IN MEMORY

of

JOHN HENRY POYNTING

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COLLECTED
SCIENTIFIC PAPERS

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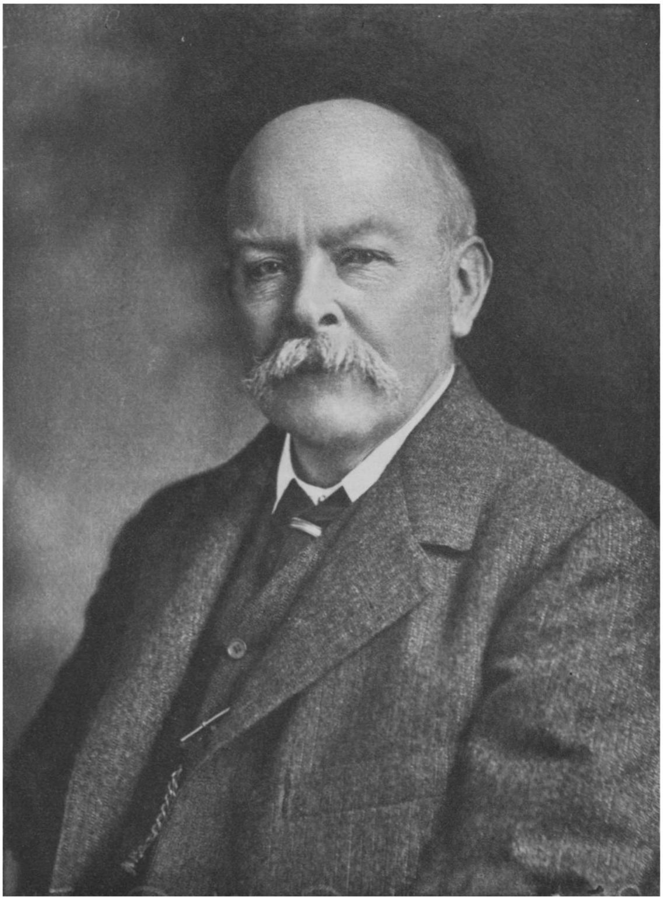

J H Poynting

COLLECTED SCIENTIFIC PAPERS

BY

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Formerly Fellow of Trinity College, Cambridge

CAMBRIDGE
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PREFACE

IN the summer of 1914 Sir Oliver Lodge summoned a meeting of the colleagues and friends of the late Professor John Henry Poynting to consider how best a suitable memorial could be established to perpetuate his memory. The following committee was appointed to carry out the purpose of the meeting: Sir Oliver Lodge (chairman), Guy Barlow, Neville Chamberlain, P. F. Frankland, Sir R. T. Glazebrook, R. S. Heath, George Hookham, Sir Joseph Larmor, Sir Napier Shaw, Sir J. J. Thomson, Sir Richard Threlfall, and T. Sydney Walker, with G. H. Morley as Secretary and G. A. Shakespear as Treasurer.

A fund was opened and subscriptions were invited, and the Committee decided that there could be no better memorial than the publication in a collected form of the Scientific Papers of John Henry Poynting, and the distribution of copies to certain scientific institutions throughout the world.

The task of editing and proof correction was gladly undertaken by his junior colleagues G. A. Shakespear and Guy Barlow who have throughout had the benefit of the advice of Sir Oliver Lodge.

Biographical and critical notices by Sir Oliver Lodge, Sir Joseph Larmor, Sir J. J. Thomson and G. A. Shakespear have been inserted as an introduction to the volume.

The papers have been arranged in groups, with the object of bringing together those dealing with kindred subjects. In each group the papers are in chronological order.

In such collections there is inevitably a certain amount of repetition or overlapping, since a subject is often dealt with from more than one point of view—the strictly scientific and the popular—but since this variety of treatment is helpful, in cases of doubt the decision has generally been given in favour of inclusion.

The popular discourses and general articles have for the most part been relegated to the last section of the volume.

His books, and other publications which are easily accessible, are not included; but a complete list of his works in chronological order is given at the end of the volume.

The papers have been reproduced as originally published except for small verbal corrections here and there. In certain cases mistakes have been corrected after reference to the original manuscripts which were kindly put at our disposal by Mrs. Poynting; but, in all cases, corrections of any importance have been indicated in footnotes, and editorial comments have been included in square brackets and marked *Ed.*

We wish to acknowledge most warmly the unstinted generosity with which permission to republish the several articles has been given both by joint authors and by the original publishers, including the representatives of the following:

The Birmingham Natural History and Philosophical Society.
The British Association.
Le Bureau International des Poids et Mesures.
The Electrician.
The Encyclopaedia of Religion and Ethics (Messrs T. and T. Clark).
The Hibbert Journal.
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The Philosophical Magazine.
The Royal Astronomical Society.
The Royal Society.
The Royal Statistical Society.
The Royal Institution.
La Société de Physique.

The Committee was fortunate in securing the services of the Cambridge University Press as publishers, and the editors take this opportunity of thanking them for their unfailing courtesy and for invaluable help in proof-correcting.

G. A. SHAKESPEAR.
GUY BARLOW.

THE UNIVERSITY, BIRMINGHAM.
1920.

A PERSONAL NOTE

JOHN HENRY POYNTING was a man admired of all who knew him, beloved of all who knew him well.

Of somewhat less than middle height and sturdy thickset build, his general appearance was suggestive rather of rural than academic interests, but even a casual observer would probably have been struck with the sense of power indicated by his fine head. His face, which was of a meditative cast when in repose, lighted with a genial and friendly warmth when he was conversing with friends, and his grey-blue eyes were expressive of the kindly gentleness of his nature.

In habit he was methodical, and indeed the condition of his health was for many years such that he could not have accomplished so much had he not economised effort with method. (He told me once that he had never been able to do more than six hours' useful work a day.) He had, moreover, great power of concentration.

He was a remarkably clear thinker and had that characteristic insight into fundamental ideas which intuitively distinguishes between hypothesis and fact; and it was probably for this reason that he viewed with suspicion some of the more recent developments of mathematical physics. He withheld his judgment when the experimental foundations were either wanting or else inadequate to bear the superstructure erected upon them.

Himself a man of wide interests and sympathies, and with a finely balanced sense of proportion, he was keenly alive to the danger of that too-exclusive specialisation which so frequently makes a man incapable of conversation except in his own particular line of work. He felt the need of guarding against atrophy of the spiritual side of his character in his outlook on life, and sought in the reading of fiction, and even more of poetry, the complement to the intellectual stimulus of scientific work. Indeed of poetry he read much, though he spoke of it but rarely even with his intimate friends; but those to whom he opened his mind on such matters knew his deep admiration for many of the English poets. Shelley, Keats and still more Wordsworth, appealed to him strongly, for he himself was imbued with that love of Nature which inspired them.

He was probably never happier than when living in the beautiful Alvechurch district of Worcestershire, where he found an unfailing source of pleasure in the rolling landscape which stretched away into the distance around his upland home. He would walk in the fields with some friend, here pointing out the haunt of a rare wildflower, there showing, with all the interest of a schoolboy, the nest of an uncommon bird, or discussing the effect of atmosphere on the landscape. The conversation would wander from the botanical affinities of the tway-blade to the mechanism whereby the grasshopper performs his prodigious leaps, or from the theory of the grinding action of a cider-press to the causes and prevention of crime in large cities. To such occasions his equable temper and ready sense of humour lent a rare charm.

Among his outdoor recreations cycling through the country lanes held a high place. Indoors, in his later years, he derived much pleasure from a piano-player; and in the evenings, when too tired to read, he would often amuse himself with a game of "patience."

In politics he was a liberal, and perhaps the thing for which he stood more strongly than anything else was freedom and liberty of thought; the thing of which he was most intolerant was bigotry, and indeed in an intimate aquaintance of 20 years' duration the only time I ever heard him speak of any man with bitterness was in reference to a case of religious intolerance.

The soundness of his judgment being well known, his advice was often sought; and, whether the matter were small or great, he always gave of his best, earning thereby the gratitude of many.

He had a great sympathy with humanity in general and especially with the poorer classes. As a magistrate he tempered judgment with mercy and his experience in this capacity confirmed him in the opinion that delinquents in general were as often sinned against as sinning.

The comradeship of his home-life was ideal, while to his students he was an object of admiration and of affectionate regard in a degree which perhaps they alone can appreciate.

His ability may to some extent be judged from his published work, but his personal worth and charm are truly measured by the affection with which he inspired those who knew him best.

G. A. S.

OBITUARY NOTICES

[From *Nature*, vol. XCIII, p. 138, with additions.]

On the evening of Monday, March 30, 1914, surrounded by his family John Henry Poynting passed quietly away. A memorial service was held in Birmingham on the Thursday following, and was attended by representatives of many universities and learned societies, including Sir J. J. Thomson, Sir Joseph Larmor, Dr Glazebrook, Sir William Tilden, Prof. W. M. Hicks, Dr W. N. Shaw, and of course by many colleagues and councillors of the University in which he occupied a chair, as well as by a large number of private citizens and friends. For he was a man universally beloved.

At the memorial service, the following true words concerning him were spoken by the Rev. Henry Gow, who knew him well:

"We remember that he did work to make him famous throughout the world of science which gave him a high place amongst the discoverers of truth; but we remember much more than that. We remember how he loved life, how interested he was in little things, how he delighted in children, in flowers, and in birds; what confidence and affection he inspired, how free he was from claims of self and from uneasy egotism; how much happiness he felt and gave. We remember his wise judgments, strong character, cheerful courage, his delightful humour, and a certain peaceful beauty and childlike joyousness of spirit behind all his multifarious gifts. He rejoiced to be the friend as well as the teacher of the young. He kept his heart free from all bitterness and disillusion which come so often to us in our later years. He knew and felt always how beautiful and great a thing it was to be alive."

He was born on September 9, 1852, at Monton, near Manchester, son of the unitarian minister of that place. His first education was at home, but the years 1867 to 1872 he passed at Owens College, Manchester, graduating B.Sc. at the London University, and proceeding, in 1872, to Trinity College, Cambridge, where he was bracketed third wrangler in 1876.

He was then appointed demonstrator at Owens College by Balfour Stewart, and began a life-long friendship with Sir J. J. Thomson, who was at that time a student. In due time Poynting became a fellow of Trinity, and in 1880 was appointed to the professorship of physics at Birmingham, which he held to the day of his death.

The four first professors of the Mason College, which was opened by Huxley in 1880 (who delivered, on this occasion, a notable address, reprinted as the first of his collected essays), were Sir Wm. Tilden, Prof. M. J. M. Hill, Dr T. W. Bridge, who died a few years ago, and Poynting. In this same year Poynting married Miss M. A. Cropper, daughter of the Rev. J. Cropper, of Stand, near Manchester. In 1887 he received the Sc.D. of Cambridge, and in 1888 the fellowship of the Royal Society. In 1893 the Adams prize was awarded to him, and in 1899 he presided over Section A of the British Association at Dover. This meeting was memorable for the clear discovery of the separate existence of electrons, which was announced to Section A by Sir J. J. Thomson on an occasion when many members of the French Association, meeting simultaneously at Boulogne, had come over for friendly fraternisation.

In 1905 Poynting became president of the Physical Society, and was awarded a Royal medal by the Royal Society "for his researches in physical science, especially in connection with the constant of gravitation and the theories of electro-dynamics and radiation." In this brief summary an immense amount of work is referred to. The work for which he is locally best known was his determination of the Newtonian constant of gravitation by the very accurate use of an ordinary balance with an adjustable mass under one or other of the arms—a determination which is popularly called "weighing the earth." His account of it appears in the *Phil. Trans.* for 1891. It is a classical memoir of its kind, and very instructive to the physical student, but the papers on electro-dynamics eclipse it in value. These were "communicated" to the Royal Society in 1884 and 1885 respectively, their titles being "On the Transfer of Energy in the Electromagnetic Field," and "On the Connection between Electric Current and the Electric and Magnetic Inductions in the Surrounding Field."

The memoir on the transfer of energy aroused universal attention. The paths by which energy travels from an electromotive source to various parts of a circuit were displayed, and their intricacies unravelled, for the first time; *identity* of energy might legitimately be urged as a supplement to *conservation* (see a paper by the present writer in *Phil. Mag.*, June, 1885); and it is to these papers that we owe that fundamental generalisation, connecting mechanical motion with electric and magnetic forces, which is known all over the world as "Poynting's Theorem."

The following letter from Sir Joseph Larmor to the writer expresses a mathematician's view of the importance of this subject:

St John's College, Cambridge, 10th May, 1915.

"Nobody before Poynting seems to have thought of tracing the flux of energy in a medium *elastically transmitting* it, and where the whole process is therefore exposed to view. The line of flow is a *ray* in optics: thus it includes a dynamical aspect of that conception *added on* to and of course consistent with the Huygenian or rather Young-Fresnelian one. The electric and optical ray is implicitly in Maxwell's equations, and is only a corollary to them. But in any other kind of elastic transmission, e.g. waves in an elastic-solid medium, a corresponding theory can be worked out.

I take it this *idea* is Poynting's main contribution, and it clarified many things, especially electrical."

A great expansion of this note is contained in a remarkable paper *On the Dynamics of Radiation* which Sir Joseph Larmor communicated to the International Congress of Mathematicians meeting at Cambridge in August, 1912. This paper is so intimately associated with Poynting's work, and so pleased him when he saw it, that I have asked and obtained permission to include extracts from it in this volume; they will be found at the end of the Section dealing with the *Pressure of Light.*

"The essential characteristic of an electrodynamic system is the existence of the correlated fields, electric and magnetic, which occupy the space surrounding the central body, and which are an essential part of the system; to the presence of this pervading aethereal field, intrinsic to the system, all other systems situated in that space have to adapt themselves. When a material electric system is disturbed, its electrodynamic field becomes modified, by a process which consists in propagation of change outward, after the manner of radiation, from the disturbance of electrons that is occurring in the core. When however we are dealing with electric changes which are, in duration, slow compared with the time that radiation would require to travel across a distance of the order of the greatest diameter of the system in fact in all electric manifestations except those bearing directly on optical or radiant phenomena—complexities arising from the finite rate of propagation of the fields of force across space are not sensibly involved: the adjustment of the field surrounding the interacting systems can be taken as virtually instantaneous, so that the operative fields of force, though in essence propagated, are sensibly statical fields. The practical problems of electrodynamics are of this nature—how does the modified field of force, transmitted through the aether from a disturbed electric system, and thus established in the space around and alongside the neighbouring conductors which alone are amenable to our observation, penetrate into these conductors and thereby set up electric disturbance in them also? and how does the field emitted in turn by these new disturbances interact with the original exciting field and with its core? For example, if we are dealing with a circuit of good conducting quality and finite cross section, situated in an alternating field of fairly rapid frequency, we know that the penetration of the arriving field into the conductor is counteracted by the mobility of its electrons, whose motion, by obeying the force, in so far annuls it by Newtonian kinetic reaction; so that instead of being propagated, the field soaks in by diffusion, and it does not get very deep even when adjustment is delayed by the friction of the vast numbers of ions which it starts into motion, and which have to push their way through the crowd of material molecules; and the phenomena of surface currents thus arise. If (by a figure of speech) we abolish the aether in which both the generating circuit and the secondary circuit which it excites are immersed, in which they in fact subsist, the changing phases of the generator could not thus establish, from instant to instant, by almost instantaneous radiant transmission, their changing fields of force in the ambient region extending across to the secondary circuit, and the ions in and along that circuit would remain undisturbed, having no stimulus to respond to. The aethereal phenomenon, viz., the radiant propagation of the fields of force. and the material phenomenon, viz., the response of the ions of material bodies to those fields, involving the establishment of currents with new fields of their own, are the two interacting factors. The excitation of an alternating current in a wire, and the mode of distribution of the current across its section, depend on the continued establishment in the region around the wire, by processes of the nature of radiation, of the changing electromagnetic field that seizes hold on the ions and so excites the current; and the question how deep this influence can soak into the wire is the object of investigation. The aspect of the subject which is thus illustrated, finds in the surrounding region, in the aether, the seat of all electrodynamic action, and in the motions of electrons its exciting cause. The energies required to propel the ions, and so establish an induced current, are radiant energies which penetrate into the conductor from its sides, being transmitted there elastically through the aether; and these energies are thereby ultimately in part degraded into the heat arising from fortuitous ionic motions, and in part transformed to available energy of mechanical forces between the conductors. The idea—introduced by Faraday, developed into precision by Maxwell, expounded and illustrated in various ways by Heaviside, Poynting, Hertz of radiant fields of force, in which all the material electric circuits are immersed, and by which all currents and electric distributions are dominated, is the root of the modern exact analysis of all electric activity."

Poynting's work on radiation appeared partly in the *Phil. Trans.* for 1904 and partly in the *Phil. Mag.* for 1905. In these memoirs the tangential pressure of radiation is analysed and demonstrated; and it is shown, both theoretically and experimentally, that a beam of light behaves essentially as a stream of momentum, and gives all the mechanical results which may thus be expected, though of a magnitude exceedingly minute. Nevertheless, he goes on to show that these radiation-pressures, however small, are of much consequence in astronomy, and have many interesting and some conspicuous results. A noteworthy part of the radiation memoirs, however, is independent of considerations of pressure or momentum, and gives a means of determining the absolute temperatures of sun and planets, and of other masses in space, in a singularly clear and conclusive manner.

A complete list of his publications is given below, but special mention must be made here of the important series of text-books on physics, written in conjunction with his friend, Sir J. J. Thomson.

He took great interest also in the philosophical aspects of physical science, and his help is acknowledged by Prof. James Ward in connection with the publication of that notable series of Gifford Lectures entitled *Naturalism and Agnosticism.* Poynting was strongly inclined, almost unduly, to limit the province of science to *description,* and to regard a law of nature as nothing but a formulation of observed correspondences. He wished to abolish the idea of *cause* in physics. In some of this he may have gone too far; but his rebellion against an excessive anthropomorphism which had begun to cling around the notion of natural laws, as if they were really legal enactments to be obeyed or disobeyed by inert matter almost as if it possessed will-power and could exercise choice, some substances being praised as good radiators while others are stigmatised as bad—most gases being admittedly unable to reach a standard of perfection held out to them as Boyle's law, though a few of excessive merit might surpass it,—Poynting's revolt against this kind of attitude to laws of nature, though doubtless more than half humorous, was in itself wholesome. Some of his philosophic views may be read, as a Presidential Address to Section A of the British Association for 1899 (infra p. 599); but I think it useful and legitimate to extract a few sentences from that address and quote them here, as an illustration of his mode of approaching the misty region where physics and metaphysics intertwine:

"To take an old but never-worn-out metaphor, the physicist is examining the garment of Nature, learning of how many, or rather of how few different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come....So, as we watch the weaving of the garment of Nature, we resolve it in imagination into threads of ether spangled over with beads of matter. We look still closer, and the beads of matter vanish; they are mere knots and loops in the threads of ether."

And then, a few pages further on, when dealing with the interaction of Matter and Mind:

"Do we, or do we not, as a matter of fact, make any attempt to apply the physical method to describe and explain those motions of matter which on the psychical view we term voluntary?

Any commonplace example, and the more commonplace the more is it to the point, will at once tell us our practice, whatever may be our theory. For instance, a steamer is going across the Channel. We can give a fairly good physical account of the motion of the steamer. We can describe how the energy stored in the coal passes out through the boiler into the machinery, and how it is ultimately absorbed by the sea. And the machinery once started, we can give an account of the actions and reactions between its various parts and the water, and if only outsiders will not interfere, we can predict with some approach to correctness how the vessel will run. All these processes can be likened to processes already studied perhaps on another scale—in our laboratories, and from the similarities prediction is possible. But now think of a passenger on board who has received an invitation to take the journey. It is simply a matter of fact that we make no attempt at a complete physical account and explanation of those actions which he takes to accomplish his purpose. We trace no lines of induction in the ether connecting him with his friends across the Channel, we seek no law of force under which he moves. In practice the strictest physicist abandons the physical view, and replaces it by the psychical. He admits the study of purpose as well as the study of motion."

In other words he recognises Mind and Purpose as dominant over and in a different category from Matter and Mechanism.

In psychical phenomena Poynting was, I judge, an agnostic, but on the question of a materialistic or naturalistic explanation of mental phenomena he expresses himself thus, in the Dover Section A address above referred to:

"It appears to me that the assumption that our methods do apply, and that purely physical explanation will suffice to predict all motions and changes, voluntary and involuntary, is at present simply a gigantic extra-polation, which we should unhesitatingly reject if it were merely a case of ordinary physical investigation. The physicist when thus extending his range is ceasing to be a physicist, ceasing to be content with his descriptive methods in his intense desire to show that he is a physicist throughout."

But I must not delay further on his scientific work; the man himself was even more than his work. When the Mason College became the University of Birmingham Poynting was elected Dean of the Faculty of Science; in that capacity his quiet wisdom and efficiency were very manifest, and keen was the regret of all his colleagues when, some twelve years later, failing health necessitated his yielding this office to another. His judgment was as sound as his knowledge, and his conspicuous fairness endeared him to colleagues and the members of his staff. By the latter it is not too much to say that he was regarded with affectionate veneration; one of them writes to me as follows:

"As to his character it is impossible to give the right impression to those who did not know him well. I consider him a man of very extraordinary ability, which might have carried him much farther if it had been associated with more self-assertion. But it was largely this modesty and self-suppression which created a very unusual degree of affection in those who had the privilege of knowing him intimately. I always associate him in my mind with Faraday and Stokes."

As a lecturer and teacher he was admirable, and the respect in which he was held by his peers was noteworthy. I am glad to remember that so recently as the 1913 meeting of the British Association, some of the greatest physicists in the world, who were staying with me—Prof. H. A. Lorentz, Lord Rayleigh, and Sir Joseph Larmor—went to his house one evening, and met there in his study Sir J. J. Thomson and Dr Glazebrook, who were staying with him; thus constituting an appropriate and representative gathering, and giving him a pleasure which he remembered to the end of his life.

There is much more that might be said; but let his position in the world of science be what it may, we in the University of his mature life knew him well, and know him best as an admirable colleague, a staunch friend, and a good man.

O. J. L.

[From the *Proceedings of the Royal Society*, A, vol. XCII, 1914.]

John Henry Poynting, the youngest son of the Rev. T. E. Poynting, Unitarian Minister at Monton, near Manchester, was born there on September 9, 1852. He received his earlier education at the school kept by his father and then went, in 1867, to the Owens College, which his elder brother, C. T. Poynting, who was for many years Unitarian Minister at Fallowfield, near Manchester, had just left. Poynting must have received a good grounding in Mathematics at his father's school, as he gained a Dalton Entrance Exhibition in Mathematics before entering the College. Owens College in those days was in a modest building, once the residence of Richard Cobden, in Quay Street, Deansgate. Neither the amenities of the locality nor the accommodation in the building were anything to boast about, but few educational institutions before or since, whatever their equipment or surroundings, have had a more efficient staff than Owens College in the old Quay Street days. As an old Quay Streeter, the writer can speak from personal experience. The cramped space was not an unmixed disadvantage. We were so closely packed that it was very easy for us to get to know each other, Arts students and Science students jostled against each other continually; a crowd of Mathematicians would be waiting outside the doors of a lecture room for it to discharge a Latin or Greek class, and thus one of the chief difficulties of non-residential colleges, the lack of social intercourse between the students, was almost absent.

The professors at Owens in Poynting's time were: Barker for Mathematics, of whom Poynting always spoke in terms of the highest appreciation, a feeling shared by all his pupils, for no abler or more conscientious teacher of Mathematics than Thomas Barker ever lived; Jack, another great teacher, was Professor of Natural Philosophy; Roscoe of Chemistry, and Williamson of Natural History; on the literary side, Greenwood, the Principal, was Professor of Classics, Ward of English History and Literature, Jevons of Logic, and that very lovable man, Theodores, lecturer on Modern Languages. At that time Owens had not the power of granting degrees, and most of the students prepared for the examinations of the University of London. In those days these covered a very wide range of subjects, and Poynting, who took the London degree, must have attended the lectures of all these professors. He was second at the London Matriculation in 1869, obtained Second Class Honours in both Physics and Mathematics in the First B.Sc. examination in 1871, and took the B.Sc. degree in 1872. In the spring of 1872 he obtained an entrance scholarship at Trinity College, Cambridge, and came into residence at Cambridge in October. At Cambridge he pursued the normal course of one destined for high honours in the Mathematical Tripos. He read with

Routh, he obtained his Major Scholarship in due course, like many of the reading men of his time at Trinity he joined the Second Trinity Boat Club and rowed in the first boat in 1875; the fortunes of that once famous club were, however, then declining and it came to an end in 1876. He took his degree in the Mathematical Tripos of 1876 as Third Wrangler, bracketed with Mr Trimmer, of Trinity College, a very brilliant man who suffered from persistent ill-health and died within a few months of taking his degree. As Dr Glazebrook and Dr Shaw both graduated in the same Tripos and Lord Rayleigh was the additional Examiner, Physics was well represented on this occasion.

After taking his degree Poynting came back for a short time to the Owens College, which was now in the buildings it at present occupies, and demonstrated in the Physical Laboratory under Prof. Balfour Stewart, who had succeeded Jack as Professor of Natural Philosophy shortly before Poynting's departure for Cambridge.

On his election to a Fellowship at Trinity College in 1878, Poynting returned to Cambridge and began, in the Cavendish Laboratory under Clerk Maxwell, those experiments on the mean density of the earth which were destined to occupy so much of his time for the next 10 years.

He remained at Cambridge until 1880, when he was elected to the Chair of Physics in Mason College, Birmingham (now the University of Birmingham), which had just been founded; this post he held until his death. The year that he went to Birmingham, he married the daughter of the late Rev. J. Cropper, of Stand, near Manchester.

He threw himself whole-heartedly into the arduous duties connected with the starting of a new University College, the preparation of his lectures and the equipment of the physical laboratory, and, as was his wont, without any bustle or hurry he soon had things working efficiently. And so in the efficient discharge of his duties as a Professor, in successful original research, in the fulfilment of municipal duties, the time passed placidly on, the only cloud on an almost idyllic domestic life being his somewhat indifferent health, the first threatenings of the disease from which he ultimately died. To see if a country life would suit his health better than a town one, the Poyntings moved from Edgbaston to Fox Hill, Alvechurch, a house about 12 miles out of Birmingham. There was a small farm attached to the house and Poynting entered into farming most heartily, though I am afraid he did not derive much pecuniary profit from it. But even farming when the agricultural depression was most acute could not impair his good temper or ruffle his equanimity. If the farm did not yield money, it gave new interests and experiences, and if something was always going wrong, at any rate it drove away monotony. The quietness and simplicity of the life were thoroughly to the taste of Mrs. Poynting and himself. Life in the country too gave free scope to his taste for Natural History, in which he always took great interest; he was a keen and excellent observer, and a favourite contention of his was that physicists were somewhat too much inclined to confine their observations to experiments made in the laboratory and did not sufficiently avail themselves of the opportunities of studying the physical phenomena going on in the sky, the sea, and the earth. The taste for Natural History was a family one; his brother, the late Mr. F. Poynting, was an excellent ornithologist, devoting himself especially to the study of the eggs of British birds, of which he made most careful and accurate water-colour drawings—some of these have been reproduced in his book *The Eggs of British Birds*.

The Poyntings stayed at Foxhill until 1901, when, his health much improved, they returned to Edgbaston. His life at this time was a busy one, for in addition to the work demanded from him as the head of a large and successful School of Physics, he acted as the Dean of the Faculty of Science, was a Justice of the Peace, and for some time Chairman of the Birmingham Horticultural Society. He had also to plan and superintend the erection of a new physical laboratory when his department was transferred from its old quarters to the new buildings of the University of Birmingham. He went with the British Association to Canada in 1909, when it met at Winnipeg, and gave one of the evening lectures; his subject was the Pressure of Light, on which he had been experimenting for several years. He went the trip to Vancouver and back and seemed thoroughly to enjoy the visit. The pressure of light was also the subject of a lecture which he gave in French at Paris before the French Physical Society at Easter, 1911.

In the spring of 1912 a severe attack of influenza was followed by a recrudescence of diabetes, a disease from which he had suffered for some time, and he was ordered to take a long rest; he was, in consequence, away from Birmingham for two terms. On his return to Birmingham he seemed much better, he took an active part in the meeting of the British Association held there in September, 1913, and he and Mrs. Poynting entertained a large party of physicists at their house in Ampton Road, and it then seemed as if he might hope to enjoy many years of useful work. Another attack of influenza in the spring of 1914 brought on a very severe attack of diabetes, and he died on March 30, 1914.

It is difficult to attempt to say what Poynting was to his friends without using terms which must appear exaggerated to those who did not know him. He had a genius for friendship, and a sympathy so delicate and acute that whether you were well or ill, in high spirits or low, his presence was a comfort and a delight. During a friendship which lasted for more than thirty years, I never saw him angry or impatient and never heard him say a bitter or unkind thing about man, woman or child.

He took pleasure in many things, in music, in literature, for he was a lover of books and a collector in a modest way, in novels of all kinds, good and bad. He was fond of the country, and especially of North Wales, where he spent most of his vacations, but happiest of all when at home with his family. Throughout his life he took considerable interest in Philosophy, and a discussion of the philosophical basis of Physics formed part of his Presidential Address to Section A at the Dover Meeting of the British Association. Views similar to those he there expressed are now held by many; he had formed his years before, when but few in this country agreed with them. The excellence of his work received many recognitions, though not in my opinion so many as it deserved. He was elected a Fellow of the Royal Society in 1888, received a Royal Medal in 1905, served on the Council from 1909 to 1911 and was Vice-President in 1910–11. He received the Adams Prize from the University of Cambridge in 1893, the Hopkins Prize from the Cambridge Philosophical Society in 1903. He was President of Section A when the British Association met at Dover in 1899 and was President of the Physical Society in 1909–11.

He was in great request as an Examiner in Physics and no one excelled him at this work, his long experience of students, his judgment and common sense, the charitable view he took of the limitations of a student's knowledge, and the fact that he was never afraid of setting easy papers, made him an eminently fair and discriminating examiner. He was very successful as a teacher of students of all kinds, those who only took Physics as a subsidiary subject as well as those who made it their life's work, the latter he inspired with an enthusiasm for research, with some of his own skill in accuracy of measurement and with the desire for thoroughness in their work.

Poynting's Scientific Work.

This may be divided into four groups: (*a*) studies on gravitational attraction, (*b*) on the change of state, (*c*) on the transfer of energy in the electromagnetic field, and (*d*) on the pressure of light.

Gravitational Attraction.

His experiments on the mean density of the earth were commenced in Cambridge in 1878 but it took twelve years' steady work before he obtained a result with which he was satisfied. The method used was to measure the attraction between two known masses A and B by suspending A from one of the arms of a balance of the ordinary type and finding the increase in weight produced when B was brought underneath it. The balance used in the later experiments was one built specially for the experiment by Oertling and had a beam 123 cm. long. With a balance of this size the difficulties arising from air currents proved very formidable. Poynting fully recognised the advantage of Boys' short torsion balance method in this respect and said that if he were designing the apparatus again, instead of using an exceptionally large balance for the sake of being able to suspend large masses, he should go to the other extreme and make the apparatus as small as possible. At the same time, as he points out, the magnitude of the effects produced by the air currents made their detection easy, whereas they might have been overlooked and not allowed for had they been smaller. The final results (*Phil. Trans.*, A, vol. CLXXXII, p. 565, 1891) he obtained for Δ, the mean density of the earth, and G, the gravitational constant, were

$$\Delta = 5{\cdot}4934,$$
$$G - 6{\cdot}6984 \quad 10^{-8}.$$

Poynting's long investigation incidentally added considerably to our knowledge of the technique of accurate weighings.

With the co-operation of Gray he made a series of most interesting experiments (*Phil. Trans.*, A, vol. CXCII, p. 245, 1899) to see if the attraction between two quartz crystals was the same when the axes of the crystals were parallel as when they were crossed. The method he used was a very ingenious application of the principle of forced oscillations, which was so effective that, though one sphere was only about 1 cm. in diameter and the other about 6 cm., the experiments showed that the attractions in the two positions could not differ by as much as one part in 10,000. Later he made with Phillips a series of experiments to see if weight depended on temperature, using as in his first experiments a balance of the ordinary type; the result of these was (*Proc. Roy. Soc.*, A, vol. LXXVI, p. 445, 1905) that between 15 C. and 100° C. the change is not greater than 1 in 10^9 and between 16·6 C. and − 186° C. it is not so great as 1 in 10^{10} per 1 C.

Change of State.

The problem of the change of state was one in which he took especial interest, and it was the subject of one of his earliest papers (*Phil. Mag.* (5), vol. XI, p. 32, 1887). His way of picturing this change was to suppose that from the surface of a liquid or solid particles were continually breaking free, so that through each unit of area of the surface there was a constant escape of molecules. This loss was balanced by the passage from the vapour above the solid of some of the gaseous particles which struck against its surface, so that when there was equilibrium the flow out from the liquid or solid was balanced by the flow inward from the gas. The proportion of gaseous molecules which after striking the surface passed across to the solid or liquid state he assumed to be the same for a solid as for a liquid and to be independent of the temperature, so that it could be measured by the vapour pressure. Thus at the same temperature the flow across water would be proportional to the vapour pressure of water, that across ice to the vapour pressure of ice, thus ice could only be in equilibrium with water when the vapour pressure over ice is equal to that over water.

Poynting supposed that the mobility of the molecules in liquids and solids is increased by pressure—the pressure as it were squeezing the molecules out: the amount of the increase depending on the density of the substance, diminishing as the density increases. Thus, if pressure increases the escape of the molecules from a liquid, a liquid under pressure will evaporate more freely, and so for it to be in equilibrium with its vapour the vapour pressure must be higher than that over the normal liquid; from the equilibrium between water and its vapour in a capillary tube, he found that if δp is the increase in the vapour pressure produced by applying a pressure P to the liquid, $\delta p = P\sigma/\rho$, where σ is the density of the vapour and ρ that of the liquid.

Poynting applied this conception of mobility to the case of solutions, taking the view that the molecules of the salt formed aggregates with some of the water molecules and thus diminished their mobility thereby diminishing the number of water molecules which passed from the liquid state through each unit of area of surface per second. The mobility of pure water is thus greater than that of the solution, so that if the two are separated by a semi-permeable membrane more molecules will pass from the water to the solution than from the solution to the water, and the water will flow into the solution. To prevent this flow the mobility of the molecules of water in the solution must be increased by the application of a pressure that will make the mobility of the solution equal to that of pure water; this pressure is the osmotic pressure. Since under this pressure the mobility of the solution is equal to that of pure water the vapour pressure in equilibrium with the pressed solution will be the vapour pressure over pure water, so that another definition of osmotic pressure would be the pressure required to raise the vapour pressure over the solution to that over pure water. On the assumption that the presence of one molecule of salt to n of water would diminish the mobility of the water in the proportion of $(n-1)/n$, which would be the case if a molecule of salt imprisoned one and only one molecule of water, Poynting showed that the osmotic pressure on his theory would be the pressure exerted by the salt molecules if they were in the gaseous state and occupying the volume of the solution. Though this theory does not connect the electrical properties of solutions with the properties associated with osmotic pressure so readily as the dissociation theory, it is so simple and fundamental that it helps to give vividness and definiteness to our picture of the processes operative in solutions.

Transfer of Energy.

The researches by which Poynting is most widely known are those published in the papers "On the Transfer of Energy in the Electromagnetic Field" (*Phil. Trans.*, A, 1884), and "On Electric Currents and the Electric and Magnetic Induction in the Surrounding Field" (*Phil. Trans.*, A, 1888). He says in the first paper, "The aim of this paper is to prove that there is a general law for the transfer of energy, according to which it moves at any point perpendicularly to the plane containing the lines of electric and magnetic force, and that the amount crossing unit of area per second of this plane is equal to the product of the two forces multiplied by the sine of the angle between them divided by 4π, while the direction of the flow of energy is that in which a right-handed screw would move if turned round from the positive direction of the electromotive to the positive direction of the magnetic intensity." He shows from the equation of the electromagnetic field that the rate of increase in the energy inside a closed surface is equal to

$$\frac{1}{4\pi}\iint[l\,(R\beta - Q\gamma) + m\,(P\gamma - R\alpha) + n\,(Q\alpha - P\beta)]\,dS,$$

where dS is an element of the closed surface, l, m, n the direction cosines of the normal to the surface, P, Q, R the components of the electromotive intensity, and α, β. γ those of the magnetic force. This expression may be regarded as showing that the energy flows across the surface, the components of flux being

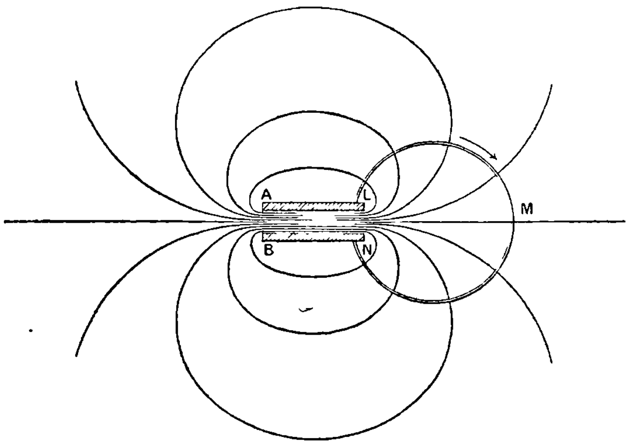
$$\frac{1}{4\pi}(R\beta - Q\gamma), \quad \frac{1}{4\pi}(P\gamma - R\alpha), \quad \frac{1}{4\pi}(Q\alpha - P\beta);$$

the vector which has those components is now universally known as Poynting's vector; it is at right angles to both the electric and magnetic forces and is proportional to the product of these forces and the sine of the angle between them. Thus when we can draw equipotential surfaces for both the electric and magnetic forces the energy flows along the lines of intersection of the two sets of surfaces. Poynting illustrates this theorem by applying it to the following cases: a constant current flowing along a straight wire, a condenser discharged by short-circuiting the plates by a wire of great resistance, a voltaic battery, a thermo-electric circuit.

The magnitude of the change in the point of view consequent on the principles brought forward in this paper is perhaps shown most clearly in the case of the discharge of the condenser and the transference of the energy which before the discharge was distributed between its plates into heat in the discharging circuit. Before the publication of this paper the general opinion was that the energy was transferred along the wire much in the same way as hydraulic power is carried through a pipe. On Poynting's view the energy flows out from the space between the plates and then converges sideways into the wire, where it is converted into heat, the paths of the energy being those represented in the figure.

As shown in this figure the paths of energy near the wire are at right angles to it. This is not so unless the wire is such a bad conductor that the lines of electric force in its neighbourhood run parallel to it; if for example the current through the wire were an alternating one with very high frequency the electric force near the wire would be at right angles to it. In this case the energy would flow parallel to the wire but outside it.

In the second paper Poynting, taking the view that the electromagnetic field consists of distributions of lines of electric and magnetic force, discusses the question of the transfer of energy from the point of view of the movement of these lines. He applies the same considerations to the question of the residual charge in Leyden jars in his fascinating and instructive paper on "Discharge of Electricity in an Imperfect Insulator" (*Phil. Mag.*, vol. v, 1886, p. 419). Poynting's vector occurs as a quantity of fundamental importance in many theories of electromagnetic action in which the subject is approached



from a point of view somewhat different from the one he adopted. It appears, for example, as a measure of the momentum per unit volume when the electromagnetic field is regarded as a mechanical system and the properties of the field as the result of the laws of motion of such a system. It appears, too, when we regard magnetic force as the result of the motion of tubes of electric force, the direction of motion of these tubes being parallel to Poynting's vector.

Pressure of Light.

For some years before his death Poynting devoted much attention to the question of radiation and the pressure of light. On the theory of this subject he published (*Phil. Trans.*, A, vol. CCII) a very valuable paper, in the first part of which he discusses the application of the fourth-power law of radiation to determine the temperature of planets (in this he found afterwards he had been anticipated by Christiansen). Among other interesting results he arrived at the conclusion that the temperature of Mars must be so low that life, as we know it, would be impossible on its surface, this result was criticised by Lowell, but Poynting maintained his ground in a paper published in the *Philosophical Magazine*, December, 1907. The second part of the paper in the *Philosophical Transactions* contains investigations of the repulsive force between two hot spheres which arises from the radiation from the one tending to repel the other. He showed that if the bodies are in radiation equilibrium with the Sun at the distance of the Earth from it, the repulsive effect will be greater than the gravitational attraction between them if their radii are less than 19·6 cm., if their density were that of water; if they were made of lead the corresponding radius would be 1·78 cm. Thus if Saturn's rings consisted of very small particles it is possible that the effect of radiation might make them repel instead of attract each other. He considers at the end of the paper the effect produced by radiation on the orbits of small bodies round the Sun and shows that this would ultimately cause them to fall into that body. To quote his own words: "The Sun cannot tolerate dust. With the pressure of his light he drives the finest particles altogether away from his system. With his heat he warms the larger particles. They give out this heat again and with it some of that energy which enables them to withstand his attraction. Slowly he draws them to himself and at last they unite with him and end their separate existence." (*Pressure of Light*, "Romance of Science" Series.)

He made important contributions to the experimental side of the subject, thus with Dr. Barlow he established the existence of the tangential force produced when light is reflected from a surface at which there is some absorption, and also the existence of a torque when light passes through a prism. They also succeeded in demonstrating the existence of the recoil from light of a surface giving out radiation: an account of these experiments was given in the Bakerian Lecture for 1910 (*Proc. Roy. Soc.*, A, vol. LXXXIII, p. 534, 1910). These investigations involved the detection of exceeding minute forces and gave ample scope for Poynting's skill in devising methods and apparatus. He had exceptionally good mechanical instincts and an excellent knowledge of the capabilities of instruments; the result was that the apparatus he designed was always simple and effective.

In addition to papers published in scientific journals and the Transactions of Societies he wrote *The Mean Density of the Earth*: the Adams Prize Essay for 1893, *The Pressure of Light* ("Romance of Science" Series) and *The Earth* (Cambridge University Press). Of the *Text Book of Physics* written in conjunction with J. J. Thomson he wrote the whole of the volumes on Sound and Heat and of the first volume of Electricity and Magnetism and the chapters on Gravitation in the Properties of Matter. His writings exhibit to the full the clearness, simplicity and thoroughness which was characteristic of all his work.

J. J. T.

[From the *Philosophical Magazine* for May, 1914, with additions.]

Although Prof. Poynting, whose loss will be universally deplored, graduated with high distinction in mathematics at Cambridge, coming out as third Wrangler in the Tripos of 1876, his interest seems always to have lain in the direct elucidation of physical laws and principles rather than in the evolution and exposition of their consequences by analysis. When he came to Cambridge, in 1872, he was already largely trained in the niceties of refined experimentation; and after graduation he embraced an early chance to resume experimental work at Manchester. The founding of the Mason University College gave him the opportunity of organising a laboratory of his own. Much work about this time was concerned with instrumental improvements, such as the design of polarimeters and other optical apparatus; and to the same period belong studies in chemical physics, such as the elucidation of osmotic pressure, theories to which in later years he returned with conviction, and which, though perhaps not yet fully appreciated, should not be lost sight of, in view of his proved insight into fundamental problems in other domains.

An example of the latter class is the memoir on the transfer of energy in the electromagnetic field, *Phil. Trans.*, 1884, culminating in the famous result that will go down to posterity as Poynting's Theorem, which not only specifies the path of transfer of electric energy from one material system to another through the aether, but also as a very special case gives for the first time (strange to say) the dynamical specification of a ray of light.

At about the same time O. Heaviside, and a little later Hertz, were engaged upon this aspect of electric transmission as an elastic effect propagated from body to body across the aether, in place of the older aspect of electric charges in movement, each carrying its field of disturbance along with it,—which latter, rejuvenated ten years later by exact conceptions of the agency of electrons, and duly modified for change of acceleration, now includes the whole field of view. But times not being yet ripe for that, he pursued his subject in 1885 in another *Phil. Trans.* Memoir "On the connexion between electric current and the electric and magnetic inductions in the surrounding field," which tracks out the relations of a current circuit by the graphical device of the motion of what are now known as Faraday tubes of force, a type of visualisation of the phenomena which is at the present time once more widely in favour.

Afterwards he broke new ground in the experimental determination of the constant of gravitation—the problem of weighing the earth—which had been solved by Cavendish with his accustomed genius by aid of Michell's principle of balancing by torsion. To Poynting's mind an ordinary balance with lever and scale-pans gave at least equal promise of practical accuracy, and his long continued experimental investigations, which were summed up in a *Phil. Trans.* Memoir of 1892 and a Cambridge Adams Prize Essay of 1893, were the starting-point of a new interest in this subject which opened up into many methods more or less cognate to his own. By this time, however, the torsion method had renewed its power through the discovery of the production and properties of quartz fibres by C. V. Boys, whose remarkable subsequent investigation with small-scale apparatus was generously acknowledged by Poynting as the last word on the subject.

The resource thus acquired in refined dynamical experimenting was to reap further successes in a more untilled field. The ancient idea of a pressure exerted by light, so obvious on the corpuscular view of optics, had been revived by Maxwell on a foundation of an accompanying electric stress in the transmitting medium. Its mere existence, as distinct from an analysis of its propagation to the place where it is in evidence, was already indubitably involved in the Amperean forces on the electric currents induced in the surface of a reflector, once the principles of the electric theory of light are admitted. It had assumed some importance in its application, notably by FitzGerald, to the elucidation of the mysterious phenomena of comets' tails. To Poynting, this pressure exerted by a ray coming say from a distant star, far out of reach of direct dynamical effect, involved that the ray carried momentum along with it, and that the pressure effect was of the nature of a thrust exerted along the ray arising from the transfer of this momentum. After long efforts, the disturbances arising from gaseous convection as a whole, and from the radiometric molecular effect, were eliminated by Lebedew, and were compensated by balance against each other by Nicholls and Hull, about the same time, and the Maxwell value of the normal component of the pressure was fully verified. But Poynting's line of thought led him straight to a tangential component of the thrust as well as a direct component, and he noted that the former could be investigated without much trouble from the gas-effect. This idea led to many beautiful determinations in conjunction with his assistants and students. His idea of convected momentum also led him in another direction to the conclusion that the pressure on a receding surface must be less than on one at rest; it also suggested that by reaction a moving radiating body would be accelerated by its own radiation an impossible result which is corrected by recognising that its radiation is greater towards the direction of its motion than towards other directions, which leads to retardation on the whole. As this effect depends on extent of surface, it is greater in proportion for small bodies. Thus he was led to consider clouds of cosmic dust revolving orbitally round the sun, each particle heated by his rays to an equilibrium temperature of the space where it is, retarded in its motion by the reaction of its own exchanges of radiation, and thus gradually sucked into the sun. This clearance of solar spaces from dust must be a prominent feature in views of stellar cosmogony; the calculation of the time that would be required aptly illustrates his latent mathematical power, which was never unduly obtruded; and the whole Memoir is an example of that simplification of reasoning and reduction to its lowest terms which is suggestive of the depth of vision that belongs to genius. When the theory of electrons came to be developed into Maxwell's channels by Lorentz and others, it appeared at once that the stress argument, on which he had based radiation pressure, was in default, and the natural first conclusion was against the objective existence of the stress as thus specified in favour of some type too complex for simple expression. But later Poincaré and Abraham introduced the idea of grouping the refractory outstanding terms as a distribution of electric momentum, specified very simply as the vector product of the aethereal and magnetic inductions. The stress in the medium is thus taken to be the sole operating cause: it is unbalanced, and so reveals itself partly in a distribution of mechanical forces exerted on the material bodies that are present, and partly in storage and expenditure of mechanical momentum of some latent type throughout the aether. This latter agrees precisely with the momentum of radiation elucidated on very simple independent grounds by Poynting. Interest in the subject is thus stimulated, and the problems now under discussion as to whether the effect is in all cases simply momentum, and whence arises the subsidiary travelling inertia which is implied in it, become of pressing interest. The application of the stress method to calculation over a boundary surrounding a material system leads in fact to an additional result—that when the system gains energy δE of electric type, its effective mass increases by $\delta E/c^2$, where c is the velocity of radiation: but this is less important practically, and would not for example affect sensibly the clearance of cosmical dust above mentioned,—though the idea that energy possesses inertia naturally assumes prominence in general relativity theory. An experimental and theoretical incursion into the different field of the elongation of a wire due to its torsion was probably prompted primarily by these problems; though not perhaps strictly pertinent to them, it opened up new views in the theory of elastic solids under stresses so great that mere superposition of strains no longer holds good.

The formulation of an exact notion of the temperature of space, above indicated, is but one phase of his interest in the theory of natural radiation; and it seems but yesterday that he was discussing, in private correspondence, with his usual acuteness and judgment and no sign of failure of powers, the theory of Stefan's law and its other fundamental relations.

J. L.

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PART I.

THE BALANCE AND GRAVITATION.

1.

ON THE ESTIMATION OF SMALL EXCESSES OF WEIGHT BY THE BALANCE FROM THE TIME OF VIBRATION AND THE ANGULAR DEFLECTION OF THE BEAM.

[*Manchester Lit. Phil. Soc. Proc.* **18**, 1879, pp. 33–38.]

Read Dec. 10, 1878.]

While working last year on an experiment to determine the mean density of the earth by the balance, I had to measure such an exceedingly small difference of weight that I could not at that time estimate it by means of a rider, but was obliged to adopt the method described in this paper. Stated generally, it consists in treating the balance as a pendulum. Knowing the nature of the pendulum (that is, its moment of inertia) and its time of vibration, we can calculate what force acting at the end of one arm of the beam will produce a given angular deflection. It is, in fact, an application to the common balance of the method which has always been used with the torsion-balance when it has been necessary to calculate the forces measured in absolute measure. I cannot find any record of a previous application of the method; and as it might be of use in very delicate weighings or in verifying the small weights in a laboratory, I have thought it worth while to give a full account of it.

When small quantities of the second order are neglected and the oscillations are of the first order, it will easily be found that the equation of motion of the beam of the balance is

$$\left(MI^2 + \frac{2Pa^2}{g}\right)\ddot{\theta} + (2Ph + Mgk)\,\theta = ap, \quad \text{..............(1)}$$

where MI^2 = moment of inertia of beam about central knife-edge,
M = mass of beam,
a = half length of beam,
P = weight of either pan and the mass in it,
h = distance of line joining terminal knife-edges below the central knife-edge,
k = distance of centre of gravity of beam below central knife-edge,
p = small excess in one pan,
θ = angular deflection in circular measure produced by p,
g = gravity.

If $\ddot{\theta} = 0$, we have the position of equilibrium given by

$$\theta = \frac{ap}{2Ph + Mgk}. \quad \text{.............................(2)}$$

The semiperiodic time is

$$t = \pi \sqrt{\frac{MI^2 + \frac{2Pa^2}{g}}{2Ph + Mgk}}. \quad \text{...........................(3)}$$

From equations (2) and (3) we can eliminate $2Ph + Mgk$, obtaining

$$p = \pi^2 \frac{MgI^2 + 2Pa^2}{ag} \frac{\theta}{t^2}. \quad \text{...........................(4)}$$

From this expression it appears that, if we know the moment of inertia of the beam, its length, and the weight at each end, we can find the excess p from the time of vibration and deflection.

The results given in this paper were obtained with a 16-inch chemical balance by Oertling. The exact length of the half beam (a) measured by a dividing-engine is 20·2484 centimetres.

To find the Moment of Inertia MI^2 of the Beam. The simplest way theoretically would appear to be this. Find the times of vibration t_1, t_2, and the deflections θ_1, θ_2, due to the same excess p with two different loads P_1, P_2 in each pan. Equating the values of p given for the two by equation (4) we have

$$\frac{MgI^2 + 2P_1a^2}{MgI^2 + 2P_2a^2} = \frac{\theta_2 t_1^2}{\theta_1 t_2^2},$$

an equation which will give MgI^2 in terms of known quantities; but on trial it was found that a very small proportional error in the observed time made a large error in the value of MgI^2; and the following method, that usually adopted in magnetic observations, was employed in preference. A stirrup was suspended by a platinum wire, and its time of vibration (t_1) against the force of torsion (μ) of the wire was observed. The moment of inertia of the stirrup being S, we have

$$t_1^2 = \frac{\pi^2 S}{\mu}$$

The time of vibration (t_2) was then observed when a cylindrical brass bar of known moment of inertia (B) was inserted in the stirrup. We now have

$$t_2{}^2 = \frac{\pi^2}{\mu}(S + B).$$

The bar was then removed and the balance-beam inserted in its place; and the time of vibration (t_3) gives

$$t_3{}^2 = \frac{\pi^2}{\mu}(S + MI^2).$$

From these three equations, eliminating S and μ, we obtain

$$MI^2 = \frac{B\,(t_3{}^2 - t_1{}^2)}{t_2{}^2 \quad t_1{}^2}.$$

Now Bg was calculated from the weight and dimensions of the bar to be 6332·83 (in centimetres and grammes). The observed times were

$$t_1 = 3{\cdot}6792 \text{ secs.,} \quad t_2 = 4{\cdot}495 \text{ secs.,} \quad t_3 \quad 7{\cdot}1483 \text{ secs.}$$

From these values we find

$$MgI^2 = 35651{\cdot}6^*.$$

To measure θ. The angle of deflection was measured by the number of divisions of the scale which the pointer moved over. As the length of the pointer is 32·1006 centimetres, while 20 divisions of the scale measure 2·5658 centimetres, a tenth of a division, in terms of which the deflection was measured, corresponds to an angle of 0·0003996. The oscillations were observed from a distance of six or eight feet by a telescope. The resting-point (i.e. the point where the balance would be in equilibrium) was found in the usual way by observing three successive extremities of two swings and taking the mean of the second and the mean of the first and third. Five determinations of the resting-point were usually made with the excess to be measured alternately added and removed. From these five, three values of the deflection (n) due to the excess were calculated in a manner which will be seen from the example below.

The Time of Vibration. This was found from several determinations of the time of ten oscillations. The method will be seen from the example. No correction was needed for the resistance of the air as long as the vibrations did not exceed two divisions of the scale. When, however, they were much more than that, the time of vibration was found to increase with the arc. As the time of vibration frequently changes slightly, probably through variations of temperature, it was usually observed before and after the determination of the deflection (n) and the mean of the two taken as the true time.

* To this a small correction should be added if the adjusting-bob is not in its lowest position. This amounts to 7·6 for each turn of the screw, and may therefore in general be neglected.

1 2

The following example of the determination of the value of a centigramme-rider by placing it halfway along the beam will sufficiently explain the details of the method.

Time of Vibration at Commencement.

No. of vibration	Observed time of passage of pointer through resting-point	No. of vibration	Observed time of passage of pointer through resting-point	Time of 10 vibrations
	Pointer apparently moving from left to right			
	h. m. s.		h. m. s.	s.
0	11 15 36	10	11 17 43	127
2	11 16 1	12	11 18 8	127
4	11 16 26·5	14	11 18 33·5	127
6	11 16 52	16	11 18 59	127
	Mean value of 10 vibrations			127
	Pointer apparently moving from right to left			
1	11 15 49	11	11 17 56	127
3	11 16 14	13	11 18 21	127
5	11 16 39·5	15	11 18 46	126·5
7	11 17 5	17	11 19 11·5	126·5
	Mean value of 10 vibrations			126·75
	MEAN OF MEANS = 126·875; t_1 = 12·6875 secs.			

Determination of Deflection (n).

Excess weight	Extremities of oscillation		Resting-point	Mean of preceding and succeeding resting-points	Deflection due to excess
Added	109 109	96	102·5		
Removed ...	93 92	40	66·25	102·25	35·25
Added	152 150	53	102	66·75	36
Removed ...	80 79	55	67·25	102·5	35·25
Added	147 145	60	103		

MEAN VALUE OF n = 35·83.

Time of Vibration at End.

No. of vibration	Observed time of passage of pointer through resting-point	No. of vibration	Observed time of passage of pointer through resting-point	Time of 10 vibrations
	Pointer apparently moving from left to right			
	h. m. s.		h. m. s.	s.
0	11 26 19	10	11 28 27	128
2	11 26 44·5	12	11 28 53	128·5
4	11 27 10	14	11 29 18	128
6	11 27 35·5	16	11 29 44	128·5
	Mean value of 10 vibrations ...		... ...	128·25
	Pointer apparently moving from right to left			
1	11 26 32·5	11	11 28 39	126·5
3	11 26 58	13	11 29 5	127
5	11 27 23·5	15	11 29 30·5	127
7	11 27 49	17	11 29 56·5	127·5
	Mean value of 10 vibrations ...		... ...	127
	MEAN OF MEANS 127·625; t_2 12·7625 secs.			

Remembering that one-tenth of a division of the scale is an angle of ·0003996 in circular measure, formula (4), expressed in milligrammes, becomes

$$p - \frac{n}{t^2} 0{\cdot}3996 \frac{\pi^2}{ag} (MgI^2 + 2P\iota^2).$$

In our present example*

$$n = 35{\cdot}83,$$

$$t = \frac{t_1 + t_2}{2} - 12{\cdot}725 \text{ secs.},$$

$$MgI^2 = 35651,$$

$$2Pa^2 = 94704,$$

$$p = 5{\cdot}724 \text{ milligrammes.}$$

The length of time occupied in this determination was not quite a quarter of an hour.

* For this, as for several other cases, I removed the pans and hung the weights directly by fine wires from the suspending-pieces. By this means the resistance of the air was very much diminished.

The following table contains a series of results which I have obtained of the weight of two centigramme-riders, the first of which was accidentally destroyed after the conclusion of the fourth determination. As the rider was always placed at division 5 on the beam, the values given in the table are double those actually obtained.

No. of experiment	$MgI^2 + 2Pa^2$	t in seconds	n	Weight of rider in milligrammes	Mean value
1	145364	8·921	13·458	9·78	9·96 milligrammes
2	309356	17·65	25·49	10·05	
3	519769	20·435	19·12	9·55	
4	130355	13·10	34·71	10·47	
5	130355	12·87	36·6	11·44	11·35 milligrammes
6	130355	12·72	35·5	11·35	
7	130355	12·725	35·83	11·45	
8	130355	12·81	35·5	11·20	
9	130355	12·903	36·37	11·31	
10	454405	19·406	22·08	10·58	

2.

ON A METHOD OF USING THE BALANCE WITH GREAT DELICACY, AND ON ITS EMPLOYMENT TO DETERMINE THE MEAN DENSITY OF THE EARTH.

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In the ease and certainty with which we can determine by the balance a relatively small difference between two large quantities, it probably excels all other scientific instruments.

By the use of agate knife-edges and planes, even ordinary chemical balances have been brought to such perfection that they will indicate one-millionth part of the weight in either pan, while the best bullion-balances are still more accurate. The greatest degree of accuracy which has yet been attained was probably in Professor Miller's weighings for the construction of the standard pound, and its comparison with the kilogramme, in which he found that the probable error of a single comparison of two kilogrammes, by Gauss's method, was $\frac{1}{14000000}$th part of a kilogramme*. (*Phil. Trans.* 1856.)

But, though the balance is peculiarly well fitted to detect the relatively small differences between large quantities, it has not hitherto been considered so well able to measure absolutely small quantities as the torsion balance. The latter, for instance, was used in the Cavendish experiment, when the force measured by Cavendish was the attraction of a large lead sphere upon a smaller sphere, weighing about $1\frac{1}{2}$ lbs., the force only amounting to $\frac{1}{50000000}$th part of this weight, or about $\frac{1}{5000}$th part of a grain.

The two great sources of error, which render the balance inferior to the torsion-balance in the measurement of small forces, are:

1. Greater disturbing effects produced by change of temperature, such as convection-currents and an unequal expansion of the two arms of the balance.

2. The errors arising from the raising of the beam on the supporting frame between each weighing, consisting of varying flexure of the beam and inconstancy of the points of contact of the knife-edges and planes.

* Even so far back as 1787, Count Rumford used a balance which would indicate one in a million and measure one in seven hundred thousand. (*Phil. Trans.* 1799.)

The disturbances due to convection-currents interfere with the torsion-balance as well as with the ordinary balance, though they are more easily guarded against with the former, by reason of the nature of the experiments usually performed with it. They might, perhaps, as has been suggested by Mr. Crookes, be removed from both by using the instruments in a partial vacuum, in which the pressure is lowered to the 'neutral point,' where the convection-currents cease, but the radiometer-effects have not yet begun. But a vacuum-balance requires such complicated apparatus to work it, that it is perhaps better to follow the course which Baily adopted in the Cavendish experiment. He sought to remove the disturbing forces as much as possible, and to render those remaining as nearly uniform as possible in their action during a series of experiments, so that they might be detected and eliminated. For this purpose the instrument was placed in a darkened draughtless room, and was protected by a thick wooden casing gilded on its outer surface. Most of the heat radiated from the surrounding bodies was reflected from the surface of the case by the gilding. The heat absorbed only slowly penetrated to the interior, and was so gradual in its action, that, for a considerable time, the effect might be supposed nearly uniform. Under this supposition it was then eliminated by the following method of taking the observations. The resting-point (that is, the central position of equilibrium, about which the oscillations were taking place) of the torsion-rod, at the ends of which were the small attracted weights, was first observed when the two large masses pulled it in one direction. The masses were then moved round to the opposite side, when they pulled the rod in the opposite direction and the resting-point was again observed. The masses were then replaced in their original position and the resting-point was observed a third time. These three observations were made at equal intervals of time; if, then, the disturbing effect was uniform during the time, the mean of the first and third observations gave what the resting-point would have been, had the rod been pulled in that one direction at the same time that it was actually observed when pulled in the opposite direction. The difference between the second resting-point and the means of the first and third might, therefore, be considered as due to the attractions of the masses alone.

In the experiments of which this paper contains an account, I have endeavoured to apply this method of introducing time as an element to the ordinary balance. But, before it could be properly applied, it was necessary to remove the errors due to the raising of the beam between successive weighings, as they could not be considered to vary in any uniform way with the time. I think I have effected this satisfactorily, by doing away altogether with the raising of the beam by the supporting frame, between the weighings. For this purpose I have introduced a clamp underneath one of the pans, which the observer can bring into action at any time, to fix that pan in whatever position it may be. The weight can then be removed from the pan, and another, which is to be compared with it, can be inserted in its place without altering the relative positions of the planes and knife edges. The counterpoise in the other pan, meanwhile, keeps the beam in the same state of flexure. The pan is then unclamped and the new position about which it oscillates is observed. The only changes are due to the change in the weight and the effect of the external disturbing forces; the latter we may consider as proportional to the time, if sufficient precautions have been taken, and by again changing the weights and again observing the position of the balance, we may eliminate their effects.

Though the method when applied to the balance does not yet give such good results as Baily obtained from the torsion-balance partly, I believe, because I have not yet been able to apply all his precautions to remove external disturbing forces—it still gives better results than would have been obtained without it. This may be seen by the numbers recorded in the tables, where a progressive motion of the resting-point may be noticed, in most cases in the same direction, during a series of experiments. Even when this is not the case, the method at once shows when the disturbing forces are irregular, and when we are justified in rejecting an observation on that account.

I give in this paper two applications of the method, one to the comparison of two weights, the other to the determination of the mean density of the earth. The latter is given only as an example of the method, but I hope shortly to continue the experiments with a large bullion balance, for the construction of which I have had the honour to obtain a grant from the Society. The balance is now in course of construction, by Mr. Oertling, of London.

Description of the Apparatus.

The balance which I have employed is one of Oertling's chemical balances, with a beam of nearly 16 inches, and fitted with agate planes and knife-edges. It will weigh up to a little more than 1 lb. To protect it from sudden changes of temperature, the glass panes of the case are covered with flannel, on both sides of which is pasted gilt paper, with the metallic surface outwards. This case is enclosed in another outer case, a large box of inch deal, lined inside and out with gilt paper. The experiments have been conducted in a darkened cellar under the chemical laboratory at Owens College, which was kindly placed at my disposal by Professor Roscoe. As the ceilings and floors of the building are of concrete, any movement near the room causes a considerable vibration of the floor and walls. It was necessary, therefore, to support the balance independently of the floor. For this purpose, six wooden posts (*A*, *B*, *C*, *D*, *E*, *F*, Fig. 1) were erected resting on the ground underneath and passing freely through the floor to a height of 6 feet 6 inches above it. They are connected at the top by a frame like that of the table, and stayed against each other to give firmness. The wider part of the frame, near the posts

E and *F*, is boarded over to form a table for the telescope (*t*, Fig. 1) and scale (*s*), by which the oscillations of the balance are observed. The box containing the balance rests on two cross-pieces, on the narrower part, *ABCD*, of the frame, with the beam parallel to *AD*, and its right end towards the telescope.

In order to observe the position of the beam, a mirror, $1\frac{1}{4}$ inches by $\frac{3}{4}$ inch, is fixed in the centre of the beam, and the reflection of a vertical scale (*s*, Fig. 1) in this is viewed with a telescope (*t*) placed close to the scale. The light from the scale passes through two small windows cut in each of the cases of the balance and glazed with plate glass. The position of the beam

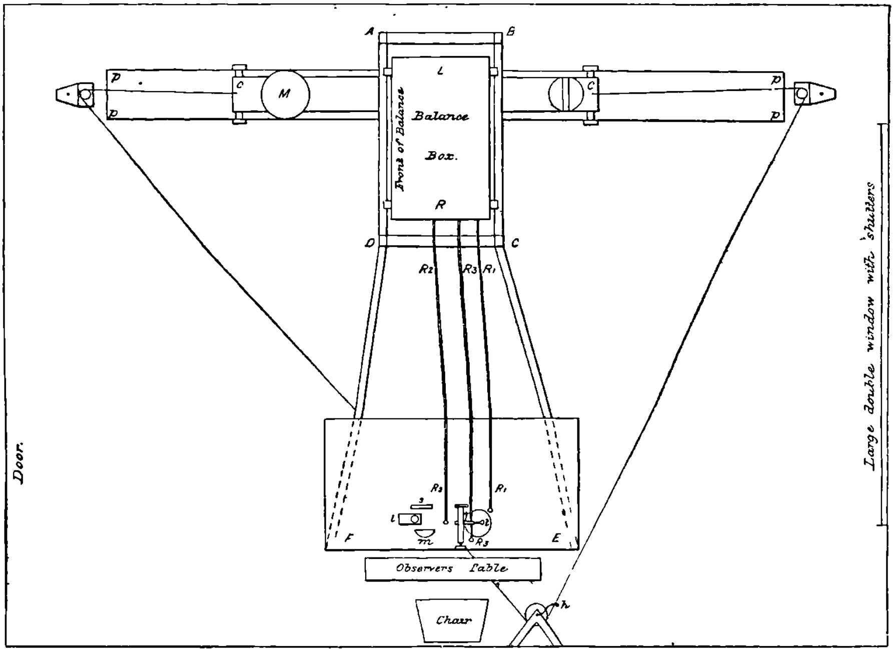


Fig. 1.

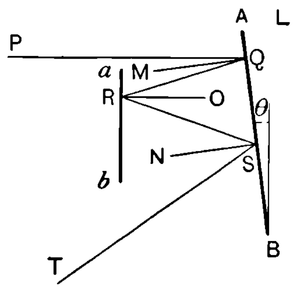
is given by the division of the scale upon the cross-line on the eyepiece of the telescope. The scale, which was photographed on glass, and reduced from a large scale, drawn very carefully, has 50 divisions to the inch. These are ruled diagonally with ten vertical cross-lines. It is possible to read, with almost certainty, to a tenth of a division, or $\frac{1}{500}$th of an inch. Since the mirror is about 6 feet from the scale, a tenth of a division means an angular deflection of the beam of about $3''$*.

The scale is illuminated from behind by a mirror (*m*), several inches in diameter, which reflects through it a parallel beam from a paraffin lamp (*l*).

* The numbers on the scale run from below upwards, so that an increase in the weight in the right-hand pan is indicated by a lower number on the scale.

A plate of ground glass between the scale and mirror diffuses the light evenly over the scale and, by altering the position of the mirror, any desired degree of brilliancy may be given to the illumination of the scale. A screen (not shown in Fig. 1) prevents stray light from striking the balance-case.

This method of reading—which, of course, doubles the deflection—has been so far sufficiently accurate for my purpose; that is to say, the errors arising from other sources are far greater than those arising from imperfections of reading. But in a long series of preliminary experiments I used the following plan to multiply the deflection still further. A rather smaller fixed mirror, *ab*, is placed opposite to and facing the beam-mirror, *AB*, fixed on the beam, and a few inches from it. Suppose the beam-mirror to be deflected from the position *BL*, parallel to *ab*, through an angle, θ, to the position *AB*. If a ray, *PQ*, perpendicular to *ab* strikes *AB* at *Q*, it will make an angle θ with *QM*, the normal at *Q*, and will be reflected along *QR*, making an angle 2θ with its original direction, and therefore with the normal *RO*, at *R*, when it strikes it. If it be reflected again to *AB* at *S*, it will make an angle 3θ with the normal *SN*, and the reflected ray, *ST*, will make an angle 4θ with the original direction, *PQ*, of the ray. It may be still further



reflected between the two mirrors, if desirable, each reflection at the mirror, *AB*, adding 2θ to the deflection of the ray. I have, for instance, employed three reflections from the beam-mirror, so multiplying the deflection six times. In this case, one division of my scale, at the distance at which it was placed from the beam, corresponded to a deflection of 7″ in the beam, and this could be subdivided to tenths by the eye. The only limit to the multiplication arises from the imperfection of the mirrors and the decrease in the illumination of the successive reflections*.

The chair of the observer is placed on a raised platform, and a small table rising from the platform and free from the frame on which the instruments rest, is between the observer and the telescope. On this he can rest his note-book during an experiment. As the differences of weight observed are sometimes exceedingly minute, the balance is made very sensitive—usually

* This method was used in the seventh and eighth series here recorded. Two reflections from the beam-mirror were employed, giving four times the actual deflection.

vibrating in periods between 30 secs. and 50 secs. The value of a division of the scale cannot be determined by adding known small weights to one pan, as the deflection would usually be too great. Any approach of the observer to the case causes great disturbances, so that the ordinary method of moving a rider an observed distance along the beam is inapplicable. In some experiments made last year I calculated the force equivalent to the small differences in weight, in absolute measure, by observing the actual angular deflection and the time of vibration. With a knowledge of the moment of inertia of the beam and treating it as a case of small oscillations, it was possible to calculate the value of the scale. But the observations and subsequent calculations were so complicated that the following method of employing riders was ultimately adopted.

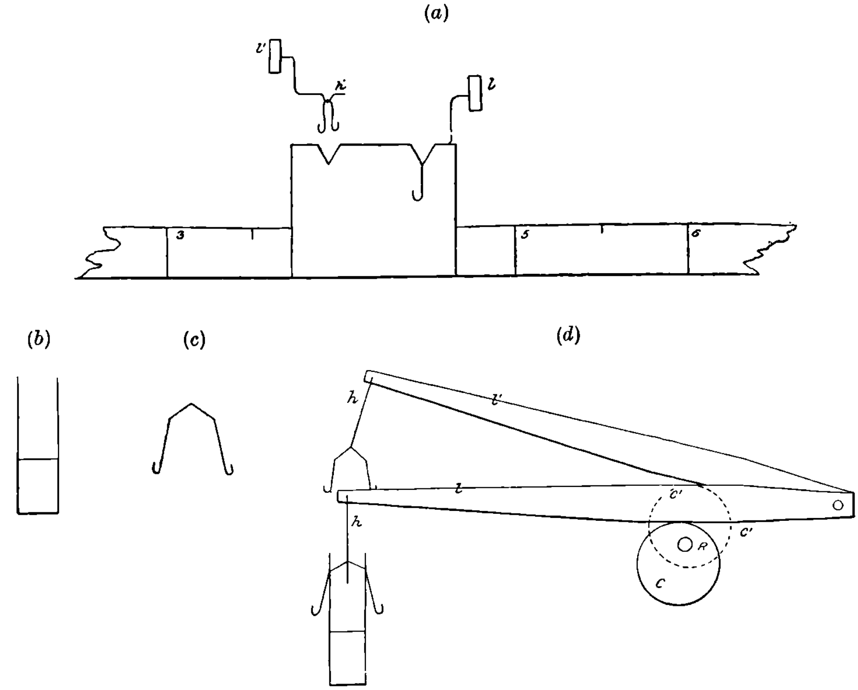


Fig. 2.

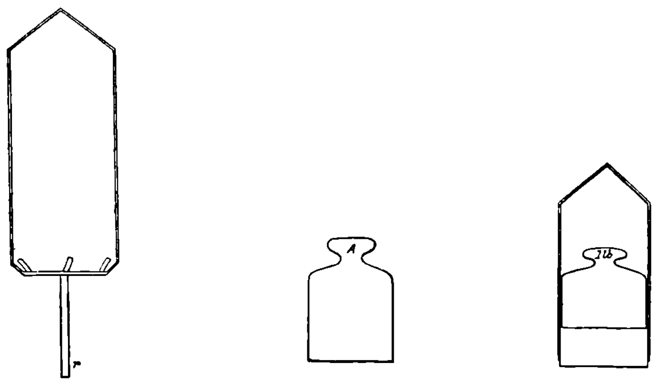
A small bridge about an inch long (Fig. 2 a) is fitted on to the beam. The sides of the bridge are prolonged about half an inch above the arch which fits on to the beam, as shown in the end view (Fig. 2 b). In each of these sides are cut two V-shaped notches directly opposite to each other, one of the opposite pairs being 6·654 millims. (about $\frac{1}{4}$ inch) distant from the other pair. Two equal riders of the shape shown in Fig. 2 c are placed across the bridge, and are of such a size that they will just fit into the bottom of the notches. When one of these rests across the bridge the other is raised up from it. The lowering of one rider and the raising of the other corresponds therefore to a transference of a single rider from one pair of notches to the other. The length of the half beam being 202·716 millims. and the distance between the notches 6·654 millims., this transference will be equivalent to the addition to one pair of 0·03282 of the weight of the rider used. As I have generally used a centigramme-rider this means 0·3282 mgm.

Two levers l, l' (Fig. 2d), with hooks h, h' are used to raise one rider while the other is lowered. These levers are worked by two cams c, c' on a rod R, which is prolonged out of the balance-case to the observer. By turning this rod round, the one lever is raised while the other is depressed. The hook at the end of the raised lever picks up its rider while the other hook deposits its rider on the bridge, and then sinks down between the raised sides (as shown in Fig. 2d), leaving the rider resting freely on the bridge.

The levers are so adjusted that the beam even in its greatest oscillations never comes in contact with the hooks.

This arrangement might probably be still further perfected by introducing two small frames for the riders to rest upon, the frames resting on the beam by knife-edges. It would then be certain that the movement of the riders was equivalent to a transference from one knife-edge to the other, whereas the rider at present may not rest exactly over the centre of the notch. But I find that I get fairly consistent results by lowering the rider somewhat suddenly so as to give it sufficient impetus to go to the bottom of the notch, and have not therefore thought it necessary as yet to introduce more complicated apparatus.

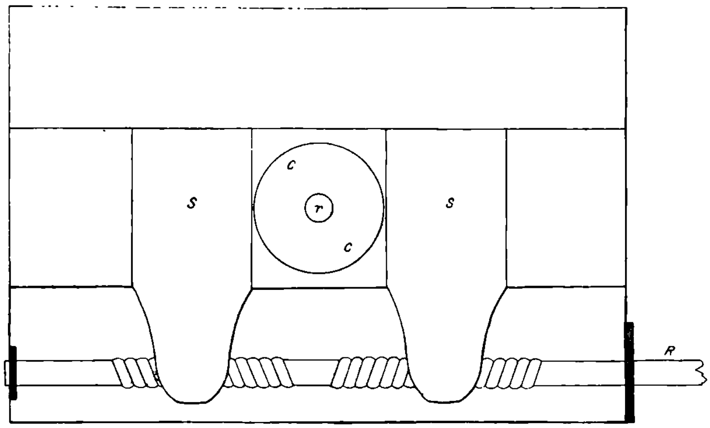
In place of the right-hand pan of the usual shape, another of the shape shown in Fig. 3a is employed. To the centre of the pan underneath is



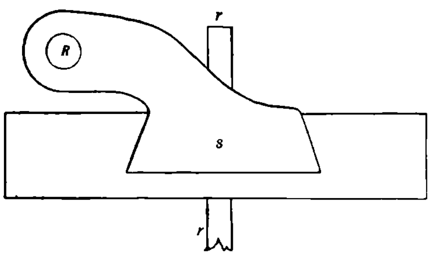
(a) (b) (c)

Fig. 3.

attached a vertical brass rod which passes downwards through the bottom of the inner case of the balance. To the under side of this case is attached the clamping arrangement before referred to. This consists of two sliding pieces (Fig. 4 *a*, *s*, *s*) working horizontally in a slot cut in a thick brass plate which is fastened to the case. Through a circular aperture in this plate (the slot is not cut through the whole thickness of the plate, but only as shown in Fig. 4*b*) and about the middle of the slot hangs the rod *r* attached to the scale-pan.



(*a*)



(*b*)

Fig. 4.

By means of right and left handed screws on a rod *R*, which is prolonged out of the case to the observer, these two sliding-pieces can be made to approach, and clamp the rod, or to recede and free it. By having the opposite surfaces of the sliding-pieces and the rod polished and clean, it is possible to clamp and unclamp without producing any disturbance. The clamp is of great use also to lessen the vibrations when they are too large, as it may be brought into action at any moment, and on releasing carefully the beam will start again from rest without any impetus. It may be used too to increase the vibrations by releasing suddenly, when the beam will have a slight impetus in one direction or the other.

The weights which I have compared are two brass pounds avoirdupois, made for me by Mr. Oertling, and marked *A* and *B* respectively. They are of the usual cylindrical shape with a knob at the top (Fig. 3*b*). Two small brass pans (Fig. 3*c*) with a wire arch by which they can be suspended, are used to carry them; these are called respectively X and Y. I found on beginning to use them that there was too great a difference between A and B. I therefore adjusted them by putting a very small piece of wax upon A, the lighter. But the difference between them increased by 0·0782 mgm. in two days, which I thought was probably due to the wax. After the fourth series I therefore removed it and scraped B till it was more nearly equal to A. The weighings I—IV have, however, been retained, for though the differences on different days vary they are fairly constant on the same day.

The weights are changed by the following apparatus which has been designed to effect the change as simply and quickly as possible.

A horizontal 'side-rod' or link (*ss*, Fig. 5) is worked by two cranks (*c*, *c*, Fig. 5*b*), which are attached to the axles of two equal toothed wheels (*t*, *t*)

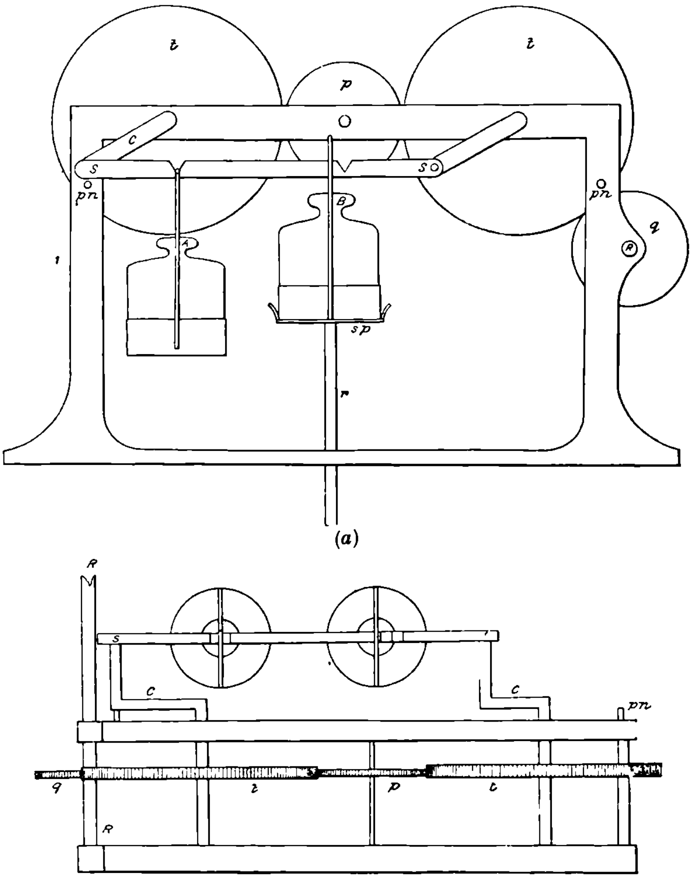


Fig. 5.

with a pinion (p) connecting them. A second pinion (q), on a rod prolonged out of the case to the observer, gears with one of the toothed wheels. By turning this rod the toothed wheels are set in motion, both in the same direction, moving the horizontal 'side-rod' from the right say upwards and over to the left. A pin (pn) stops its motion downwards further than is shown in Fig. 5a. Near each end of the rod is cut a notch, and across these are hung the pans carrying the weights. The apparatus is fastened to the floor of the case between the central upright, supporting the beam, and the scale-pan, the side-rod being perpendicular to the direction of the beam, and exactly over the centre of the pan. In Fig. 5a, one of the weights B is supposed to be resting on the scale-pan (the wires suspending the pan from the beam not being shown), the side-rod having moved down so far below the wire of the smaller pan carrying the weight that it leaves it quite free. If, now, it is desired to change the weights the rod R is turned, setting the wheels in motion, the side-rod moves up, picks up B—the notch catching the wire—then travels over round to the extreme right, when A will be just over and nearly touching the scale-pan. By continuing the motion slightly A will be gently deposited on the pan, and the side-rod will move slightly down leaving the weight quite free. On the scale-pan are four pins, turned slightly outwards, acting as guides for the small pan, and ensuring that it shall always come into the same position. The wheels and pinions are of such a size that two revolutions of the rod just suffice to change one weight for the other.

It will be seen that all the manipulation required from the observer during a series of weighings is the simple turning of three rods, which are prolonged out of the balance case to where he is stationed at the telescope. By turning one of these he can change the position of the rider on the beam by a known amount, and so find the value of his scale. By turning a second he clamps the scale-pan, and so steadies the balance while the weights are changed by turning a third rod. I have made this arrangement not only because it seems as simple as possible to secure the end required, but also because it seemed more applicable to a vacuum-balance (with which I hope ultimately to test it).

I take this opportunity of expressing my thanks to Mr. Thomas Foster, mechanician of Owens College, for his aid in the construction of the apparatus, and in the planning of many of its details.

Method of conducting a Series of Weighings.

After the counterpoise has been adjusted so that the beam swings nearly about its horizontal position, the frame is lowered so that the balance is ready for use. The pan is then clamped and the balance is left to come to a nearly permanent state of flexure if possible, sometimes for the night or even longer. The lamp is lighted usually half-an-hour or more before beginning to observe, so that its effect on the balance may attain a more or less steady state. It is necessary also to wait some time after coming into the room, for the opening of the door will always cause a considerable and immediate deflection of the beam. When a sufficient time has elapsed, the observations are commenced with a determination of the value of one scale-division by means of the riders. The three extremities of two successive oscillations are observed with one of the riders resting on the beam. These are then combined as follows: The mean of the first and third is taken, and the mean again of this and the second, this constituting the 'resting-point,' that is, the position of equilibrium of the beam at the middle of the time. For instance, in weighing No. I (see tables at the end) the three extremities of successive oscillations were 280·5, 312·0, and 286·0 (column 2). The resting-point was taken as

$$\frac{280{\cdot}5 + 286{\cdot}0 + 2 \times 312{\cdot}0}{4} = 297{\cdot}62,$$

the rider on the beam being the right-hand one denoted by R (column 1). The balance is then clamped, and the other rider is brought on to the beam while the first is taken up. The resting-point is again observed. In No. I it was 270·05. The balance is again clamped, and the first rider again brought on to the beam, and, on unclamping, the resting point again observed. In the same weighing it was 296·75. These three are sufficient to give one determination of the deflection due to the transference of a rider. This will be the difference between the second resting-point and the mean of the first and third. For instance, $\frac{297{\cdot}62 + 296{\cdot}75}{2} - 270{\cdot}05 \quad 27{\cdot}13$ divisions. This number is found in the fifth column.

This process is continued, the resting-points being combined in threes till several values of the deflection due to the rider have been obtained, and the mean of these is taken as the true value. This plan of combining the resting-points requires that the observations should be taken at nearly equal intervals. After a little practice it will always take the observer about the same time to go through the same operations of clamping, changing the riders, unclamping, clamping again to lessen the vibrations about the new resting-point, and then beginning to observe, and I have considered that this was a sufficiently correct method of timing the observations.

When a series has been taken it will at once be seen whether they were begun too soon after entering the room, or whether any irregular disturbing force has acted. For instance, in weighing No. II, determination of one scale-division, the first resting point is so much lower than the succeeding with the same rider that evidently the balance was still affected by my entrance into the room. It was, therefore, rejected. Again, in weighing No. III, determination of the difference between the weights, the fourth resting-point was much lower than the others with the same weight in the pan.

The resting-points, when the other weight was in the pan, showed no similar sudden drop of such magnitude. This observation was, therefore, rejected as being affected by some irregular disturbance.

When the value of the deflection is determined, the value of one scale-division is at once found by dividing ·3282 mgm. by the number of divisions of the deflection, since the change of the sides is equivalent to the addition of ·3282 mgm. to one pan.

The determination of the difference between the weights is then begun. This is carried on in a precisely similar manner, the only difference being that the rod changing the weights is now turned round in place of the rod changing the riders. I have usually taken a greater number of observations of the difference between the weights than of the deflection due to the riders, as the former is somewhat more irregular than the latter. This irregularity I believe to arise from slight differences of temperature of the two weights, and perhaps from air currents caused by their motion inside the case. They do not seem to be due to any fault in the clamping arrangement, since that is employed equally in both, and the changing of the weights, if effected gently, does not move the beam at all.

When the deflection has been determined, it is multiplied by the number of milligrammes corresponding to one scale-division, and this, of course, gives the difference between the weights. I have interchanged the weights in the two pans X and Y, between the series of weighings, in order to make the experiments like those conducted in the weighings for the standard pound. But my object has not been to show at all that the method gives consistent results day after day, and, in fact, the difference between the weights has varied. For instance, according to weighings I and II, $A - B = \cdot 0446$, while, according to weighings III and IV, $A - B = \cdot 0116$. There is a greater difference between these than can be accounted for by errors of experiment, and it probably arose from the small piece of wax with which I made A nearly equal to B. The difference between the weights when measured to such a degree of accuracy as that which I have attempted, will, no doubt, vary from time to time, partly with deposits of dust, partly with changes in the moisture in the atmosphere, and so on.

But I think the numbers which are given in the tables are sufficient to show that the difference between two weights in any one series of weighings can be measured with a greater degree of accuracy than has hitherto been supposed possible. I give in the tables a full account of the weighings, each series containing a determination of the value of one scale-division and a determination of the difference between the weights. The greatest deviation of any one of a series from the mean of that series of differences is always given. This I consider a better test of accuracy of weighing than the probable error. What is wanted in weighing is rather a method which will give at once a good determination of the difference between two weights. But I may state, that if the error of any one of a series be taken as its difference from the mean of that series, the probable error of a single determination of the difference between the weights in the first four series is ·4344 of a division, or ·0054 mgm., that is, $\frac{1}{84000000}$th of the total weight, while the greatest error is 1·8 divisions, or ·0224 mgm., that is, $\frac{1}{20000000}$th of the total weight. It may be remarked that these weighings were all made during peculiarly unfavourable weather when there were frequent heavy showers, causing sudden changes of temperature, and thus seriously affecting the working of the balance. In the series V VIII the greatest error is only $\frac{1}{50000000}$ of the total weight, the weather having improved considerably.

On the Employment of the Balance to determine the Mean Density of the Earth.

In the Cavendish experiment, the attraction of a large sphere of lead of known mass and dimensions upon another smaller sphere, also of known mass and dimensions, is measured when the two are an observed distance apart. Comparing this attraction with the weight of the small sphere that is the attraction of the earth upon it and knowing the dimensions of the earth, we can deduce the mass of the earth in terms of the mass of the large lead sphere, and so obtain its mean density. The torsion-balance, which was invented for the purpose by Mitchell, the original contriver of the experiment, has hitherto been used to determine the force exerted by the mass upon the small sphere. In the arrangement here described, I have replaced the torsion-balance by the ordinary balance, and have so been able to compare the attraction of a lead sphere with that of the earth upon the same mass somewhat more directly. The results which I have obtained have no value in themselves, but they serve as an example of the employment of the balance for more delicate work than any which it has as yet been supposed able to perform.

The method is shortly this: A lead weight (called 'the weight') weighing 452·92 grms. (nearly 1 lb.) hangs down by a fine wire from one arm of a balance, from which the pan has been removed, at a distance of about six feet below it, and is accurately counterpoised in the other pan, suspended from the other arm. A large lead mass (called 'the mass') weighing 154,220·6 grms. (340 lbs.) is then introduced directly under the hanging 'weight.' The attraction of this mass increases the weight slightly and the beam is deflected through an angle which is observed. The value of this deflection in milligrammes is measured by the employment of riders in the manner described above, and so the attraction of the 'mass' is known. The increase of the weight caused by the 'mass' has been in my experiments about ·01 of a milligramme, or $\frac{1}{45000000}$th of the whole weight.

The balance which I have used is that which I have described above. It was placed in the same room and in the same position as in the weighing experiments. The same method was used to observe the oscillations with a single mirror on the beam. The scale was a simple one etched on glass and not diagonally ruled. It had about 50 divisions to the inch, and the numbers increased from above downwards, so that an increase in the weight hanging from the left arm was indicated by a lower number on the scale.

The 'weight' which is suspended by a very fine brass wire from the left arm, passing through a hole in the bottom of the balance-case, hangs in a double tin tube, 4 inches in diameter, to protect it from air-currents. At the bottom of the tube is a window, through which can be seen the bottom of the 'weight' as it hangs. The 'weight' is 4·248 centims. in diameter and is gilded. The 'mass' is a sphere of an alloy of lead and antimony. It was cast with a 'head' on and then accurately turned. Its vertical diameter is 30·477 centims. (about 1 foot). The specific gravity of a specimen of the metal was found to be 10·422. Its weight given by a weighing-machine is 340 lbs. about, and this agrees very nearly with the weight calculated from the specific gravity. I am obliged to accept this as the true weight provisionally, until it is found more correctly by the large balance referred to above and now being constructed.

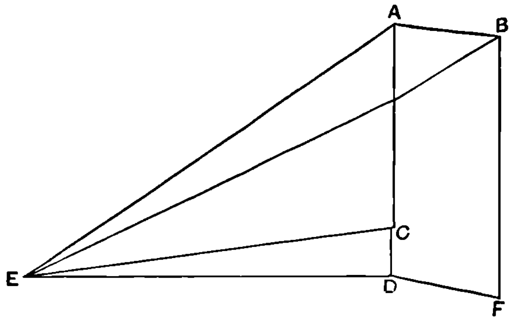
This mass (Fig. 1, *M*) is placed in a shallow wood cup at one end of a 2-inch plank, 8 inches wide and 6 feet 11 inches long, mounted on four flanged brass wheels, and serving as a carriage for it (Fig. 1). A plank about 12 feet long nailed to the floor in a direction perpendicular to the beam of the balance, as shown in Fig. 1, *pp*, acts as a railway for the carriage, and a firm stop at each end prevents the carriage from running off the rail. The distance between the stops is rather less than twice the length of the carriage, and the 'weight' hangs down from the balance exactly midway between the stops. The 'mass' is placed on the carriage so that it is exactly under the 'weight' when the carriage is at one end of its excursion against one of the stops. An empty cup (*c*, Fig. 1) of the same dimensions as that in which the 'mass' rests is placed at the other end of the carriage, and is just under the 'weight' when the carriage is against the other stop. By this arrangement no correction is needed for the attraction of the carriage upon the 'weight' or counterpoise, and the effect caused by the removal of the carriage from one end of its excursion to the other is entirely due to the difference of attractions of the 'mass' upon the 'weight' and counterpoise in its two positions. The position of the 'mass' when directly under the 'weight' is called its 'in' position, and that when it is at the other end of its excursion is called the 'out' position. The length of the excursion is 5 feet 7·3 inches.

To draw the carriage along the rail a vertical iron shaft with a wood cylinder at the lower end pivots on the floor, and is prolonged up to the level of the observer as he sits at the telescope with a handle by which he can turn it. The two ends of a rope which winds round the cylinder pass through pulleys on the stops, and are attached to the ends of the carriage.

The observer can then move the 'mass' with great ease by turning the handle, even while looking through the telescope.

When a series of observations is made, the general method is this. The deflection (r) due to the transference of a rider from one notch to the other on the beam is first observed exactly in the manner before described, the mean of four or five values being taken as the true value. Then the deflection (n) due to the difference of attraction of the 'mass' in its two positions is found in exactly the manner in which the difference between two weights is found, except that now when three successive extremities of oscillations have been observed for a resting-point the 'mass' is moved from one position to the other where the weights were changed in the former experiments, the clamp not being brought into action. The second extremity of the oscillation which is proceeding while the 'mass' is moved, is observed as the first of the next three. When nine or more resting-points have been observed they are combined in threes, and the mean of the resulting values of the deflection n is used in the subsequent calculation.

This deflection is, of course, less than that which would be observed were there no attraction on the counterpoise, and were the 'out' position of the



'mass' at an infinite distance. To find the factor f by which the deflection n due to the change of position of the 'mass' must be multiplied in order to reduce it to the deflection which would be observed under these conditions, let AB be equal and parallel to the beam of the balance at the level of the counterpoise of which B is the centre. Let C be the centre of the 'weight,' D that of the 'mass' in its 'in' position, E that of the 'mass' in its 'out' position. Draw BF, DF, parallel to AD, AB. Let $\mu =$ the mass of the 'mass.'

The vertical attraction of the 'mass' in its 'in' position will be

$$\frac{\mu}{CD^2} - \frac{\mu BF}{BD^3}.$$

The vertical attraction in its 'out' position will be

$$\frac{\mu CD}{CE^3} - \frac{\mu BF}{BE^3}.$$

The difference between these is actually observed, viz.:

$$\frac{\mu}{CD^2}\left\{1 - \frac{CD^2 . BF}{BD^3} - \frac{CD^3}{CE^3} + \frac{CD^2 . BF}{BE^3}\right\}.$$

The factor by which we must multiply the observed difference to reduce it to the attraction of the 'mass' on the 'weight' in its 'in' position is therefore

$$\begin{aligned} f &= 1 + \frac{CD^2 . BF}{BD^3} + \frac{CD^3}{CE^3} - \frac{CD^2 . BF}{BE^3} \\ &= 1{\cdot}0185 \end{aligned}$$

$$\begin{aligned} \text{since } CD &= 22{\cdot}13 \text{ centimetres.} \\ BD &= 192{\cdot}03 \quad ,, \\ BF &= 187{\cdot}70 \quad ,, \\ CE &= 172{\cdot}36 \quad ,, \\ BE &= 257{\cdot}09 \quad ,, \end{aligned}$$

The values of r and n being observed, the distance d between the centres of the 'mass' and 'weight' is then measured by adding to 17·362 centims. (the sum of their radii) the distance from the top of the 'mass' to the bottom of the 'weight' as measured by a cathetometer.

It now remains to explain the calculation of the mean density Δ from the observed values of r, n, and d. We have

$$\frac{f \times \text{increase in weight observed}}{\text{Weight of 'weight'}} = \frac{\text{Attraction of 'mass' on 'weight' when 'in'}}{\text{Attraction of earth on 'weight'}}.$$

But the increase in weight is $\dfrac{n \times 6654}{r \times 202716}$ mgms., since the distance between the notches is 6·654 millims., and the half beam 202·716 millims. The weight of the 'weight' is 453·29 grammes.

The attraction of the 'mass' per gramme of the 'weight'

$$\begin{aligned} &= \frac{\text{Volume} \times \text{density}}{(\text{distance } d \text{ between centres of 'mass' and 'weight'})^2}, \\ &= \frac{\text{Mass in grammes}}{d^2}, \\ &= \frac{154220{\cdot}6}{d^2}. \end{aligned}$$

The attraction of the earth is similarly

$$\Delta \times \tfrac{4}{3}\pi R\left\{1 + M - \left(\tfrac{5}{2}M - \epsilon\right)\cos^2\lambda\right\},$$

where

$$\begin{aligned} \Delta &= \text{mean density of the earth,} \\ R &= \text{earth's polar radius in centimetres,} \\ M &= \frac{\text{centrifugal force at the equator}}{\text{Equatorial gravity}}, \\ \epsilon &= \text{ellipticity,} \\ \lambda &= \text{latitude.} \end{aligned}$$

The logarithm of the coefficient of Δ when R is in inches is 9·0209985 (*Astron. Soc. Mem.*, vol. 14, p. 118), or if R is in centimetres it is 9·4258322.

Inserting these values in the equation we obtain

$$\Delta = \frac{\dfrac{154220{\cdot}6}{d^2}}{\frac{4}{3}\pi R\,\{1+M-(\frac{5}{2}M-\epsilon)\cos^2\lambda\}} \times \frac{453290}{f \times \dfrac{n}{r}\dfrac{6654}{202716}}.$$

$$\therefore\ \Delta = C \times \frac{r}{nd^2},$$

where
$$C = \frac{154220{\cdot}6 \times 453290 \times 202716}{\frac{4}{3}\pi R\,\{1+M-(\frac{5}{2}M-\epsilon)\cos^2\lambda\} \times f \times 6654}$$

and
$$\log C = 1{\cdot}8951337,$$

$$\therefore \log \Delta = 1{\cdot}8951337 + \log r - \log n - 2\log d.$$

The following table is an account of an experiment made on May 30th, 1878, and will serve as a specimen of the method of making the observations. It is the best which I have yet made in the closeness with which all the values of n agree with each other.

VII. *May* 30, 1878. *Determination of* r.

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Differences $= r$
R	260·9 250·9 260·6	255·82	—	—
L	214·6 205·0 212·3	209·22	256·74	47·52
R	271·9 244·7 269·4	257·67	210·31	47·36
L	214·3 209·2 212·9	211·40	257·62	46·22
R	249·7 263·8 253·0	257·57	212·23	45·34
L	204·9 220·7 206·0	213·07	—	—

Mean $r = 46{\cdot}61$.

Determination of n.

Position of mass	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Differences $= n$
In	216·8 208·1 215·9	212·22	—	—
Out	210·9 216·1 211·0	213·52	212·27	1·25
In	213·9 210·8 213·8	212·32	213·61	1·29
Out	212·7 214·6 212·9	213·70	212·41	1·29
In	211·5 213·6 211·3	212·50	213·78	1·28
Out	215·9 211·8 216·0	213·87	212·60	1·27
In	210·2 215·0 210·6	212·70	213·93	1·23
Out	216·7 211·6 216·1	214·00	212·73	1·27
In	209·9 215·4 210·4	212·77	214·08	1·31
Out	217·0 211·4 216·9	214·17	212·98	1·19
In	209·8 216·2 210·6	213·20	—	—

Mean n = 1·26.

At the close of the experiment d was found to be 22·226 centimetres.

We have therefore

$$\begin{aligned}\log \Delta &= \log C \\ &\quad + \log 46{\cdot}61 \\ &\quad - \log 1{\cdot}26 \\ &\quad - 2 \log 22{\cdot}226 \\ &= 1{\cdot}8951337 \\ &\quad + 1{\cdot}6684791 \\ &\quad - 0{\cdot}1003705 \\ &\quad - 2{\cdot}6937226 \\ &= 0{\cdot}7695197. \\ \therefore \quad \Delta &= 5{\cdot}882.\end{aligned}$$

I have made in all eleven experiments with this method. The resulting values of Δ are

1	May	20	5·393.
2	„	23	5·570.
3	„	24	4·415.
4	„	28	7·172.
5	„	29	5·109.
6	„	29	6·075.
7	„	30	5·882.
8	„	30	6·336.
9	June	5	5·977.
10	„	5	5·580.
11	„	6	5·100.

The resulting mean value of the mean density of the earth is 5·69.

If the eleven determinations be supposed to have equal weight, the probable error of their value is 0·15.

The various determinations differ very much among themselves, but they seem to me sufficiently close to justify the hope that with a large balance and a large weight, which will not be so easily affected by air-currents, and with greater precautions to prevent those air-currents, a good determination of the mean density of the earth may ultimately be obtained by this method.

I. *June* 12. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of three successive oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R – L
L	280·5 312·0 286·0	297·62	—	—
R	253·1 284·9 257·3	270·05	297·18	27·13
L	306·2 288·5 303·8	296·75	268·68	28·07
R	258·8 275·0 260·5	267·32	294·81	27·49
L	285·8 298·7 288·3	292·87	266·79	26·08
R	272·9 260·6 271·0	266·27	292·88	26·61
L	296·1 290·0 295·5	292·90	—	—

Mean R – L = 27·08 divisions.

$$\therefore \ 1 \text{ division} = \frac{0{\cdot}3282}{27{\cdot}08} = 0{\cdot}01212 \text{ milligramme.}$$

Determination of $(B + X) - (A + Y)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$A + Y$	254·5 272·9 257·3	264·40	—	—
$B + X$	267·5 249·8 264·8	257·97	263·27	5·30
$A + Y$	258·5 265·4 259·3	262·15	257·04	5·11
$B + X$	259·3 253·6 258·0	256·12	261·87	5·75
$A + Y$	253·5 268·5 255·9	261·60	255·63	5·97
$B + X$	244·6 264·5 247·0	255·15	261·87	6·72
$A + Y$	273·6 252·0 271·0	262·15	255·85	6·30
$B + X$	252·7 260·0 253·5	256·55	262·92	6·37
$A + Y$	275·0 253·7 272·4	263·70	—	Mean difference 5 93

$\therefore (B + X) - (A + Y) = 0·01212 \times 5·93 = 0·0718$ milligramme.

Greatest deviation from the mean = 0·82 division = 0·0099 milligramme.

The weather during this series of weighings was very unfavourable, with frequent heavy showers.

II. *June* 13. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
L	302·8 299·9 302·9	301·37	This is rejected as it is so much lower than the succeeding	
R	321·8 335·7 326·8	330·00	—	—
L	299·6 308·7 301·0	304·50	330·68	26·18
R	322·9 337·5 327·6	331·37	305·10	26·27
L	299·5 310·9 301·5	305·70	331·77	26·07
R	325·2 337·1 329·3	332·17	305·90	26·27
L	290·4 319·4 295·2	306·10	—	—

Mean R − L = 26·20 divisions.

∴ 1 division = 0·01252 milligramme.

The weather was as unfavourable as on the previous day.

The weights were changed shortly before the commencement of this series and the balance then worked so irregularly that for some time I was unable to begin the rider-determination. Even then the first resting-point had to be rejected.

Determination of $(A+X)-(B+Y)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$B+Y$	307·8 326·0 311·6	317·85	These are all rejected, as the motion was so irregular. The weights had been changed a short time before, and had probably not reached an uniform temperature	
$A+X$	293·7 307·8 295·8	301·27		
$B+Y$	309·6 322·3 312·6	316·70		
$A+X$	295·5 309·6 297·6	303·07		
$B+Y$	304·1 322·7 308·1	314·40		
$A+X$	289·3 304·4 291·5	297·40		
$B+Y$	305·5 315·0 307·5	310·75	297·35	13·40
$A+X$	294·2 300·1 294·8	297·30	309·95	12·65
$B+Y$	304·6 312·7 306·6	309·15	296·51	12·64
$A+X$	290·0 300·7 291·5	295·72	—	—

Mean $(A+X)-(B+Y)=12{\cdot}89$ divisions.

$\therefore\ (A+X)-(B+Y)=0{\cdot}1614$ milligramme.

Greatest deviation from the mean $=0{\cdot}51$ division $=0{\cdot}0062$ milligramme.

III. *June* 13. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
R	301·3 290·6 299·7	295·55	—	—
L	313·7 328·7 318·1	322·30	296·38	25·92
R	293·6 300·4 294·5	297·22	322·47	25·25
L	311·0 331·6 316·4	322·65	297·88	24·77
R	281·6 313·2 286·2	298·55	323·57	25·02
L	310·1 335·8 316·3	324·50	298·62	25·88
R	287·8 308·1 290·8	298·70	—	—

Mean R − L = 25·37 divisions.

$$\therefore\ 1 \text{ division} = \frac{0{\cdot}3282}{25{\cdot}37} = 0{\cdot}01293 \text{ milligramme.}$$

Determination of $(A + X) - (B + Y)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$A + X$	303·3 294·5 301·8	298·52	—	
$B + Y$	303·6 315·8 306·2	310·35	297·07	13·28
$A + X$	288·9 301·5 290·6	295·62	—	—
$B + Y$	300·2 311·6 302·5	306·47	This is evidently due to some irregular and short disturbing cause, and is rejected	
$A + X$	288·8 304·2 291·1	297·07	—	—
$B + Y$	301·5 319·3 303·9	311·00	297·16	13·84
$A + X$	291·7 302·0 293·2	297·22	310·33	13·11
$B + Y$	301·0 316·8 304·1	309·67	—	—

Mean $(A + X) - (B + Y) = 13{\cdot}40$ divisions.

$\therefore\ (A + X) - (B + Y) = 0{\cdot}1732$ milligramme.

Greatest deviation from the mean = 0·44 division = 0·0057 milligramme.

IV. *June* 14. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
R	252·5 253·4 251·7	252·25	This was taken soon after entering the room. It is so much lower than the succeeding that it is rejected	
L	290·8 273·7 288·8	281·75	—	—
R	242·4 266·7 245·1	255·22	281·96	26·74
L	272·6 289·7 276·7	282·17	255·89	26·28
R	259·8 253·9 258·7	256·57	282·67	26·10
L	286·2 280·0 286·5	283·17	—	—

Mean R − L = 26·37 divisions.

$$\therefore\ 1 \text{ division} = \frac{0{\cdot}3282}{26{\cdot}37} = 0{\cdot}01244 \text{ milligramme.}$$

Being interrupted, I could not continue the series of rider-determinations further.

Determination of $(B+X)-(A+Y)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$A+Y$	291·7 275·0 289·5	282·80	—	—
$B+X$	261·0 280·3 263·4	271·25	282·85	11·60
$A+Y$	285·7 280·4 285·1	282·90	271·66	11·24
$B+X$	280·2 265·3 277·5	272·07	283·90	11·83
$A+Y$	294·9 276·1 292·5	284·90	273·43	11·47
$B+X$	276·0 273·9 275·4	274·80	286·40	11·60
$A+Y$	267·3 305·4 273·5	287·90	275·11	12·79
$B+X$	278·5 272·9 277·4	275·42	289·28	13·86
$A+Y$	296·6 285·5 295·1	290·67	—	—

Mean $(B+X)-(A+Y)=12{\cdot}06$ divisions.

$\therefore (B+X)-(A+Y)=0{\cdot}1500$ milligramme.

Greatest deviation from the mean = 1·8 divisions = 0·0224 mgm.

The previous determination of $(B+X)-(A+Y)$ was ·0718 mgm. The difference is too great, ·0782 mgm., to be accounted for by errors of experiment. There must have been some deposit on one of the weights, either of dust or moisture. I therefore took them out, cleaned and adjusted them by scraping B till nearly equal to A, and removing the wax from A.

V. *June* 14. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
R	212·6 230·3 214·7	221·97	—	—
L	252·3 242·5 251·2	247·12	222·16	24·96
R	225·0 220·5 223·6	222·40	246·67	24·27
L	238·6 252·8 240·7	246·22	221·87	24·35
R	224·9 218·7 223·3	221·40	245·98	24·58
L	249·7 242·2 248·9	245·75	221·26	24·49
R	226·8 216·7 224·3	221·12	—	—

Mean R − L = 24·53 divisions.

$$\therefore\ 1 \text{ division} = \frac{0{\cdot}3282}{24{\cdot}53} = 0{\cdot}01339 \text{ milligramme.}$$

Determination of $(B+Y)-(A+X)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$A + X$	206·3 212·7 206·8	209·62	This was rejected as being so much higher than the rest	
$B + Y$	209·0 206·4 208·6	207·60	—	—
$A + X$	212·4 204·0 211·3	207·92	207·71	·21
$B + Y$	206·8 208·8 206·9	207·82	207·88	·06
$A + X$	211·8 204·4 210·8	207·85	207·64	·21
$B + Y$	209·1 206·1 208·6	207·47	207·86	·39
$A + X$	210·7 205·5 209·8	207·87	207·11	·76
$B + Y$	208·5 205·3 207·9	206·75	207·53	·78
$A + X$	209·7 205·1 208·9	207·20	206·45	·75
$B + Y$	203·3 208·7 203·9	206·15	—	—

Mean $(B + Y) - (A + X) = 0·45$ division $= 0·00602$ milligramme.

Greatest deviation from the mean $= 0·39$ division $= 0·00522$ mgm.

A and *B* had here been cleaned and *B* readjusted by scraping. A small vessel containing calcium chloride was put inside the balance to dry the air. This improved the action of the clamp, diminishing the cohesion.

VI. *June* 17. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
R	210·4 214·3 210·7	212·42	—	—
L	238·4 233·3 238·1	235·77	212·93	22·84
R	209·5 217·0 210·3	213·45	236·17	22·72
L	246·2 228·1 243·9	236·57	213·38	23·19
R	224·2 204·0 221·1	213·32	236·59	23·27
L	233·7 239·1 234·6	236·62	213·53	23·09
R	207·6 219·4 208·6	213·75	—	—

Mean R − L = 23·02 divisions.

$$\therefore\ 1 \text{ division} = \frac{0{\cdot}3282}{23{\cdot}02} = 0{\cdot}01425 \text{ milligramme.}$$

Determination of $(B + X) - (A + Y)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$B + X$	216·9 214·6 216·6	215·67	—	—
$A + Y$	256·7 233·8 253·9	244·55	216·08	28·47
$B + X$	220·1 213·7 218·5	216·50	244·90	28·40
$A + Y$	254·8 236·7 252·8	245·25	217·07	28·18
$B + X$	221·5 214·5 220·1	217·65	245·48	27·83
$A + Y$	257·5 235·3 254·8	245·72	217·37	28·35
$B + X$	220·9 214·0 219·5	217·10	245·68	28·58
$A + Y$	246·8 244·5 246·8	245·65	216·83	28·82
$B + X$	223·7 210·5 221·6	216·57	—	—

Mean $(B + X) - (A + Y) = 28·38$ divisions $= 0·4043$ milligramme.

Greatest deviation from the mean $= 0·55$ division $= 0·00784$ milligramme.

VII. *June* 18. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
R	171·6 190·7 172·7	181·42	—	—
L	210·3 225·3 212·4	218·32	180·47	37·85
R	189·2 171·7 185·5	179·52	217·54	38·02
L	223·2 210·8 222·3	216·77	179·04	37·73
R	185·0 173·2 182·9	178·57	216·17	37·60
L	220·2 211·3 219·5	215·57	178·22	37·35
R	189·1 168·8 184·8	177·87	—	—

Mean R − L = 37·71 divisions.

$$\therefore \ 1 \text{ division} = \frac{0{\cdot}3282}{37{\cdot}71} = 0{\cdot}00870 \text{ milligramme.}$$

Determination of $(B+Y)-(A+X)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$B+Y$	208·5 221·7 209·5	215·35	—	—
$A+X$	246·9 214·4 244·1	229·95	216·22	13·73
$B+Y$	219·2 215·6 218·0	217·10	230·91	13·81
$A+X$	240·8 224·0 238·7	231·87	217·12	14·75
$B+Y$	226·1 209·4 223·7	217·15	232·24	15·09
$A+X$	221·8 242·3 224·1	232·62	216·96	15·66
$B+Y$	211·5 221·8 212·0	216·77	232·21	15·44
$A+X$	219·0 243·0 222·2	231·80	—	—
$B+Y$	208·3 229·0 209·9	219·05	This sudden change of resting-point must be due to some irregular disturbance. It is therefore rejected. It was slowly returning to nearly its former values	

Mean $(B+Y)-(A+X) = 14·75$ divisions $= 0·12831$ milligramme.

Greatest deviation from the mean $= 1·02$ division $= 0·0089$ mgm.

The great difference between the result here and that in series V is probably due to deposit of dust. The new mirrors had to be fixed up just before the experiment began, and the doors were open for some time. At the conclusion of the weighing I found a good deal of dust on the weights.

VIII. *June* 19. *Determination of* 1 *Scale-Division.*

Rider on beam	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference due to R − L
L	235·8 222·3 233·3	228·42	This is so much higher than the rest, probably through being observed soon after I entered the room, that it is rejected	
R	197·0 181·2 193·9	188·32	—	—
L	228·2 222·7 228·0	225·40	188·24	37·16
R	197·4 180·7 193·9	188·17	225·46	37·29
L	233·6 218·0 232·5	225·52	187·63	37·89
R	191·7 183·3 190·1	187·10	225·28	38·18
L	230·2 220·3 229·4	225·05	187·35	37·70
R	193·8 182·5 191·8	187·60	—	—

Mean R − L = 37·64 divisions.

$$\therefore\ 1 \text{ division} = \frac{0{\cdot}3282}{37{\cdot}64} = 0{\cdot}00872 \text{ milligramme.}$$

Determination of $(B+X)-(A+Y)$.

Weight in pan	Extremities of oscillations	Resting-point	Mean of preceding and succeeding resting-points	Difference
$A+Y$	245·3 238·7 244·2	241·72	In one observation not recorded just before this the clamp had been loose, and the scale-pan had slipped, and the resting-point was thereby changed. The disturbance had apparently not subsided when this was taken, it is therefore rejected	
$B+X$	193·0 185·7 191·4	188·95	—	—
$A+Y$	226·6 243·3 229·2	235·60	188·25	47·35
$B+X$	182·2 192·8 182·4	187·55	235·22	47·67
$A+Y$	229·4 239·2 231·6	234·85	187·72	47·13
$B+X$	194·4 182·5 192·2	187·90	234·71	46·81
$A+Y$	239·8 229·7 239·1	234·57	187·75	46·82
$B+X$	194·7 181·6 192·5	187·60	234·51	46·91
$A+Y$	230·6 237·4 232·4	234·45	187·50	46·95
$B+X$	186·0 189·0 185·6	187·40	—	—

Mean $(B+X)-(A+Y) = 47·09$ divisions $= 0·4100$ milligramme.
Greatest deviation from the mean $= 0·58$ division $= 0·00506$ mgm.

Summary.

Series		Mgms.	Greatest deviation from mean in milligrammes	
I	$(B+X)-(A+Y)=$	·0718	·0099	$A=B+\cdot 0446$ mgm.
II	$(A+X)-(B+Y)=$	·1614	·0062	
III	$(A+X)-(B+Y)=$	·1732	·0057	$A=B+\cdot 0116$ mgm.
IV	$(B+X)-(A+Y)=$	·1500	·0224	
V	$(B+Y)-(A+X)=$	·0060	·0052	$B=A+\cdot 2051$ mgm.
VI	$(B+X)-(A+Y)=$	·4043	·0078	
VII	$(B+Y)-(A+X)=$	·1283	·0089	$B=A+\cdot 2691$ mgm.
VIII	$(B+X)-(A+Y)=$	·4100	·0051	

The greatest error—that is the greatest deviation of any one value from the mean of its series—in the first four series is $\frac{1}{20000000}$th of a pound. The greatest error in the four series V—VIII is $\frac{1}{50000000}$th of a pound.

3.

ON A DETERMINATION OF THE MEAN DENSITY OF THE EARTH AND THE GRAVITATION CONSTANT BY MEANS OF THE COMMON BALANCE.

[*Phil. Trans.* A, **182**, 1892, pp. 565 656.]

[*Received* May 13. *Read* June 4, 1891.]

I. Account of Apparatus and Method.

In a paper printed in the *Proceedings of the Royal Society*, No. 190, 1878 (vol. 28, pp. 2–35)*, I gave an account of some experiments undertaken in order to test the possibility of using the Common Balance in place of the Torsion-Balance in the Cavendish Experiment. The success obtained seemed to justify the intention expressed in that paper to continue the work, using a large bullion-balance, instead of the chemical balance with which the preliminary experiments were made.

As I have had the honour to obtain grants from the Royal Society for the construction of the necessary apparatus, I have been able to carry out the experiment on the larger scale which appeared likely to render the method more satisfactory, and this paper contains an account of the results obtained.

At the time I was making the preliminary experiments the late Professor von Jolly was already employing the balance for gravitation investigations (*Wiedemann's Annalen*, vol. 5, p. 112), though I was not aware of the fact. Later he published an account (*Wied. Ann.*, vol. 14, p. 331) of a determination of the Mean Density of the Earth by the use of the Balance. Still more recently Drs. Koenig and Richarz have devised a method of using the balance for the same purpose (*Nature*, vol. 31, pp. 260 and 475), and I believe that their work is still in progress. It might appear useless to add another to the list of determinations, especially when, as Mr. Boys has recently shown, the torsion-balance may be used for the experiment with an accuracy quite unattainable by the common balance. But I think that in the case of such a constant as that of gravitation, where the results have hardly as yet begun

* [*Collected Papers*, Art. 2.]

to close in on any definite value, and where, indeed, we are hardly assured of the constancy itself, it is important to have as many determinations as possible made by different methods and different instruments, until all the sources of discrepancy are traced and the results agree.

The apparatus for the experiments described in this paper was first set up in the Cavendish Laboratory at Cambridge through the kindness of Professor Clerk Maxwell. After spending some months in working at the experiment, but without much success beyond the detection of some sources of error, I left Cambridge, and ultimately the apparatus was again set up at the Mason College, Birmingham. The difficulties in carrying out the work with any approach to exactness have been far greater than were anticipated, and many times work has been begun and results have been obtained, but examination has shown them to be affected by large errors which could be traced and eliminated by further improvements in the apparatus.

At the beginning of 1890, however, the apparatus was brought into fair working order, and during the course of the year I made a number of experiments with the results recorded in this paper.

The Principle of the Experiment.

The object of the experiment, in common with all of its class, may be regarded, primarily, as the determination of the attraction of one known mass M on another known mass M' a known distance d away from it. The law of universal gravitation states that when the masses are spheres with centres d apart this attraction is GMM'/d^2, G being a constant—the gravitation constant—the same for all masses. Astronomical observations fully justify the law as far as M'/d^2 is concerned. They do not, however, give the value of G, but only that of the product GM for various members of the solar system.

To determine G we must measure GMM'/d^2 in some case in which both M and M' are known, whether they be a mountain and a plumb-bob, as in Maskelyne's experiment, the surface strata and a pendulum-bob, as in Airy's experiment, or two spheres of known mass and dimensions, as in all the various forms of Cavendish's experiment.

Knowing the gravitation constant G, we may at once find the mean density of the earth Δ. For if V be the volume of the earth—regarded as a sphere of radius R—the weight of any mass M', being the attraction of the earth on it, is

$$GV\Delta M'/R^2.$$

But if g is the acceleration of gravity the weight is also expressible as $M'g$. Equating these we get

$$\Delta = gR^2/GV.$$

Method of Using the Common Balance.

In using the common balance to find the attraction between two masses, perhaps the most direct mode of proceeding would consist in suspending a mass from one arm of a balance by a long wire, and counterpoising it in the other pan. Then bringing under it a known mass, its weight would be slightly increased by the attraction of this mass. The increase would be the quantity sought if the attracting mass had no appreciable effect before its introduction beneath the hanging mass, and if, when beneath it, the effect on the balance could be neglected. This is very nearly the principle of the method used by von Jolly, and it is that of the method used in the preliminary experiments referred to above, in which a mass of 453 grms. of lead was hung from one arm of a chemical balance (about 40 centims. beam) by a wire 1·8 metres long, and was attracted by a mass of 154 kilogrms. of lead. But the attraction to be measured was exceedingly small, rather less than 0·01 milligrm., and it therefore appeared advisable to use a much larger balance with a larger hanging mass so that the attraction might be made comparable with the weight of exactly determined riders. Other anticipations as to proportionate increase of sensibility and diminution of effect of air-currents, have hardly been justified in the way I expected, though, by the ultimate form of the apparatus, they have, I think, been more than realised.

With increase in the length of beam, a differential method became applicable, by means of which the attraction of the mass on the beam was eliminated, and the necessity for prolonging the case to allow of a long suspending wire was removed. This will be seen from a consideration of Fig. 1. Let A, B represent equal masses suspended from the two arms of the balance, and let M be the attracting mass put first under A, the position of the beam being noted. If M is then placed under B its attraction is not only taken away from A but added to B, so that the tilting of the beam is that due to nearly double the attraction to be measured. Of course there are what we may term *cross-attractions*, in the first position, of M on B, and in the second position, of M on A, but these may be allowed for in the calculations. We cannot give any mathematical expression for the attraction of M on the beam and suspending wires, owing to their irregularity of shape. But this attraction is eliminated if a second experiment is made in which A and B are raised equal known distances to A' and B'. For the *difference* between the two increments of weight on the right, is due solely to the alteration of the positions of A and B relative to M, the attraction on the beam remaining the same in each. From the observed effect of a known alteration of distance the attraction at any distance can be found.

This is, shortly, the method adopted. The arrangement was ultimately complicated by the addition of a second mass m. Originally the mass M

was alone on a turn-table which revolved about a vertical axis immediately under the central knife-edge of the balance. And some experiments which I made led me to suppose that mere change of position of the mass did not affect the level of the balance. However, after a complete determination in 1888 of the mean density, when I supposed that the work was finished, an examination of the results showed some curious anomalies, which I could only ascribe to a tilting of the whole floor on the displacement of the mass. Making new tests as to the effect of removal of the mass, I found that the

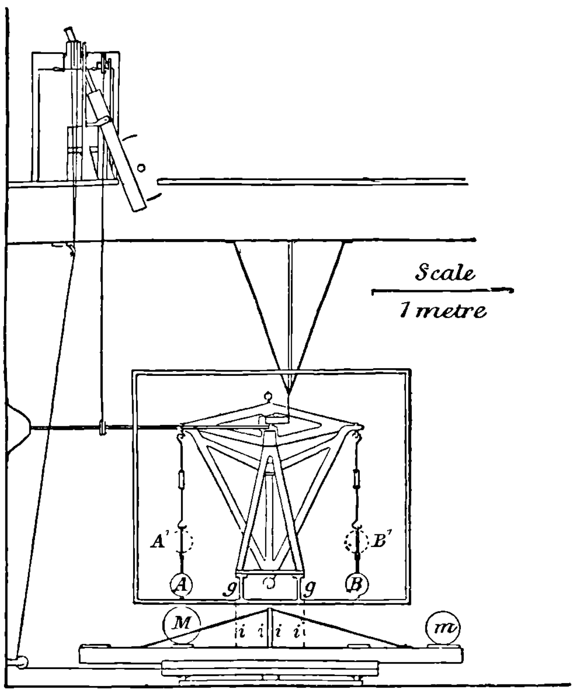


Fig. 1. Elevation of balance-room and observing-room. The front of the case is removed, and the front pillar is not shown. The pointer and mirrors are at the back.

previous tests had been quite wrong in principle, and that there was a very appreciable effect quite visible in the telescope when the masses A and B were removed, and M was removed from one side to the other, the slope of the floor changing by an angle comparable with a third of a second. If this had been absolutely constant in amount, the differential method would have eliminated it; but, probably, it varied slightly in successive motions of the turn-table, and the results showed that there was also a secular change, the amount of tilt gradually increasing. This secular change was probably due to increasing rigidity of the floor, so that it tilted over bodily, moving the supports of the balance with it, an increase partly due, perhaps, to the pressure of the building, which had only been erected ten or twelve years, but chiefly, I think, to a gas-engine recently erected next door. When this was doing heavy work, the vibrations were very plainly felt, and no doubt they greatly aided the floor in 'settling down.' A second balancing mass m was therefore added, half as great as M, and on the opposite side of the turn-table, but twice as far from the axis. The resultant pressure was now always through the axis, and I could detect no tilting of the floor when the turn-table was moved. Of course the balancing mass acted somewhat to reduce the effect of the larger attracting mass, but in a calculable ratio.

Finally, in order to eliminate or reduce the effect of any want of symmetry in the moving parts or in the masses, a second set of experiments was made with all the masses turned over and moved from left to right, and the mean of the first and second sets was taken.

I now proceed to a detailed description of the various parts of the apparatus and the mode of experiment.

The Balance-Room. The balance-room is in the basement of the Mason College, immediately under my room, and about 20 metres from the street. On one side were three windows looking on to a small courtyard, entirely surrounded by high buildings, but the windows have been bricked up. On the two adjacent sides are two other rooms, and on the opposite side a closely fitting door opening on a short corridor with doors at each end. There is no chimney in the room, and only an opening in the ceiling through which the balance was observed from the room above. The floor is of brick, resting on earth, and is very firmly laid.

The temperature of the room was taken by means of a thermometer with a protected bulb at the end of a long wooden rod hanging down from the room above. The thermometer was about 6 feet from the floor, near one end of the case, and it could be rapidly pulled up into the room above and read by the observer before its temperature sensibly varied. The temperature never appeared to vary so much as 0·1 C. in the course of two or three hours.

The Balance-Case and its Supports. The case (Fig. 1) is a large cabinet of $1\frac{1}{4}$ inch wood, 1·94 metres high, 1·63 metres wide, ·61 metre deep, with three large doors in front giving access to the hanging masses and riders, and a small door at the back near the mirror hereafter described. It is lined inside and out with tinfoil, and under each of the suspended masses is a double bottom with a layer of wool between, making a total thickness of about $1\frac{1}{2}$ inches or 4 centims. At the top is a small window about 10 centims. square, through which the oscillations of the beam were observed. On each side within the case are placed three horizontal partitions, like shelves, to hinder circulation of the air.

The larger attracting mass and the attracted masses are gilded, and it is possible that some advantage may arise from having the surface of the case of different metal. For if it, too, were gilded, it would readily absorb radiation from the large mass, and when the inside temperature changed, the suspended masses would readily absorb radiation from the inner surface of the case. But gold probably absorbs considerably less of tin radiation than it absorbs of gold radiation, and so temperature changes are probably lengthened out more than if the case were gilded.

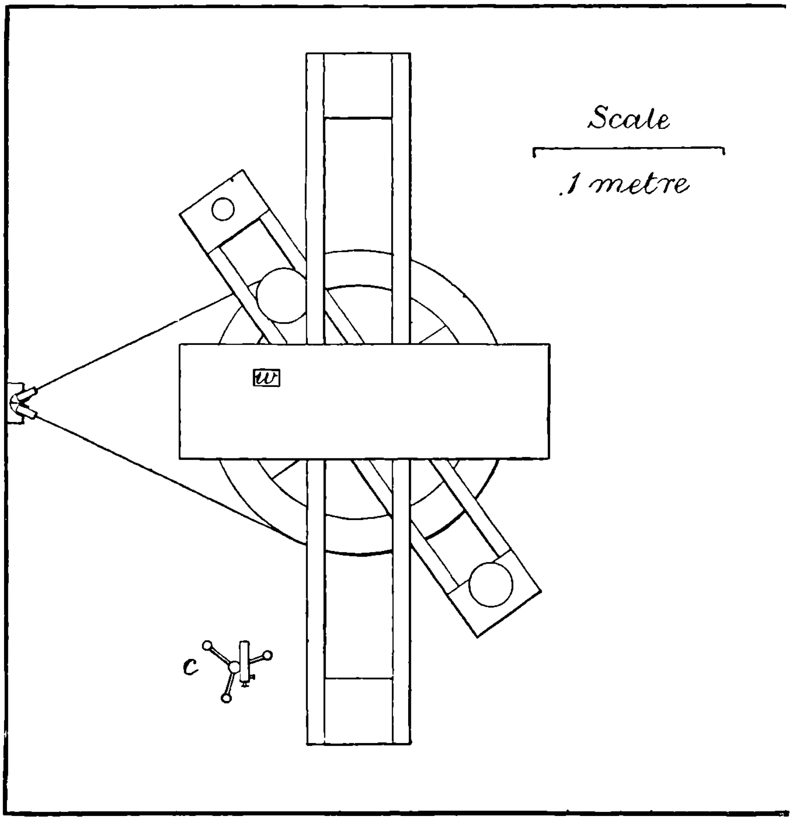


Fig. 2. Plan of turn-table, girders, pillars, and balance-case. *w*. Window in case. *c*. Usual position of cathetometer.

It was necessary to support the case so that the attracting masses could be moved about underneath it, and also to make it independent of the floor. Two brick pillars, 58 centims. × 36 centims. and 56 centims. high, were therefore built on thick beds of concrete under the floor, and about $3\frac{1}{2}$ metres apart. They rise up free from the bricked floor. Stretching between them are two parallel iron girders (g, g), about 30 centims. apart, and with their under side 56 centims. above the floor. The balance-case is placed across the middle of these girders (see plan, Fig. 2), with its under surface level with that of the girders. The square base-plate of the balance is placed on the girders on three levelling screws. Two horizontal screws attached to the girders bear against each edge of the base-plate, so that it can be adjusted and fixed in any position.

To lessen vibration one tier of bricks is removed from each pillar, and in its place are inserted eight cylindrical blocks of indiarubber (*i*, *i*, Fig. 1), originally 7·5 centims. diameter and 7·5 centims. high. These crushed down almost 1 centim. at once, but have not shown any further measurable contraction in the course of several years. Their effect in deadening vibration has been surprisingly great.

The Turn-table. On a bed of concrete, and quite free from the brickwork of the floor, is a circular rail of cast iron, 1·3 metres in diameter. On this, on conical brass wheels and pivoted at the centre, runs the turn-table, about 1·5 metres in diameter. This is made of wood and covered with tinfoil. It is like a wheel with a flat circular rim, and with four flat spokes arranged as a cross. It is as nearly symmetrical as possible, and at opposite ends of a diameter are placed two shallow cups, in either of which the large attracting mass may rest. The centres of these cups are a distance apart, equal to the length of the balance-beam. There are cut slots through the bottom of each cup, so that the bottom of the mass can be seen for the purpose of measuring the vertical diameter.

Two beams, 2·74 metres long, run across the turn-table 26 centims. apart, with the cups between them, and across the ends are two boards, each with a circular hole 12 centims. in diameter, and in either of these the smaller, or balancing mass, may rest. These beams are braced by brass rods to brass uprights at their middle points to diminish bending.

The turn-table is moved by an endless gut rope passing round it, and fixed at one point of the rim. The two sides of the rope pass over pulleys on to a drum in the room above. There are stops on the circular rail, against which come brass pieces on the turn-table when the masses are in position at either end of the motion. The drum can be turned easily by the observer at the telescope. Since the knife-edges and planes of the balance are of steel, all other moving parts of the apparatus were made free from iron. As an illustration of the necessity of this, I may mention that for some time I used what I supposed to be a brass wire rope to move the turn-table, but on looking out for the explanation of some irregularities, I found that the brass was wrapped round a core of steel wire, which acquired poles at the highest and lowest points in the position in which it always rested between different sets of weighings. These poles had quite an appreciable action on the balance-beam.

The Balance. This is of the large bullion-balance type, with gun-metal beam and steel knife-edges and planes. It was made specially for the experiment by Mr. Oertling, with extra rigidity of beam. Its performance has shown the great excellence of the design. The central knife-edge is supported on a steel plane by a framework rising 107 centims. above the base-plate, and the usual moveable frame can be raised or lowered from outside the case, fixing the beam or setting it free to oscillate. The beam has often been left free to oscillate for months at a time, with the full load of 20 kilogrms. on each side, but I have no reason to suppose that the knife-edges have suffered at all.

The length of the beam was measured by taking the length of each half separately by a beam-compass, and the mean of several measurements gave 123·329 centims. as the total length. The standard scale used throughout was that of a cathetometer made by the Cambridge Scientific Instrument Company. This scale has been verified at the Standards Office, and taking its coefficient of expansion as $\frac{1}{60000}$, it may be regarded for our purpose as perfectly correct at 18°, any errors being at that temperature much less than the errors of experiment. Comparing the beam-compass with this scale, it was found that ·06 centim. must be subtracted, reducing the length to 123·269 centims. Now both beam and scale are of gun-metal and may, therefore, without serious error, be assumed to have the same coefficient of expansion, so that this is the length of the beam at 18°. At 0° it is 123·232 centims.

Mirrors, Telescope, and Scale. At first a mirror was attached to the centre of the beam and the reflection of a scale in it was observed, either in the ordinary method or in the method described in the former paper (*Roy. Soc. Proc.*, vol. 28, 1879)*, where a second fixed mirror is used to throw the ray of light a second, or even a third time back on to the moving mirror, each return increasing the deflection of the ray. But it was then necessary to make the time of vibration very long, and even when the time was three minutes, the tilt due to the attraction, i.e. the change of resting-point, did not amount to more than two or three scale-divisions. Now certain irregularities observed when the apparatus was first set up at Cambridge, led to experiments on the time taken by heat to get through the case in sufficient quantity to affect the balance, and I found that a coil of copper wire placed close under the case on one side (the bottom of the case being then solid, 1 inch thickness), heated by a current yielding 100 calories per minute, began to produce an appreciable disturbance on the balance in about 10 minutes, doubtless by the creation of air-currents from the heated floor of the case. It appeared advisable, therefore, to reduce the time of a complete experiment to less than this if possible, and, consequently, the time of a single swing very much below 3 minutes. This could only be done if at the same time the optical sensibility were very greatly increased.

The employment of what may be termed the double-suspension mirror method due I believe to Sir William Thomson, and used by Messrs. G. H. and

* [*Collected Papers*, Art. 2.]

Horace Darwin in their experiments on the Lunar Disturbance of Gravity (*Brit. Assoc. Rep.*, 1881), has very satisfactorily solved the problem, giving a greatly increased deflection on the scale, even when the time of oscillation is as short as twenty seconds.

This method, which deserves to be more generally known and applied for the detection of small motions, consists in suspending a mirror by two threads,

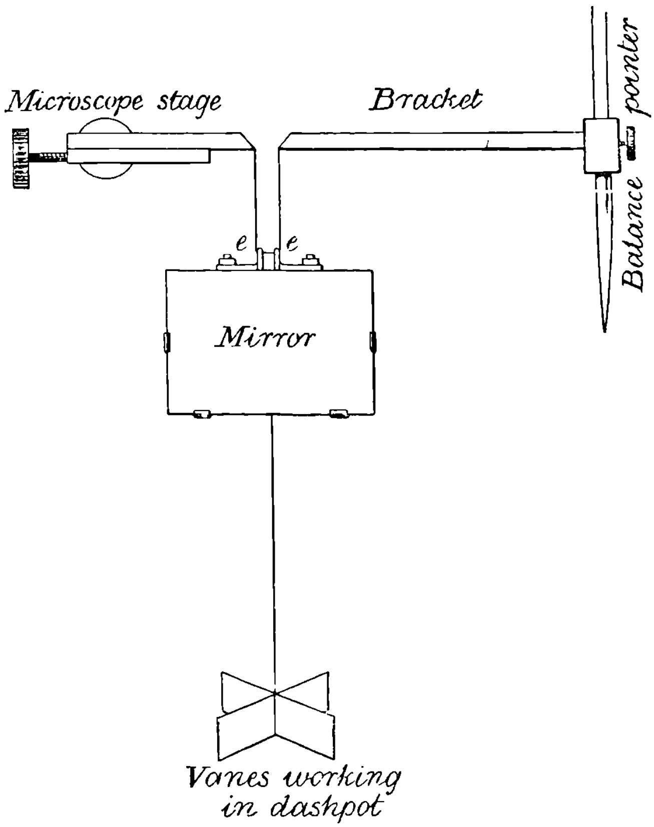


Fig. 3. Double-Suspension Mirror (half size).

one from a fixed point, the other from the point which moves. The angle through which the mirror turns for a given motion of the latter point is inversely as the distance between it and the fixed point, so that by diminishing this distance the sensibility of the arrangement may be almost indefinitely increased.

To apply it to the balance, a small bracket (Fig. 3) is fixed to the ordinary pointer of the balance, about 60 centims. below the central knife-edge. This projects horizontally at right angles to the axis of the beam, and it is bevelled at the edge. Close to it is another bevelled edge attached to a microscope-stage movement which is fixed on to the central pillar of the balance. A thread of silk (as supplied for the Kew magnetometer) is fastened to the stage, passes over the bevelled edge, through two eyes (e, e) on a light frame holding the mirror, up over the bevelled edge of the bracket, and is fastened to the bracket. The microscope-stage movement allows the distance between the threads to be adjusted, and also enables the azimuth of the mirror to be altered.

Of course, if the mirror were weightless, it would not affect the sensibility of the balance, and the threads might be brought very close together. But the weight of the mirror—it is silver on glass, 56 millims. × 38 millims. × 10 millims.—has a considerable effect on the sensibility, diminishing it with decrease of distance between the points of suspension. In practice it has been found convenient to work with the threads parallel, and from 3 to 4 millims. apart, the time of swing one way being adjusted to about 20 seconds. A less time hardly suffices for a correct determination and record of the scale reading. Taking 4 millims. as the distance, and supposing the bracket to be 600 millims. below the knife-edge of the balance, the mirror evidently turns through an angle 150 times as great as that through which the beam turns.

The drawback to this method of magnification is that the mirror has its own time of swing and is easily disturbed. The swings of the mirror and the disturbances are, however, effectually damped by having four light copper vanes attached to the end of a thin wire, projecting down from the mirror and working in a dashpot with four radial partitions not quite meeting in the centre, one vane being in each compartment. I found that mineral lubricating oil is very suitable for the dashpot, as the surface keeps quite clean and there is little evaporation. The swings of the balance are also very greatly damped by this arrangement, but the effect of this will be discussed later.

The telescope and scale are in the room over the balance-room (see Fig. 1), a hole being cut through the floor, and a small glass window being fixed in the top of the case. As the suspended mirror is in a vertical plane it is necessary to have an inclined mirror fixed in front of it to direct the light from the scale horizontally on to it and back again to the telescope. With the magnification used it was necessary, for good definition, to have an exceedingly good inclined mirror, and several were rejected before a suitable one was obtained. That finally used is a silver on glass oval mirror, 60 millims. × 40 millims., by Browning. The glass window in the case is optically worked and carefully adjusted to be normal to the path of the light.

The telescope has a 3-inch object-glass of about 4 feet focal length. It is fixed on a brick pillar, on one of the brick arches which form the ceiling of the balance-room, and it rises free from the floor of the observing-room. To destroy vibration one course of bricks is replaced by blocks of india-rubber. The scale has 50 divisions to the inch (say $\frac{1}{2}$ millim.), ruled diagonally, and divided to tenths by cross-lines. It is photographed on glass from a scale drawn on paper with very great care, 50 inches long (say 127 centims.), and with 500 divisions. The photograph is $\frac{1}{10}$th of this length, and only the central part of the scale, about 60 divisions in length, has been used. The diagonal ruling enables a tenth of a division to be read with certainty, and the readings recorded in the Tables, pp. 98–128, are in tenths. Though the lines appear somewhat coarse, I have not been able to find another scale equal to it in distinctness and in ease of reading. As all the results depend on the ratio of measurements, taken almost simultaneously, of deflection due to attraction and rider respectively, in the same part of the scale, I have not thought it necessary to calibrate it.

The scale is fixed horizontally on the end of the telescope close to the object-glass with a piece of ground glass over it. It was illuminated in general by an incandescent lamp placed above it, once by an Argand burner.

The distance from the scale to the mirror and back is about 5 metres. It follows that 1 division of the scale corresponds to an angular motion of the mirror through ·0001 radian. But this is at least 150 times the angle through which the beam turns for the same deflection. So that 1 scale division implies an angular motion of $\cdot 000000\dot{6}$ radian, or $\frac{2}{15}''$, in the beam. As the total length of swing in Table III is never more than 12 divisions, the angular vibrations of the beam are at the most about $1''\cdot 6$, and the linear vibrations of the masses, since the half beam is about 60 centims., are at the most about ·005 millim. This shows that it is quite unnecessary to consider any change of distance due to vibration. The greatest deviation from the mean in any of the series of weighings recorded is about 1 per cent. of the rider-value, corresponding to about $\frac{1}{10}$th of a division, or an angle of $\frac{1}{75}''$ in the beam, and a distance of ·00004 millim., say $\frac{1}{600000}$ inch, in the motion of the masses. This seems to show that the method is accurate as well as sensitive.

Determination of the Value of the Scale-Divisions by means of Riders. This was done by means of centigramme-riders (Fig. 4), these being the least weights which appeared capable of sufficiently accurate determination. Instead of transferring the same rider from point to point, it was much easier to use two equal riders, and to take one up while the other was being let down a given distance from it. The distance selected was about 2·5 centims., since the deflection due to the transfer of one centigramme so far along the beam was nearly equal to that due to the greatest attraction to be measured.

At first the riders when on the beam rested in V-notches in a pair of parallel brass strips fixed on and parallel to the beam. But this plan was soon abandoned, as there was no certainty about the position of the rider in the notches. The riders were then supported in little wire frames, each hung by two cocoon-fibres from the edges of a plate fixed to the beam, the edges being parallel to the central knife-edge. The only objection to this method was the very considerable time spent in replacing the fibres after the breakages which occurred on dusting or any readjustment of the balance.

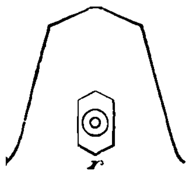


Fig. 4. Rider, actual size, and end of lifting-rod, *r*.

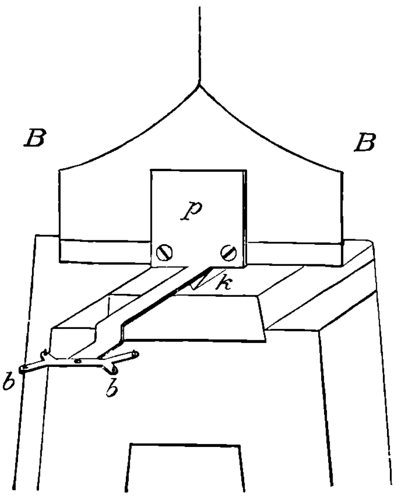


Fig. 5 *a*. Subsidiary rider-beam, *bb*, attached to centre of balance-beam, *BB*, by plate *p* just above central knife-edge, *k* (half size).

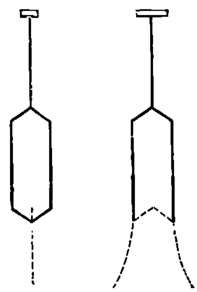


Fig. 5 *b*. Wire frames depending like scale-pans from ends of *bb*, Fig. 5 *a*, side and end views (half size).

Ultimately a small subsidiary beam, about 2·5 centims. long, was attached to the centre of the balance-beam just above the knife-edge (Fig. 5*a*), the scale-pans being represented by small wire frames in which the riders could rest (Fig. 5*b*). These frames depend from agate pieces resting on steel points at the extremities of the subsidiary beam in the way now usually adopted in delicate assay-balances. This mode of supporting the riders appears to be perfectly satisfactory.

To raise or lower the riders two short horizontal lifting-rods parallel to the beam move up and down within the supporting wire frames with a nearly parallel motion, and on them are two metal pieces with their upper surfaces shaped so that the riders rest on them without swinging (Fig. 4, *r*). They are the extremities of L-shaped projections from a jointed parallelogram-framework (Fig. 6), supported on an upright in front of the subsidiary beam.

The framework is moved by a tongue engaging with it, and projecting from a horizontal rod, which rotates about its axis in bearings, one within the case and the other outside. The rod is turned through an angle of about 30 between stops by an endless string passing upwards and round a wheel in the observing-room.

The parallelogram-framework and the bearing of the rotating rod within the case are both supported, independently of the case, from the ceiling. At first they were supported respectively on the central pillar of the balance and on the case; but when the increase of optical sensitiveness enabled me to detect small irregularities, I realised how essential it was for accurate weighing that all parts of the apparatus moved from the outside should be supported quite independently of the balance. Even the string moving the

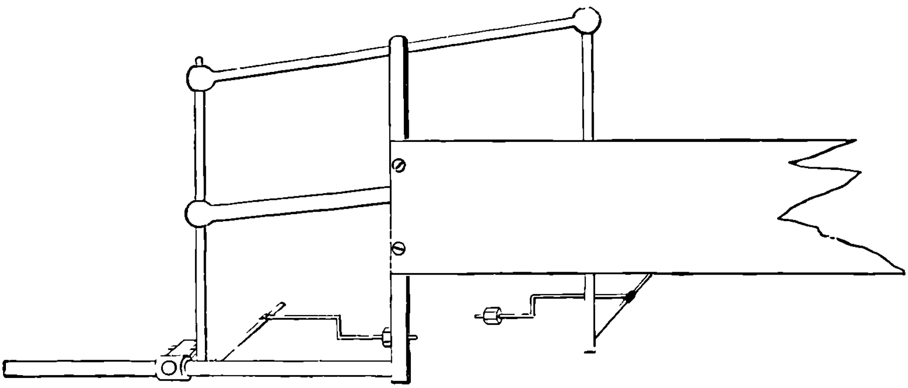


Fig. 6. Lifting-rods to raise or lower riders (half size).

rod transmitted great and continual vibration. The rod and the framework with the lifting-levers were, therefore, supported by iron rods coming down from the ceiling through holes in the top of the case, large pieces of cardboard stretching from these rods over the holes to hinder the passage of dust into the case. Once or twice in the course of preliminary experiments irregularities were traced to accidental contact of outside bodies with the case.

It appeared just possible that there might be electrification of the riders by friction with the lifting-rods, especially when they were supported by cocoon-silk. It was, therefore, advisable that the surface of the lifting-rods should be of the same kind as that of the riders. As the latter are silver wire gilded, the lifting-rods are also gilded. It may not be uninteresting to note here a curious phenomenon which occurred during some early preliminary experiments. The shaped pieces on the lifting-rods were then of wood covered with gold leaf, put on with ordinary paste. After they had been on for some months, I obtained some very various results for the deflection due to the riders, and on examining the lifting-rods I found that a number of long needle-growths projected from the wood pieces and interfered with the supporting wire frames. At first I thought these were organic, but my colleague, Professor Hillhouse, examined them and found that they were crystalline. Doubtless, the hygroscopic paste set up electric action between the gold leaf and the brass to which the wood pieces were attached, and the crystals were probably zinc sulphate. The wood was then replaced by brass gilded, and no further difficulty of the kind was experienced.

The length of the subsidiary beam was kindly determined for me by Mr. Glazebrook at the Cavendish Laboratory. The steel points are hardly sharp enough to determine the distance to 1 in 10,000, but the mean of the results is sufficiently exact. The following are Mr. Glazebrook's determinations; the four points being denoted by a, b, c, d:

Date	Temperature	Number of readings	a to b	Number of readings	c to d
	° C.		inches		inches
1889 July 4	22·5	6	·9985	6	·9979
July 11	21·5	3	·9990	3	·9982
July 12	23	3	·9988	3	·9979

These are in terms of a gun-metal standard of which the error is only 3 in 100,000 at 0 , and, therefore, for my purpose negligible. The beam is of brass, and we may assume with sufficient exactness that it has the same expansion as the standard. The temperature may, therefore, be left out of account. The mean value of $\frac{1}{2}(ab + cd)$ is therefore ·9983375 inch, or taking 2·539977 centims. to the inch we obtain

Length of beam at 0°, 2·53575 centims.

There is an advantage in fixing this beam at the centre, which should be noted here. Suppose the riders are not quite equal, but have values w and $w + \delta$. Let the two ends of the subsidiary beam be distant a and $a + l$ from the central knife-edge. Then the effect of picking up the rider w from the nearer, and letting down the rider $w + \delta$ on the further end, is equivalent to putting at unit distance

$$(w + \delta)(a + l) - wa = wl + \delta(a + l) = wl\left(1 + \frac{\delta}{w}\frac{a + l}{l}\right),$$

or the error $\delta\, w$ is multiplied by $(a + l)/l$, and, if the beam is not central, $(a + l)\, l$ may be greater than 1, so that the error is magnified.

If, however, the small beam is central, l is equal to $- 2a$, and the error is multiplied by $+ \frac{1}{2}$.

If the riders are interchanged and the weighings are then repeated, the mean result is the same as if riders with the mean value were used, for

$$w(a + l) - (w + \delta)a = wl - \delta a = wl\left(1 - \frac{\delta}{w}\frac{a}{l}\right),$$

and the mean of this and the above is

$$\left(w + \frac{\delta}{2}\right) l.$$

The Attracting and Attracted Masses. These are all made of an alloy of lead and antimony, for the sake of hardness, the specific gravity in each case being about 10·4. They were made at various times and places, the large attracting mass M being made more than 12 years ago by Messrs. Storey, of Manchester. The smaller balancing mass m was made in 1889 by Messrs. Heenan, of Manchester and Birmingham. These were both cast with a 'head' on, and then turned. The attracted masses A and B were made by Messrs. Whitworth, and subjected to hydraulic pressure before turning. The dimensions have been measured from time to time, and there is no evidence of any sensible change of shape.

The larger mass M and the attracted masses A and B were weighed at the Mint through the kindness of the Deputy Master and Professor Roberts-Austen. For the weight of the balancing mass m, I am indebted to Messrs. Avery, of Birmingham. The large mass M has suffered two accidents since it was weighed, once being slightly cut into by a saw during some alteration of the case, and once being scratched by coming into contact with a piece of metal fixed to the turn-table in taking it out of its place. The saw-cut was carefully filled in with lead, and the scratch removed only a fraction of a gramme, as was determined by taking a mould of the hollow. I should be glad to think that the determination of the attraction was sufficiently exact to make reweighing necessary, but I am afraid that the alteration in weight is very far beyond the important figures, and I therefore take the original weight as sufficiently near the truth. The masses A and B have been gilded since the original weighing, but I carefully determined their increase of weight by the balance used in the gravitation experiment.

The values given below in the second column are the true masses. In the third column are the masses of M and m, less the air displaced by them, this being taken as 18·41 and 9·2 grms. respectively. It will be shown later that the true masses of A and B and the reduced masses of M and m may be used in the calculation of the result.

	True mass in grammes	Mass less that of air displaced
M	153407·26	153388·85
m	76497·4	76488·2
A	21582·33	
B	21566·21	

Suspension of the Attracted Masses. Each of the attracted masses is drilled through along a diameter, the hole being ·6215 centim. in diameter, and a brass rod (Fig. 7) terminating in an eye e below, is passed through the hole. The mass is secured in position by a nut n working in a screw-thread cut for a short distance in the rod. An exactly similar rod terminating in a similar eye e', and with a similar nut n', is fastened end to end to this by a union u. The nuts and the inner sides of the enlargements for the eyes are hollowed out so as to fit exactly on to the spheres.

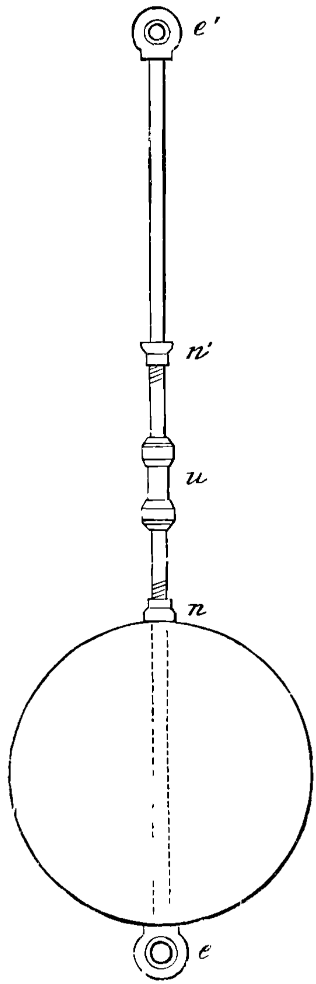


Fig. 7. Suspender for Attracted Mass (one-fourth size).

From the ends of the balance beam hang down stout brass wires terminating in hooks. If these hooks are passed through the eyes e' the attracted masses are close to the floor of the balance-case, and their centres are adjusted to be about 32 centims. from the centre of the large attracting mass when under either of them. If the masses are turned over so that the hooks pass through the eyes e, they are about 30 centims. higher or at nearly double the distance, the length ee' being about 48 centims. The rods being perfectly symmetrical about the union u, the attraction on them is the same in either position. The weight of each is about 212 grms., or about $\frac{1}{100}$ of the attracted mass, so that any small variation in their position would produce a negligible variation in the total attraction. By the differential method, the attraction on them entirely disappears from the results.

The Mode of Support of the Attracting Masses M and m. This has already been described when describing the turn-table.

The Riders. Four centigramme-riders, A, B, C, D, of silver wire gilt were made by Mr. Oertling of the form shown in Fig. 4. These were weighed in 1886 at the Bureau International des Poids et Mesures, by M. Thiesen. The following is an extract from the certificate:

'*Densité et volume.* Comme densité on a accepté celle de l'argent, et par conséquent comme volume de chacun des cavaliers, 0·0010 millilitre.

'*Détermination des poids des cavaliers.* L'étude des poids de ces quatre cavaliers a été faite par M. le Dr. Thiesen, adjoint du Bureau International, chargé de la section des pesées. M. Thiesen au moyen de la balance Stüchrath, destinée à des poids au dessous du gramme, a d'abord déterminé les différences entre les quatre cavaliers pris deux à deux dans les six combinaisons possibles, et ensuite la différence entre l'ensemble des quatre cavaliers et le poids de 40 milligrms. de la série 0 du Bureau, série en platine iridié récemment étalonnée par M. Thiesen. Les comparaisons ont été faites du 19 au 29 Mars, 1886.

'*Résultats.* De l'ensemble de ces comparaisons résultent les poids:

$$\begin{aligned} A &= 10{\cdot}1247 \text{ milligrms.} \\ B &= 10{\cdot}0615 \quad \text{,,} \\ C &= 10{\cdot}1196 \quad \text{,,} \\ D &= 10{\cdot}1262 \quad \text{,,} \end{aligned}$$

'L'incertitude de ces déterminations ne dépasse pas 0·001 milligrm.'

A and D were selected for use as being the nearest to each other in value. B and C were kept untouched in boxes till 1890. In the various experiments made between 1886 and the final weighings, A and D had necessarily been handled to some extent, especially through the frequent breaking of the silk fibre suspension used before the subsidiary beam described above, and it appeared possible that their weights might be altered. It was also necessary to determine whether an appreciable amount of dust was deposited on them in the course of several weeks as it was inconvenient to dust them frequently. The riders B and C might be assumed to have the same weight as in 1886, and could be taken as standards.

To make the weighings a 16-inch chemical balance was arranged with a double-suspension mirror on exactly the principle already described for the large balance. The apparatus was put together quickly with materials at hand, and might easily be greatly improved. It is only described here to show how accurate the method is, even with such rough apparatus, and that it is applicable to a small as well as a large balance.

A cork sliding on the pointer with a horizontal needle stuck in it, served to support one thread of the mirror; a stand with a projecting arm one made to hold platinum wires in a Bunsen flame—served to support the other thread. A wire with a small copper vane depended from the mirror and was immersed in an oil dashpot. The telescope and a millimetre-scale were on a level with the mirror about 2 metres distant on one side of the balance. Two brass strips, parallel to each other and the beam, were fixed on the top of one arm of the beam, and in each of these were two V-notches in which centigramme-riders could rest. Two levers, worked by cams on a rod rotated by the observer, picked one rider up and let down the other, so that the effect was equivalent to the transfer of 1 centigrm. from one notch to the other. Their distance apart was such that this was equivalent to the addition of

·3284 milligrm. to one pan of the balance. This was the arrangement described in my former paper. Attached to one pan was a pair of brass strips parallel to each other, and such that the riders A, B, C, or D, would just rest across them. Two lifting-rods worked up and down between these strips, so that of the two riders to be compared, one could be picked up at the instant the other was let down. The lifting-rods were worked by a rod rotated by the observer and supported quite independently of the balance, and of the slab on which it rested. By this plan the value of the scale-divisions and the shifting of the centre of swing on changing the weights to be compared, could all be determined without raising the beam of the balance between the successive weighings, an essential condition, I believe, for exact work.

The weighings were made in the large room of the Physical Laboratory, and no precaution was taken to protect the balance-case beyond placing a board in front of it. The room is draughty and subject to great variations of temperature, so that the weighings were made under very disadvantageous circumstances. One result of this was a rapid and sometimes very great change of resting-point in the course of a few hours, so that the scale passed out of the field of view. In order to bring it back without opening the case, two glass tubes passed through the top of the case, almost down to the scale-pans, and small bits of wire could be dropped through these on to either pan as needed. Caps fitted on to the tubes to prevent draughts. This plan appears worthy of mention, as it suggests a mode of determining the value of a scale-division when a balance is either too sensitive for riders or has no special arrangement for their accurate use. If a piece of wire weighing, say, 1 milligrm. is cut into say ten nearly equal parts, and if these are dropped on to the two pans alternately the shiftings of the centre of swing will be to and fro, about equal distances, due to about ·1 milligrm., but the sum of the shiftings will be that due to 1 milligrm., and the balance at the end will be nearly in the same position as at the beginning.

The following is an abstract of the comparisons of the riders. They were made soon after the first determinations of attraction on February 4, when A and D had not been dusted for three months.

In each case three extremities of swing were observed, and the centre of swing was determined from these by the graphic construction described later (p. 72).

The centres of swing were combined in consecutive threes in the usual way to give the differences in scale-divisions.

Thus, in the first series, the successive centres of swing with D and A alternately in the scale-pan were

D	A	D	A	D
231	223	217	211·9	208

whence $$(D-A)_1 = \frac{217+231}{2} - 223 = +1{\cdot}0 \text{ division,}$$

$$(D-A)_2 = 217 - \frac{223+211{\cdot}9}{2} = -0{\cdot}45 \text{ division,}$$

$$(D-A)_3 = \frac{217+208}{2} - 211{\cdot}9 = +0{\cdot}6 \text{ division.}$$

$$\text{Mean } D-A = {\cdot}38 \text{ division.}$$

Successive values of the differences alone are given below.

The time of swing one way was about 16 seconds.

February 16, 1890.

(1) Comparison of A and D, undusted.

Deflection due to ·328 *milligrm.*, 83·45, 82·45, 84·45 divisions. Mean 83·45 divisions.

$$D-A = 1{\cdot}0,\ -{\cdot}45,\ +{\cdot}06 \text{ division. Mean } {\cdot}38 \text{ division;}$$

therefore $$D = A + {\cdot}0015 \text{ milligrm.}$$

(2) Comparison of A undusted, D dusted.

Value of scale-division taken as in the last.

$$D-A = -{\cdot}5,\ +{\cdot}3,\ -{\cdot}1,\ -{\cdot}4,\ +{\cdot}25. \text{ Mean } -{\cdot}09 \text{ division;}$$

therefore $$D = A - {\cdot}0004 \text{ milligrm.}$$

February 17, 1890.

(3) Comparison of A and D, both dusted.

Value of scale-division taken as below (4).

$$D-A = -{\cdot}1,\ -{\cdot}2,\ +{\cdot}3,\ -{\cdot}3,\ -{\cdot}8. \text{ Mean } -{\cdot}22 \text{ division;}$$

therefore $$D = A - {\cdot}0008 \text{ milligrm.}$$

(4) Comparison of C and D.

Deflection due to ·328 *milligrm.*, 85·35, 85·4, 84·65. Mean 85·13 divisions.

$$D-C = +{\cdot}15,\ {\cdot}00,\ +{\cdot}05,\ -{\cdot}15,\ +{\cdot}05,\ +{\cdot}3,\ +{\cdot}05,\ -{\cdot}05,\ -{\cdot}35,$$
$${\cdot}05,\ {\cdot}35,\ {\cdot}45,\ {\cdot}50,\ {\cdot}2. \text{ Mean } {\cdot}114 \text{ division;}$$

therefore $$D = C + {\cdot}00044 \text{ milligrm.}$$

February 18, 1890.

(5) Comparison of C and D repeated.

Deflection due to ·328 *milligrm.*, 92·75, 92·3, 91·65. Mean 92·23 divisions.

$$D-C = {\cdot}35,\ -{\cdot}05,\ -{\cdot}8,\ -{\cdot}95,\ -{\cdot}1,\ +{\cdot}05,\ 0,\ -{\cdot}15,\ -{\cdot}1,\ +{\cdot}05.$$
$$\text{Mean } -{\cdot}17 \text{ division;}$$

therefore $$D = C - {\cdot}0006 \text{ milligrm.}$$

Combining this with the last, and weighting them in the ratio of the numbers of determinations in each,

$$D = C + (\cdot 00044 \times 14 - \cdot 0006 \times 10) \div 24 = C - \cdot 0000 \text{ milligrm.}$$

(6) Comparison of A and D.

Value of scale-division taken as above, ·328 milligrm. = 92·23 divisions.

$D - A$ = ·45, ·25, ·1, − ·2, − ·1, ·35, ·25, ·45, ·60, ·5, ·5, ·55, ·5, ·75, ·55, ·1, ·05, ·45, ·10, ·30, ·8, ·9, ·35, ·2, ·30, ·55, ·50, ·35, ·45, ·45.

Mean ·378 division;

therefore $$D = A + \cdot 00134 \text{ milligrm.}$$

Examining the values obtained in (1), (2), and (3), it will be seen that no trustworthy evidence is given of a difference due to dusting. Any existing difference was probably under ·002 milligrm., and since the weighings on February 4, before dusting, were made with the attracted masses in the upper position, when the attraction was only one-fourth of that on which the final results depend, we may safely neglect the effect. After this the riders were dusted more frequently, so that we may probably assume their values more constant.

The comparisons of C and D, and of A and D, in (4), (5), and (6), were made more carefully. That of A and D in (6) is much the best of the series, the air in the laboratory happening to be steadier while it was made. The range between the greatest and least values of the difference is one scale-division, or ·0036 milligrm., and the different results are grouped fairly closely about the mean.

The centres of swing and the differences are plotted in Diagram VIII (p. 136). I do not claim that these results show any remarkable accuracy when compared with those obtained at the Bureau International des Poids et Mesures, but remembering how rough the apparatus was, and how little precaution was taken to ward off air-currents, I have not the slightest doubt that, with special design of apparatus and more suitable locality, the results could be very greatly improved, and the accuracy carried far beyond anything hitherto reached. As they stand, they seem to show the value of the combination of a short time of swing with optical magnification.

The results of comparisons (4), (5), and (6), is, that if C has its Paris value, viz., $C = 10{\cdot}1196$ milligrms., then, $A = 10{\cdot}1183$ milligrms., and $D = 10{\cdot}1196$ milligrms.; whence $\frac{1}{2}(A + D) = 10{\cdot}119$ milligrms. This value may be used in calculating the result, since the riders were interchanged before Set II was taken.

The losses experienced since 1886 by A and D are respectively, by A ·0064 milligrm., and by D ·0066 milligrm., i.e., they have diminished by practically equal amounts. This was to be expected as they have probably received equal amounts of rough usage.

The substitution of the subsidiary beam for the cocoon-fibre suspension of the riders having greatly diminished the handling to which they were subjected, I have not thought it necessary to weigh them again during the work.

Linear Measurements.

In the mathematical theory it will be shown that the lengths required are those marked in Fig. 14, viz., the horizontal distances, L and l, and the vertical distances, $D_1\, D_2$, $d_1\, d_2$, $H_1\, H_2$, $h_1\, h_2$.

The Horizontal Distances. Except when estimating the moment of the rider, the distance L is really that between the verticals through the centre of M and the centre of the more distant attracted mass. But the verticals through the centre of M in each position so nearly passed through the centre of the mass above it, and, therefore, through the knife-edge from which it hung, that L was taken as equal to the length of the beam (p. 50).

The accuracy of this adjustment was secured as follows. A horizontal cross-piece was fixed on the top of each attracted mass, with two horizontal cards at its two ends, each with a portion of a circular arc on it, with radius equal to that of the large mass M, and with centre over that of the attracted mass (Fig. 8). A plumb-line was then hung just in front of the case, and the balance was moved by the horizontal screws bearing against the base-plate until the plumb-line always appeared to touch the circular arc above, when it appeared to touch the large mass below. The adjustment was not quite perfect, but the error in the worst case was probably not more than 1 millim., and certainly less than 2 millims. Such an error in the horizontal distance is negligible.

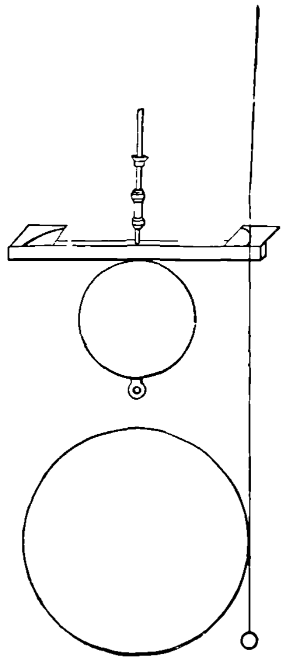


Fig. 8. Plumb-line Adjustment of Masses.

The distance l had different values for the two positions occupied by m on the turn-table. Calling these values l_1 and l_2 respectively, $l_1 + l_2$ was found by measuring a, the inside distance between M and m, arranged as in Set II, and b, the inside distance between them, when m was put on the same side of the turn-table as M, and adding to $a + b$ the sum of the diameters of M and m in the radial direction of the turn-table as taken by square calipers.

The following are the values in terms of the cathetometer-scale already referred to, the temperature being 15° C.:

$$
\begin{array}{rr}
a = & 157{\cdot}01 \\
b = & 33{\cdot}95 \\
\text{Diameter of } M = & 30{\cdot}52 \\
\text{,, \quad ,, \quad } m = & 24{\cdot}23 \\
\hline
\end{array}
$$

therefore $$l_1 + l_2 = 245{\cdot}71$$

The value of $l_1 - l_2$ was found by measuring the shortest distance of m from the wall when respectively in the first and the second positions on the turn-table. It was found that

$$l_1 - l_2 = \quad {\cdot}12$$

whence

$$l_1 = 122{\cdot}915$$

$$l_2 = 122{\cdot}795.$$

We may obtain from these measures an independent value of the radius of the circle in which the centre of M moves. With perfect adjustment this should be $\frac{1}{2}L = 61{\cdot}66$ at 18°.

It is equal to a + radius of M + radius of $m - l_2$, or, by the above measures,

$$= 157{\cdot}01 + 15{\cdot}26 + 12{\cdot}115 - 122{\cdot}795$$

$$= \quad 61{\cdot}59,$$

which is only ·07 centim. less than $\frac{1}{2}L$.

Inasmuch as the wood probably expanded less than the cathetometer-scale, while the metal expanded more, I have assumed as a rough approximation that the total expansion equalled that of the scale, so that the values of l_1 and l_2 are correct at 18° (see p. 50). No importance is, however, to be attached to this temperature-correction.

The Vertical Distances. At the conclusion of each set of weighings with the attracted masses in a given position, the vertical distances between the top of the attracting masses and the bottom or top of the attracted masses (accordingly as they were in the upper or lower position) were measured by the cathetometer already referred to.

This instrument is of the well-known design of the Cambridge Scientific Instrument Company, and is especially adapted for measuring differences of level at different distances in different vertical planes. It reads to ·002 centim. The account of these measurements will be found in Table II (p. 89, *et seq.*).

To find the distances D, d, H, h (Fig. 14), it was necessary to add to the actual distances measured the sum or difference of the vertical radii of the attracting and attracted masses, and, therefore, the vertical diameters of all the masses were measured.

For this purpose I used a cathetometer which has lately been constructed for me by Messrs. Bailey, of Bennett's Hill, Birmingham. I have to thank Mr. Potts, of that firm, for his care in its construction, and also for the trouble which he has taken in the construction and alteration of much of the apparatus used throughout the work recorded in this paper. As the cathetometer is, I believe, new in design and satisfactory in its performance, it appears worthy of description.

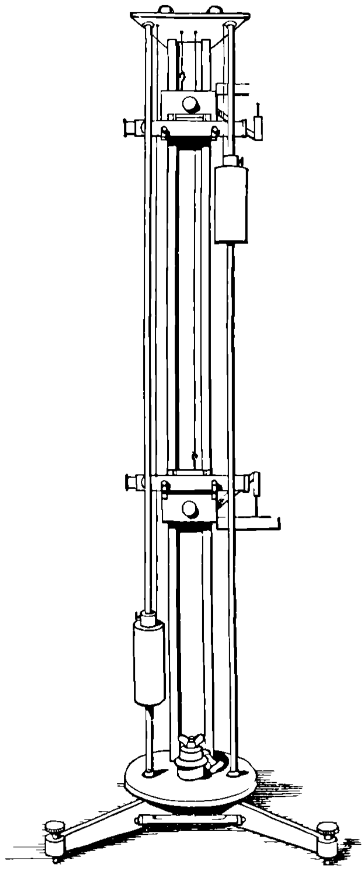


Fig. 9. Cathetometer used to measure Vertical Diameters.

The Cathetometer used to measure Vertical Diameters (Fig. 9). There are two telescopes, one to sight the upper the other to sight the lower of the points between which the vertical height is required. There is no scale on the instrument, but after the telescopes are fixed to sight the two points the instrument is turned round a vertical axis, so that the telescopes sight a vertical scale at the same distance from them as the points. In general, of course, the cross-wire will appear to lie between two divisions, but by means of the fine adjustment, to be described below, the two nearest scale-divisions are brought in succession on to the cross-wire, and by interpolation the reading corresponding to the point first sighted by the telescope is determined.

The telescopes are fixed on collars running up and down the main pillar, which has a section of the form shown in Fig. 10 (shaded).

The guides consist of three knobs, *k*, *k*, on the inside of the collar, two sliding in a vertical V-groove and one on a plane, both groove and plane being at the back of the pillar. A screw, *s*, clamps the collar in any position. Gut strings running up over pulleys and supporting counterpoises, sliding on the thinner pillars (see Fig. 9), are attached to the collars so that these move easily. At first springs were used to keep the knobs always in contact, but I found it much better to remove these and trust merely to hand-pressure to keep the collars in the proper position before clamping with the screw *s*.

The fine adjustment is secured by the use of a piece of plate-glass (g, Fig. 10), placed in the front of each object-glass and capable of rotation about a

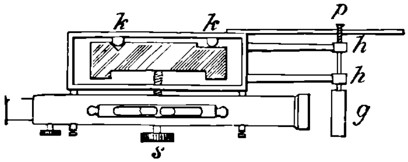


Fig. 10. Section of pillar and collar of new Cathetometer.
s, clamping screw. k, k, guiding knobs. g, glass plate for fine adjustment, turning on axis hh, with pointer at p perpendicular to plane of figure.

horizontal axis, hh. A pointer is fixed on the end of this axis at p, and at its end is a small glass plate with a scratch on it moving close against a straight scale. If the plate is initially normal to the optic axis of the telescope, on turning it through an angle ϕ, the ray which now comes along the optic axis has been shifted, by transmission through the plate, parallel to itself, a distance $t \sin(\phi - \psi) \cos\psi$, where t is the thickness of the plate and ψ is the angle of refraction within it (see Fig. 11).

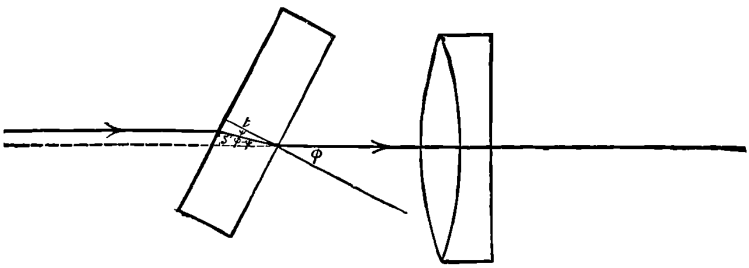


Fig. 11. Section of fine adjustment plate.

For small angles this shifting happens to be nearly proportional to $\tan\phi$, and, therefore, to the reading on the straight scale. To show how nearly this is the case the following table gives the shifting for angles of 5°, 10°, and 20 , with a thickness of $t = 1$ centim. and a refractive index $\mu = \frac{3}{2}$:

Angle ϕ	Shifting*
5	$\frac{1}{3}\tan 5\ (1 + \cdot 00042)$
10°	$\frac{1}{3}\tan 10°\ (1 + \cdot 00160)$
20	$\frac{1}{3}\tan 20\ (1 + \cdot 00526)$

The error in taking the shifting as proportional to $\tan\phi$ is, up to 20°, quite negligible in ordinary telescope-cathetometer work. If it is desirable to have greater accuracy, it is probably best to use a table of corrections to the tangent; but it is possible to get an exact scale thus:

* [These expressions are given with the wrong sign in the original paper. A slight correction has also been made in the values of the numerical coefficients. Ed.]

Let OP, Fig. 12, represent the pointer of length r, making an angle ϕ with a line MN. Let a pointer PM jointed to this at P be of length μr, and let its extremity M move on the line MN. Drawing OD at right angles to MN, if s is the shifting, we have

$$OD = OP \frac{\sin(\phi - \psi)}{\cos \psi}$$

$$= \frac{rs}{t},$$

or

$$s = \frac{t}{r} OD.$$

Probably the practical difficulties in the use of such an arrangement would render it troublesome and uncertain.

The plate is used as follows: Adjust it normal to the optic axis of the telescope, and move the telescope till the required point is brought as near to the cross-wire as is possible by the hand. Clamp the telescope, and then turn the plate till the point is exactly on the cross-wire. Read on its scale the position of the pointer attached to the plate. Repeat these operations with the other telescope on the other point, then turn the instrument about its vertical axis till the telescopes sight the vertical scale placed at the same distance away as the two points. Looking through one of the telescopes the cross-wire is in general not exactly on a division. Turn the plate so that first the nearest division above, and next the nearest division below, is on the cross-wire. Reading the position of the pointer in each case, interpolation gives us the reading on the vertical scale corresponding to the position of the pointer when the cross-wire was between the two scale divisions. Doing this for each telescope the difference between the two points is found in terms of the vertical scale.

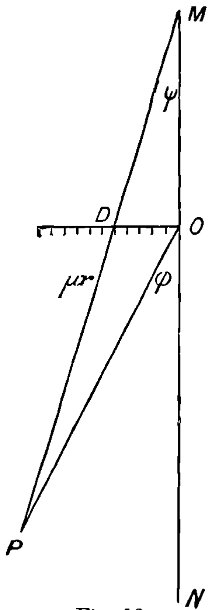


Fig. 12.

The plates I have used are about 9 millims. thick, and the pointers about 9 centims. long. They move over scales such that 25 to 27 divisions correspond to a shifting of 1 millim. The lower scale is graduated from 0 to 50, the upper from 50 to 100, to prevent confusion. The 50 divisions occupy a distance of 66 millims.

It will be observed that in this form of instrument the 'level error' is practically entirely obviated. It can only come in if the scale is not at the same distance as the height to be measured, and may then be made negligible in practice by levelling the telescopes. Indeed, the uncertainty of measurement appears only to depend on the uncertainty with which the cross-wire can be brought to the proper point, that is, it depends only on the magnifying power and definition of the telescopes used.

To illustrate the use of the instrument, a full account of the determinations of the vertical diameters is given in Table II. Below are the results, and for the sake of showing that there has certainly been no great change in shape, I give results obtained with a cathetometer more than 10 years earlier at the Cavendish Laboratory at Cambridge.

	1890	1880
	centims.	centims.
Large attracting mass M ...	30·526	30·5192
Small „ „ m ...	24·176	
Attracted mass A	15·8203	15·8166
„ „ B	15·7829	15·7842

The diameters of M and m in a horizontal direction parallel to a radius of the turn-table measured by square calipers were

$$M = 30{\cdot}52 \text{ centims.}$$
$$m = 24{\cdot}23 \quad ,,$$

Temperature-Correction. Though the expansion of the masses was to be expected of an unimportant amount, I thought it advisable to attempt to measure it, in case there might be anything anomalous. One of the attracted spheres, B, was for this purpose placed between two vertical levers, in a tank through which could be run a continuous stream of cold or warm water. These levers depended from horizontal rods which could rock or slightly rotate on fine point-suspensions. This was, in fact, a kind of double Lavoisier and Laplace apparatus. The motion of each lever was shown by another lever of about the same length, rising vertically up from each horizontal axis, and serving as the moving support for a double-suspension mirror in which was viewed the reflection of a millimetre-scale. Two telescopes and one scale were used for the two mirrors, though it would not have been difficult to arrange one telescope and two scales. The value of one scale-division was determined by inserting a piece of thin glass between the sphere and each lever in turn. The method is exceedingly sensitive, but I have not been able to make it exact, owing to the warping produced in the rods due to unequal temperatures.

The measures of the expansion varied between ·0000214 and ·0000277, both vertical and horizontal diameters (in the position in the balance) being tested. The true value is probably nearly ·000025 or 1/40000. It will, therefore, lead to no appreciable error if we take the expansion as equal to that of the scale of the cathetometer, say 1/60000 (see p. 92, Table II).

Determination of the Attraction by the Balance.

When the balance is used to measure such small forces and weights as those with which we are here concerned, it must be left swinging on its knife-edge throughout any set of weighings in which the deflections are to be compared one with another. For there is not the slightest reason to suppose that if the beam is lifted up and let down again, its new position of equilibrium will coincide with the old. And again, the beam, especially with such loads as the attracted masses, is put into a state of considerable strain, and continues to alter its shape sensibly for hours, and probably even days, after the masses are put on to it. I have, therefore, always left the beam free for at least two or three days before commencing work with the balance, and it has of course remained free during the course of each day's work. The balance-room was never entered just before any weighing, as it took many hours for the disturbance due to entrance and interference with the case to die away.

When the turn-table supporting the attracting masses is moved half round, from one stop to the other, the bulk of the attraction is taken away from one attracted mass and put on to the other. The balance, being free, is slightly tilted over to the side on which is the larger attracting mass. But the deflection in the apparatus as arranged is so very small—at the most only 10 scale-divisions—that errors of reading can only be neutralised by making a great number of successive measures.

Probably other errors are also largely eliminated, such as those due to the deposition of dust particles, shaking, change of ground level, and varying air-currents. Of such errors I have found those due to varying air-currents by far the worst. Sometimes—especially in autumn and winter the balance will move quite irregularly through more than a scale-division, and continue to move to and fro in this way for days or weeks. When in such an unsteady condition it is useless for accurate work. In spring and summer, however, it is much more steady as a rule, and frequently the scale can hardly be seen to move. I have never worked when on looking into the telescope for some time the irregular movements appeared to be more than a fraction of a tenth, i.e., a fraction of one of the diagonal divisions, though, doubtless, irregularities comparable with a tenth of a whole division have often made their appearance in the work. It is perhaps not safe to ascribe these always to air-currents.

I have always found the air steadiest in warm quiet weather, with a slowly rising temperature in the balance-room, and most unsteady after a sudden fall of temperature. As the alteration of temperature spreads downwards, this is fully in accord with Lord Rayleigh's observation that when the air is steady the ceiling is warmer than the floor, and that when it is unsteady the floor is the warmer of the two. In the observing-room I had a gas stove often kept burning day and night, in the hope that the higher temperature it produced in the ceiling of the balance-room below might steady the air. But the vertical walls of the balance-room interfered with the action of the ceiling, and often produced unsteadiness.

A door opening or shutting anywhere in the building had a visible though transient effect, doubtless through an air-wave. In a high wind the balance was always unsteady, partly, I suspect, through rushes of air into and out of the case with sudden pressure-changes, and partly through changes of ground-level, with variations of wind-pressure against the building.

At all times there was a march, in one direction or the other, of the centre of swing. This was especially marked soon after the frame was lowered and the beam left free. As already remarked, readings were not taken till changes due to change in strain of the beam had subsided. But the march was very appreciable at other times, as will be seen from the diagrams. Perhaps the change was sometimes due to tilting of the ground, with barometric variation, since the balance was a very delicate level, and sometimes due to the change in buoyancy of the air affecting the two sides unequally, though I have not been able to make out any direct connection between barometric height and position of centre of swing. I believe that the explanation is to be sought for the most part in unsymmetrical effect on the beam of slight changes of temperature, for I have frequently noticed that a rising temperature produced an upward march, and a falling one a downward march. This explanation is supported by the following table (p. 71) of observations of the centre of swing, extending from May 9 to May 22, 1890, the balance being free, and the balance-room undisturbed meanwhile.

The relation between temperature and centre of swing is represented in Diagram IX (p. 136).

Of course, after a change in the position of the attracting masses or of the riders, the balance does not at once settle in a new position of equilibrium, but oscillates about it. Inasmuch as the balance never rests in this position, it is better to term it the centre of swing rather than the equilibrium position or resting-point. The dashpot used to damp the vibrations of the mirror reflecting the scale serves also to damp those of the balance-beam, and they die down rapidly. Instead of waiting, however, to observe directly the point on which they are closing in, it is much more exact, and also saves much time, to find the centre of swing, as with an undamped balance, from the extremities of the swings. I have always observed and recorded four extremities of three successive swings, occupying in all a little more than a minute.

Notwithstanding the very considerable damping, the successive lengths of swing are still in geometrical progression, but the rate of reduction is too great to allow the ordinary approximation, in which the geometrical is assumed

Date, 1890	Time	Centre of swing	Temperature: Balance-room	Temperature: Observing-room	Barometer
May 9	11.5 a.m.	136·0	12·0°	13·4°	739·8
	12.55 p.m.	133·0	12·0	15·0	739·2
,, 12	11.15 a.m.	133·8	12·05	14·5	738·6
	1.15 p.m.	131·9	12·05	15·8	738·3
	2.40 p.m.	133·7	12·05	16·6	738·1
Stove left on all night of 12th–13th					
,, 13	11.0 a.m.	181·7	12·6	17·5	740·2
	12.35 p.m.	181·5	12·6	18·4	740·3
	3.15 p.m.	185·0	12·7	18·6	740·3
Stove turned off					
	5.25 p.m.	189·4	12·7	16·5	740·5
,, 14	11.20 a.m.	167·4	12·6	14·3	745·8
	1.10 p.m.	165·5	12·6	14·4	746·0
,, 15	11.5 a.m.	156·8	12·4	13·8	749·5
	2.45 p.m.	160·0	12·4	14·0	749·3
,, 16	1.25 p.m.	158·3	12·4	14·0	744·3
,, 17	8.50 p.m.	171·0	12·55	13·7	741·9
,, 19	10.30 a.m.	174·3	12·55	14·0	743·5
	6.5 p.m.	181·8	12·6	14·0	741·0
,, 20	11.30 a.m.	175·2	12·75	14·0	739·8
	1.5 p.m.	173·7	12·75	14·1	740·0
	5.20 p.m.	172·3	12·7	13·8	741·7
,, 21	11.10 a.m.	172·7	12·6	13·7	751·9
,, 22	11.30 a.m.	192 about	12·85	14·1	757·6

to be an arithmetical progression. The exact method of determining the centre of swing is as follows:

Let a, b, c, d be four successive readings of extremities of swing, and let x be the reading of the required centre.

Let the constant ratio of each swing length to the next be λ.

Then
$$a - x = \lambda (x - b), \quad \text{......(1)}$$
$$x - b = \lambda (c - x), \quad \text{......(2)}$$
$$c - x = \lambda (x - d). \quad \text{......(3)}$$

Eliminating λ from (1) and (2), we may readily obtain x in the form

$$x = c - \frac{(c-b)^2}{(a-b)+(c-b)}, \quad \text{......(4)}$$

and from (2) and (3)

$$x = b + \frac{(c-b)^2}{(c-b)+(c-d)}. \quad \text{......(5)}$$

With no disturbances and no errors of reading, the values of x in (4) and (5) will coincide; but usually there is some small difference, the result of error or disturbance, and it is better to find both and take the mean. A third value might be obtained from (1) and (3); but it appears unadvisable to combine directly observations so far separated in time.

These formulae lend themselves to easy arithmetical treatment, especially with the aid of a slide-rule; but the following graphic method of finding the centre of swing is much less tiring and quite sufficiently exact.

Let the line OA, Fig. 13, represent the scale; O its zero, and A, B, C, D the points distant respectively a, b, c, d from O.

Let $O'C'$ be a parallel line, B', C', D' being points opposite to B, C, D respectively. Let AB' and BC' intersect in K_1. Draw $X_1K_1X_1'$ perpendicular to OA. Then X_1 is the centre of swing given by equation (4). For

$$\frac{AX_1}{X_1B}=\frac{AX_1}{X_1'B'}=\frac{K_1X_1}{K_1X_1'}=\frac{X_1B}{X_1'C'}=\frac{X_1B}{X_1C},$$

i.e., X_1 is the point dividing AB and BC in the same ratio. Similarly if BC' and CD' intersect in K_2, and $X_2K_2X_2'$ be drawn perpendicular to OA, X_2 is the point given by equation (5).

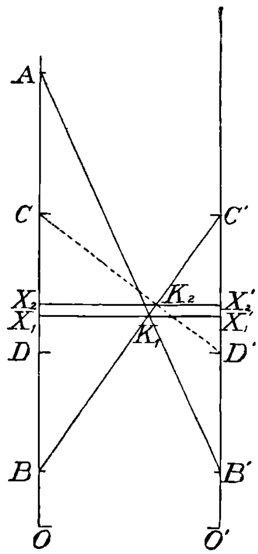


Fig. 13.

The third point given by equations (1) and (3) is obtained from the intersection of AB' and CD', but evidently a small error in C or D' may considerably alter the position of this point, and it is better not to use it.

The construction was carried out thus: a large opal glass plate, 10 in. × 11 in., was etched with cross-lines 10 to the inch, so as to present the appearance of ordinary section-paper. The glaze was taken off so that pencil-marks could be made. A diagonal line ran at 45° across the plate through the corners of the inch squares, and this was always taken as the line BC' in the figure. Taking any convenient horizontal line, usually, of course, far below the plate, as zero, each inch represented a scale-division, each tenth a diagonal division. The values of b and c fixed the lines to be taken as OA, $O'C'$, and on these were marked the points A, C, B', D'. A long glass slip, with a straight scratch on it, was then laid across from A to B' so that the scratch passed through A and B', and its intersection K_1 with the diagonal BC' was x_1 from the zero line. The slip was then laid with the scratch passing through C and D', and its intersection K_2 with BC' gave x_2. It will be observed that all the actual construction for a set of readings of the balance-swings consisted in marking four points on the plate.

The following cases, the first of very regular, the second of very disturbed swing, will serve to compare the results by this exact method with those obtained from the ordinary arithmetic-mean method. At the same time they will show how nearly constant is the ratio of swing decrease.

Date and Number, 1890	Scale-readings in diagonal divisions	Length of swing	Ratio of each to preceding	Centre of swing, exact	Centre of swing, approximate $\frac{a+2b+c}{4}$
May 4, No. 3	865 939 896 921	74 43 25	·581 ·581	911·8 911·8	909·75 913·0
				Mean 911·8	Mean 911·375
Sept. 17, No. 45	1118 1053 1093 1068	65 40 25	·615 ·625	1077·8 1077·6*	1079·25 1076·75
				Mean 1077·7	Mean 1078·0

In finding the attraction the observations were always made in the same order, the determination of the scale-value of rider and attraction being sandwiched so that each might be equally affected by any comparatively slow changes. Starting with the initial position, the attracting masses and riders were so arranged that, on moving either, the balance was deflected in the same direction and over the same part of the scale.

The following was the order of proceeding always observed, the column headed 'Centre of swing' being supposed to contain the values of the position in each case determined from four swing extremities as just explained:

		Centre of swing.
(1)	Initial position	i_1
(2)	Riders moved	r_1
(3)	Riders moved back to initial position	i_2
(4)	Masses moved round	m_1
(5)	Masses moved back to initial position	i_3
(6)	Riders moved	r_2
(7)	Riders moved back to initial position	i_4
(8)	Masses moved round	m_2
	and so on.	

To minimise the effect of progressive changes these observations were always combined in threes in the following way. Denoting the scale-value of rider by R, and of attraction by M:

* [The original has 1078·6 which is evidently a slip; hence this is not really a good example of a 'very disturbed' swing. Ed.]

From (1), (2), (3) .... $R_1 = r_1 - \frac{i_1 + i_2}{2}$,

,, (3), (4), (5) .... $M_1 = m_1 - \frac{i_2 + i_3}{2}$,

,, (5), (6), (7) .... $R_2 = r_2 - \frac{i_3 + i_4}{2}$,

and so on.

These again were combined in threes, so that (the notation being continued) the successive values of attraction/rider are

$$\frac{2M_1}{R_1 + R_2}, \quad \frac{M_1 + M_2}{2R_2}, \quad \frac{2M_2}{R_2 + R_3}, \text{ and so on.}$$

The successive centres of swing i_1, r_1, i_2, m_1, etc., correspond to instants of time following each other at intervals of about 2 minutes, rather more than 1 minute being taken up in making and recording the four readings for each, and the rest in making the change of position in rider or mass and waiting for the next readings. It will be seen that each value of M or R is based on three successive centres of swing, the weighings extending over about 6 minutes, while each value of M/R is based on seven successive centres of swing determined in about 14 minutes.

A series of readings was usually continued for about 2 or 3 hours. The temperature in both observing and balance rooms was read at the beginning and end of the series, and the barometric height was also observed. As soon as possible after the desired number of determinations was completed with the attracted masses in one of the two positions, the vertical distances between attracting and attracted masses were measured by the cathetometer in the manner explained in Table II, and the position of the attracted masses was then altered.

A full account of all the weighings is given in Table III, and the results are represented in Diagrams I–VI (pp. 130–135). The three upper rows of points in each diagram represent the centres of swing, those in the initial position being marked •. After movement of the rider they are marked ×, and after movement of the masses they are marked o. The base-lines for the different rows are altered to save space, as described on the diagrams, for on the scale adopted the rider series would always be about 10 inches above or below the initial series. In Diagram I the rider and mass series are also brought down and superposed on the initial series, so that each of the three has the same average height. It will be seen that all three are affected by the same disturbances. The advantage of the short time of swing and the mode of combining the results in threes will be realised more easily from this superposition.

The base-line may be regarded as a time-scale, as the instants corresponding to successive centres of swing were almost exactly equidistant.

In each case, under the representation of the centres of swing, are plotted the resulting values of M/R, and at the side will be found a representation of the distribution of results about the mean.

Assuming that each day's mean value is correct, and that the differences for different days are to be set down to variation of distance, etc., we can find the distribution of all the values about the mean by simply superposing the marginal curves at the side of the figures. The result fairly shows the accuracy as far as the weighing alone is concerned. It is represented in Diagram VII, where A is the mean value of the attraction in the lower, and a that in the upper position. A and a are brought near together to save space, but really they should be 40 inches apart. It will be seen that the range is about 2 per cent. of $A - a$ on each side of the mean, or taking the value of $A - a$ in milligrammes weight as about $\frac{1}{3}$ milligrm., and the load on each side as 20 kilogrms., the range is about $1/3 \times 10^9$ of this load on each side of the mean.

A comparison of the values of M/R in Diagrams I and II, shows a very curious similarity in the fluctuations, and at first I was inclined to think there was some common external disturbance producing these fluctuations. But an analysis of the two sets of values appeared to show that the resemblance is merely accidental. When the values of M and R are set out separately, it is seen that the fluctuations depend chiefly on M, of which the fluctuations are slightly like each other for the two series, while those of R are quite different, but such that they make the fluctuations in M R resemble each other more closely than those in M alone. Further, it is not easy to see how fluctuations due to some external source would affect the values of M equally in the upper and lower positions and not have any effect on R. Some periodic change of level might be suspected, but this ought certainly to be traced in R. I have examined all the other diagrams and plotted out the component values of M and R, but have found no trace of resemblance, so that I think the curious likeness in I and II must be set down to accident.

There is a curious step by step descent of the centre of swing in the initial position on September 23, Diagram VI, which I cannot explain. It may be due to some error in the method of finding the centre of swing which comes in with a rapid march of that centre. The effect on the result is probably only small, for the value of M/R obtained with a march in the reverse direction on September 25 is very nearly the same, the two values being

September 23 ...... ·2112753.
,, 25 ...... ·2112533.

The following is a list of the weighings recorded, with the distances measured and the mean values of the attraction:

SET I.

Date 1890	Position of attracted masses	No. of values of M/R	Mean value of M/R	D or d in centims.	H or h in centims.
Feb. 4	Upper	50	·2142212	62·318	61·416
April 30 and May 4	Lower	100	1·0109685	31·783	30·824
May 25	Upper	50	·2157379	62·308	61·373

SET II.

Date 1890	Position of attracted masses	No. of values of M/R	Mean value of M/R	D or d in centims.	H or h in centims.
July 28	Lower	25	·9973168	32·106	30·965
Sept. 17	Lower	25	·9984148	32·116	30·954
Sept. 23 and 25 ...	Upper	52	·2112647	62·708	61·566

On the completion of Set I the four masses were inverted, and changed over from right to left or left to right, and the initial position was after this always arranged so that movement of rider or mass decreased the reading. This was done in order to lessen errors due to want of symmetry. If reversal had no effect, Set II should, with the increased distance recorded above, give a value of M/R in the lower position of about ·990, instead of ·998. The larger value actually found is no doubt chiefly due to a want of symmetry in the large attracting mass M. The effect of this want of symmetry will be discussed after the investigation of the mathematical formula, and an account will be given of an independent method of detecting it. I think there is still outstanding a small difference, due, perhaps, to want of symmetry in the turn-table or in the attracted masses. The result of the reversal shows how necessary it was to make it. I should have liked to have in Set II as many determinations as in Set I, so that the mean should be based on values of equal weight. During June and July, 1890, a complete set of 100 in each position, upper and lower, was made; but, owing to the pressure of other work, I was unable to calculate the results till the completion of the set. I then found that the value of M/R was still more than in Set II, and, on plotting out the results, it appeared that occasionally the rider-value fell very considerably, and in an irregular way. On examination, there was little doubt that the rider came in contact occasionally with the suspending frame, when it was raised and should have been clear from it. Very likely temperature-changes had brought about a displacement of the lever-apparatus. Comparison with Set I seemed to show that during that set no such contact had taken place, for there was no comparable irregularity. As it appeared dangerous to attempt to disentangle the good from the bad, the set of June and July was rejected, and Set II was taken as recorded. When I had made the weighings giving 50 and 52 values in the two positions respectively, the balance became so irregular, through the cooler weather, that it was useless to continue work. Rather than carry over the experiment into another season, when it might be necessary to repeat the whole of the work, I have preferred to take Set II as it stands, and give it the same weight as Set I. The final results are calculated from the means of Sets I and II, as explained hereafter. I may here state the results obtained:

$$\text{Constant of attraction} \;\ldots\ldots\; G = \frac{6{\cdot}6984}{10^8}.$$

$$\text{Mean density of the Earth} \;\ldots\; \Delta = 5{\cdot}4934.$$

General Remarks on the Method.

Comparing the common balance with the torsion-balance, there is no doubt that the former labours under the great disadvantage that the disturbances due to air-currents are greatest in the vertical direction, that of the displacement to be measured. But even with this disadvantage the common balance may, I believe, be made to do much more than has hitherto been supposed possible. As an instrument in itself, apart from the external disturbances of air-currents, dust, etc., I believe its accuracy would be far beyond anything approached when these external disturbances are, as they always are, present to interfere with its action. I have always found that every precaution to ward off air-currents and external disturbance has been accompanied by a corresponding increase in steadiness; and I have seen no sign of a limit of accuracy depending on the instrument itself.

Besides the protection from air-currents, there are two conditions essential above all others for accurate work:

1st. That during any set of weighings in which the deflections are to be compared with each other, the beam should be supported on its knife-edge, and should be under constant strain.

2nd. That all moving parts, such as apparatus for changing riders or weights, should be supported quite independently of the balance or its case.

With regard to the first condition, it seems impossible to make the supporting frame move so truly and with so little disturbance that the knife-edge shall return exactly to the same line. Even were it possible, the beam after raising and lowering would be practically a different beam, for, as my observations show, the condition of strain changes considerably after the load is first put on, and it would be merely a chance coincidence if the mean state of strain were the same during successive weighings. I have, in my former paper (*Proceedings of the Royal Society*, vol. 28, 1879)*, described one method of comparing weights of nearly equal value with the beam throughout on its knife-edge and equally strained†, and I should now only modify that

* [*Collected Papers*, Art. 2.]

† I am glad that Dr. Thiesen urges the importance of this condition (*Travaux et Mémoires du Bureau International des Poids et Mesures*, vol. 5, 'Études sur la Balance').

method in having regard to the second condition, of which I have since realised the importance when working with the large balance and with increased optical sensitiveness. It is surprising to find how much disturbance is produced by having the moving parts of the apparatus connected with the balance or its case.

As to air-currents there is no doubt that, as Professor Boys has shown, the greater the apparatus the greater the errors produced by them. At the time my apparatus was designed I did not know this, and there seemed to be a great advantage in making it large, as riders could be used of weight large enough to be measured accurately. Were I about to start with a new design I should certainly go towards the other extreme and make the apparatus small, attempting to get over the rider-difficulty by some such method as that explained on p. 60. For not only is a smaller apparatus kept more easily at a uniform temperature, and, therefore, freer from the source of air-currents, but it is much more handy to adjust, and even if the adjustments are not more accurate they will at least take much less time to make.

At the same time it is only fair to say, on behalf of the large apparatus, that some errors have been magnified on a like scale till they have become observable, and so could be investigated and eliminated. Starting with a small apparatus they would probably never have been detected, and would, therefore, have appeared in the final result.

II. Mathematical Investigation.

The Value of the Attraction Expressed in Terms of the Masses and Distances, and the Investigation of the Effect of Want of Symmetry in the Masses.

Let us suppose that initially the attracting masses are in the positions M_1, m_1, Fig. 14, the larger on the left, the smaller on the right, and that the attracted masses are in the lower positions A, B. When the turn-table is moved round so that the positions of the masses are M_2, m_2, the greater attraction is taken from the left and put on to the right. Let the centre of swing of the balance alter by an amount corresponding to a total change of vertical pull of n dynes. Assuming that a spherical mass M attracts another spherical mass M' when their centres are D centimetres apart with a force of GMM'/D^2 dynes, we can express the change of vertical pull due to the change of position of the masses as $G \times$ a function F of the masses and distances. There is also a change of pull on the suspending-rods and the balance-beam which we may denote by E.

Then $$n = GF + E.$$

In order to eliminate E let the attracted masses be moved into their upper positions A', B', and let the change on moving round the attracting masses be n' dynes. If f is the function of the masses and new distances corresponding to F,

$$n' = Gf + E.$$

Subtracting $$n - n' = G(F - f),$$

whence $$G = \frac{n - n'}{F - f},$$

and knowing G, the mean density of the earth may be at once found in the manner shown later.

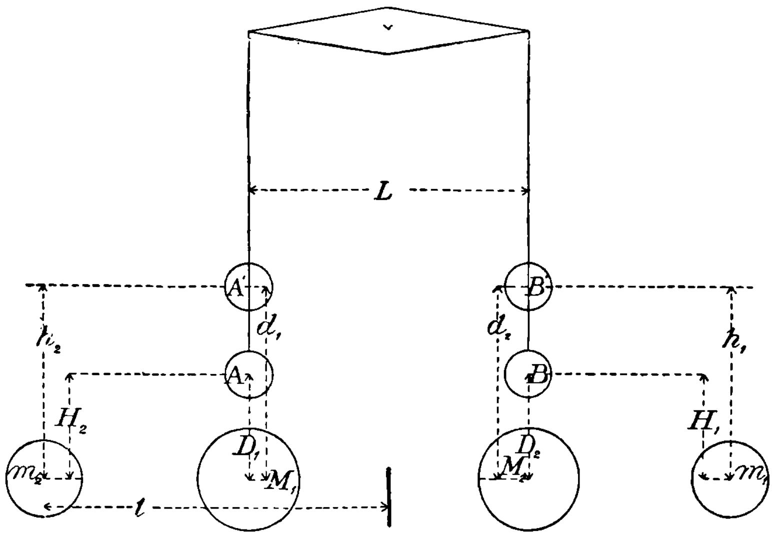


Fig. 14.

We have then to find the form of the functions F, f, and as a preliminary step it is necessary to find the effect of the holes bored through the attracted masses A, B. This may be made to take the form of a correcting factor to the attraction which would be exercised on them if they were spheres.

The piece bored out in each case has radius ·31 centim. This we denote by c. It may be taken as practically a cylinder with plane ends and length equal to 15·8 centims., the diameter $2r$ of the spheres. The intensity due to such a cylinder of mass μ at D from its centre is (Todhunter's *An. Stat.*, Ed. 5, p. 292),

$$G\mu \frac{2r - \sqrt{\{(D + r)^2 + c^2\}} + \sqrt{\{(D - r)^2 + c^2\}}}{c^2 r},$$

which equals, to a sufficient approximation,

$$\frac{G\mu}{D^2 - r^2}.$$

If the mass remaining after μ is removed is A, and if the centre of the mass M is D below that of A, the attraction of M on A is

$$\begin{aligned} &\frac{GM(A+\mu)}{D^2} - \frac{GM\mu}{D^2 - r^2} \\ &= \frac{GMA}{D^2} + GM\mu\left(\frac{1}{D^2} - \frac{1}{D^2 - r^2}\right) \\ &= \frac{GMA}{D^2}\left\{1 - \frac{\mu}{A}\left(\frac{r^2}{D^2} + \text{higher powers of } \frac{r^2}{D^2}\right)\right\}. \end{aligned}$$

Now $$\frac{\mu}{A} = \frac{2\pi c^2 r}{\frac{4}{3}\pi r^3 - 2\pi c^2 r} = \frac{3}{2}\frac{c^2}{r^2} \text{ nearly} = \tfrac{3}{2}\left(\tfrac{31}{790}\right)^2 = \cdot 00231,$$

and the greatest value of

$$\frac{r^2}{D^2} = \left(\tfrac{79}{320}\right)^2 = \cdot 061.$$

Then the higher powers may be neglected, and the attraction may be written

$$\frac{GMA}{D^2}\left(1 - \frac{3}{2}\frac{c^2}{D^2}\right) = \frac{GMA}{D^2}(1-\theta), \text{ say.}$$

When A and B are in the lower position, $D = 32$, and $1 - \theta = \cdot 99986$. When they are in the upper position, $D = 62$ and $1 - \theta = \cdot 99996$, a value so near 1 that we shall in this position omit the correction, since it is only applied to one-fourth of the final result.

In the cross-attractions we shall also omit the correction.

Referring to Fig. 14 let the vertical differences of level between the centres of the various spheres be denoted as follows, the suffixes to M and m denoting their first and second positions respectively:

$$\begin{aligned} A - M_1 &= D_1, \qquad & B - M_1 &= D_1', \\ B - M_2 &= D_2, & A - M_2 &= D_2', \\ B - m_1 &= H_1, & A - m_1 &= H_1', \\ A - m_2 &\quad H_2, & B - m_2 &= H_2'. \end{aligned}$$

When the masses A, B are placed in their upper positions, let the corresponding distances be denoted by small letters.

Let the horizontal distance between the centres of A and B be L, being within sensible limits equal to that between the centres of M in its two positions, and to the length of the beam, and let the radius of the circle in which m moves be l.

Then we have the following horizontal distances:

$$\begin{aligned} A - M_2 &= B - M_1 = L, \\ A - m_1 &= B - m_2 = l + \tfrac{1}{2}L, \\ A - m_2 &= B - m_1 = l - \tfrac{1}{2}L. \end{aligned}$$

We may now write the change in vertical pull on the left by the motion of M from left to right, and of m from right to left, as follows—the first four terms representing the vertical attractions on A and B by M and m in their first position, the next four their attractions when moved round, and the last term E representing the change in attraction on the beam and suspending rods:

$$G\left\{\frac{MA(1-\theta)}{D_1^2}-\frac{MBD_1'}{(D_1'^2+L^2)^{\frac{3}{2}}}-\frac{mBH_1}{\left\{H_1^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}+\frac{mAH_1'}{\left\{H_1'^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right.$$
$$\left.+\frac{MB(1-\theta)}{D_2^2}-\frac{MAD_2'}{(D_2'^2+L^2)^{\frac{3}{2}}}-\frac{mAH_2}{\left\{H_2^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}+\frac{mBH_2'}{\left\{H_2'^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right\}+E.$$

We may arrange all but the last term in nearly equal pairs.

Thus the first and fifth go together, and if we put $D_1+D_2=2D$ and $D_1+\delta=D_2-\delta=D$, their sum is

$$GM(1-\theta)\left(\frac{A}{D_1^2}+\frac{B}{D_2^2}\right)$$
$$=GM(1-\theta)\left\{\frac{A}{D^2}\left(1+\frac{2\delta}{D}+\frac{3\delta^2}{D^2}+\ldots\right)+\frac{B}{D^2}\left(1-\frac{2\delta}{D}+\frac{3\delta^2}{D^2}-\ldots\right)\right\}$$
$$=GM(1-\theta)\frac{A+B}{D^2}\left\{\left(1+\frac{3\delta^2}{D^2}+\text{higher powers of }\frac{\delta^2}{D^2}\right)\right.$$
$$\left.+\frac{A-B}{A+B}\cdot\frac{\delta}{D}\left(2+\frac{4\delta^2}{D^2}+\ldots\right)\right\}.$$

Now $(\delta/D)^2$ is negligible, as will be seen by reference to the table of distances, p. 92, and $(A-B)/(A+B)$ is less than $\frac{4}{10000}$, or less than $\delta\ D$.*

To a sufficiently close approximation then the sum of the two terms is

$$\frac{GM(A+B)(1-\theta)}{D^2}.$$

The second and sixth terms may also be taken together, and putting

$$D_1'+D_2'=2D' \quad\text{and}\quad D_1'+\delta'=D_2'-\delta'=D',$$

we may show that to a sufficient approximation

$$GM\left\{\frac{BD_1'}{(D_1'^2+L^2)^{\frac{3}{2}}}+\frac{AD_2'}{(D_2'^2+L^2)^{\frac{3}{2}}}\right\}=\frac{GM(A+B)D'}{(D'^2+L^2)^{\frac{3}{2}}}.$$

The two pairs with m give similar results with

$$H=\tfrac{1}{2}(H_1+H_2) \quad\text{and}\quad H'=\tfrac{1}{2}(H_1'+H_2').$$

* [A slight correction of the original, obviously required, has been made here. ED.]

Now
$$2D = D_1 + D_2 = A - M_1 + B - M_2$$
$$= B - M_1 + A - M_2 = D_1' + D_2' = 2D',$$

and similarly $2H = 2H'$, so that we may put the expression in the form

$$G\left\{\frac{M(A+B)(1-\theta)}{D^2} - \frac{M(A+B)D}{(D^2+L^2)^{\frac{3}{2}}} - \frac{m(A+B)H}{\left\{H^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} + \frac{m(A+B)H}{\left\{H^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right\}$$
$$+ E = GF + E \text{ say.}$$

It is evident that we may combine experiments at different distances on different occasions in the same way by taking D and H to represent the mean values of these distances, so long as there is only a small variation from the mean.

If the attracted masses are now moved into their upper positions the expression for the change in attraction may be at once deduced from that in the lower position by replacing D and H by d and h, and omitting the factor $1 - \theta$. Let it be denoted by $Gf + E$.

Subtracting one expression from the other E is eliminated, and we have

$$G(F-f)$$
$$= G\left\{\frac{M(A+B)(1-\theta)}{D^2} - \frac{M(A+B)D}{(D^2+L^2)^{\frac{3}{2}}} - \frac{m(A+B)H}{\left\{H^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} + \frac{m(A+B)H}{\left\{H^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right.$$
$$\left. - \frac{M(A+B)}{d^2} + \frac{M(A+B)d}{(d^2+L^2)^{\frac{3}{2}}} + \frac{m(A+B)h}{\left\{h^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} - \frac{m(A+B)h}{\left\{h^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right\}.$$

This is to be equated to the difference in the values of the change in attraction in the two positions, as determined by the rider.

Let

b — the length of the small rider-beam,

w — the mass of each rider,

A mass deflection ÷ rider deflection in lower position*,

a = ,, ,, ,, ,, upper ,, *,

g_B = acceleration of gravity, or dynes weight per unit mass at Birmingham.

Then
$$G(F-f) = \frac{(A-a)\,bwg_B}{\frac{1}{2}L}.$$

Whence we may find the gravitation-constant

$$G = \frac{2bwg_B(A-a)}{L(F-f)},$$

where all the quantities on the right-hand are given in the tables at the end.

* [It will be noted that this symbol is used with two distinct significations. ED.]

The value of g_B may be found sufficiently nearly from the formula (Everett's *Units*, p. 21): $g = 980{\cdot}6056 - 2{\cdot}5028 \cos 2\lambda - {\cdot}000003h$, where λ is the latitude $= 52° \, 28'$ at Birmingham, and h is the height above sea-level, which may be taken as 450 feet, or 13,725 centims. Whence $g_B = 981{\cdot}21$.

Since all the operations are conducted in air, the effective masses should throughout be less by the mass of air each displaces. But since they all have nearly the same densities, and w and $A + B$ appear respectively in numerator and denominator, it is sufficient to take their true masses, and to correct for air displaced in the case of M and m only.

To obtain the mean density of the earth Δ, we must express the acceleration of gravity in terms of G and the mass and dimensions of the earth.

The ordinary formula (Pratt, *Figure of the Earth*, 4th ed., p. 119) is based on the assumption that the earth is a spheroid. It is sufficiently correct for our purpose, the departure of the assumed spheroid from the actual shape being very small. Adding a term $-3 \times 10^{-9}h$, or approximately, $-41 \times 10^{\ 6}$, since the balance-room is taken as 13,725 centims. above sea-level (see above), the value of gravity at Birmingham may be written

$$g_B = \frac{GV\Delta}{a^2}\left\{1 + \frac{\epsilon}{3} - \frac{3}{2}m + \left(\frac{5}{2}m - \epsilon\right)\sin^2 52° \, 28' - 41 \times 10^{-6}\right\},$$

where

V = volume of the earth $= 1{\cdot}0832 \times 10^{27}$ (Everett's *Units*, p. 57),

a = mean radius of the earth $= 6{\cdot}3709 \times 10^{8}$ (*loc. cit.*),

Δ = mean density of the earth,

m = equatorial 'centrifugal force' ÷ gravity $= \frac{1}{289}$,

ϵ = ellipticity of the earth $= \frac{1}{282}$.

The value of the ellipticity is taken to make the formula agree with that quoted above from Everett's *Units*. The uncertainty in the value is quite unimportant, for were ϵ as low as $\frac{1}{295}$, the error in Δ, introduced by taking it as $\frac{1}{282}$, would be less than 1 in 50,000.

Substituting for G, the value of the mean density of the earth is

$$\Delta = \frac{a^2 L (F - f)}{2bwV\left\{1 + \frac{\epsilon}{3} - \frac{3}{2}m + \left(\frac{5}{2}m - \epsilon\right)\sin^2 52° \, 28' - 41 \times 10^{\ 6}\right\}(A - a)}.$$

Here, as in the value of G, w and $A + B$ may have their true values, M and m their values less the mass of air displaced.

In the foregoing investigation we have supposed that all the masses are homogeneous and spherical, with the exception of the borings through A and B. We have supposed, also, that the turn-table is exactly symmetrical about a vertical plane through its axis, so that its motion through two right angles is without effect. Doubtless, these suppositions and the formula based on

6—2

them are not quite true. But, if we invert all the masses and change their sides, or pervert the whole arrangement of them, on taking the mean of the results obtained in the original and inverted and perverted positions we ought to greatly reduce the errors. Indeed, those due to want of symmetry in the turn-table should evidently be quite eliminated, and those due to want of homogeneity in the masses should certainly be lessened.

To show this, we shall calculate the effect of a spherical 'blow-hole,' or gas-cavity in M, in the first and most important term of F. This we shall take as being

$$\frac{GM(A+B)}{D^2},$$

on the supposition that M is homogeneous and spherical.

If the mass of metal which would fill the blow-hole is λ, supposing it to be placed there, the sphere is completed and its attraction is

$$\frac{G(M+\lambda)(A+B)}{D^2};$$

but the vertical attraction is less than this in reality by the vertical component of the attraction of λ.

Let B be the centre of the cavity,
P the centre of the attracted mass,
O the centre of the attracting mass,
δ the distance of B from the centre of M,
θ the angle BOP.

The vertical component of the attraction of λ is

$$\frac{G\lambda(A+B)\cos BPO}{PB^2},$$

but $$BP^2 = D^2 + \delta^2 - 2D\delta\cos\theta,$$

and $$\cos BPO = \frac{D - \delta\cos\theta}{BP},$$

whence the attraction of λ may be put

$$\frac{G\lambda(A+B)(D-\delta\cos\theta)}{(D^2+\delta^2-2D\delta\cos\theta)^{\frac{3}{2}}} = -G\lambda(A+B)\frac{d}{dD}\frac{1}{\sqrt{D^2+\delta^2-2D\delta\cos\theta}}$$
$$= \frac{G\lambda(A+B)}{D^2}\left(1 + 2P_1\frac{\delta}{D} + 3P_2\frac{\delta^2}{D^2} + \dots\right),$$

where $P_1, P_2, \dots$ are zonal harmonics. The attraction of the sphere with the cavity is therefore

$$\frac{GM(A+B)}{D^2}\left\{1 - \frac{\lambda}{M}\left(2P_1\frac{\delta}{D} + 3P_2\frac{\delta^2}{D^2} + 4P_3\frac{\delta^3}{D^3} + \dots\right)\right\}.$$

If the mass is inverted, the vertical component is obtained by changing the sign of δ, and the mean of the two values is

$$\frac{GM(A+B)}{D^2}\left\{1-\frac{\lambda}{M}\left(3P_2\frac{\delta^2}{D^2}+5P_4\frac{\delta^4}{D^4}+\ldots\right)\right\},$$

the first power of δ/D being eliminated.

If $\theta=0$, P_2 and all the other harmonics $=1$.

If $\theta=90°$, $P_2=-\frac{1}{2}$, $P_4=\frac{3}{8}$, etc.

Now, with the actual dimensions of the apparatus, $(\delta/D)^2$ cannot be so great as $(\frac{1}{2})^2$ or $\frac{1}{4}$, and may, of course, be much smaller. The first term of those involving λ, therefore, is the most important, and it lies between $+\frac{3}{2}(\lambda/M)(\delta^2/D^2)$ and $-3(\lambda/M)(\delta^2/D^2)$, changing sign for the value of θ given by $P_2=0$.

The second set of experiments recorded in this paper was taken after inversion and change of side of all the masses, and the final result obtained from this set differs by a little more than 1 per cent. from that obtained from the first set, the observed attraction being slightly greater at the same distance. The difference may be due to irregularities in any or all of the masses and in the turn-table, and to other undetected effects, such as change of level on rotating the turn-table. It would be a very long task to disentangle these, and I have contented myself with trying to find how much must be set down to irregularity in the large mass M, by taking a set of weighings with it alone inverted.

After the weighings on July 28, and the subsequent measures of distances, M was inverted only, and the other masses remained as in Set II. Some weeks later, on September 14, 25 values of $M/R=A$ were obtained, the mean being ·9926. The distances were $D=32{\cdot}118$, H 30·978. The mass M was then put in its original position, as in Set II, and on September 17, as will be seen on referring to the tables, the value of M R obtained was ·9984, the distances being $D=32{\cdot}117$ and $H=30{\cdot}955$, practically the same as on September 14.

Assuming that the difference in attraction is due to cavities in various places. and that, for each, the term $3P_2\delta^2$ D^2 is negligible, we have, approximately,

$$\frac{1-2\dfrac{\Sigma\lambda P_1\delta}{MD}}{1+2\dfrac{\Sigma\lambda P_1\delta}{MD}}=\frac{9926}{9984}.$$

Whence, approximately, since $D=32$,

$$\frac{\Sigma\lambda P_1\delta}{M}=\cdot 0464 \text{ centim.}$$

This result may be tested by independent experiment. For, let the centre of gravity be x below the horizontal plane through the point bisecting the vertical diameter (i.e., the centre of figure), in the position of September 14. The distance of any missing particle λ from the horizontal plane is $\delta \cos \theta = P_1 \delta$. Completing the sphere by the addition of all such particles, the centre of gravity is brought to the centre of figure, so that we have

$$Mx = \Sigma \lambda P_1 \delta,$$

and

$$x = \frac{\Sigma \lambda P_1 \delta}{D}.$$

We have, therefore, to determine the vertical distance of the centre of gravity from the centre of figure.

In order to do this, a large flat-bottomed scale-pan (one belonging to the balance used in the gravitation-experiment) was suspended by two parallel wires about 8 centims. apart and 3 metres long. In the middle of the pan was a shallow cup about 7·5 centims. internal diameter, arranged so that it could turn freely but truly about a vertical axis. The mass, M, was placed on this cup with the diameter, which had been vertical, arranged horizontal, and perpendicular to the plane of the suspending wires. A vertical flat plate, worked by a horizontal micrometer-screw, could be brought just in contact with the end of the diameter, and the reading of the micrometer gave the position of the point of contact. The position of the scale-pan was determined by a plumb-line hanging over one edge in front of a horizontal scale. On turning the cup and mass through 180°, and repeating the readings, knowing the weight of the scale-pan, and the position of its centre of gravity, x could at once be found.

Two separate experiments gave

$$x = \cdot 0536 \text{ centim.},$$

and

$$x = \cdot 0516 \text{ centim.},$$

not very different from the value ·0464 obtained from the attraction-experiments. The agreement is, I think, very close when it is noted that a difference of 1 in 1000 in the attraction in one of the sets of weighings would make x either ·038 or ·054.

This result appears to justify the rejection of all terms in the expansion above the first, and so supports the belief that the reversal largely eliminates errors due to irregularity of shape. For it is in the case of M that there is the greatest danger of a large value for δ/D, and the above experiments seem to indicate that even in this case it is small.

It is, perhaps, noteworthy that the largest term rejected in the attraction of M, viz., $3\lambda P_2 \delta^2 MD^2$ is, if we give P_2 its maximum value 1,

$$\frac{3\lambda\delta}{MD} \cdot \frac{\delta}{D} = \frac{3x}{D} \cdot \frac{\delta}{D},$$

which is not greater than

$$\frac{3 \times \cdot 0464}{32} \times \frac{15}{32} = \cdot 0020,$$

since the radius of the mass is 15.

This is in a term about 5/4 of the final result, so that the greatest error which can be introduced by neglecting this term is ·0025, or 1 in 400.

In calculating the results of the experiments the means of Sets I and II have been taken. Equal weights have been given to each set. It would have been more satisfactory if the number of experiments had been the same in each set; but I should have had to wait for another season to obtain more, and then it would, probably, have been necessary to repeat the whole series in both arrangements, as it is not safe to assume that the various disturbing causes remain the same over a wide interval of time. The second set, though fewer in number, are, in some respects, I believe, better; partly owing to the additional experience gained when they were taken.

In order that the various terms in $F - f$ may be compared, I give below their numerical values, as determined from the values of the masses and distances given in the tables. The meaning of each term in the first column will be seen on referring to Fig. 14. The second column contains the actual values; the third column the values in terms of the lowest term, the fourth.

Value of $F - f$.

$\dfrac{M(A+B)(1-\theta)}{D^2}$	+ 6483938·8	416
$-\dfrac{M(A+B)D}{(D^2+L^2)^{\frac{3}{2}}}$	− 102416·3	6·6
$-\dfrac{m(A+B)H}{\left\{H^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	− 316243·3	20
$+\dfrac{m(A+B)H}{\left\{H^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	+ 15579·9	1
$-\dfrac{M(A+B)}{d^2}$	− 1693687·2	109
$+\dfrac{M(A+B)d}{(d^2+L^2)^{\frac{3}{2}}}$	+ 156728·0	10
$+\dfrac{m(A+B)h}{\left\{h^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	+ 310695·0	20
$-\dfrac{m(A+B)h}{\left\{h^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	− 27597·7	1·7

Whence $F - f = 4826997{\cdot}2$.

The mean value of $A - a$ (see Table III) is

$$A - a = \cdot 791295;$$

substituting these values of $F - f$ and $A - a$ in the formula for G (p. 82), we obtain

$$G = \frac{6 \cdot 6984}{10^8};$$

substituting them in the formula for Δ we obtain

$$\boldsymbol{\Delta = 5 \cdot 4934.}$$

The values given by Sets I and II, treated separately, are to two figures of decimals:

Set I. $\boldsymbol{\Delta = 5 \cdot 52}$
Set II. $\boldsymbol{\Delta = 5 \cdot 46.}$

III. Tables.

Table I. Constants of the Apparatus and Dimensions of the Earth.

Masses.

		grms.
Attracting mass M, in vacuo	=	153407·26
Less air displaced, say	=	153388·85
Attracting mass m, in vacuo	=	76497·4
Less air displaced, say	=	76488·2
Attracted mass A, in vacuo	=	21582·33
,, ,, B, ,, ...	=	21566·21
,, ,, $A + B$, in vacuo ...	=	43148·54
Riders each, in vacuo	=	0·010119

Vertical Diameters of Masses in terms of Cathetometer-Scale correct at 18°.

The masses are taken as having the same coefficient of expansion as the scale.

		centims.
M	=	30·526
m	=	24·176
A	=	15·8203
B	=	15·7829

The diameters of the masses A and B are taken between the nuts securing them on the suspending wires.

		centims.
Balance beam at 0°, L	=	123·232
Rider beam at 0°, b	=	2·53575
$L\,b$ (as occurring explicitly in G and Δ, independent of temperature, assuming them to have the same coefficient of expansion)	=	48·59775

Latitude of Birmingham	$= 52° 28'$
Height of balance-room above sea-level ...	$= 13725$ centims.
Gravity at Birmingham, g_B	$= 981{\cdot}21$ centims./sec.^{2}
Mean radius of earth	$= 6{\cdot}3709 \times 10^8$ centims.
Volume of earth	$= 1{\cdot}0832 \times 10^{27}$ cub. centims.
Equatorial 'centrifugal force'/gravity ...	$= \frac{1}{289}$
Ellipticity of earth	$= \frac{1}{282}$

$$1 + \frac{\epsilon}{3} - \frac{3}{2}m + \left(\frac{5}{2}m - \epsilon\right)\sin^2 52° 28' - 41 \times 10^{-6} = {\cdot}999161.$$

TABLE II. Vertical and Horizontal Distances.

Vertical Diameters of Masses taken by the Cathetometer, described p. 65.

In the tables below P.S. signifies divisions on the scale over which moves the pointer, which is attached to the small adjustment-plate. V.S. signifies divisions on the vertical millimetre-scale.

Diameter of Large Attracting Mass M.

	Reading on pointer-scale	Mean
Upper telescope sighting top of mass ...	73·2, 73·4, 73·2	73·27 P.S.
Lower „ „ bottom „ ...	23·0, 23·0, 23·2	23·07 P.S.

Turning round to the Vertical Scale.

	Reading on pointer-scale	Mean
Upper telescope sighting 459 millims. V.S. ...	94·6, 94·9, 94·4, 95·0, 94·0	94·58 P.S.
„ „ „ 458 „ ...	68·8, 68·8, 69·7, 69·0, 68·7, 70·0, 69·6, 69·4	69·25 P.S.

Therefore 25·33 P.S. divisions = 1 millim. V.S.,

$$\text{and scale-reading for top of mass} = 458 + \frac{73{\cdot}27 - 69{\cdot}25}{25{\cdot}33}$$

$$= 458{\cdot}158 \text{ millims. V.S.}$$

	Reading on pointer-scale	Mean
Lower telescope sighting 153 millims. V.S. ...	27·3, 27·4, 27·7, 27·0	27·35 P. .
„ „ „ 152 „ ...	0·0, 0·3, − 0 5, 0·0	− 0·05 P.S.

Therefore 27·40 P.S. divisions = 1 millim. V.S.,

$$\text{and scale-reading for bottom of mass} = 152 + \frac{23{\cdot}07 + 0{\cdot}05}{27{\cdot}40}$$

$$= 152{\cdot}844 \text{ millims. V.S.}$$

The difference = 30·5314 centims.

This is rather greater than the diameter of the mass, as the cross-wire was made to touch the image of the mass in each case. A series of measures of 1 millim. on the scale, in which the cross-wire was on the centre of each division, and of 1 millim. between the jaws of a wire-gauge, in which the wire touched the images of the jaws, showed that at the distance at which the scale was, ·005 centim. must be subtracted, leaving

$$\text{Diameter of } M = 30{\cdot}526 \text{ centims.}$$

Vertical Diameter of Small Attracting Mass *m*.

		Reading on pointer-scale	Mean
Upper telescope sighting top of mass	...	75·6, 75·6, 75·0	75·40 P.S.
Lower ,, ,, bottom ,,	...	26·5, 26·3, 26·8	26·53 P.S.

Turning round to the Vertical Scale.

		Reading on pointer-scale	Mean
Upper telescope sighting 388 millims. V.S.	...	100, 99·9, 99·7	99·87 P.S.
,, ,, ,, 387 ,,	...	73·9, 73·4, 74·0	73·77 P.S.

Therefore 26·10 P.S. divisions = 1 millim. V.S.,

$$\text{and scale-reading for top of mass} = 387 + \frac{75{\cdot}40 - 73{\cdot}77}{26{\cdot}10}$$
$$= 387{\cdot}062 \text{ millims. V.S.}$$

		Reading on pointer-scale	Mean
Lower telescope sighting 146 millims. V.S.	...	45·9, 45·9, 45·0	45·60 P.S.
,, ,, ,, 145 ,,	...	20·4, 19·4, 20·0	19·93 P.S.

Therefore 25·67 P.S. divisions = 1 millim. V.S.,

$$\text{and scale-reading for bottom of mass} = 145 + \frac{26{\cdot}53 - 19{\cdot}93}{25{\cdot}67}$$
$$= 145{\cdot}257 \text{ millims. V.S.}$$

$$\text{The difference} = 24{\cdot}1805 \text{ centims.}$$

Subtracting the same correction as in the last case for the cross-wire,

$$\text{Diameter of } m = 24{\cdot}176 \text{ centims.}$$

Vertical Diameters of Attracted Masses A and B taken between the Junctions of the Securing Nuts with the Sphere.

A.

		Reading on pointer-scale	Mean
Upper telescope sighting top of mass	...	82·7, 83·0, 82 9	82·9 P.S.
Lower ,, ,, bottom ,,	...	31·0, 31·5, 31·3	31·3 P.S.

Turning round to the Vertical Scale.

		Reading on pointer-scale	Mean
Upper telescope sighting 429 millims. v.s.	...	95·0, 95·2, 95·3	95·2 P.S.
,, ,, ,, 428 ,,	...	69·0, 69·0, 68·9	69·0 P.S.

Therefore 26·2 P.S. divisions = 1 millim. v.s.,

$$\text{and scale-reading for top of mass} = 428 + \frac{82{\cdot}9 - 69{\cdot}0}{26{\cdot}2}$$

$$= 428{\cdot}531 \text{ millims. v.s.}$$

		Reading on pointer-scale	Mean
Lower telescope sighting 271 millims. v.s.	...	48·0, 47·8, 48·4	48·1 P.S.
,, ,, ,, 270 ,,	...	23·4, 23·0. 22·8	23·1 P.S.

Therefore 25·0 P.S. divisions = 1 millim. v.s.,

$$\text{and scale-reading for bottom of mass} = 270 + \frac{31{\cdot}3 - 23{\cdot}1}{25{\cdot}0}$$

$$= 270{\cdot}328 \text{ millims. v.s.}$$

The difference gives the diameter since the middle of the cross-wire was used, so that

$$\text{Diameter of } A = 15{\cdot}8203 \text{ centims.}$$

B.

		Reading on pointer-scale	Mean
Upper telescope sighting top of mass	...	72·0, 71·0, 71·0	71·3 P.S.
Lower ,, ,, bottom ,,	...	24·6, 25·0, 25·2	24·9 P.S.

Turning round to the Vertical Scale.

		Reading on pointer-scale	Mean
Upper telescope sighting 430 millims. v.s.	...	94·0, 94·6, 94·0	94·2 P.S.
,, ,, ,, 429 ,,	...	68·0, 68·1, 68·1	68·1 P.S.

Therefore 26·1 P.S. divisions – 1 millim. v.s.,

$$\text{and scale-reading for top of mass} - 429 + \frac{71{\cdot}3 - 68{\cdot}1}{26{\cdot}1}$$

$$= 429{\cdot}123 \text{ millims. v.s.}$$

		Reading on pointer-scale	Mean
Lower telescope sighting 272 millims. v.s.	...	43·3, 43·0, 43·0	43·1 P.S.
,, ,, ,, 271 ,,	...	17·1, 17·3, 17·4	17·3 P.S.

Therefore 25·8 P.S. divisions = 1 millim. v.s.,

$$\text{and scale-reading for bottom of mass} = 271 + \frac{24{\cdot}9 - 17{\cdot}3}{25{\cdot}8}$$

$$= 271{\cdot}294 \text{ millims. v.s.}$$

$$\text{And diameter of } B = 15{\cdot}7829 \text{ centims.}$$

Vertical Distances between the Levels of the Centres of the Attracting and Attracted Masses Measured by Cathetometer.

The measurements were made as soon as possible after the completion of a set of weighings, usually on the following day.

It was necessary to fix the attracted masses in the position occupied during the weighings, and with the beam of the balance in the same strained condition. This was done in some cases by gripping the left suspending wire by a pair of jaws; in others, by adding a small weight to one side, and placing a block of the right thickness under the mass on that side.

The cathetometer was placed in front of the left side of the balance-case, from which position all the masses could be viewed by turning the telescope round the central pillar (Fig. 2). It was read when sighting the top of each attracting mass and the top of each attracted mass when in the lower position, the bottom of each attracted mass when in the upper position, the top and bottom being taken at the junctions of the securing nuts with the masses. It is therefore necessary to add to the distances measured by the cathetometer the difference of the radii of attracting and attracted masses in the lower position, and their sum in the upper position (see p. 88). The work is shown in full for February 5 and May 5.

Tests were made at various times, showing that there was no change in the distances (at least within errors of reading), either through moving the turn-table or in the course of a few days (see February 5 and May 5 for examples).

Temperature-Correction. The cathetometer-scale is taken as correct at 18°, and its coefficient of expansion is assumed to be 1/60000. That of the masses is probably about 1/40000, but, for simplicity, is taken as equal to that of the scale, the difference, 1/120000, never amounting to as much as the errors of reading, since the greatest length concerned is 23 centims.

The temperature was estimated to be about 1° above that observed during the immediately preceding weighings, the presence of the observer and the lights used tending to raise it.

The cathetometer rested always on the brick floor of the room. Its vernier reads to ·002 centim.

SET I.

Attracted masses A on the left, B on the right. Attracting mass M moving round from left to right in front of the balance-case.

February 5, 1890. Attracted masses in upper position. Assumed temperature 11°.

Half-way through the measurements the cathetometer was accidentally moved, and could not be exactly replaced. Repeating the reading of A it was found that ·197 must be added to the previous readings to compare with the following ones. This addition is made where the numbers have an asterisk.

	A 64·999* 65·001*	B 65·284 65·282	
m_2 23·448	M_1 25·895* 25·889*	M_2 26·070 26·064	m_1 23·947*
Differences: $A - m_2 = 41·552$	$A - M_1 = 39·108$	$B - M_2 = 39·216$	$B - m_1 = 41·336$

From Table I, p. 88, the sums of the radii of the masses are

$$R_M + R_A = 23·173,$$
$$R_M + R_B = 23·154,$$
$$R_m + R_A = 19·998,$$
$$R_m + R_B = 19·979,$$

whence $$d = \tfrac{1}{2}\{39·108 + 23·173 + 39·216 + 23·154\}$$
$$= 62·326,$$

and $$h = \tfrac{1}{2}\{41·336 + 19·979 + 41·552 + 19·998\}$$
$$= 61·433.$$

These are in terms of a scale correct at 18°, so that the value is too great by about 7/60000. We take as true values

Corrected $$d = 62·318,$$
$$h = 61·425.$$

Test Experiment. At the conclusion, the distance $A - M_1$ was measured again and found to be 39·110.

May 28, 1890. Attracted masses in upper position.
Assumed temperature 14°.

	A 64·674 64·674	B 65·286 65·288	
m_2 23·422 23·424	M_1 25·726 25·724	M_2 25·920 25·920	m_1 23·766 23·756
Differences: $A - m_2 = 41·251$	$A - M_1 = 38·949$	$B - M_2 = 39·367$	$B - m_1 = 41·526$

whence $$d = 62·312,$$
$$h = 61·377.$$

Subtracting temperature-correction ·004,

Corrected $d = 62{\cdot}308,$
$h = 61{\cdot}373.$

Mean values in Set I, $d - 62{\cdot}313,$
$h = 61{\cdot}399.$

May 5, 1890. Attracted masses in lower position.
Assumed temperature 13°.

		A	B	
		50·324	50·622	
		50·324	50·634	
		50·328	50·630	
	m_2	M_1	M_2	m_1
	23·672	25·972	26·138	23·998
	23·674	25·970	26·138	24·008
		25·972	26·132	
Differences: $A - m_2$	26·652	$A - M_1 = 24{\cdot}354$	$B - M_2 - 24{\cdot}493$	$B - m_1 = 26{\cdot}626$

From Table I, p. 88,

$$R_M - R_A = 7{\cdot}353,$$
$$R_M - R_B = 7{\cdot}372,$$
$$R_m - R_A = 4{\cdot}178,$$
$$R_m - R_B = 4{\cdot}197,$$

whence $$D = \tfrac{1}{2}\{24{\cdot}354 + 7{\cdot}353 + 24{\cdot}493 + 7{\cdot}372\}$$
$$= 31{\cdot}786,$$

and $$H - \tfrac{1}{2}\{26{\cdot}626 + 4{\cdot}197 + 26{\cdot}652 + 4{\cdot}178\}$$
$$= 30{\cdot}827.$$

Subtracting temperature-correction ·0025,

Corrected values for Set I,

$$D = 31{\cdot}783,$$
$$H = 30{\cdot}824.$$

Test Experiment. The balance was set free at the end of these measures, and two days later, on May 7, it was again fixed, and the distance D was determined by the cathetometer described on p. 65. The value obtained was $D = 31{\cdot}786$.

NOTE. If the apparatus were perfectly rigid and constant in its dimensions we should expect $D - H = d - h =$ constant. The values actually given by the above experiments are

February 5 ... ... ·892,
May 5 ... ... ·959,
May 28 ... ... ·935.

There is apparently a slight increase during the course of the spring, probably due to the warping of the wood supporting the mass m. But there was some uncertainty in sighting the top of the mass m, especially when in the distant position on the right.

Set II.

Attracted masses A on the right, B on the left. Attracting mass M moving round from left to right behind the balance-case. All the masses inverted.

July 29, 1890. Attracted masses in lower position.
Assumed temperature 16°.

	B	A	
	49·014	49·846	
	49·014	49·844	
m_1	M_2	M_1	m_2
22·434	24·584	24·788	22·868
22·436	24·586	24·782	22·864
Differences: $B - m_1 = 26·579$	$B - M_2 = 24·429$	$A - M_1 = 25·060$	$A - m_2$ 26·979

whence $D = 32·107,$
$H = 30·967.$

Subtracting temperature-correction ·001,

Corrected $D = 32·106,$
$H = 30·966.$

September 18, 1890. Attracted masses in lower position.
Assumed temperature 16°.

	B	A	
	49·076	49·768	
	49·074	49·766	
m_1	M_2	M_1	m_2
22·467	24·576	24·756	22·840
	24·576	24·758	
Differences: $B - m_1 = 26·608$	$B - M_2 - 24·499$	$A - M_1$ 25·010	A m_2 26·927

whence $D = 32·117,$
$H = 30·955.$

Subtracting temperature-correction ·001,

Corrected $D = 32·116,$
$H = 30·954.$

Mean values in Set II,

$D = 32·111,$
$H = 30·960.$

September 27, 1890. Attracted masses in upper position.
Assumed temperature 16°.

	B 63·880 63·876	A 64·540 64·544	
m_1 22·450 22·448	M_2 24·570 24·572	M_1 24·756 24·758	m_2 22·810 22·816
Differences: $B - m_1 = 41·429$	$B - M_2 = 39·307$	$A - M_1 = 39·785$	$A - m_2 = 41·729$

whence $d = 62·710$,

$h = 61·568$.

Subtracting temperature-correction ·002,

Corrected values for Set II,

$d = 62·708$,

$h = 61·566$.

NOTE. The values of $D - H$ and $d - h$, which should be constant, are from the above, and from another set of measures (not here recorded, see p. 85) on September 15, as follows. (We have no reason to expect the same value as in Set I, as the masses M, m have changed sides.)

July 29 ... ... 1·140,
September 15 ... ... 1·140,
September 18 ... ... 1·162,
September 27 ... ... 1·142.

From July 29 to September 15 inclusive, the balance was swinging freely without alteration. The values of H should, therefore, be the same on those dates. They were

July 29 ... ... 30·967,
September 15 ... ... 30·978,

equal almost within errors of reading for the top of m.

Means of Sets I and II:

$$D = \tfrac{1}{2}(31·783 + 32·111)$$
$$= 31·947.$$

$$H = \tfrac{1}{2}(30·824 + 30·960)$$
$$= 30·892.$$

$$d = \tfrac{1}{2}(62·313 + 62·708)$$
$$= 62·511.$$

$$h = \tfrac{1}{2}(61·399 + 61·566)$$
$$= 61·483.$$

Horizontal Distances.

SET I.

$$L = 123{\cdot}269 \text{ centims.}$$

At 18° $\quad l_1 = 122{\cdot}915$,,

and $\quad \dfrac{L}{2} = 61{\cdot}635$,,

whence $\quad l_1 + \dfrac{L}{2} = 184{\cdot}550$,,

$$l_1 - \frac{L}{2} = 61{\cdot}280 \quad ,,$$

Taking the mean temperature of the Set as 12°, and assuming 1 60000 as the coefficient of expansion, on correcting to 12°,

$$l_1 + \frac{L}{2} = 184{\cdot}532 \text{ centims.}$$

$$l_1 - \frac{L}{2} = 61{\cdot}274 \quad ,,$$

SET II.

At 18° $\quad l_2 = 122{\cdot}795$ centims.

$$\frac{L}{2} = 61{\cdot}635 \quad ,,$$

Whence $\quad l_2 + \dfrac{L}{2} = 184{\cdot}430$,,

$$l_2 - \frac{L}{2} = 61{\cdot}160 \quad ,,$$

Taking the mean temperature of the Set as 15°, and correcting to 15°,

$$l_2 + \frac{L}{2} = 184{\cdot}421 \text{ centims.}$$

$$l_2 - \frac{L}{2} = 61{\cdot}157 \quad ,,$$

Mean values for the two Sets

$$L = 123{\cdot}260 \quad ,,$$

$$l + \frac{L}{2} = 184{\cdot}477 \quad ,,$$

$$l - \frac{L}{2} = 61{\cdot}216 \quad ,,$$

TABLE III. Determination of Attraction by the Balance.

Determinations of the Attraction in terms of the Riders by the Balance.

In each case four turning-points of three successive swings are recorded in tenths of a division, i.e., in divisions on the diagonal lines. In the columns headed *i* the masses and riders are in the initial position, in those headed *r* the riders are moved, and in those headed *m* the masses are moved. Under each set of four readings is the calculated centre of swing (see p. 71). In the next line are the deflections due to movements of riders and masses, each placed under the middle one of the three centres of swing from which it is calculated. In the next line are the values of deflection due to mass ÷ deflection due to rider, or M/R (see p. 74).

SET I.

I. Attracted Masses in Upper Position. Feb. 4, 1890, 7.59 p.m. to 10.49 p.m. Temperature: in Observing Room, 15°·7–16°·5; in Balance Room, 10°·05. Barometer, 752·2–752·0 millims. Weather mild and still, after slight frost on the two previous nights. Time between successive passages of centre about 20 seconds.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	725 798 759 780	912 838 878 856	725 799 759 781	804 787 796 791	764 779 771 776	913 838 879 857	726 800 759 781	804 787 797 791
Centre of swing	772·55	863·85	773·00	792·80	773·90	864·60	773·40	793·20
Deflection due to rider or mass...	...	91·075	...	19·350	...	90·950	...	19·700
Mass deflection : rider deflection	...	...	...	·212608	...	·214688	...	·217110

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	763 779 771 775	913 837 879 857	724 801 759 782	804 787 796 792	764 779 771 776	914 838 880 857	725 801 760 783	805 789 796 792
Centre of swing	773·60	864·25	773·85	793·05	773·90	865·05	774·50	793·65
Deflection due to rider or mass...	...	90·525	...	19·175	...	90·850	...	18·950
Mass deflection : rider deflection	...	·214720	...	·211440	...	·209824	...	·208758

TABLE III (*continued*).

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	765 780 772 777	916 839 881 859	726 803 762 784	803 788 797 792	765 779 771 776	914 838 879 857	725 800 759 782	805 787 796 791
Centre of swing	774·90	866·30	776·30	793·65	774·00	864·50	773·60	792·85
Deflection due to rider or mass...	...	90·700	...	18·500	...	90·700	...	19·225
Mass deflection ÷ rider deflection	...	·206174	...	·203966	...	·207966	...	·211438

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	763 780 770 776	914 838 880 857	727 800 760 782	805 789 797 792	764 779 771 775	913 838 879 857	725 800 760 781	804 786 796 791
Centre of swing	773·65	865·05	774 15	793·90	773·65	864·65	773·80	792·50
Deflection due to rider or mass...	...	91·150	...	20·000	...	90·925	...	19·275
Mass deflection ÷ rider deflection	...	·215167	...	·219690	...	·215975	...	·211494

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* 40)
Scale-readings...	763 778 770 774	913 838 879 857	724 801 759 782	803 789 796 792	763 778 769 774	912 837 879 857	726 799 759 780	802 787 797 792
Centre of swing	772·65	864·60	773·85	793·50	772·30	864·20	772·95	793·30
Deflection due to rider or mass...	...	91·350	...	20·425	...	91·575	...	19·875
Mass deflection : rider deflection	...	·217296	...	·223316	...	·220038	...	·216503

TABLE III (*continued*).

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	764 779 771 776	915 839 880 858	725 800 759 780	803 786 795 791	763 778 770 774	913 838 879 857	726 799 759 781	803 787 796 792
Centre of swing	773·90	86·555	773·15	792·25	772·70	864·60	773·15	792·95
Deflection due to rider or mass...	...	92·025	...	19·325	...	91·675	...	19·200
Mass deflection ÷ rider deflection	...	·212986	...	·210397	...	·210117	...	·209693

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)	*m* (56)
Scale-readings...	764 779 772 776	914 839 880 858	724 802 759 781	803 787 795 790	762 778 769 774	912 836 877 855	721 798 755 779	801 785 794 788
Centre of swing	774·35	865·55	773·85	792·10	772·20	862·55	770·35	790·55
Deflection due to rider or mass...	...	91·450	...	19·075	...	91·275	...	20·275
Mass deflection ÷ rider deflection	...	·209267	...	·208784	...	·215557	...	·222161

	i (57)	*r* (58)	*i* (59)	*m* (60)	*i* (61)	*r* (62)	*i* (63)*	*i* (63 *a*)	*m* (64)
Scale-readings...	760 776 767 772	911 836 877 855	724 799 758 779	803 785 795 789	762 777 769 773	911 836 877 855	722 800 758 780	725 799 759 780	803 786 796 790
Centre of swing	770·20	862·50	772·20	791·35	771·70	862·50	772·50	772·90	792·30
Deflection due to rider or mass...	...	91·250	...	19·425	...	90·400	...	...	19·900
Mass deflection ÷ rider deflection	...	·217534	...	·213873	...	·217506	...	...	·217873

* After 63 the riders were moved by mistake instead of the masses, therefore it was necessary to return to the initial position, and take the readings in (63 *a*).

TABLE III (*continued*).

	i (65)	*r* (66)	*i* (67)	*m* (68)	*i* (69)	*r* (70)	*i* (71)	*m* (72)
Scale-readings...	762 777 769 774	913 838 879 857	725 800 758 780	802 785 794 789	759 776 767 772	909 834 875 854	722 797 757 779	802 784 794 789
Centre of swing	771·90	864·60	772·75	790·80	770·15	860·80	771·05	790·55
Deflection due to rider or mass...	...	92·275	...	19·350	...	90·200	...	19·450
Mass deflection ÷ rider deflection	...	·213170	...	·212084	...	·215078	...	·214947

	i (73)	*r* (74)	*i* (75)	*m* (76)	*i* (77)	*r* (78)	*i* (79)	*m* (80)
Scale-readings...	760 777 768 773	911 835 877 854	724 798 757 779	803 785 795 790	762 777 769 773	911 835 877 854	721 797 756 778	800 784 793 788
Centre of swing	771·15	862·05	771·40	791·50	771·70	862·05	770·30	789·75
Deflection due to rider or mass...	...	90·775	...	19·950	...	91·050	...	20·150
Mass deflection ÷ rider deflection	...	·217020	...	·219442	...	·220209	...	·221064

	i (81)	*r* (82)	*i* (83)	*m* (84)	*i* (85)	*r* (86)	*i* (87)	*m* (88)
Scale-readings...	759 774 766 771	910 833 876 854	722 796 757 778	801 783 793 787	759 775 767 771	910 834 876 854	723 797 757 778	802 783 793 787
Centre of swing	768·90	860·95	770·50	789·30	769·60	861·25	770·90	789·35
Deflection due to rider or mass...	...	91·250	...	19·250	...	91·000	...	19·300
Mass deflection ÷ rider deflection	...	·215990	...	·211248	...	·211813	...	·212995

TABLE III (*continued*).

	i (89)	*r* (90)	*i* (91)	*m* (92)	*i* (93)	*r* (94)	*i* (95)	*m* (96)
Scale-readings...	759 775 766 771	908 831 874 852	719 795 754 776	800 783 792 788	760 775 767 772	910 835 876 854	723 798 756 779	800 785 793 789
Centre of swing	769·20	859·00	768·35	789·05	769·90	861·60	770·90	790·30
Deflection due to rider or mass...	...	90·225	...	19·925	...	91·200	...	19·350
Mass deflection ÷ rider deflection	...	·217373	...	·219650	...	·215323	...	·212462

	i (97)	*r* (98)	*i* (99)	*m* (100)	*i* (101)	*r* (102)	*i* (103)	*m* (104)	*i* (105)
Scale-readings...	761 777 768 772	910 835 876 853	721 796 756 777	798 783 791 787	759 775 765 770	909 833 874 852	721 796 756 777	800 783 793 787	759 775 767 771
Centre of swing	771·00	861·35	769·80	788·35	768·60	859·65	769·80	789·35	769·60
Deflection due to rider or mass...	...	90·950	...	19·150	...	90·450	...	19·650	
Mass deflection : rider deflection	...	·211655	...	·211136	...	·214483			

Feb. 4, 1890. Mean of 50 determinations of $M/R = a$ Attracted masses in upper position $\Big\}$ ·21422122.

TABLE III (*continued*).

II. Attracted Masses in Lower Position. April 30, 1890, 7.45 p.m. to 10.32 p.m. Temperature: in Observing Room, 17°–16 ·1; in Balance Room, 11°·1. Barometer, 748·6–749·2 millims. Weather clear; S.E. wind; sunny during day. Time between successive passages of centre not quite 20 seconds.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	1046 969 1012 988	1133 1055 1098 1075	951 1024 984 1007	1127 1062 1099 1078	955 1025 986 1007	1134 1059 1102 1077	952 1028 985 1009	1123 1069 1099 1082
Centre of swing	996·60	1082·85	998·35	1085·50	999·80	1086·25	1000·40	1088·25
Deflection due to rider or mass...	...	85·375	...	86·425	...	86·150	...	87·350
Mass deflection ÷ rider deflection	...	...	...	1·00772	...	1·00856	...	1·01437

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	962 1023 989 1009	1136 1060 1104 1079	955 1029 987 1012	1129 1067 1102 1083	961 1027 989 1012	1136 1064 1105 1081	956 1031 989 1013	1133 1069 1103 1085
Centre of swing	1001·40	1088·00	1002·45	1089·55	1003·15	1090·00	1004·15	1091·25
Deflection due to rider or mass...	...	86·075	...	86·750	...	86·350	...	86·350
Mass deflection ÷ rider deflection	...	1·01133	...	1·00623	...	1·00232	...	1·00101

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	967 1027 994 1012	1141 1064 1108 1083	957 1034 990 1015	1135 1070 1106 1086	965 1031 994 1015	1143 1066 1110 1085	958 1036 993 1017	1134 1073 1108 1089
Centre of swing	1005·65	1092·00	1006·00	1093·20	1007·40	1094·00	1008·35	1095·40
Deflection due to rider or mass...	...	86·175	...	86·500	...	86·125	...	86·825
Mass deflection ÷ rider deflection	...	1·00290	...	1·00406	...	1·00624	...	1·01106

TABLE III (*continued*).

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	976 1027 998 1016	1143 1069 1110 1087	963 1037 996 1019	1141 1073 1112 1090	966 1037 996 1019	1145 1070 1112 1089	960 1040 995 1020	1141 1075 1113 1092
Centre of swing	1008·80	1095·35	1010·65	1097·90	1010·85	1097·05	1011·15	1099·30
Deflection due to rider or mass...	...	85·625	...	87·150	...	86·050	...	87·575
Mass deflection : rider deflection	...	1·01591	...	1·01529	...	1·01264	...	1·01713

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	973 1034 1000 1020	1147 1071 1114 1090	962 1041 996 1022	1137 1079 1112 1094	975 1034 1002 1020	1146 1075 1114 1091	965 1042 999 1021	1138 1080 1113 1094
Centre of swing	1012·30	1098·55	1012·50	1100·20	1013·40	1099·80	1014·00	1101·00
Deflection due to rider or mass...	...	86·150	...	87·250	...	86·100	...	86·650
Mass deflection : rider deflection	...	1·01465	...	1·01306	...	1·00987	...	1·00858

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	977 1035 1003 1022	1150 1073 1116 1093	964 1043 1000 1025	1089 1110 1098 1104	976 1038 1004 1023	1153 1074 1118 1094	968 1044 1001 1025	1148 1078 1118 1095
Centre of swing	1014·70	1100·80	1015·45	1102·20	1016·15	1102·35	1016·45	1103·40
Deflection due to rider or mass...	...	85·725	...	86·400	...	86·050	...	86·475
Mass deflection ÷ rider deflection	...	1·00933	...	1·00597	...	1·00450	...	1·00625

TABLE III (*continued*).

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)	*m* (56)
Scale-readings...	978 1039 1005 1025	1153 1076 1119 1094	969 1045 1002 1027	1149 1080 1118 1097	976 1043 1005 1026	1153 1079 1121 1096	968 1047 1004 1028	1145 1085 1118 1099
Centre of swing	1017·40	1103·35	1017·65	1104·45	1018·60	1105·55	1019·25	1106·15
Deflection due to rider or mass...	...	85·825	...	86·325	...	86·625	...	86·875
Mass deflection ÷ rider deflection	...	1·00670	...	1·00116	...	·99971	...	1·00973

	i (57)	*r* (58)	*i* (59)	*m* (60)	*i* (61)	*r* (62)	*i* (63)	*m* (64)
Scale-readings...	984 1039 1008 1026	1155 1078 1120 1097	971 1048 1004 1029	1151 1083 1122 1100	977 1046 1007 1030	1157 1081 1123 1100	972 1051 1007 1031	1152 1087 1123 1102
Centre of swing	1019·30	1105·10	1020·00	1107·85	1021·25	1108·10	1022·60	1109·95
Deflection due to rider or mass...	...	85·450	...	87·225	...	86·175	...	86·850
Mass deflection ÷ rider deflection	...	1·01872	...	1·01646	...	1·01011	...	1·00798

•	*i* (65)	*r* (66)	*i* (67)	*m* (68)	*i* (69)	*r* (70)	*i* (71)	*m* 72)
Scale-readings...	983 1046 1011 1031	1159 1082 1125 1102	976 1051 1008 1033	1153 1088 1126 1104	983 1049 1012 1032	1161 1083 1127 1102	978 1053 1011 1034	1158 1088 1127 1106
Centre of swing	1023·60	1109·80	1023·70	1112·05	1025·15	1111·05	1025·95	1113·15
Deflection due to rider or mass...	...	86·150	...	87·625	...	85·500	...	86·700
Mass deflection ÷ rider deflection	...	1·01262	...	1·02097	...	1·01944	...	1·01226

TABLE III (*continued*).

	i (73)	*r* (74)	*i* (75)	*m* (76)	*i* (77)	*r* (78)	*i* (79)	*m* (80)
Scale-readings...	985 1051 1014 1033	1163 1086 1129 1104	980 1056 1013 1036	1156 1092 1129 1108	990 1051 1017 1036	1163 1087 1131 1107	982 1057 1015 1039	1161 1093 1132 1109
Centre of swing	1026·95	1113·40	1028·25	1115·50	1029·20	1115·20	1030·15	1117·65
Deflection due to rider or mass...	...	85·880	...	86·775	...	85·525	...	87·100
Mass deflection ÷ rider deflection	...	1·01093	...	1·01299	...	1·01652	...	1·01782

	i (81)	*r* (82)	*i* (83)	*m* (84)	*i* (85)	*r* (86)	*i* (87)	*m* (88)
Scale-readings...	991 1054 1018 1038	1167 1090 1133 1108	984 1059 1018 1041	1158 1098 1132 1112	992 1056 1021 1041	1169 1092 1136 1111	984 1063 1018 1042	1161 1100 1135 1115
Centre of swing	1030·95	1117·40	1032·60	1119·55	1033·55	1120·00	1034·00	1122·20
Deflection due to rider or mass...	...	85·625	...	86·475	...	86·225	...	87·600
Mass deflection : rider deflection	...	1·01358	...	1·00640	...	1·00942	...	1·01890

	i (89)	*r* (90)	*i* (91)	*m* (92)	*i* (93)	*r* (94)	*i* (95)	*m* (96)
Scale-readings...	996 1058 1022 1043	1171 1094 1137 1114	987 1064 1022 1045	1165 1100 1137 1117	995 1061 1024 1045	1172 1097 1137 1116	989 1066 1023 1046	1169 1099 1140 1117
Centre of swing	1035·20	1121·75	1036·85	1123·75	1037·35	1123·15	1038·20	1125·05
Deflection due to rider or mass...	...	85·725	...	86·650	...	85·375	...	86·575
Mass deflection ÷ rider deflection	...	1·01633	...	1·01286	...	1·01450	...	1·01065

TABLE III (*continued*).

	i (97)	r (98)	i (99)	m (100)	i (101)	r (102)	i (103)	m (104)	i (105)
Scale-readings...	998 1062 1026 1045	1175 1097 1141 1116	992 1066 1025 1047	1174 1102 1141 1119	995 1064 1027 1048	1176 1098 1143 1118	995 1067 1026 1049	1169 1105 1143 1121	1001 1066 1029 1049
Centre of swing	1038·75	1125·05	1039·45	1127·15	1040·15	1126·70	1040·75	1128·90	1042·20
Deflection due to rider or mass...	...	85·950	...	87·350	...	86·250	...	87·425	
Mass deflection ÷ rider deflection	...	1·01178	...	1·01452	...	1·01319			

April 30. Mean of 50 determinations of $M/R = A$, Attracted masses in lower position } 1·010905.

May 4, 1890, 11.11 to 11.50 a.m. Temperature: in Observing Room, 13 ·5 to 13°·8; in Balance Room, 11°·7. Barometer, 742·0 to 741·7 millims. Weather inclined to rain; a little cooler than previous day; wind S. to S.W.

	i (1)	r (2)	i (3)	m (4)	i (5)	r (6)	i (7)	m (8)
Scale-readings...	875 936 900 920	1045 969 1013 988	865 939 896 921	1044 970 1014 989	865 938 897 920	1045 967 1013 986	861 940 894 921	1041 971 1012 989
Centre of swing	913·10	996·95	911·80	997·75	911·60	996·00	910·95	997·10
Deflection due to rider or mass...	...	84·500	...	86·050	...	84·725	...	86·275
Mass deflection ÷ rider deflection	...	...	...	1·01699	...	1·01697	...	1·01950

TABLE III (*continued*).

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	869 936 896 919	1046 966 1012 985	862 939 894 920	1036 972 1009 988	867 936 896 918	1045 964 1011 984	860 937 893 919	1040 968 1009 986
Centre of swing	910·700	995·10	910·45	995·55	910·40	993·80	909·20	994·20
Deflection due to rider or mass...	...	84·525	...	85·125	...	84·000	...	85·450
Mass deflection ÷ rider deflection	...	1·01390	...	1·01024	...	1·01533	...	1·01454

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)	*i* (25)
Scale-readings...	863 934 894 916	1043 964 1009 983	859 936 892 917	1033 971 1006 985	863 932 892 915	1040 963 1007 982	857 936 889 915	1035 966 1006 983	862 930 891 914
Centre of swing	908·30	992·60	908·00	993·15	906·60	991·00	906·10	991·40	905·25
Deflection due to rider or mass...	...	84·450	...	85·850	...	84·650	...	85·725	
Mass deflection ÷ rider deflection	...	1·01421	...	1·01538	...	1·01344			

May 4, morning. Mean of 10 determinations of $M/R = A$ Attracted masses in lower position $\Big\}$ 1·015050.

TABLE III (*continued*).

Same Day. 2.40 to 4.54 p.m. Temperature: in Observing Room, 13°·9–14°·1; in Balance Room, 11°·7–11°·75. Barometer, 740·3–739·7 millims.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	847 933 883 912	1035 957 1003 977	853 930 886 911	1031 961 1002 979	864 925 890 909	1035 960 1003 977	853 931 885 912	1026 965 1001 980
Centre of swing	901·40	986·20	902·00	987·05	902·55	987·10	902·00	987·65
Deflection due to rider or mass...	...	84·500	...	84·775	...	84·825	...	85·300
Mass deflection ÷ rider deflection	...	...	...	1·00133	...	1·00251	...	1·00783

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	866 924 891 909	1035 959 1004 977	853 932 886 912	1023 968 1000 982	864 925 889 910	1036 960 1004 978	854 931 886 912	1024 968 1000 982
Centre of swing	902·70	987·20	902·80	988·35	902·30	987·75	902·50	988·45
Deflection due to rider or mass...	...	84·450	...	85·800	...	85·350	...	85·925
Mass deflection ÷ rider deflection	...	1·01303	...	1·01060	...	1·00601	...	1·00940

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	864 925 890 909	1039 958 1005 978	855 931 887 913	1025 969 1001 982	866 925 891 911	1040 958 1005 978	855 932 888 913	1011 977 996 985
Centre of swing	902·55	987·80	903·25	989·20	903·50	987·90	904·00	989·10
Deflection due to rider or mass...	...	84·900	...	85·825	...	84·150	...	85·225
Mass deflection ÷ rider deflection	...	1·01148	...	1·01538	...	1·01634	...	1·01232

TABLE III (*continued*).

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	864 927 890 912	1036 961 1004 979	854 933 887 914	1024 970 1001 984	865 926 892 912	1037 962 1005 980	854 934 888 915	1031 967 1004 983
Centre of swing	903·75	988·10	904·00	989·85	904·35	989·25	904·90	990·55
Deflection due to rider or mass...	...	84·225	...	85·675	...	84·625	...	85·625
Mass deflection : rider deflection	...	1·01454	...	1·01481	...	1·01211	...	1·01182

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	864 928 892 912	1039 961 1006 980	855 934 888 914	1024 972 1002 985	864 929 891 913	1041 961 1006 980	856 934 888 915	1025 971 1002 984
Centre of swing	904·95	989·50	904·80	991·10	905·00	989·65	905·00	990·65
Deflection due to rider or mass...	...	84·625	...	86·200	...	84·650	...	85·500
Mass deflection ÷ rider deflection	...	1·01521	...	1·01846	...	1·01418	...	1·00796

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	866 927 893 913	1038 962 1007 981	857 934 889 915	1030 960 1004 984	865 930 893 915	1043 962 1008 981	858 934 891 915	1035 966 1006 984
Centre of swing	905·30	990·40	905·50	991·30	906·60	991·15	906·55	991·55
Deflection due to rider or mass...	...	85·000	...	85·250	...	84·575	...	84·95
Mass deflection ÷ rider deflection	...	1·00441	...	1·00546	...	1·00621	...	1·00741

TABLE III (*continued*).

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)	*m* (56)
Scale-readings...	869 927 895 914	1041 961 1008 982	858 935 891 916	1037 965 1007 984	867 929 894 914	1041 963 1007 982	856 936 890 916	1029 972 1004 986
Centre of swing	906·65	990·90	907·00	991·80	906·60	991·05	906·70	992·50
Deflection due to rider or mass...	...	84·075	...	85·000	...	84·440	...	85·750
Mass deflection ÷ rider deflection	...	1·01070	...	1·00905	...	1·01155	...	1·01509

	i (57)	*r* (58)	*i* (59)	*m* (60)	*i* (61)	*r* (62)	*i* (63)	*m* 64)
Scale-readings...	871 928 895 913	1041 963 1008 982	859 936 890 917	1035 969 1008 985	869 931 895 916	1044 963 1010 983	860 937 892 917	1030 973 1005 986
Centre of swing	906·80	991·50	907·10	993·50	908·20	992·85	908·25	993·30
Deflection due to rider or mass...	...	84·550	...	85·850	...	84·625	...	84·775
Mass deflection ÷ rider deflection	...	1·01478	...	1·01493	...	1·00812	...	1·00162

	i (65)	*r* (66)	*i* (67)	*m* (68)	*i* (69)	*r* (70)	*i* (71)	*m* (72)
Scale-readings...	863 935 894 917	1042 965 1010 984	863 935 894 917	1039 969 1008 985	840 949 886 923	1042 965 1009 985	861 937 893 918	1037 971 1009 987
Centre of swing	908·80	993·45	908·80	993·75	909·15	993·25	909·05	995·05
Deflection due to rider or mass...	...	84·650	...	84·775	...	84·150	...	85·750
Mass deflection : rider deflection	...	1·00148	...	1·00444	...	1·01322	...	1·01449

TABLE III (*continued*).

	i (73)	*r* (74)	*i* (75)	*m* (76)	*i* (77)	*r* (78)	*i* (79)	*m* (80)
Scale-readings...	865 935 895 918	1045 965 1011 985	860 938 893 919	1038 969 1010 987	868 934 895 918	1045 965 1012 985	863 937 894 919	1041 969 1011 988
Centre of swing	909·55	994·30	909·25	995·00	909·50	994·75	909·80	995·80
Deflection due to rider or mass...	...	84·900	...	85·625	...	85·100	...	85·625
Mass deflection ÷ rider deflection	...	1·00928	...	1·00735	...	1·00617	...	1·00765

	i (81)	*r* (82)	*i* (83)	*m* (84)	*i* (85)
Scale-readings...	864 938 895 919	1044 967 1012 986	860 940 894 920	1036 974 1010 989	867 936 896 919
Centre of swing	910·55	995·45	910·65	996·80	910·60
Deflection due to rider or mass...	...	84·850	...	86·175	
Mass deflection ÷ rider deflection	...	1·01238			

May 4, afternoon. Mean of 40 determinations of $M/R = A$ Attracted masses in lower position $\}$ 1·0100278.

April 30 and May 4. Mean of 100 determinations of $M/R = A$ Attracted masses in upper position $\}$ 1·0109685.

TABLE III (*continued*).

III. Attracted Masses in Upper Position. May 25, 1890, 11.20 to 12.53 noon. Temperature: in Observing Room, 15°·4–16°; in Balance Room, 13°·3. Barometer, 748·5–748·1 millims. Weather, E. wind, warm, very bright. Time of swing not recorded.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	1071 986 1033 1005	1175 1085 1134 1108	960 1047 998 1025	1049 1028 1041 1034	1003 1021 1010 1017	1173 1085 1134 1107	956 1047 996 1024	1049 1028 1040 1034
Centre of swing	1015·90	1116·90	1015·50	1036·20	1014·25	1116·55	1014·25	1035·80
Deflection due to rider or mass...	...	101·200	...	21·325	...	102·300	...	21·500
Mass deflection ÷ rider deflection	...	...	...	·209582	...	·209311	...	·210320

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	1001 1021 1011 1016	1173 1085 1134 1106	958 1046 996 1024	1049 1028 1038 1033	1002 1020 1010 1015	1173 1084 1134 1105	957 1046 995 1023	1049 1028 1040 1032
Centre of swing	1014·35	1116·35	1014·05	1034·70	1013·50	1115·80	1013·25	1035·90
Deflection due to rider or mass...	...	102·150	...	20·925	...	102·425	...	22·850
Mass deflection ÷ rider deflection	...	·207660	...	·204571	...	·213688	...	·223297

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	1003 1019 1009 1015	1173 1082 1133 1104	956 1043 994 1023	1048 1025 1038 1032	1001 1019 1008 1014	1172 1081 1131 1103	958 1042 994 1021	1048 1026 1038 1030
Centre of swing	1012·85	1114·60	1011·90	1033·60	1012·05	1113·10	1011·35	1033·50
Deflection due to rider or mass...	...	102·225	...	21·625	...	101·400	...	22·375
Mass deflection ÷ rider deflection	...	·217535	...	·212400	...	·216962	...	·220172

TABLE III (*continued*).

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	1000 1017 1007 1014	1171 1081 1130 1103	953 1044 992 1021	1047 1025 1037 1031	1000 1016 1007 1013	1171 1080 1130 1103	955 1041 992 1021	1046 1025 1036 1030
Centre of swing	1010·90	1112·70	1010·80	1032·90	1010·45	1112·40	1010·00	1032·15
Deflection due to rider or mass...	...	101·850	...	22·275	...	102·175	...	22·050
Mass deflection ÷ rider deflection	...	·219195	...	·218356	...	·216907	...	·215885

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	999 1017 1006 1013	1168 1080 1131 1102	952 1043 992 1021	1046 1024 1037 1030	999 1018 1007 1012	1170 1082 1130 1102	955 1043 993 1021	1046 1026 1039 1030
Centre of swing	1010·20	1112·40	1010·40	1032·35	1010·70	1112·65	1011·05	1033·75
Deflection due to rider or mass...	...	102·100	...	21·800	...	101·775	...	22·100
Mass deflection ÷ rider deflection	...	·214740	...	·213857	...	·215672	...	·216858

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	998 1019 1009 1014	1171 1082 1132 1104	955 1043 994 1022	1046 1027 1038 1031	1000 1019 1009 1014	1173 1082 1131 1104	956 1046 995 1023	1048 1028 1039 1032
Centre of swing	1012·25	1114·00	1011·65	1033·85	1012·35	1113·80	1013·20	1034·85
Deflection due to rider or mass...	...	102·050	...	21·850	...	101·025	...	21·900
Mass deflection ÷ rider deflection	...	·215388	...	·215197	...	·216531	...	·216350

TABLE III (*continued*).

	i (49)	r (50)	i (51)	m (52)	i (53)	r (54)	i (55)
Scale-readings...	1001 1019 1009 1015	1172 1083 1132 1105	956 1046 996 1023	1048 1027 1039 1032	1000 1020 1009 1016	1173 1082 1133 1105	956 1044 995 1023
Centre of swing	1012·70	1114·60	1013·65	1034·60	1013·15	1114·70	1012·65
Deflection due to rider or mass...	...	101·425	...	21·200	...	101·800	
Mass deflection ÷ rider deflection	...	·212472	...	·208636			

May 25, morning. Mean of 25 determinations of $M\ R = a$ Attracted masses in upper position } ·21446168.

Same Day. 3.15 to 4.50 p.m. Temperature: in Observing Room, 16°·0 to 16°·25; in Balance Room, 13°·3 to 13°·35. Barometer, 747·7 747·4 millims.

	i (1)	r (2)	i (3)	m (4)	i (5)	r (6)	i (7)	m (8)
Scale-readings...	1001 1069 1031 1052	1205 1116 1165 1138	990 1077 1029 1057	1081 1061 1073 1066	1034 1055 1044 1049	1207 1120 1168 1139	991 1080 1030 1059	1083 1062 1076 1068
Centre of swing	1044·55	1147·60	1046·30	1068·55	1047·60	1150·50	1048·35	1070·65
Deflection due to rider or mass...	...	102·175	...	21·600	...	102·525	...	21·675
Mass deflection ÷ rider deflection	...	...	...	·211041	...	·211046	...	·212162

	i (9)	r (10)	i (11)	m (12)	i (13)	r (14)	i (15)	m (16)
Scale-readings...	1037 1056 1046 1052	1209 1119 1169 1141	995 1082 1030 1059	1086 1066 1078 1071	1039 1058 1048 1054	1213 1121 1172 1145	994 1086 1034 1064	1088 1069 1078 1073
Centre of swing	1049·60	1151·10	1049·00	1073·50	1051·60	1154·10	1052·90	1074·90
Deflection due to rider or mass...	...	101·800	...	23·200	...	101·850	...	21·675
Mass deflection ÷ rider deflection	...	·220408	...	·227842	...	·220299	...	·216738

TABLE III (*continued*).

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	1045 1059 1050 1056	1212 1126 1175 1147	996 1088 1037 1066	1088 1071 1080 1076	1043 1063 1053 1058	1215 1126 1177 1148	1002 1088 1039 1066	1094 1072 1083 1077
Centre of swing	1053·55	1157·20	1055·30	1077·10	1056·30	1158·50	1056·55	1079·25
Deflection due to rider or mass...	...	102·775	...	21·300	...	102·075	...	22·350
Mass deflection ÷ rider deflection	...	·209073	...	·207957	...	·213813	...	·219575

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	1044 1064 1053 1061	1217 1127 1178 1150	999 1093 1041 1069	1095 1073 1086 1079	1048 1067 1056 1061	1221 1131 1180 1152	1002 1094 1042 1071	1096 1076 1088 1081
Centre of swing	1057·25	1159·80	1059·35	1081·35	1059·70	1162·50	1060·70	1083·55
Deflection due to rider or mass...	...	101·500	...	21·825	...	102·300	...	22·800
Mass deflection ÷ rider deflection	...	·217611	...	·214181	...	·218112	...	·223147

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	1049 1067 1057 1064	1221 1131 1182 1154	1004 1095 1045 1072	1097 1079 1089 1082	1053 1069 1059 1066	1224 1134 1183 1156	1007 1096 1047 1075	1101 1079 1091 1085
Centre of swing	1060·80	1163·75	1062·60	1085·20	1062·95	1165·70	1064·65	1086·90
Deflection due to rider or mass...	...	102·050	...	22·425	...	101·900	...	22·000
Mass deflection ÷ rider deflection	...	·221583	...	·219907	...	·217983	...	·215898

TABLE III (*continued*).

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	1054 1072 1061 1068	1226 1135 1185 1157	1009 1096 1048 1076	1101 1081 1093 1086	1054 1073 1064 1068	1225 1135 1187 1159	1008 1099 1048 1080	1102 1081 1094 1089
Centre of swing	1065·15	1167·15	1065·35	1088·55	1066·85	1168·40	1067·00	1089·70
Deflection due to rider or mass...	...	101·90	...	22·450	...	101·475	...	21·625
Mass deflection ÷ rider deflection	...	·218106	...	·220774	...	·217172	...	·213607

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)
Scale-readings...	1058 1075 1066 1072	1228 1138 1189 1161	1014 1102 1053 1080	1104 1086 1097 1091	1059 1076 1068 1074	1229 1141 1192 1163	1019 1103 1055 1081
Centre of swing	1069·15	1170·80	1070·45	1093·00	1070·95	1173·40	1072·15
Deflection due to rider or mass...	...	101·000	...	22·300	...	101·850	
Mass deflection ÷ rider deflection	...	·217458	...	·219867			

May 25, afternoon. Mean of 25 determinations of $M/R = a$ } ·21701412.
Attracted masses in upper position }

Mean of 50 determinations, morning and afternoon, ·2157379.

Summary of Set I.

February 4 ... $a =$ ·2142212
May 25 ... ... $a =$ ·2157379
Mean value of ... $a =$ ·2149791

April 30 ... ... $A =$ 1·010905
May 4 ... ... $A =$ 1·011032
Mean value of ... $A =$ 1·0109685

therefore $A - a =$ ·7959894.

TABLE III (*continued*).

SET II.

All Attracting and Attracted Masses inverted and changed over, each to the other side. The Suspending Rods also reversed and Riders interchanged. The initial position always the higher reading on the scale.

I. Attracted Masses in Lower Position. July 28, 1890, 8.10 to 9.43 p.m. Temperature: in Observing Room, 17°–16°·9; in Balance Room, 15°·4. Barometer, 747·6–748 millims. Weather fine and calm; wind W.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	1099 1051 1081 1063	912 1007 951 985	1130 1034 1093 1057	917 1005 952 985	1126 1036 1091 1058	914 1008 951 986	1131 1035 1093 1057	922 1005 954 984
Centre of swing	1069·65	971·95	1070·55	972·10	1070·20	972·60	1070·95	973·15
Deflection due to rider or mass...	...	98·150	...	98·275	...	97·975	...	97·575
Mass deflection ÷ rider deflection	...	...	...	1·00217	...	·99949	...	·99541

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	1128 1035 1092 1058	913 1010 951 987	1134 1035 1095 1058	924 1006 956 987	1130 1038 1094 1061	915 1013 953 989	1137 1034 1098 1060	919 1012 955 989
Centre of swing	1070·50	973·30	1072·25	975·00	1073·05	975·65	1073·80	976·40
Deflection due to rider or mass...	...	98·075	...	97·650	...	97·775	...	97·700
Mass deflection ÷ rider deflection	...	·99528	...	·99719	...	·99898	...	·99719

TABLE III (*continued*).

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	1132 1040 1095 1062	917 1014 954 989	1140 1036 1099 1060	924 1009 958 990	1134 1042 1098 1064	916 1016 955 993	1136 1041 1098 1064	960 991 972 983
Centre of swing	1074·40	976·55	1075·05	977·40	1076·90	978·20	1076·70	979·05
Deflection due to rider or mass...	...	98·175	...	98·575	...	98·600	...	98·100
Mass deflection ÷ rider deflection	...	·99962	...	1·00191	...	·99734	...	·99506

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	1133 1044 1098 1065	916 1018 956 994	1142 1039 1103 1064	925 1013 960 994	1134 1045 1101 1068	918 1019 957 997	1143 1042 1103 1066	925 1018 961 996
Centre of swing	1077·60	979·55	1078·65	980·30	1079·80	981·00	1080·00	982·65
Deflection due to rider or mass...	...	98·575	...	98·925	...	98·900	...	97·075
Mass deflection ÷ rider deflection	...	·99937	...	1·00190	...	·99090	...	·99031

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	1136 1046 1099 1068	924 1019 961 996	1145 1042 1104 1067	930 1016 964 995	1140 1046 1104 1069	918 1022 959 997	1143 1045 1104 1068	928 1018 962 996
Centre of swing	1079·45	982·95	1080·75	983·50	1082·00	982·80	1081·80	983·30
Deflection due to rider or mass...	...	97·150	...	97·875	...	99·100	...	98·875
Mass deflection ÷ rider deflection	...	1·00335	...	·99745	...	·99268	...	1 00051

TABLE III (*continued*).

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	1138 1047 1104 1071	926 1020 963 997	1146 1045 1107 1069	928 1022 964 998	1142 1047 1106 1071	927 1021 963 999	1144 1047 1107 1070	937 1015 967 996
Centre of swing	1082·55	984·45	1083·45	985·75	1083·70	985·10	1084·05	985·10
Deflection due to rider or mass...	...	98·550	...	97·825	...	98·775	...	98·900
Mass deflection ÷ rider deflection	...	·99797	...	·99151	...	·99582	...	1·00139

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)
Scale-readings...	1140 1049 1105 1072	923 1024 962 999	1148 1045 1108 1071	932 1021 966 998	1141 1050 1106 1072	924 1024 963 1001	1144 1048 1107 1072
Centre of swing	1083·95	985·40	1084·35	986·60	1084·80	986·25	1084·75
Deflection due to rider or mass...	...	98·750	...	97·975	...	98·525	
Mass deflection : rider deflection	...	·99684	...	·99328			

July 28, 1890. Mean of 25 determinations of $M/R = A$ Attracted masses in lower position $\Big\}$ ·9973168.

TABLE III (*continued*).

September 17, 1890, 8.0 to 9.31 p.m. Temperature: in Observing Room, 17°–17°·5; in Balance Room, 15°·8. Barometer, 746·2–746·4 millims. Weather warm, cloudy.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	1085 1051 1073 1058	908 1004 945 981	1118 1029 1085 1050	921 995 949 978	1109 1036 1081 1053	905 1006 944 981	1126 1026 1087 1050	921 996 951 978
Centre of swing	1064·20	967·35	1063·40	966·70	1063·75	967·35	1063·95	967·90
Deflection due to rider or mass...	...	96·450	...	96·875	...	96·500	...	96·450
Mass deflection ÷ rider deflection	...	...	...	1·00415	...	1·00168	...	·99613

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	1113 1034 1084 1053	907 1006 944 982	1126 1027 1088 1052	929 993 953 978	1110 1038 1083 1056	910 1007 947 984	1131 1027 1092 1052	934 993 956 979
Centre of swing	1064·75	967·75	1065·05	968·40	1065·90	969·90	1067·10	970·20
Deflection due to rider or mass...	...	97·150	...	97·075	...	96·600	...	96·850
Mass deflection ÷ rider deflection	...	·99601	...	1·00206	...	1·00375	...	1·00026

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	1104 1044 1081 1059	910 1008 947 985	1129 1030 1091 1054	924 1001 953 983	1116 1040 1086 1057	909 1009 947 986	1121 1036 1088 1056	927 1000 956 984
Centre of swing	1067·00	970·44	1067·90	971·45	1066·70	970·85	1067·05	972·80
Deflection due to rider or mass...	...	97·050	...	958·50	...	96·025	...	95·550
Mass deflection ÷ rider deflection	...	·99279	...	·99288	...	·99662	...	·99105

TABLE III (*continued*).

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	1112 1043 1086 1060	914 1009 951 987	1131 1033 1093 1056	929 1002 957 984	1114 1044 1087 1061	916 1011 952 988	1132 1035 1094 1058	934 1002 960 986
Centre of swing	1069·65	973·10	1070·15	974·00	1070·70	974·50	1071·70	976·00
Deflection due to rider or mass...	...	96·800	...	96·425	...	96·700	...	96·550
Mass deflection ÷ rider deflection	...	·99161	...	·99664	...	·99780	...	·99690

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	1097 1058 1083 1067	916 1015 954 991	1135 1037 1098 1061	942 999 965 986	1119 1048 1093 1066	919 1017 957 993	1136 1039 1099 1063	935 1006 963 989
Centre of swing	1073·40	977·10	1074·80	977·85	1075·80	979·70	1076·25	979·10
Deflection due to rider or mass...	...	97·000	...	97·450	...	96·325	...	97·025
Mass deflection ÷ rider deflection	...	1·00000	...	1·00815	...	1·00947	...	1·00362

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	1122 1048 1093 1065	917 1018 956 994	1141 1038 1101 1062	929 1011 962 993	1118 1053 1093 1068	921 1019 958 996	1141 1041 1103 1065	941 1009 966 993
Centre of swing	1076·00	979·45	1076·95	980·65	1078·20	981·40	1079·40	982·65
Deflection due to rider or mass...	...	97·025	...	96·925	...	97·400	...	96·975
Mass deflection ÷ rider deflection	...	·99948	...	·99704	...	·99538	...	·99628

TABLE III (*continued*).

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)
Scale-readings...	1134 1047 1100 1067	920 1022 958 998	1143 1041 1104 1065	932 1016 964 996	1134 1048 1102 1068	925 1021 962 997	1140 1045 1104 1068
Centre of swing	1079·85	982·65	1080·00	983·85	1081·15	984·20	1081·55
Deflection due to rider or mass...	...	97·275	...	96·725	...	97·150	
Mass deflection ÷ rider deflection	...	·99563	...	·99499			

September 17, 1890. Mean of 25 determinations of $M/R = A$, Attracted masses in lower position } ·9984148.

July 28 and September 17. Mean of 50 determinations of $M/R = A$, ·9978658.

II. Attracted Masses in Upper Position. September 23, 1890, 7.52 to 9.30 p.m. Temperature: in Observing Room, 15°·3–15°·4; in Balance Room, 15°·05. Barometer, 749·8–750·2 millims. Weather, light S.W. wind and clear after heavy showers. Scale-readings between about 1100 and 1300; 1000 omitted.

	i (1)	*r* (2)	*i* (3)	*m* (4)	*i* (5)	*r* (6)	*i* (7)	*m* (8)
Scale-readings...	307 248 285 263	113 210 151 186	329 235 293 257	235 257 243 251	281 261 273 265	112 208 149 185	326 232 290 256	233 256 241 249
Centre of swing	271·00	173·10	270·95	248·25	268·35	171·45	268·25	246·60
Deflection due to rider or mass...	...	97·875	...	21·400	...	96·850	...	21·175
Mass deflection ÷ rider deflection	...	...	...	·219797	...	·219799	...	·218581

TABLE III (*continued*).

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	279 260 272 264	110 207 148 183	331 228 290 253	232 255 239 248	277 258 271 262	110 205 147 182	324 229 288 252	230 254 239 247
Centre of swing	267·30	170·15	266·80	245·15	265·80	168·90	265·55	244·50
Deflection due to rider or mass...	...	96·900	...	21·150	...	96·775	...	20·225
Mass deflection ÷ rider deflection	...	·218395	...	·218407	...	·213769	...	·209179

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	275 256 269 261	108 204 145 181	323 228 286 251	228 253 237 247	276 255 268 260	107 203 145 179	328 224 287 249	226 252 236 245
Centre of swing	263·90	167·40	264·10	243·15	263·00	166·65	263·25	241·85
Deflection due to rider or mass...	...	96·600	...	20·400	...	96·475	...	20·225
Mass deflection ÷ rider deflection	...	·210274	...	·211317	...	·210547	...	·209652

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	274 254 265 258	106 199 143 176	320 224 283 246	232 245 237 241	271 252 262 255	100 197 138 175	317 221 281 243	222 247 232 241
Centre of swing	260·90	163·90	260·40	239·85	258·25	160·50	257·90	237·60
Deflection due to rider or mass...	...	96·750	...	19·475	...	97·575	...	19·100
Mass deflection ÷ rider deflection	...	·205323	...	·200437	...	·197668	...	·196730

TABLE III (*continued*).

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	266 249 259 254	98 197 136 173	317 219 279 241	221 244 228 237	265 246 257 251	97 193 135 171	311 218 275 240	218 242 226 235
Centre of swing	255·50	159·10	255·90	234·20	253·05	157·00	253·30	232·10
Deflection due to rider or mass...	...	96·600	...	20·275	...	96·175	...	20·150
Mass deflection ÷ rider deflection	...	·203804	...	·210351	...	·210164	...	·209271

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	264 243 256 249	95 191 133 168	310 216 273 238	215 239 223 232	261 241 253 246	91 188 128 166	311 210 272 234	212 236 220 229
Centre of swing	251·20	154·90	251·40	229·10	248·55	151·10	248·40	226·10
Deflection due to rider or mass...	...	96·400	...	20·875	...	97·375	...	20·675
Mass deflection ÷ rider deflection	...	·212785	...	·215456	...	·213350	...	·213585

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)	*m* (56)
Scale-readings...	257 237 250 243	90 186 127 162	306 208 269 232	211 234 218 227	256 236 248 242	88 184 125 160	303 209 265 231	208 232 216 225
Centre of swing	245·15	149·25	245·80	224·10	244·25	147·20	243·95	222·10
Deflection due to rider or mass...	...	96·225	...	20·925	...	96·900	...	20·350
Mass deflection ÷ rider deflection	...	·216160	...	·216699	...	·212977	...	·209956

TABLE III (*continued*).

	i (57)	r (58)	i (59)	m (60)	i (61)
Scale-readings...	253 233 246 238	86 180 122 157	301 205 263 227	203 228 213 222	293 204 280* 245
Centre of swing	240·95	144·00	240·95		
Deflection due to rider or mass...	...	96·950			

September 23, 1890. Mean of 27 determinations of $M/R = a$ Attracted masses in upper position } ·2112753.

September 25, 1890, 7.10–8.43 p.m. Temperature: in Observing Room, 15°–15°·2; in Balance Room, 15°. Barometer, 760·8 millims., steady. Weather cloudy, with westerly airs. Time of swing 21 seconds. 1000 omitted in scale-readings.

	i (1)	r (2)	i (3)	m (4)	i (5)	r (6)	i (7)	m (8)
Scale-readings...	246 238 243 239	84 179 121 156	301 205 263 228	206 229 215 224	248 233 243 236	82 178 119 156	297 204 260 226	202 228 212 222
Centre of swing	240·90	142·90	240·95	220·40	238·95	141·60	238·95	218·10
Deflection due to rider or mass...	...	98·025	...	19·550	...	97·350	...	20·850
Mass deflection ÷ rider deflection	...	...	...	·200128	...	·207499	...	·213163

* This is a considerable rise, showing either a sudden disturbance or a displacement of the apparatus; possibly the telescope was touched. The rise was maintained and therefore the observations were discontinued.

TABLE III (*continued*).

	i (9)	*r* (10)	*i* (11)	*m* (12)	*i* (13)	*r* (14)	*i* (15)	*m* (16)
Scale-readings...	248 233 243 236	83 176 119 155	300 203 261 226	204 228 214 224	252 233 245 239	84 182 122 158	303 206 265 228	207 232 217 226
Centre of swing	238·95	140·80	239·20	219·50	240·70	144·60	242·45	222·60
Deflection due to rider or mass...	...	98·275	...	20·450	...	96·975	...	20·825
Mass deflection ÷ rider deflection	...	·210125	...	·209475	...	·212813	...	·214718

	i (17)	*r* (18)	*i* (19)	*m* (20)	*i* (21)	*r* (22)	*i* (23)	*m* (24)
Scale-readings...	255 237 249 242	87 184 125 162	307 207 267 232	210 234 219 228	257 238 251 244	90 184 127 163	271 233 255 241	215 233 222 229
Centre of swing	244·40	147·55	244·70	224·65	246·05	148·75	246·70	226·15
Deflection due to rider or mass...	...	97·000	...	20·725	...	97·625	...	20·725
Mass deflection ÷ rider deflection	...	·214175	...	·212974	...	·212292	...	·212564

	i (25)	*r* (26)	*i* (27)	*m* (28)	*i* (29)	*r* (30)	*i* (31)	*m* (32)
Scale-readings...	258 241 251 244	90 186 129 164	307 213 270 236	213 237 223 232	262 242 253 246	93 189 131 167	307 215 272 237	215 239 225 233
Centre of swing	247·05	150·45	248·60	228·30	248·85	153·00	250·25	230·05
Deflection due to rider or mass...	...	97·375	...	20·425	...	96·550	...	19·750
Mass deflection ÷ rider deflection	...	·211297	...	·210649	...	·208053	...	·204425

TABLE III (*continued*).

	i (33)	*r* (34)	*i* (35)	*m* (36)	*i* (37)	*r* (38)	*i* (39)	*m* (40)
Scale-readings...	261 243 253 247	93 189 132 167	312 214 273 237	215 241 225 235	263 245 257 250	96 192 135 168	312 217 275 240	217 243 227 237
Centre of swing	249·35	153·40	250·80	231·10	252·40	156·05	253·05	233·05
Deflection due to rider or mass...	...	96·675	...	20·500	...	96·675	...	20·525
Mass deflection ÷ rider deflection	...	·208172	...	·212051	...	·212180	...	·212254

	i (41)	*r* (42)	*i* (43)	*m* (44)	*i* (45)	*r* (46)	*i* (47)	*m* (48)
Scale-readings...	264 247 259 251	98 194 136 171	314 219 277 242	220 243 231 238	267 249 260 255	100 197 138 174	321 223 281 246	224 246 233 242
Centre of swing	254·10	157·85	255·05	235·20	256·25	160·35	259·30	238·05
Deflection due to rider or mass...	...	96·725	...	20·450	...	97·425	...	21·250
Mass deflection ÷ rider deflection	...	·211812	...	·210662	...	·214011	...	·218650

	i (49)	*r* (50)	*i* (51)	*m* (52)	*i* (53)	*r* (54)	*i* (55)
Scale-readings...	271 252 264 256	102 200 139 176	321 221 282 245	224 247 233 242	271 251 264 256	102 198 140 174	314 226 280 247
Centre of swing	259·30	162·20	259·00	238·40	258·95	161·60	259·50
Deflection due to rider or mass...	...	96·950	...	20·575	...	97·625	
Mass deflection ÷ rider deflection	...	·215704	...	·211487			

September 25, 1890. Mean of 25 determinations of $M/R = a$ } ·21125332.
Attracted masses in upper position }

September 23 and September 25 } Mean of 52 determinations of $M/R = a$, ·2112647.

TABLE III (*continued*).

Summary of Set II.

July 28	... ...	$A = \cdot 9973168$
September 17	...	$A = \cdot 9984148$
Mean value of	...	$A = \cdot 9978658$
September 23	...	$a = \cdot 2112753$
,, 25	...	$a = \cdot 2112533$
Mean value of	...	$a = \cdot 2112647$

Therefore $$A - a = \cdot 7866011.$$

Mean value, giving equal weights to Sets I and II,

$$A - a = \cdot 791295.$$

[The experimental data in these tables have been verified from the original MS. Certain slips in calculations from the data occur in Table III, but it was not considered advisable to correct these. ED.]

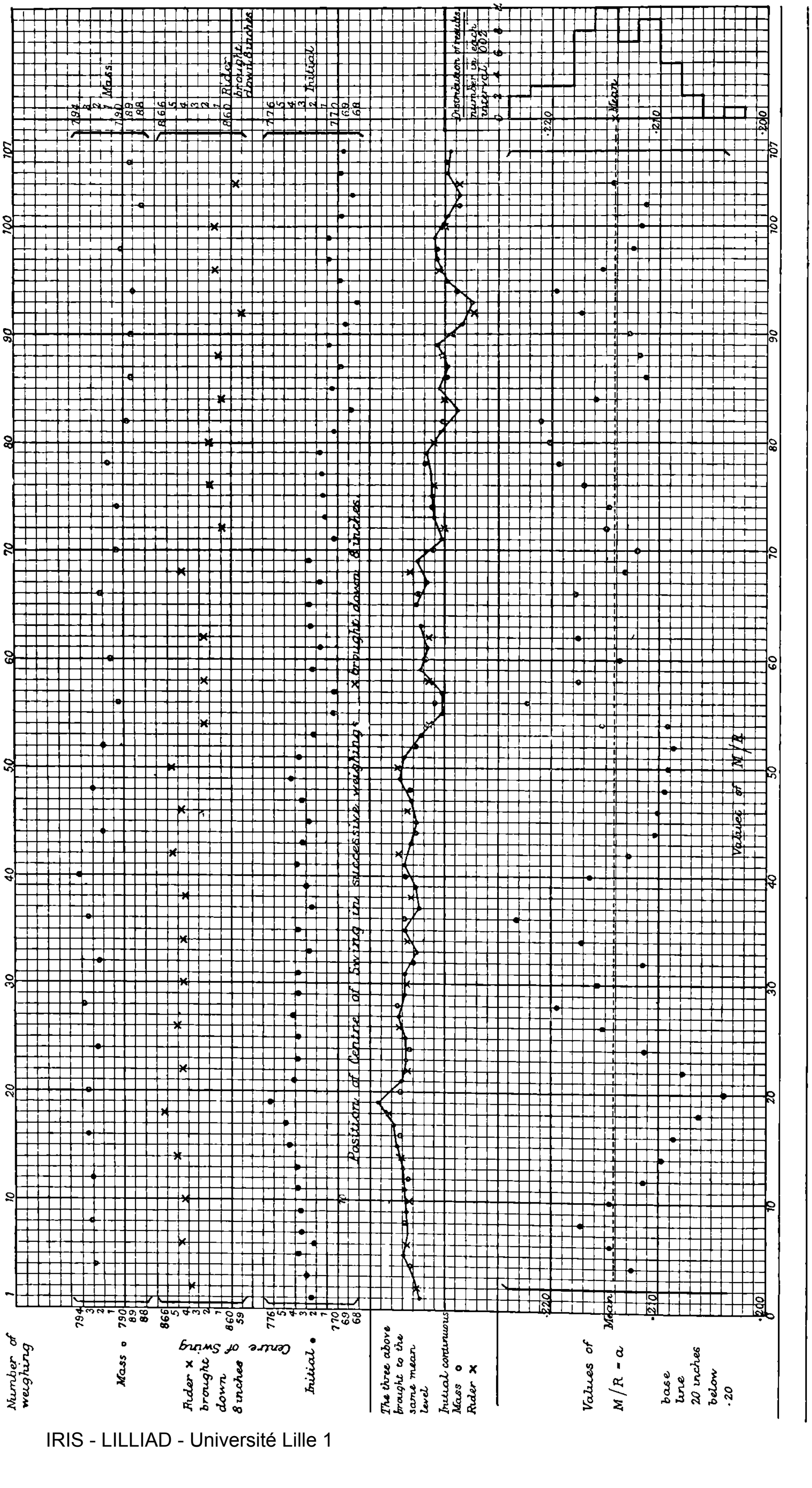


Diagram I. February 4, 1890. Attracted masses in upper position.
[N.B. The large squares are one-inch squares in original. Ed.]

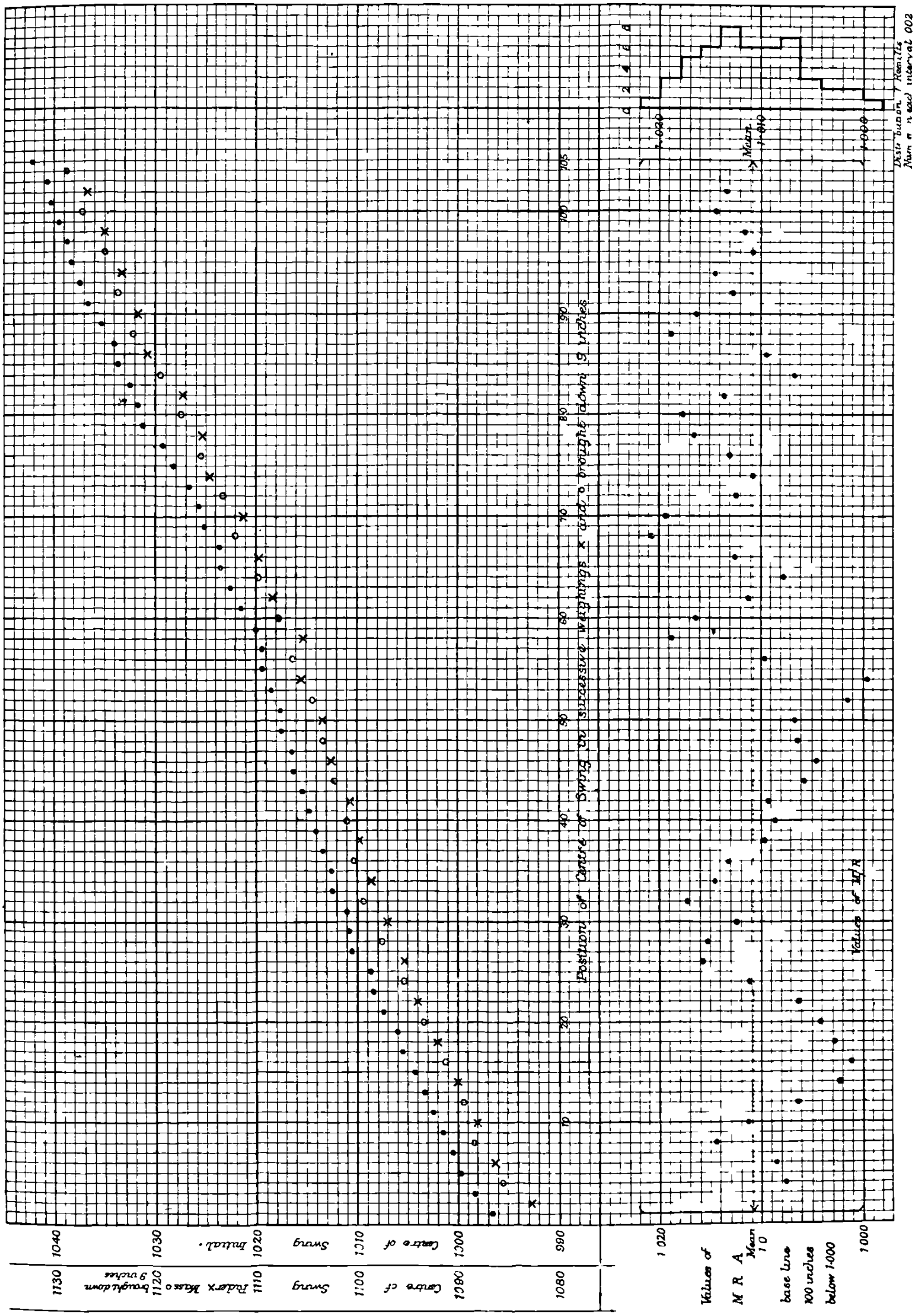


Diagram II April 30, 1891. Attracted masses in lower position.

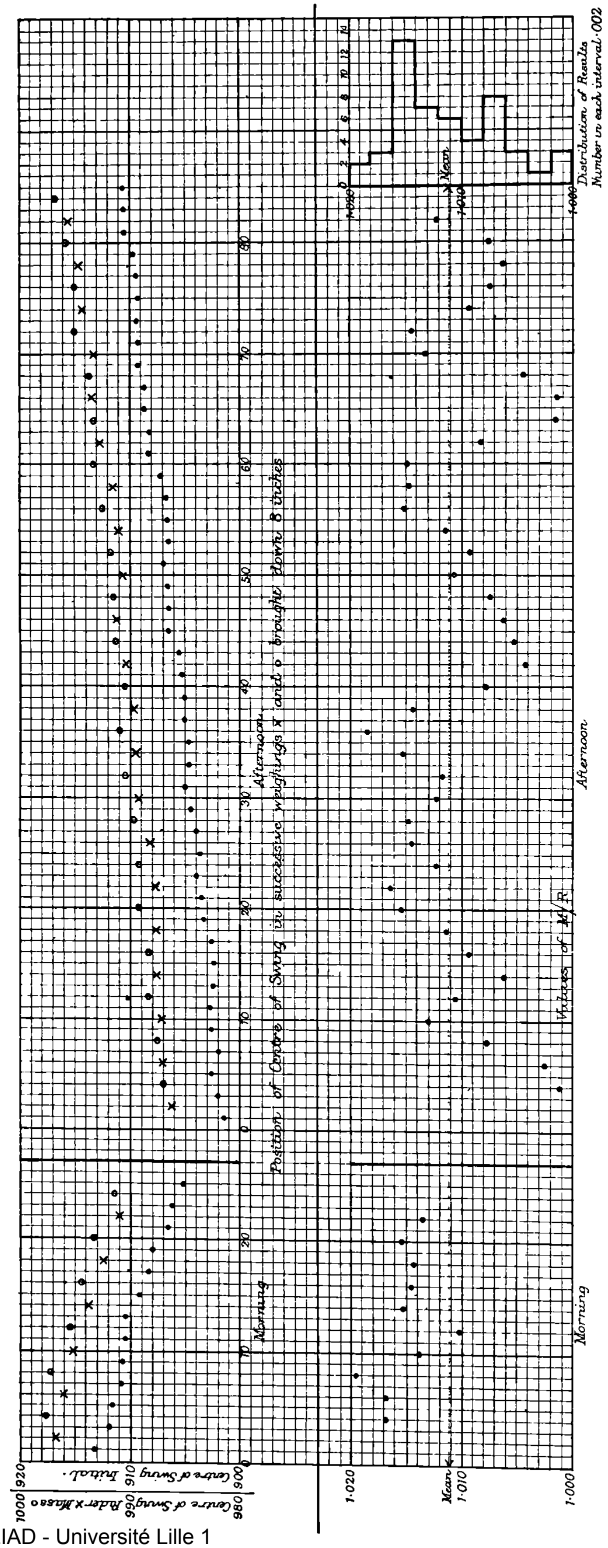


Diagram III May 4, 1890. Attracted masses in lower position.

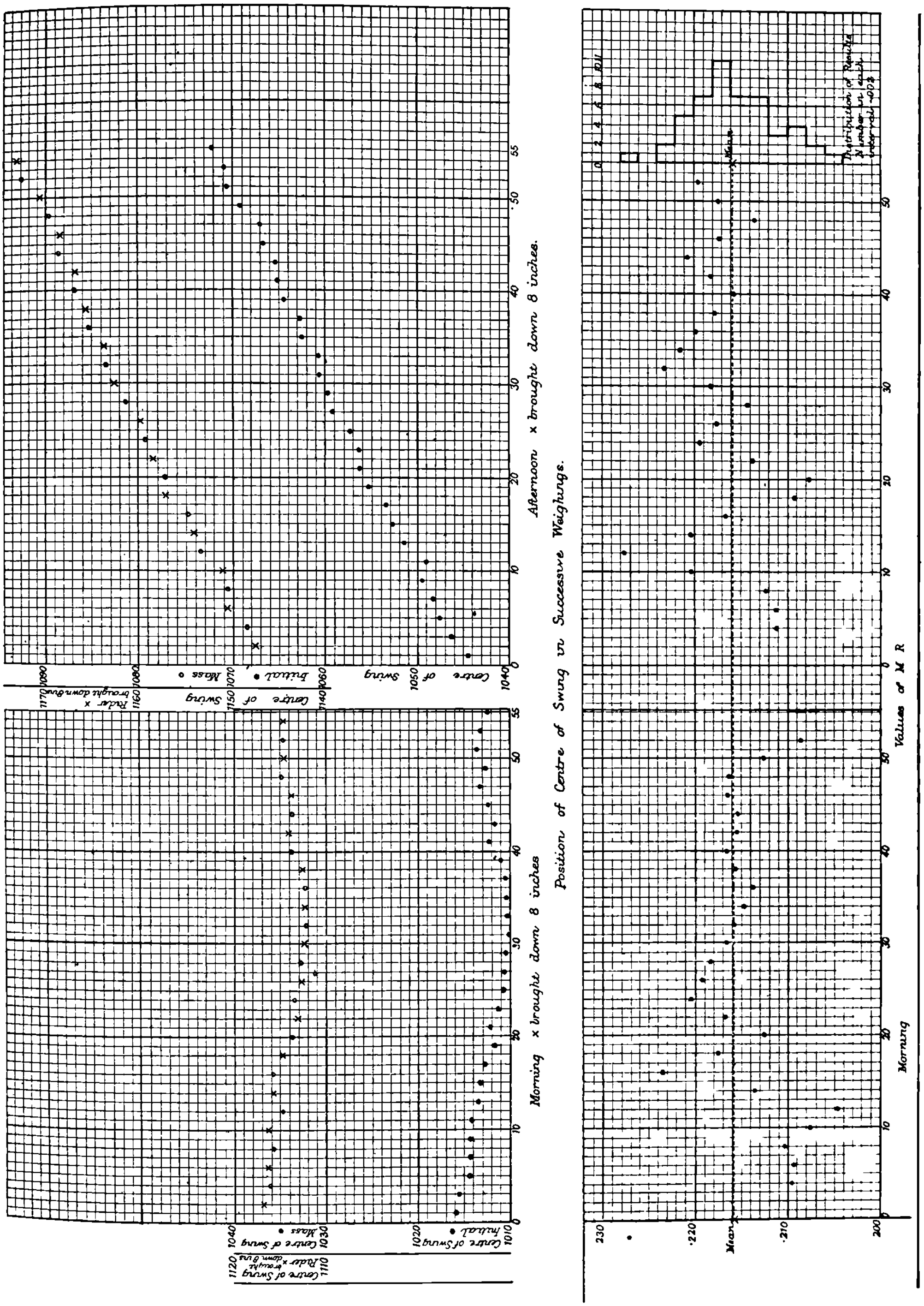


Diagram IV. May 25, 1890. Attracted masses in upper position.

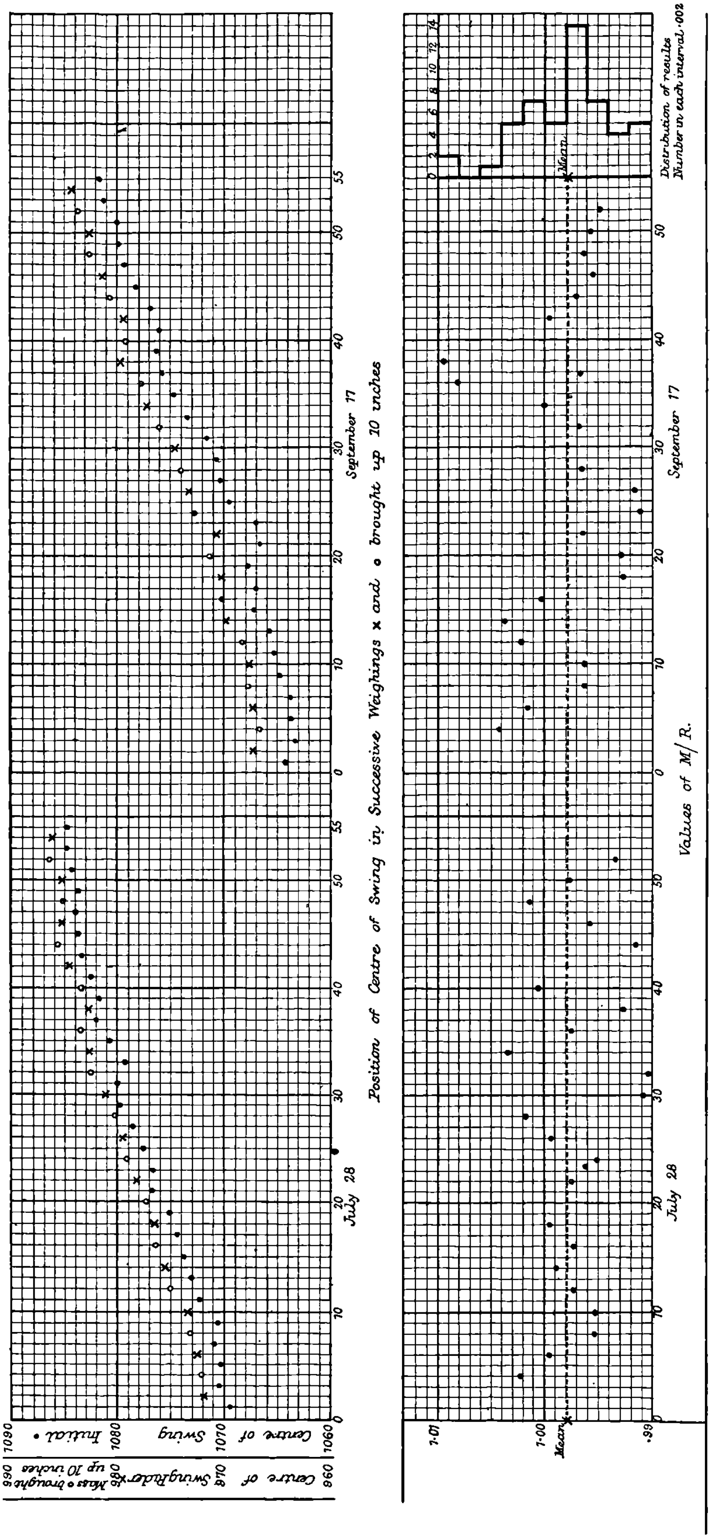


Diagram V. July 28 and September 17. Attracted masses in lower position. Distances nearly equal, on the two dates.

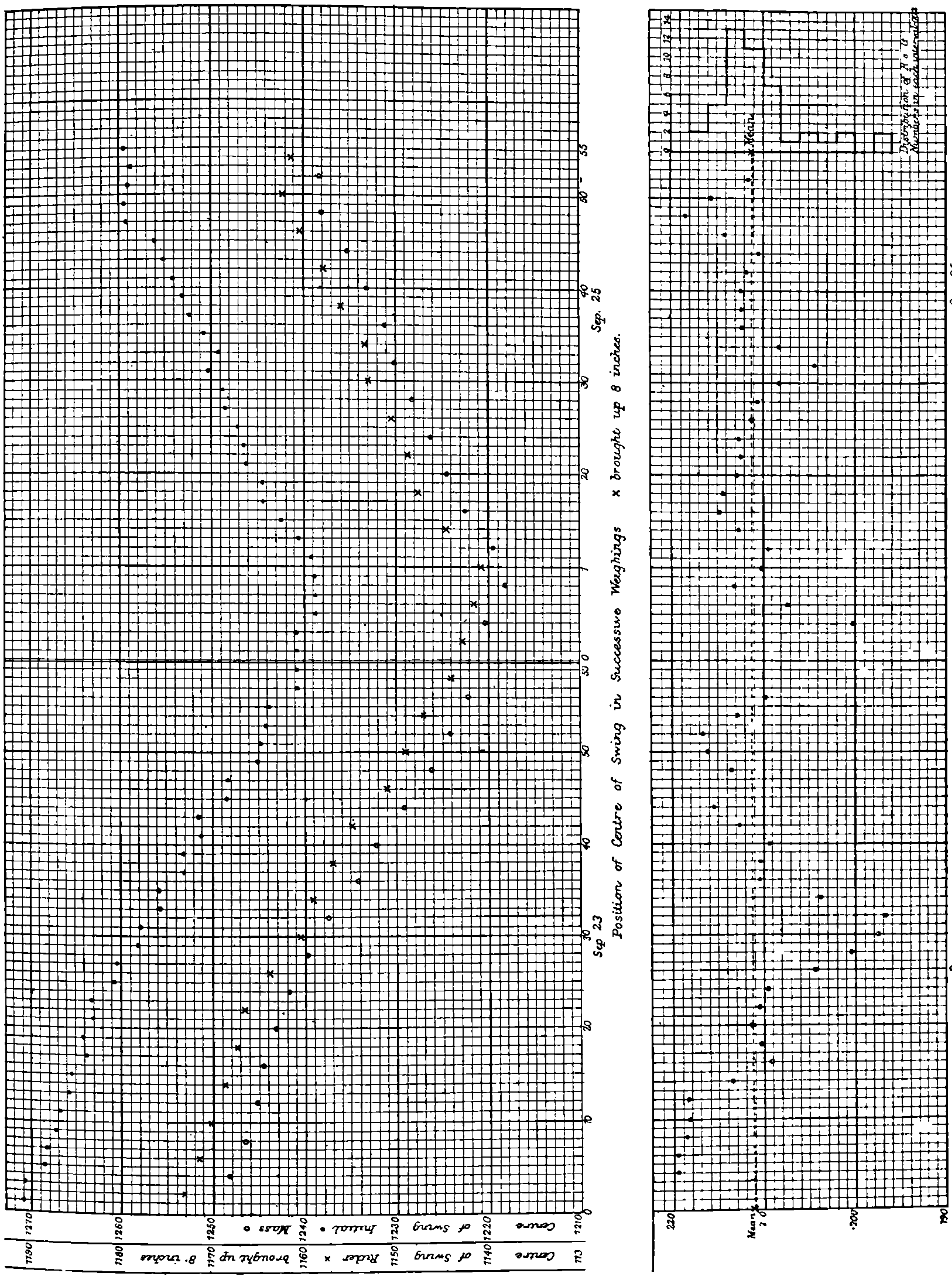


Diagram VI September 23 and 25. Attracted masses in upper position. Distances equal.

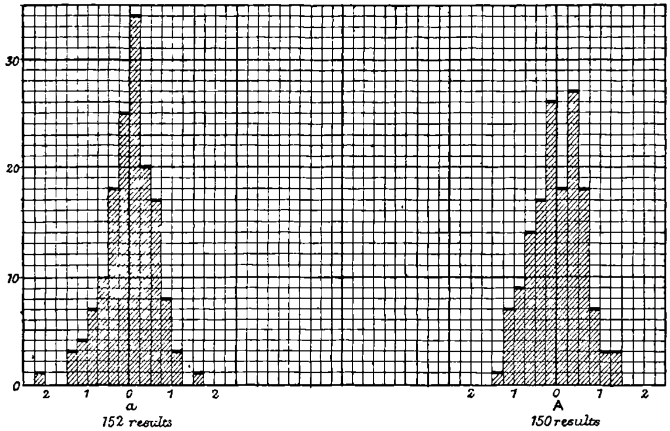


Diagram VII. Distribution of results about the means assumed correct for each set of distances. The numbers along the base are in percentages of the distance aA. The numbers on the side-line show the numbers in each interval 0·25 per cent. from the mean. The distance aA should be 40 inches to show both on the same diagram.

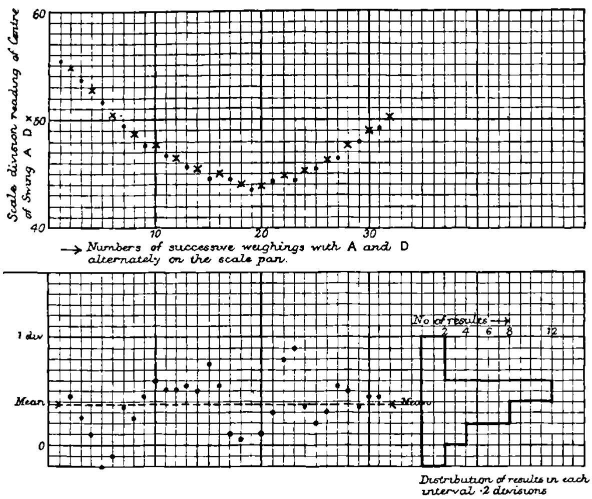


Diagram VIII. Comparison of Riders A and D.

Successive values of $D - A$ in terms of 1 scale-division = ·0035 mgm.

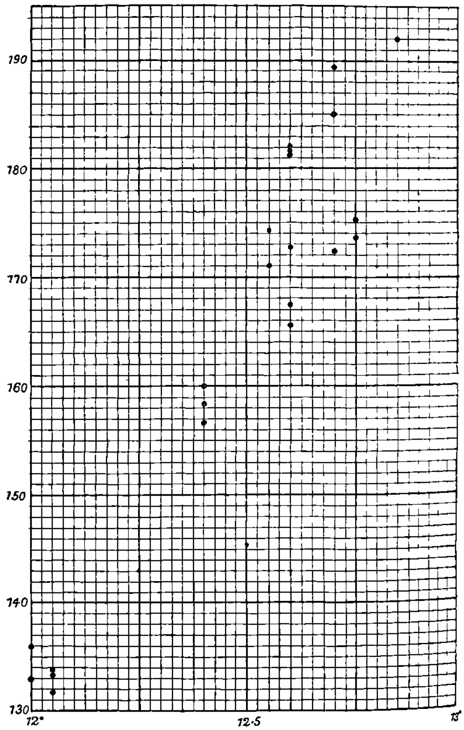


Diagram IX. Relation between temperature and scale-ıeading, May 9—22.

4.

AN EXPERIMENT IN SEARCH OF A DIRECTIVE ACTION OF ONE QUARTZ CRYSTAL ON ANOTHER.

By J. H. Poynting and P. L. Gray, B.Sc.

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Since so many of the physical properties of crystals differ along the different axes, our ignorance of the nature and origin of gravitation allows us to imagine that the gravitative field of crystals may also differ along those axes. Dr. A. S. Mackenzie (*Phys. Rev.*, vol. 2, 1895, p. 321) has described an experiment in which he failed to find any such difference. Using Boys's form of the Cavendish apparatus, he showed that the attraction of calc spar crystals on lead and on other calc-spar crystals was independent of the orientation of the crystalline axes, within the limits of experimental error about one-half per cent. of the total attraction. He further showed that the inverse-square law holds in the neighbourhood of a crystal, the attractions at distances 3·714 centims., 5·565 centims., and 7·421 centims. agreeing with the law to one-fifth per cent.

One of the authors of this paper had already pointed out (*The Mean Density of the Earth*, 1894, p. 7) that if the attraction between two crystal spheres were different for a given distance, according as their like axes were parallel or crossed, such difference should show itself by a directive action on one sphere in the field of the other. This directive action is suggested by the growth of a crystal from solution, where the successive parts are laid down in parallel arrangement—a fact which we might perhaps interpret on the molecular hypothesis as showing that, within molecular range at least, there is directive action.

The experiment now to be described is a modification of one indicated in the work above referred to, carried out for two quartz spheres, and we may say at once that we have certainly not succeeded in proving the existence of a directive action of the kind sought for.

To bring out the principle of the method, let us suppose that the law of the attraction between two spheres with their like axes parallel, as in Fig. 1 (a), is GMM'/r^2, where M, M' are the masses, r the distance between the centres, and G a constant for this arrangement. Let us further suppose that the law of attraction when the axes are crossed, as in Fig. 1 (b), is $G'MM'/r^2$, where G' is a constant for this arrangement, and different from G.

Let us start with the spheres r apart, as in Fig. 1 (a). The work done in removing M' to an infinite distance, in a line perpendicular to the parallel axes, is GMM'/r. Now turn M' through 90° to cross the axes, and bring it back to the original position, but with the axes crossed.

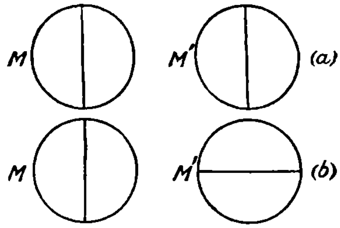

Fig. 1.

The force will do work $G'MM'/r$. Then turn M' through 90° into its original orientation. Assuming that the forces are conservative, the total work vanishes, so that there must be a couple acting during the last rotation, which does work equal to the difference between the works done on withdrawal and approach.

If we take the average value of the couple as L, then

$$\frac{\pi}{2} L = (G - G') \frac{MM'}{r}.$$

Our suppositions as to the law of force are doubtless arbitrary, but they serve to show the probability of the existence of a directive couple accompanying any axial difference in the gravitative field.

In the absence of any distinction between the ends of an axis we may assume that the couple is 'quadrantal,' that is, that it goes through its range of values with the rotation of the sphere through 180° and vanishes in every quadrant, and we shall suppose that it is zero when the crystals are in the positions shown in Fig. 1 (a), and Fig. 1 (b).

Taking the couple as a sine-function of amplitude F, we have

$$\frac{\pi}{2} L = \int_0^{\frac{\pi}{2}} F \sin 2\theta \, d\theta = F,$$

whence
$$F = (G - G') \frac{MM'}{r}.$$

But it is conceivable that the two ends of an axis are different, having polarity of the magnetic type. The couple would then be 'semicircular,' going through its range of values once and vanishing twice in the revolution. We shall suppose that the couple is zero when the axes are parallel. We should now have G and G' constants for the axes parallel, the one when like ends are in the same direction, the other when they are in opposite directions, and we have

$$\pi L = (G - G')\,\frac{MM'}{r}.$$

But if F is the amplitude of the couple

$$\pi L = \int_0^\pi F \sin\theta\, d\theta = 2F,$$

and

$$2F = (G - G')\,\frac{MM'}{r}.$$

To seek for the directive action we have made use of the principle of forced oscillations, thereby obtaining to some extent a cumulative effect, and at the same time largely eliminating the errors due to accidental disturbances.

Briefly the method was as follows: A small quartz sphere, about 0·9 centim. in diameter, was carried in a frame to which a light mirror was attached, and suspended by a quartz fibre inside a brass case, the position being determined by the reflection of a scale in the usual way. The complete time of torsional vibration was about 120 seconds.

Outside the case was a larger quartz sphere, about 6·6 centims. in diameter, its centre being level with that of the suspended sphere, and 5·9 centims. from it. The larger sphere could be rotated about a vertical axis through its centre at any desired rate. The crystalline axes of both were horizontal, that of the smaller sphere being perpendicular to the line joining the centres.

To test for the quadrantal couple, the larger sphere was rotated once in 230 seconds—a period nearly double that of the smaller sphere. To test for the semicircular couple, the larger sphere was rotated once in 115 seconds, or nearly the period of the smaller sphere.

Assuming that a couple exists, a continuous rotation of the larger sphere would set up a forced oscillation in the smaller sphere of the same period as the couple, and since the damping was very considerable, this forced oscillation would soon rise to approximately its full value. Meanwhile, any natural vibrations of the suspended system would be rapidly damped out. Though continually renewed by disturbances due to convection-currents and tremors, they would be irregularly distributed, and there was no reason to suspect that their maximum amplitude would recur at any particular phase of the period of the applied couple. To secure the distribution of successive maxima of natural vibrations of the smaller sphere over all phases of the forced period, the latter was made sensibly different from the natural period in the ratio 23 : 24; and though the cumulative effect of the forced oscillations was reduced by the largeness of this difference, we did not think it advisable to make the periods more nearly coincident, lest the distribution of the disturbances, which were sometimes large, should not be sufficient. This conclusion was arrived at from the results of preliminary experiments with more nearly equal periods.

During each complete period of the supposed applied couple, the position of the smaller sphere was read ten times at equi-distant intervals of time, and the scale-readings were entered in ten parallel columns, one horizontal line for each period. The observations were continued usually for 70 or 80 periods. Adding up the columns and dividing by the number of periods, any forced oscillation would be indicated by a periodicity in the quotients. The periodicities found were too irregular to be taken as evidence of the existence of a couple.

Description of the Apparatus.

The quartz spheres were placed in a cellar at Mason College, Birmingham, below the room in which the observing telescope and rotating-apparatus were fixed.

The smaller sphere, 0·9 centim. diameter and weighing 1·004 grams, was held in an aluminium wire cage, and was suspended by a long, fine quartz fibre in a brass case from a torsion-head at the top of the case.

A light plane mirror was fixed to the cage, and opposite this mirror was a glass window in the case; in front of the window was a plane mirror at 45°, by means of which the light from the scale was reflected into the case and back again to the telescope, as shown in Fig. 2.

The case was surrounded by a double-sided wooden box, lined within and without with tin-foil, and with cotton-wool between its inner and outer walls. The box was supported on indiarubber blocks to lessen tremors.

The larger sphere, 6·6 centims. diameter and weighing 399·9 grams, was held at the lower end of a vertical brass tube which terminated in a very carefully turned shallow brass bell, in which the sphere was held by tapes. The tube passed upwards through the top of the wooden casing without contact, a kind of air stuffing-box indicated in the figure serving to prevent currents through the hole. The tube came into the room above, and was there connected with a train of wheels, driven by an electromotor, the rotation of the motor being geared down from 1000 to 1. The observing telescope was fixed to a heavy stone slab resting on indiarubber blocks, standing on a brick-pillar, which was built on the brick arches forming the cellar-roof. A diagonal scale (of half-millimetre graduations, divided into tenths by the diagonal ruling) was clamped to the telescope-tube and illuminated by an incandescent lamp, aided by a concave mirror. A tenth of a division could be read with certainty, and as the distance from scale to mirror was 358 centims., the position of the suspended sphere could be determined within a little more than one second of arc.

The steady rotation of the larger sphere was maintained by a regulator, for which we are indebted to Mr. R. H. Housman. It consisted of two parts: (1) the governor proper, which automatically maintained approximate steadiness, and (2) a fine hand-adjustment, by which the motion could be accelerated or retarded when it got 'out of time.'

One lead to the motor went through two mercury-cups, and the circuit was completed by a fork of platinum-wire dipping into the cups. This wire

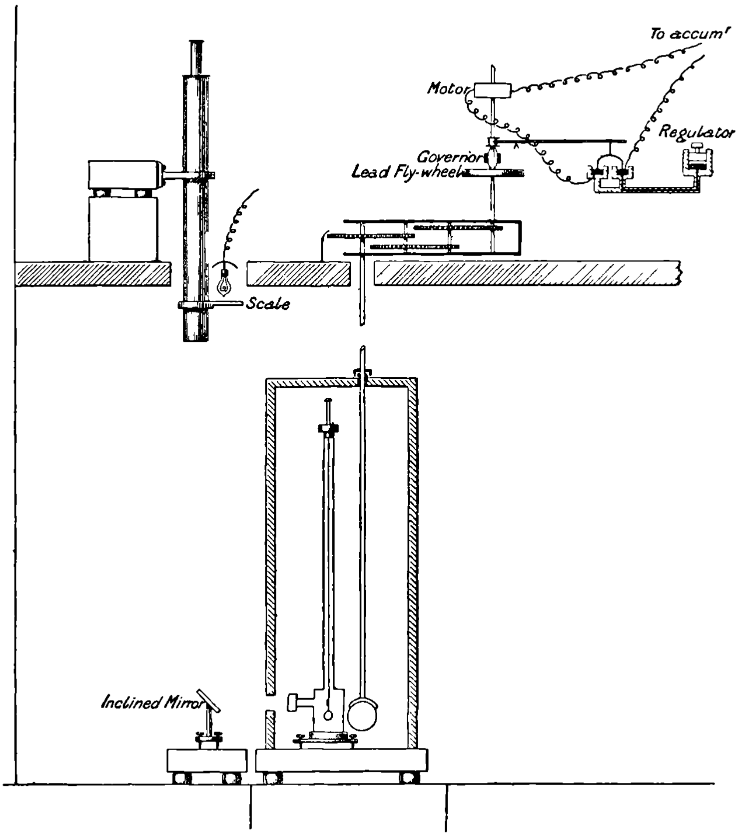

Fig. 2. Diagrammatic sketch of the apparatus.

was fastened to one end of a wooden lever, the other end of which was attached to a sliding collar on the axle of the motor. To this collar were fastened the upper ends of the loaded springs of the governor, as shown in the figure. If the speed increased, the loads flying out pulled the collar down and so raised the wire out of the mercury-cups, and broke the circuit. As the speed diminished, the wire again dipped into the mercury and re-established the current. To diminish sparking the mercury was covered with alcohol, and the two cups were permanently connected by a high resistance shunt.

The fine hand-adjustment consisted of a small wooden plunger working in a tube connected with one of the mercury-cups; by means of a screw the plunger could be raised or lowered, and the level of the mercury in the cup varied accordingly.

If the revolving sphere was found to be gaining or losing, it was quite easy to bring it 'up to time' again by working the screw of the plunger.

The last of the train of driving-wheels was fixed on the tube supporting the larger sphere; its rim was divided into equal parts by numbered marks, the use of which will be explained directly. There were 20 numbered marks, at intervals of 18°; of these only 10 alternate ones were used for the quicker rotation, while the whole 20 were used for the slower speed.

The Observations.

Two observers were required, one at the telescope to note the position of the smaller sphere, the other to regulate the speed of rotation of the larger sphere, and to notify when readings were to be taken by the first observer. The motion having been started, and brought to about the right speed, a time-table was rapidly prepared, showing the times, on the chronometer used, at which each of the numbered marks above mentioned should pass a fixed mark throughout the whole set of observations for one occasion. A signal was given at each passage of a mark past the fixed point, the observer at the telescope putting down the simultaneous scale-reading in a manner which will be understood from Table I, which may serve as a typical record. It does not appear to be necessary to give the full details in other cases. If the motion did not keep to the time-table, it was easily corrected by the hand-adjustment already described.

Every reading in the same column is taken at the same phase in the rotation of the larger sphere, and therefore the mean readings of the columns should preserve any periodicity in the motion of the smaller sphere equal to that of the larger sphere, and more or less eliminate all others. These mean readings are given at the foot of Table I, and appear to indicate a slight periodic vibration, but this might be due to a want of symmetry in the larger sphere and its attachments about its axis of rotation, since the system supporting the smaller sphere and mirror was necessarily not symmetrical. The observations for each couple were on this account divided into two sets: for the semicircular couple the larger sphere was, in the second set, turned through 180° about a vertical axis from its position in the first set; for the quadrantal couple the rotation was 90°. For the final results the means of the results of the two sets were taken, in each case after the second set had been advanced by an amount corresponding to the change of position of the sphere.

Table II contains all the mean results obtained in the same way as the figures at the foot of Table I, the greatest range being given in the last column as an indication of the magnitude of the disturbances.

In Table III are given the means for each azimuth of the larger sphere in its support, the B and D series being advanced as mentioned above.

In combining the results it appeared useless to attempt to weight them according to the number of periods taken, since no accurate conclusion could be expected. It will be seen that in each case there is an outstanding periodicity, but the amplitude is less when the disturbances (as indicated by the greatest range during a period) are less, and it diminishes when the results are combined so as to lessen the effect of want of symmetry.

In the 'quadrantal' observations (Series C, D), where the effect of want of symmetry of the apparatus should almost be eliminated, since it is approximately semicircular, the mean range is much smaller than in Series A and B.

For these reasons we do not think that our observations can be taken as indicating the existence of a couple of the kind sought, but only as giving a superior limit to its value, should it exist.

We now proceed to the Calculation of the Superior Limit of the Couple.

Equation of Motion of the Smaller Sphere.

Let I be the moment of inertia of sphere and cage.
,, μ ,, torsion-couple per radian.
,, λ ,, damping couple per unit angular velocity.
,, $F \cos pt$ be the supposed couple due to the larger sphere, having period $2\pi/p$.

Then $$I\ddot{\theta} + \lambda\dot{\theta} + \mu\theta - F \cos pt.$$

Putting $$\kappa = \lambda/I; \quad n^2 = \mu/I; \quad E = F/I$$

we have $$\ddot{\theta} + \kappa\dot{\theta} + n^2\theta = E \cos pt. \quad \text{.......................(1)}$$

The solution of this is

$$\theta = \frac{E \sin \epsilon}{p\kappa} \cos (pt - \epsilon) + Ae^{-\frac{1}{2}\kappa t} \cos \{\sqrt{(n^2 - \tfrac{1}{4}\kappa^2)}\, t - a\}, \quad \text{......(2)}$$

where $\tan \epsilon = \dfrac{p\kappa}{n^2 - p^2}$ and A, a are constants.

The first term in the value of θ in (2) gives the forced, and the second term the natural vibrations, the period of the latter being

$$\frac{2\pi}{\sqrt{(n^2 - \frac{1}{4}\kappa^2)}} = T. \text{ say.}$$

The value of T was always very near to 120 secs., and the mean of various determinations during the observations gave

$$T = \frac{2\pi}{\sqrt{(n^2 - \frac{1}{4}\kappa^2)}} = 120{\cdot}8 \text{ secs.} \qquad\qquad (3)$$

Value of κ. When there are only natural vibrations

$$\frac{\text{any complete swing}}{\text{next complete swing}} = e^{\frac{1}{2}\kappa\,.\,\frac{1}{2}T}.$$

The value of this ratio was usually near 1·4. The mean of a number of determinations taken at various times was 1·3953. Putting

$$e^{30\cdot 2\kappa} = 1{\cdot}3953,$$

we get

$$\kappa = 0{\cdot}011033.$$

Value of n. Substituting for κ in the value of T in (3) we get

$$n^2 = 0{\cdot}0027359,$$

and

$$n = 0{\cdot}052306.$$

Value of ϵ. The forced period $2\pi/p$ was always 115 secs., whence

$$\tan\epsilon = \frac{p\kappa}{n^2 - p^2} = 2{\cdot}420,$$

and

$$\epsilon = 67^\circ\, 33',$$

$$\sin\epsilon = 0{\cdot}9242.$$

From equation (1) it will be seen that the steady deflection due to F is

$$\frac{E^2}{n}$$

while from (2) the amplitude of the forced oscillations is

$$\frac{E\sin\epsilon}{p\kappa} \quad \text{or} \quad \frac{n^2\sin\epsilon}{p\kappa}\cdot\frac{E}{n^2}.$$

Using the values found for $n\kappa$ and ϵ we have

$$\frac{n^2\sin\epsilon}{p\kappa} = 4{\cdot}196,$$

or the forced oscillations give a cumulative effect, about four times the steady deflection due to the couple at its maximum value.

Value of Moment of Inertia, I. This was found by vibrating the cage hung by a short quartz fibre, (1) when empty, (2) when containing the sphere, the times of vibration being respectively 8·38 secs. and 11·22 secs. The sphere weighs 1·004 grams, and its radius is 0·45 centim., so that its moment of inertia $\frac{2}{5}Mr^2 = {\cdot}08132$.

From this, and the times of vibration, we get

$$I = 0{\cdot}1821.$$

Value of F. The vibrations were observed in scale-divisions, each 0·05 centim., the distance between mirror and scale being 358 centims. If

N is the number of scale-divisions in the amplitude of vibration, i.e., in half the range, we have from (2)

$$\frac{E \sin \epsilon}{p\kappa} = \frac{5N}{2 \times 35800},$$

whence $$F = EI = 0{\cdot}8293N \times 10^{-8},$$

using the values already found for ϵ, κ, I.

Taking the limiting values of the amplitudes as half the mean ranges given in Table III, the vibration due to the quadrantal couple has amplitude not greater than 0·033 div., and that due to the semicircular couple, amplitude not greater than 0·095 div. Whence

$$F \text{ (quadrantal) is not greater than } 2{\cdot}737 \times 10^{-10},$$

and $$F \text{ (semicircular) is not greater than } 7{\cdot}878 \times 10\ ^{10}.$$

Perhaps some idea of these values may be obtained by noticing that the times of vibration of the small sphere under couple F per radian would be respectively 32 hours and 25 hours. But it is probably best to interpret the value in terms of the assumptions we made as to the force in the introduction. We found for the quadrantal couple

$$F = (G - G')\, MM'/r,$$
$$= \frac{G - G'}{G} \cdot \frac{GMM'}{r},$$

where M, M' are the masses of the spheres, r the distance between their centres, G, G' the parallel and crossed gravitation-constants.

Now M, the mass of the larger sphere, is 399·9, say 400 grams,
M' ,, ,, smaller ,, 1·004 grams,
r is 5·9 centims.,
G and G' are exceedingly near $6{\cdot}66 \times 10^{-8}$,

whence $$\frac{G - G'}{G} = \frac{Fr}{GMM'} = \frac{1}{16500}.$$

On the assumed law of force this implies that the attractions between the two spheres, with distance 5·9 centims. between their centres, do not differ in the parallel and crossed positions by as much as $\frac{1}{16500}$ of the whole attraction.

We may compare this result with Rudberg's values of the refractive indices of quartz for the mean D line

$$\frac{\mu_e - \mu_0}{\mu_0} = \frac{1{\cdot}55328 - 1{\cdot}54418}{1{\cdot}54418} = \frac{1}{170} \text{ about.}$$

For the semicircular couple

$$2F = \frac{G - G'}{G} \cdot \frac{GMM'}{r},$$

whence $$\frac{G - G'}{G} = \frac{1}{2850}.$$

P C W. 10

On the assumed law of force, this implies that the attractions between the two spheres, with distance 5·9 centims. between their centres, with their axes parallel and respectively in like and unlike directions, do not differ by as much as $\frac{1}{2850}$ of the whole attraction.

This limit is large, undoubtedly owing to the want of axial symmetry in the apparatus which produced a semicircular couple as already pointed out. This couple was large, and though we attempted to eliminate it by the two sets of observations with the different azimuths of the larger sphere, in all probability we failed.

TABLE I. *Showing Scale-Readings in Tenths of a Division at Phases at Heads of Columns. Time of Revolution of Larger Sphere* 115 *secs.*

0	1	2	3	4	5	6	7	8	9
...	...	...	...	55	61	61	64	61	42
25	26	31	40	50	55	60	53	52	45
44	40	45	50	54	51	49	49	48	49
52	57	52	54	57	52	40	33	28	25
30	44	57	66	70	64	52	40	38	36
39	46	55	60	63	61	52	44	44	45
44	43	49	50	52	48	42	30	32	37
45	50	62	71	69	62	52	42	39	38
44	55	58	65	65	66	61	51	45	45
41	38	40	49	56	61	62	60	56	50
48	42	39	37	40	42	58	69	69	68
58	48	41	38	38	42	48	54	60	57
55	50	43	41	41	42	47	49	55	57
63	60	58	49	46	47	46	44	51	52
50	54	48	45	44	40	36	40	50	60
67	67	62	54	44	33	35	35	38	50
57	62	68	62	52	45	36	36	39	44
51	56	59	53	47	48	53	51	50	49
50	49	52	50	50	51	53	52	54	55
48	47	44	41	44	52	55	58	60	56
49	41	41	42	43	47	50	55	60	60
60	56	58	47	43	47	49	50	50	50
54	54	54	48	50	51	49	52	52	45
42	43	48	49	55	56	52	52	52	57
56	51	46	42	42	43	49	51	55	55
55	52	49	57	50	50	50	44	43	50
50	50	43	43	46	50	58	54	55	50
49	49	49	48	50	51	54	53	56	56
57	58	56	56	51	43	40	38	41	51
60	60	60	58	52	48	48	48	52	57
58	60	57	47	41	41	51	62	63	59
53	46	40	40	40	43	49	51	61	60
60	56	51	48	42	42	43	51	59	63
62	61	55	52	51	50	51	51	52	56
58	58	53	45	40	41	49	60	70	70
60	52	48	48	50	50	54	55	53	51
50	50	47	50	50	52	53	53	50	48
48	46	48	50	51	51	50	50	52	52
49	46	44	44	49	50	55	59	57	58
51	49	46	43	44	51	59	68	64	56
50	42	40	49	57	68	71	70	59	50

TABLE I (*continued*).

	0	1	2	3	4	5	6	7	8	9
	47	41	43	56	65	66	56	45	39	35
	33	40	48	59	68	70	64	51	43	42
	48	51	60	64	70	67	56	42	39	38
	40	47	52	65	60	61	59	51	51	50
	48	47	50	54	53	60	52	50	41	39
	40	44	51	58	66	70	71	63	50	38
	35	38	41	43	50	56	70	75	70	59
	50	46	45	51	61	70	71	70	62	52
	41	40	40	40	47	54	60	71	71	68
	60	50	48	45	39	39	42	49	51	60
	64	61	50	46	47	49	52	60	72	75
	70	62	57	32	23	21	30	42	57	77
	84	79	63	51	42	33	34	38	49	60
	66	64	57	51	49	44	47	49	52	52
	55	55	52	58	59	56	57	51	42	40
	43	47	55	61	66	64	60	52	50	45
	41	45	49	59	67	67	56	50	49	43
	38	45	48	53	55	56	57	55	54	56
	53	49	42	42	51	61	69	70	65	54
	45	41	40	47	51	56	61	59	55	49
	48	52	60	60	60	58	50	46	44	43
	45	51	53	60	63	67	62	60	55	48
	44	46	49	50	52	54	53	50	50	52
	60	62	63	61	51	41	39	38	42	50
	55	59	54	51	48	47	42	47	48	55
	58	61	62	60	59	54	52	52	50	50
	58	54	55	55	58	56	56	50	51	51
	56	58	51	52	48	48	54	55	50	51
	52	51	51	50	45	44	42	46	51	55
	56	53	56	59	59	60	58	59	59	54
	49	46	46	49	50	52	58	56	57	53
	51	50	50	46	49	51	58	66	67	69
	65	62	51	46	39	39	39	45	51	56
	62	61	53	48	40	38	47	62	67	63
	55	52	57	56	56	53	49	42	38	41
	51	60	65	71	73	72	60	52	50	40
	42	49	52	62	71	73	73	65	59	44
	39	38	40	43	51	51	59	61	61	50
	49	41	49	51	52	58	58	52	52	50
	53	56	57	51	50	49	49	49	51	52
	49	52	53	52	...	...	...	...	...	...
Mean of 80 in divisions ...	5·175	5·163	5·143 min.	5·186	5·246	5·294	5·355 max.	5·284	5·300	5·216

Mean range 5·355 — 5·143 — 0·212 division.

Greatest range in one period 7·5 — 3·5 – 4·0 divisions.

TABLE II.

Series	Azimuth of large sphere	Period of revolution	Number of periods observed	Mean readings at phases (whole numbers omitted)										Mean range in Scale-divisions	Greatest range in a period in Scale-divisions
				0	1	2	3	4	5	6	7	8	9		
	°	secs.													
A 1	0	115	80	·175	·163	·143	·186	·246	·294	·355	·284	·300	·216	·212	4·0
A 2	0	115	80	·653	·558	·566	·653	·813	·950	1·030	1·008	·929	·769	·472	3·1
B 1	180	115	80	·485	·590	·624	·648	·556	·464	·379	·328	·284	·364	·364	3·0
B 2	180	115	70	·423	·503	·650	·836	·941	·961	·843	·714	·540	·464	·538	7·5
B 3	180	115	54	·650	·632	·619	·648	·656	·680	·717	·739	785	·739	·166	2·7
	°	secs.													
C 1	0	230	72	·708	·731	·708	·739	·717	·711	·676	·678	·642	·688	·097	1·3
C 2	0	230	80	·370	·400	·358	·326	·271	·214	·173	·158	·253	·310	·242	3·4
C 3	0	230	80	·616	·654	·686	·673	·663	·627	·571	·560	·566	·584	·126	2·0
D 1	90	230	50	1·024	1·042	1·034	1·004	·988	·920	·926	·954	·994	1·010	·122	2·2
D 2	90	230	70	·031	·090	·150	·210	·220	·230	·223	·176	·126	·096	·199	3·1

TABLE III.

Series	Mean readings at phases										Mean range
	0	1	2	3	4	5	6	7	8	9	
A	·414	·361	·355	·420	·530	·622	·693	·646	·615	·493	·338
B (advanced 180°)	·702	·646	·594	·536	·522	·519	·575	·631	·711	·718	·199
Means of *A* and *B*	·558	·503	·474	·478	·526	·570	·634	·638	·663	·605	·189
C	·565	·595	·584	·579	·550	·517	·473	·465	·487	·527	·130
D (advanced 90°)	·575	·575	·565	·560	·553	·528	·566	·592	·607	·604	·079
Means of *C* and *D*	·570	·585	·575	·570	·552	·523	·520	·529	·547	·566	·065

5.

AN EXPERIMENT WITH THE BALANCE TO FIND IF CHANGE OF TEMPERATURE HAS ANY EFFECT UPON WEIGHT.

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In all the experiments hitherto made to determine the gravitative attraction between two masses, the temperature has not varied more than a few degrees, and there are no results which would enable us to detect with certainty any dependence of attraction upon temperature even if such dependence exists. It is true, as Professor Hicks has pointed out*, that Baily's results for the Mean Density of the Earth, if arranged in the order of the temperature of the apparatus when they were obtained, show a fall in value as the temperature rises. But this is almost certainly some secondary effect, due to errors in the measurements of the apparatus, or to the seasons at which different attracted masses were used†.

The ideal experiment to find if temperature has an effect on gravitation would consist in one determination of the gravitative attraction between two masses at, say 15° C., and another determination at, say, the temperature of boiling liquid air. But the difficulties of exact determination at ordinary temperatures are not yet overcome, and at any very high or very low temperatures, they would be so much increased that the research seems at present hopeless.

The question can, however, be attacked in a somewhat less direct method by examining whether the weight of a body—the gravitative attraction of the earth upon it—varies when the temperature of the body varies. The various parts of one of the attracting masses—the Earth remain, each part, at the same temperature throughout, and this is, no doubt, a weakness of the method. For it is perhaps conceivable that in the expression for the attraction a temperature factor might exist of some such form as $1 + \kappa\,(Mt + mt')/(M + m)$, where M and m are the two masses, and t and t' are their temperatures. If $m\ M$ is negligible, this reduces to $1 + \kappa t$, and

* *Proc. Camb. Phil. Soc.*, vol. 5, p. 156.

† Poynting, *Mean Density of the Earth*, p. 56.

is independent of the temperature t' of the smaller mass. But it seems more likely that each mass would have a separate temperature factor. If such a factor exists, and if its variation is appreciable, then we ought to be able to detect a change of weight with change of temperature.

Observations on pendulums suffice to show that at the most any such effect must be small. The nearly constant period of vibration with the nearly constant length of a compensated or an 'invar' pendulum shows constancy of weight of the bob to a considerable degree of exactness. Again, the agreement of weight-methods and volume-methods of measuring the expansion of liquids with rise of temperature shows, though less conclusively, that there is no great variation.

It appeared to us that it would be possible to go much further in testing constancy of weight by a direct weighing experiment, in which the weight on one side of a balance should be subjected to great changes of temperature while the counterpoise should remain at a uniform temperature. We give an account in this paper of a series of experiments carried out on the following principle. A brass cylinder weighing 266 grammes was hung by a wire from one arm of a balance so as to be near the bottom of a tube depending from the floor of the balance-case, the tube being closed at the bottom and opening at the top into the case, the wire passing down through the opening. The brass cylinder was counterpoised by an equal cylinder hung by a short wire from the other arm inside the case. To this short wire was attached a finely divided scale on which the swings of the balance could be read by a microscope looking through a window in the case. The balance was released and left free to swing. Then the case was exhausted till the pressure was not more than a small fraction of a millimetre of mercury. Steam was passed round the lower part of the tube where the weight hung, and after a time the weight was allowed to cool again. In other experiments the lower part of the tube was cooled by liquid air and again brought up to the temperature of the room.

While the changes of temperature were in progress there were considerable apparent variations in weight. But ultimately, when the temperature became steady, the weight, too, became steady. At 100° C. it was slightly less than at the temperature of the room. This difference was partly due to a rise in the temperature of the case, such a rise being always accompanied by an apparent diminution of the weight in the tube, whether steam was applied or the balance was merely left to follow the temperature of the room. Probably this effect was due to some change in the balance-beam. But the difference was partly due to convection-currents, or at any rate to the residual air in the case, for it varied with the disposition of diaphragms in the tube. There were no doubt convection-currents, as there was always a tendency for the case to rise in temperature when steam was applied, and this could hardly be accounted for by conduction or radiation, under the conditions of the apparatus. As effects due to residual air should depend upon surface and not upon volume, similar experiments were made with hollow weights, each about 58 grammes, and of the same size and form as the solid weights. There was again an apparent diminution in weight when steam was applied. Any true diminution due to change of temperature should be shown by a difference in the diminution with the solid and with the hollow weights, the surface effects being eliminated, and this diminution should be that of 266 – 58 – 208 grammes.

The net result of all the experiments was that there was not a greater change in 208 grammes between 15° C. and 100° C., than 0·003 milligramme. But an inspection of the detailed account given later shows that this result is probably accidentally small—within the limits of experimental error. It would imply that there is not a change greater than 1 in 6 10^9 per 1 C. But the experiments hardly justify us in saying more than that there is not a change greater than 1 in 10^9 per 1°.

When liquid air was used, air-currents were absent, and the temperature variations of the case were much less. The net result of these experiments was that there is not a change of weight in 208 grammes between 16 C. and – 186° C. greater than 0·002 milligramme. This would imply that there is not a change greater than 1 in $1{\cdot}3 \times 10^{10}$ per 1 change of temperature. We may probably assert that the change is not greater than 1 in 10^{10} per 1 C.

We now proceed to a detailed account of the apparatus and of the mode of using it.

The Balance.

The balance has a 6-inch beam and was specially constructed for the experiment by Mr. Oertling. The general arrangement will be seen from Fig. 1. The base-plate is of gun-metal, as are also two sides and the top of the case. The front and back of the case are of thick plate-glass fixed to the metal by marine glue. In the experiments the base-plate was supported on levelling-screws on a slate slab, and between it and the slab was a gas-pipe with pinhole burners so that it could be warmed. When the case was to be fixed in position the jets were lighted and sealing-wax was smeared on to the area of contact of plate and case. When the wax was quite liquid the case was put down on the plate and the gas was turned off. When the metal was cool the joint was perfectly air-tight.

The tube *T* in which the weight *W* hung was of brass, 4·1 cm. internal diameter and 62·5 cm. long. It consisted of three parts. The topmost was brazed to the base-plate and the two lower parts were attached to it and to each other by flanged joints *ff*. Between the flanges was placed a circular lead washer of diamond-shaped section. When the flanges were pressed together by bolts the joint was quite air-tight. Round the middle section of the tube was a water-jacket *wj* through which water flowed while an experiment was in progress, and round the lowest section was a steam-jacket *sj* through which either water or steam could be passed. This jacket could be removed and could be replaced by a vacuum-vessel 30 cm. long containing liquid air.

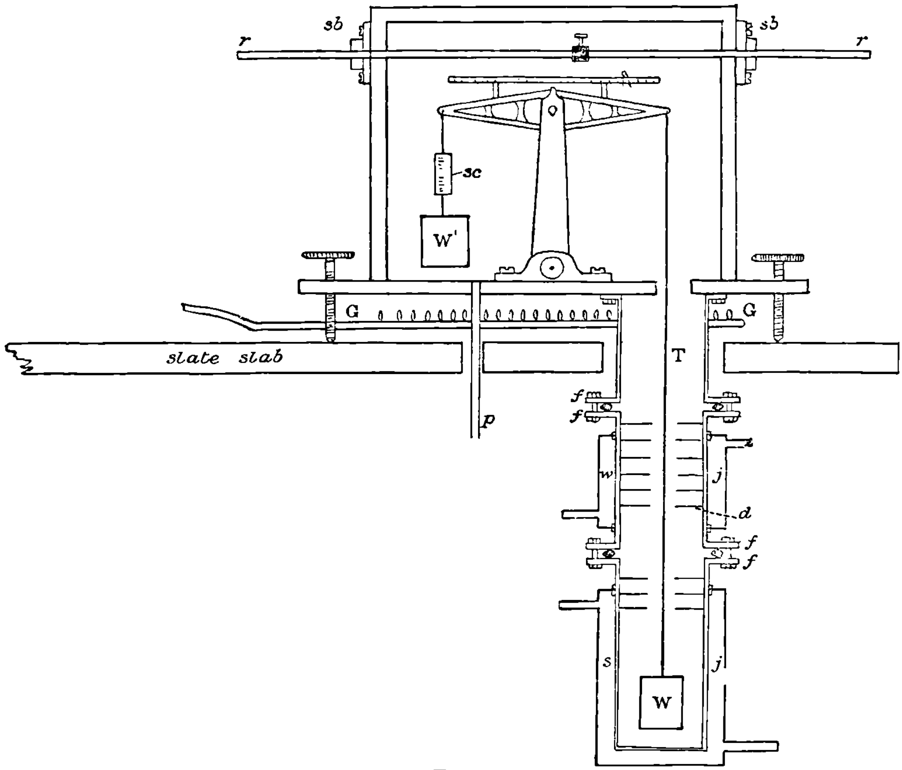


Fig. 1.

W, weight of which the temperature is to be raised, *W′* counterpoise.
T, tube in which it hangs, with a number of diaphragms with ½-inch holes.
sj, steam-jacket, replaced by liquid-air jacket.
ff, flanged joint with lead washer.
wj, water-jacket.
p, pipe to the exhausting pump.
sc, scale read by a microscope not shown.
rr, rider-rod passing through stuffing-boxes, *sb*, enlarged in Fig. 2.
GG, gas-burners to heat the base-plate before sealing up.

The weights *W*, *W′* were turned from the same gun-metal bar. The length of each was 4·45 cm., the diameter 3 cm., and the solid weights were each 266·17 grammes, while the hollow ones were 57·86 grammes. They were hung directly from the end-plates of the balance by platinum wires, and any residual inequality was compensated by moving a centigramme-rider along the beam by the rider-rod *rr*. This rod passed through stuffing-boxes *sb*, designed for us by Mr. G. O. Harrison, the mechanical assistant in the laboratory, to whom we are much indebted for this and many other valuable suggestions, and for the careful construction of all the apparatus except the balance. These stuffing-boxes were perfectly air-tight when screwed up and the rod could still be rotated without any leakage. But to draw it in or out it was necessary to loosen the screws slightly, and in one case when this was done some leakage occurred. As the construction appears to give an efficient mode of moving apparatus inside a vacuum from without, we give in Fig. 2 a section of a stuffing-box.

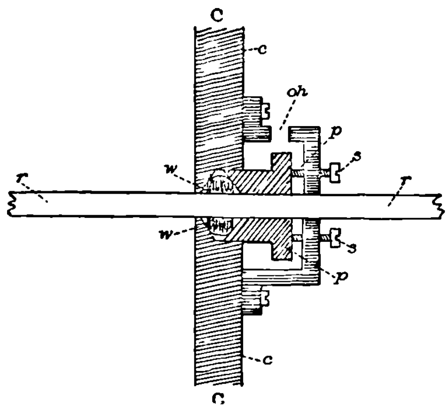


Fig. 2.

rr, rider-rod.
CC, side of case.
ww, two or three circular washers punched out of soft leather and soaked in oil.
p, plunger driven in by screws, *ss*.
oh, oil-hole through which valvoline, a thick lubricating oil, was inserted.

The position of the balance-beam was read by a microscope viewing a glass scale *sc*, Fig. 1, interposed in the suspension of *W′*. The scale was divided to 0·1 mm. and numbered in millimetres. The objective of the microscope was placed inside the case and the eye-piece with cross-hairs was fixed outside it. The axis of the microscope was horizontal and a lamp at the back illuminated the scale. The case was surrounded by felt and a tin cover was placed over the whole, small windows through the felt then allowing the scale to be seen. A thermometer placed between the felt and the case was taken to give the temperature of the case.

A brass pipe *p* from the floor of the case led to the pumping apparatus. This pipe was connected to a branched glass tube, one branch going to a Fleuss pump and the other to a 4-fall Sprengel, made continuous in its action by a steel pump which was worked by a motor, and which raised the mercury again from the cistern at the base to the reservoir at the top. When the case was to be exhausted the Fleuss pump was first used and then sealed off and the exhaustion was carried on by the Sprengel. The degree of exhaustion was estimated by sending a discharge through a vacuum-bulb 10 cm. diameter connected with the tube to the pump, and usually the pumping was continued till the negative dark space was of the order of 4 cm. As a rule the vacuum held without serious change for days or even for weeks.

Mode of Experiment.

A large number of preliminary experiments was made with a pair of brass weights each about 187 grammes. These were only useful in bringing to the front the difficulties in obtaining good results and in suggesting means for overcoming them. We shall only record the final results with the 266 grammes and 58 grammes weights.

The weights and the lower section of the tube were first cleaned by boiling in caustic potash solution and washing in distilled water. They were then suspended, being handled with gloves only, and the lowest section of the tube was screwed on.

Steam-Heating.

The jacket *sj* (Fig. 1) was fixed on the lower section of the tube and the balance was set free to vibrate, being left free during a whole series of experiments. The case was then sealed on and the value of a scale-division was determined by the rider. Any change in the value during a series could be determined from the change in period of the swing. The time of swing in different series ranged from 24 to 42 seconds. After the stuffing-boxes were tightened the case was exhausted till the pressure was estimated to be not more than $\frac{1}{50}$ mm. of mercury. The weight of air displaced by a weight was then of the order 0·001 milligramme and the change in this with change of temperature was quite negligible.

Cold water was passed through the water-jacket *wj*, and sometimes, while steam was being got up in a boiler at some distance well screened from the balance, through *sj* also. The centre of swing and the temperature of the case were observed, and before any heating occurred the balance was usually quite steady. Steam was then blown through *sj*, water still flowing through *wj*. After considerable changes, which will be described later, the centre of swing in the course of five or six hours settled down to a steady march which appeared to correspond to change in temperature of the case. Sometimes steam was turned off after eight or nine hours, but in some cases it was kept on for 24 and 48 hours and even longer. Then it was turned off and the jacket was allowed to cool. The centre of swing was observed when steady, several hours later or next day.

The results in the first few heatings and coolings, after an exhaustion of the case, were rejected, as there was evidence that the earlier heatings drove gas from the weight. Only after successive heatings gave fairly consistent values were these taken into account.

One effect of the steam-heating was always to raise the temperature of the case, probably through convection of the residual air. A rise of temperature in the case was always accompanied by a lowering of the scale-reading, corresponding to a diminution in weight. The effect was somewhat irregular, but an average value of the lowering per 1° rise was determined by observing the centre of swing of the balance at intervals through several days, when the balance was left to follow the varying temperature of the room and no steam was flowing. The value thus obtained was used to correct all the readings to 15° C.

As an example of the method pursued, we give in Table I the series of readings used to obtain the temperature-correction for the hollow weights.

TABLE I. *Change of Centre of Swing with Change of Temperature of Case.*

Date	Time	Centre of swing in millimetres of scale	Temperature of case
6.12.04	1.0 p.m.	14·34	14·75
,,	3.0 ,,	14·355	14·55
,,	5.40 ,,	14·35	14·65
7.12.04	12.35 ,,	14·51	12·70
,,	5.5 ,,	14·465	13·60
8.12.04	11.5 a.m.	14·43	14·70
,,	12.55 p.m.	14·20	14·95
9.12.04	9.51 a.m.	14·555	12·80
,,	2.27 p.m.	14·525	13·3
10.12.04	9.37 a.m.	14·46	15·0

The temperature-correction deduced from these numbers by the method of least squares is a decrease of 0·13 division per 1 C. rise, and as the sensibility was 1 division for 0·248 milligramme, there was an apparent decrease of weight of 0·032 milligramme per 1° rise.

Two similar series with the solid weights gave a decrease of 0·044 division per 1° C. rise, and as the sensibility was now 1 division per 0·803 milligramme, there was an apparent decrease of weight of 0·035 milligramme per 1 rise.

Another series with the solid weights when steam was passing all the time for several days, gave a decrease of 0·052 division per 1° rise, but as the values were more irregular, the series giving 0·044 division were used. This series with steam sufficed to show that very nearly the same temperature-correction applied when the weight was hot as when it was cold.

The irregularity of the observations is only to be expected when it is remembered that the balance was subjected to some considerable vibration at times through machinery running in the same building, and that the observations extended over several days. Indeed it is remarkable that there was not more irregularity, and the fair consistency of the observations illustrates once more the marvellous accuracy of a well-made balance.

The following Table II will serve as an example of a complete experiment in which one of the hollow weights was cold initially, was then surrounded with steam for 24 hours, and was then allowed to get cold again. The observations recorded are at about hourly intervals, but intermediate ones, not used, were frequently taken to be sure that there were no sudden changes.

TABLE II. *Experiment with Hollow Weight raised to* 100° *C. and then cooled,* 1 *mm.* = 0·248 *milligramme.*

Correction for temperature of case − 0·13 division per 1°.

Date	Time	Condition of weight	Centre of swing, 1 = 1 mm.	Temperature of case	Centre of swing corrected to 15° C.	Remarks
17.11.04	9.25 a.m.	Cold	14·905	14°·75	14·872	Steam put on just after 9.25 and kept on till 10 a.m. next day
,,	4.0 p.m.	Hot	14·40	15 ·0	14·400	
,,	5.20 ,,	,,	14·39	15 ·0	14·390	
,,	6.12 ,,	,,	14·375	15 ·0	14·375	
,	7.8 ,,	,,	14·365	15 ·05	14·372	
,,	7.56 ,,	,,	14·36	15 ·1	14·373	
18.11.04	9.5 a.m.	,,	14·30	15 ·05	14·307	
,,	9.55 ,,	,,	14·295	15 ·1	14·308	Steam turned off just after 9.55
,,	5.36 p.m.	Cold	14·55	16 ·00	14·680	
,,	6.39 ,,	,,	14·56	15 ·95	14·684	
,,	7.56 ,,	,,	14·58	15 ·75	14·678	
19.11.04	9.40 a.m.	,,	14·67	14 ·30	14·579	
,,	11.38 ,,	,,	14·665	14 ·20	14·561	

Initial reading, cold at 15°	14·872 divisions
Final mean reading, cold at 15°	14·636 ,,
Mean reading, cold	14·754 ,,
,, hot	14·360 ,,
Cold − hot	0·394 division

The following Table III gives the results of the various experiments with the hollow weight, treated as in Table II, the readings of the centre of swing being at about hourly intervals when on the same day.

TABLE III. *Experiments with Hollow Weight raised to* 100° *C. and then cooled,* 1 *mm.* = 0·248 *milligramme.*

Correction for temperature of case − 0·044 division per 1°.

Date	Condition of weight	Centre of swing corrected to 15°	Number of readings from which centre of swing is found	Greatest deviation from the mean	Excess of cold above hot	Remarks
16.11.04 ...	Hot	14·749	4	0·020	0·123	Temperatures 14 ·75 to 16°·3 The initial cold reading was rendered useless by a subsequent shift of scale-reading, probably due to slight displacement of the eye-piece
17.11.04 ...	Cold	14·872	1	—		
17.11.04 ...	Cold	14·872	1	—	0·394	Temperatures 14 ·2 to 16 The set given in full in Table II. The last of the preceding used as the first of this
17–18.11.04	Hot	14·360	7	0·058		
18–19.11.04	Cold	14·636	5	0·075		
21.11.04 ...	Cold	14·218	1	—	0·165	Temperatures 10 ·45 to 14 ·8
22–23.11.04	Hot	14·091	6	0·095		
23.11.04 ...	Cold	14·294	3	0·040		
25.11.04 ...	Cold	14·421	1	—	0·187	Temperatures 10 ·95 to 14 ·5
,, ...	Hot	14·029	2	0·001		
26.11.04 ...	Cold	14·010	2	0·016		
1.12.04 ...	Cold	14·500	1	—	0·334	Temperatures 16 to 16 ·9
,, ...	Hot	14·100	3	0·007		
2.12.04 ...	Cold	14·367	1	—		
2.12.04 ...	Cold	14·367	1	—	0·166	Temperatures 16 ·9 to 17 ·45
2–3.12.04	Hot	14·218	3	0·048		
3.12.04 ...	Cold	14·400	1	—		
12.12.04 ...	Cold	14·379	1	—	0·279	Temperatures 13 ·65 to 15 ·75
,, ...	Hot	14·106	3	0·018		
13.12.04 ...	Cold	14·390	1	—		

Mean value cold − hot = 0·235 division = 0·058 milligramme.

The following Table IV gives the results with the solid weight. They are not so consistent as those with the hollow weight, probably because they were spread over a longer time on the average. This was done to secure that the weight should be more nearly at the temperature of its surroundings. A rough estimate shows that if heat be gained by radiation alone and the brass is taken as a full radiator, three hours will be required to bring it within 1° of the temperature of the steam. The last two experiments were incomplete in that no final cold weighing was taken, but the results obtained were regarded as probably sufficient.

TABLE IV. *Experiments with Solid Weight raised to* 100° *C. and then cooled,* 1 *mm.* = 0·803 *milligramme.*

Correction for temperature of case − 0·044 division per 1°.

Date	Condition of weight	Centre of swing corrected to 15°	Number of readings from which centre of swing is found	Greatest deviation from the mean	Excess of cold above hot	Remarks
26.12.04 ...	Cold	16·295	1	—		Temperatures 9°·9 to 11°·0
27–28.12.04	Hot	16·032	2	0·017	0·139	
29.12.04 ...	Cold	16·046	1	—		
30.12.04 ...	Cold	16·016	1	—		Temperatures 9°·1 to 12°·05
30–31.12.04	Hot	15·914	4	0·021	0·112	
2.1.05......	Cold	16·035	1	—		
2.1.05......	Cold	16·035	1	—		Temperatures 9°·1 to 12°·1
3.1.05......	Hot	16·022	2	0·018	0·019	
4.1.05......	Cold	16·047	1	—		
5.1.05......	Cold	15·987	1	—		Temperatures 11°·95 to 12°·65 Experiment interrupted by stoppage of steam tubes
,, ...	Hot	15·919	3	0·002	0·068	
9.1.05......	Cold	16·048	1	—		Temperatures 13°·7 to 15°·65 Heating continued several days after this to obtain temperature-correction. No final cold reading taken
10.1.05......	Hot	16·039	4	0·020	0·009	

Mean value cold − hot = 0·069 division = 0·055 milligramme.

From Tables III and IV we have:

Solid weight, 266 grammes: cold − hot ... = 0·055 milligramme.
Hollow weight, 58 grammes: cold − hot ... = 0·058 ,,
For the difference, 208 grammes: hot − cold = 0·003 ,,

Taking the rise in temperature as 85°, this gives a change of the order of 1 in 6×10^9 per 1° rise. But evidently the smallness of the result is accidental, and probably all we can assert from the work is that any change of weight with change of temperature between 15° C. and 100° C. is not greater than 1 in 10^9.

Cooling with Liquid Air.

Experiments were made in which heating by steam was replaced by cooling with liquid air. This was supplied to us by Sir William Ramsay, and we desire to express our hearty thanks to him for his ready kindness in helping us to increase the temperature range so considerably. In these experiments the steam-jacket was removed and replaced by a vacuum-vessel 30 cm. deep and 6 cm. inside diameter, kept full of liquid air. After the steady state was reached the liquid air was removed, the jacket was replaced and cold water was again passed round the tube.

Owing to the evaporation of the air the experiments had to be carried out more rapidly than those with steam, but through the absence of convection-currents a steady state was more rapidly reached, and the variation in the temperature of the case was very small.

The temperature-correction was not observed, but as in the subsequent observations with both solid and hollow weights it was found to be about 0·03 milligramme per 1°, this value was assumed to hold here. In any case its effect is very small, as the temperature varied so little.

The centre of swing was observed nearly continuously from the time when the liquid air was applied and again after it was removed. After a time in each case it became steady and only these steady values are recorded in the following Tables.

TABLE V. *Experiment with Solid Weight cooled by Liquid Air, 1 mm. — 0·315 milligramme.*

Correction for temperature of case — 0·1 division per 1 .

Date	Time	Condition of weight	Centre of swing	Tempera ture of case	Centre of swing corrected to 16 ·6	Remarks
28.7.04	3.25 p.m.	Normal	11·085	16 ·6	11·085	Liquid air applied just after 3.25
,,	5.50 ,,	Cold	11·07	16 ·65		
,,	6.0 ,,	,,	11·07	16 ·65	11·075	
,,	6.10 ,,	,,	11·07	16 ·65		Liquid air removed and water applied just after 6.10
,,	8.45 ,,	Normal	11·095	16 ·4	11·075	
,,	9.15 ,,	,,	11·095	16 ·4		

Normal - cold = 0·005 division — 0·0016 milligramme.

TABLE VI. *Experiment with Hollow Weight cooled by Liquid Air, 1 mm. = 0·343 milligramme.*

Correction for temperature of case — 0·1 division per 1 .

Date	Time	Condition of weight	Centre of swing	Tempera-ture of case	Centre of swing corrected to 16 ·6	Remarks
9.9.04	9.40 a.m.	Normal	14·485	16°·3	14·455	Liquid air applied at 9.43
,,	11.40 ,,	Cold	14·480	16 ·4	14·460	
,,	11.50 ,,	,,	14·480	16 ·4		Removed at 11.52
,,	5.25 p.m.	Normal	14·480	16 ·4	14·460	Steady The balance next morning read 14·48 at 16 ·3

Normal - cold = - 0·002 division — - 0·0007 milligramme.

From Tables V and VI we have:

Solid weight, 266 grammes: normal − cold ...	= 0·0016	milligramme.
Hollow weight, 58 grammes: normal − cold...	= − 0·0007	,,
For the difference, 208 grammes: normal − cold	= 0·002	,,

Taking the fall in temperature as 200°, this gives a change of the order of 1 in 2×10^{10} per 1° fall.

These liquid-air experiments were not repeated. But the conditions are probably much less disturbed than with the steam experiments, and we may safely say that if there is any change of weight with change of temperature between 16°·6 C. and − 186° C., it is not so great as 1 in 10^{10} per 1° C.

Note on the Change of Apparent Weight on First Heating or Cooling.

We have mentioned that while the changes in the temperature of the weight were in progress there were considerable apparent variations in weight. These, in a few cases, amounted to as much as 0·6 milligramme. They were almost certainly due to radiometric forces or to other gas-action, for they were very dependent on the disposition of the diaphragms in the tube *T* (Fig. 1), and also on the way in which the steam was blown through the jacket.

In the preliminary experiments with solid weights the lowest diaphragm was 5 to 6 inches above the weight, and the steam was blown into the top of the jacket. Under these circumstances the following variations occurred when the steam was turned on:

At first the weight apparently increased, until in 15 to 20 minutes it reached a maximum, which was in some cases as much as 0·6 milligramme above the real weight. After reaching this maximum the weight apparently decreased, till in four hours it had reached a nearly steady value, which was a little less than the value at the temperature of the laboratory.

If, now, the jacket was filled with cold water, the apparent weight first increased for about one minute and then decreased for about two hours to a minimum, which was a little lower than the final weight at 100°. After this the weight very slowly increased, till in five to six hours it had recovered the value which it had before the experiment.

These changes did not vary very much with the pressure, but at lower pressures they took place more rapidly than at higher ones.

On cooling the weight with liquid air, changes occurred exactly similar to those which occurred when the weight was cooled from 100° to the temperature of the laboratory; and when the weight was warmed up from the temperature of liquid air to the temperature of the laboratory, the changes were similar to those when the weight was warmed from the temperature of the laboratory to 100° C.

So long as the arrangement of the diaphragms and the weight remained the same, and so long as the steam was blown through in the same way, these changes were exactly similar, but as soon as any alteration was made in these arrangements the character of the changes altered.

In one series of experiments a sealed glass bulb containing mercury was used in place of the brass weight. In this case, immediately after the steam was turned on there was a rapid decrease in weight, and a minimum was reached in less than one minute. After this the changes were very similar to those occurring with the brass weight. On cooling, however, the changes were almost exactly the reverse of the changes on heating, and were not at all like the changes with the brass weight.

In the final experiments, those recorded, the lowest diaphragm was within $\frac{1}{2}$-inch of the top of the brass weight. With this arrangement and with the steam blown into the top of the jacket, the following changes occurred:

The apparent weight first increased rapidly, reaching a maximum in about one minute, then it rapidly decreased, reaching a minimum in about four minutes, and again increased to another and lower maximum in 8 to 10 minutes. After this it slowly decreased to a nearly steady value a little lower than the original value.

On passing cold water through the jacket the apparent weight rapidly increased for about one minute, and then slowly decreased to its original value.

Still another variation was arranged by blowing the steam in at the bottom of the jacket instead of at the top, all the other things remaining as in the last experiment.

In this case, on turning on the steam, the apparent weight first decreased to a minimum in about one minute, then increased to a maximum in about six minutes, and finally decreased slowly to a nearly steady value a little below the original value.

The cooling and the consequent changes were exactly similar to those in the last experiment.

It is somewhat difficult to follow out exactly the changes which would be caused by radiometer-action and by convection-currents in these different arrangements of the apparatus, but the fact that these changes depend entirely on the arrangement is sufficient evidence that they are caused by gas-action, and, as we have before said, we have some reason to believe that even the small final difference is due to air-currents.

6.

ON A METHOD OF DETERMINING THE SENSIBILITY OF A BALANCE.

By J. H. Poynting and G. W. Todd, M.Sc.

[*Phil. Mag.* **18**, 1909, pp. 132–135.]

[*Read* June 25, 1909.]

In the method, as we have arranged it, a small frame (Fig. 1, end-view) is fixed at the centre of the beam of a 16-inch Oertling balance. This carries two **V**s about 2 cm. apart, and in the **V**s lies a straight wire or fibre about $3\frac{1}{2}$ cm. long, parallel to the beam and level with the central knife-edge. This wire takes the place of the ordinary rider, and we shall call it 'the rider.' Its weight is determined before use as accurately as possible by weighing on an assay-balance. The sensibility is determined by moving the rider either to right or left a measured distance. If this distance is d, if the half-length of beam is b, and if the weight of the rider is R, the movement is equivalent to an addition of weight to one pan, Rd/b.

In order to move the rider a definite distance a stout horizontal rod (Fig. 2) passes through the balance-case from side to side without contact with the case, and is supported at its ends outside, and independent of, the case. It is parallel to the beam and a little lower than the **V** frame. On the rod are fixed horizontally two Brown and Sharp micrometer-screws divided to 0·01 mm. and allowing an estimate of 0·001 mm. Their axes are in one line coinciding with the axis of the rider, and they are fixed so that one can bear against one end and the other against the other end of the rider. Their ends are plane and the ends of the rider are bluntly pointed. Each micrometer-screw head has a cross-piece fixed on it; and a fork, which can be rotated about an axis in the continuation of the axis of the screw by a pulley outside the case, can engage with the cross-piece and so advance or withdraw the screw. The pulley is worked by an endless string passing to a pulley at the side of the observer, who is about 2 metres in front of the balance. The micrometer-divisions are illuminated and each micrometer is viewed by its own telescope. The position of the balance-beam is read by a double-suspension mirror, telescope and scale. The scale is divided to millimetres and is about 3 metres from the mirror. The double-suspension mirror is fully described in the *Phil. Trans.* A, vol. 182, p. 572*. It is of course not essential to the method, but was chosen because of the great magnification of the deflection which it gives.

Let us suppose that the value of the scale-divisions of the deflection is to be determined by a movement of the rider from right to left. The two micrometer-screws are withdrawn so that neither is in contact with the rider, that on the left so far that the rider will not touch it in its subsequent travel. The beam is lowered and allowed to swing. Then the right-hand screw is advanced till it bears against the end of the rider and pushes it some small distance. The contact is seen to have occurred by the interference with freedom of swing, as watched in the telescope. The micrometer is then

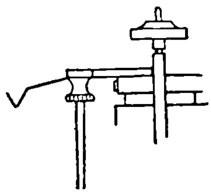


Fig. 1. End-view of V frame fixed to balance-beam.

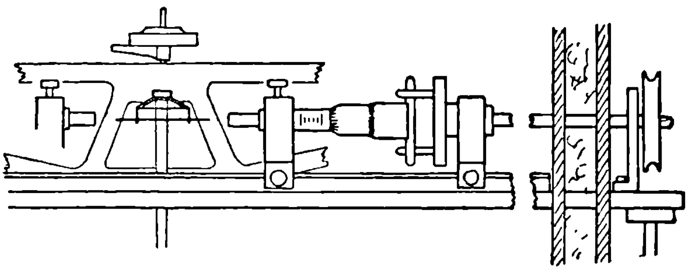


Fig. 2. Arrangement of right-hand micrometer-screw.

read. Let its reading be m_1. It is then withdrawn a little so as to leave the rider free, and the centre of swing C_1 is determined in the usual way from three successive turning-points. Then the micrometer is advanced again so as to push the rod a little further, and its reading m_2 is taken. It is then withdrawn and the new centre of swing C_2 is taken. If $m_1 \quad m_2 \quad d$, $C_1 - C_2$ divisions of deflection are due to an addition of Rd b to the left pan.

The right-hand micrometer may then be withdrawn and the left-hand micrometer may be brought into action in a similar manner, and so on, the two screws being used alternately.

The balance-case was fixed on a shelf and was enclosed in a tin-foiled wooden box with wool loosely packed between box and case. The case and

* [*Collected Papers*, Art. 3.]

box were provided with plate-glass windows to view the mirror and the micrometer-divisions. The following abstract of some determinations of sensibility will serve to show what accuracy may be attained:

I. Rider German silver wire, 7·35 mgm.
Half-length of beam, 20·272 cm.
10 determinations alternately left and right.
Mean travel of rider ... ... 2·4850 mm.
Mean deflection ... ... 21·26 divisions.
Mean value for 20 divisions ... 0·0848 mgm.
The separate determinations range between
20 divisions = 0·0877
and 20 „ = 0·0824.

II. The same rider.
10 determinations alternately left and right.
Mean travel of rider ... ... 5·2713 mm.
Mean deflection ... ... 45·47 divisions.
Mean value for 40 divisions ... 0·1681 mgm.
The separate determinations range between
40 divisions = 0·1722
and 40 „ = 0·1632.

III. Rider German silver wire, 189·05 mgm.
7 determinations alternately left and right.
Mean travel of rider ... ... 0·1764 mm.
Mean deflection ... ... 38·70 divisions.
Mean value for 40 divisions ... 0·1691 mgm.
The separate determinations range between
40 divisions = 0·1709
and 40 „ = 0·1654.

IV. The same rider.
7 determinations alternately left and right.
Mean travel of rider ... ... 0·3004 mm.
Mean deflection ... ... 64·84 divisions.
Mean value for 60 divisions ... 0·2578 mgm.
The separate determinations range between
60 divisions = 0·2612
and 60 „ = 0·2518.

PART II.

ELECTRICITY.

7.

ON THE LAW OF FORCE WHEN A THIN, HOMOGENEOUS, SPHERICAL SHELL EXERTS NO ATTRACTION ON A PARTICLE WITHIN IT.

[*Manchester Lit. Phil. Soc. Proc.* **16**, 1877, pp. 168 171.]

[*Read* March 6, 1877.]

If a homogeneous, thin, spherical shell of uniform thickness exert no attraction on a particle within it, then the law of the force is the law of nature.

Professor Maxwell uses this proposition (*Electricity*, vol. 1, § 74) to deduce the law of the force between electrified bodies, and shows that it proves, far more conclusively than any direct measurements of electrical forces, that the law is that of the inverse square. It would therefore be an advantage to have a simpler proof of such an important proposition than that given by Laplace (*Méc. Céleste*, LIV. ii, ch. 2) and followed by Maxwell. The following seems more simple, as it requires neither integration nor the solution of a functional equation:

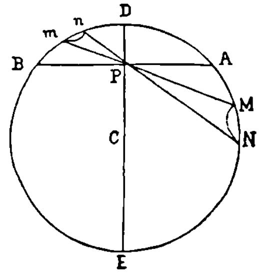


Fig. 1.

Let P be any point inside the spherical shell, C the centre of the sphere, $DPCE$ the diameter through P, and APB perpendicular to CD. In Newton's proof of the proposition that, if the law of attraction be that of the inverse square, the force at P is zero, the surface is divided into an indefinitely great number of opposite elements by small cones having their vertices at P, and the attractions of each of these pairs of elements are shown to balance each other. We shall first show that if the attraction at P is zero, then it follows inversely that, for at least one position (if not for all positions) of the cone *MPm* besides the position *APB*, the attractions of the opposite elements balance each other; and we shall thence prove that the law of attraction must be that of the inverse square.

Let us suppose the cone, with vertex at *P*, to move round from the position where *AB* is its axis to any other position *MPm*. At *AB* the attractions of the opposite sections on *P* are equal, whatever the law of the force. As the cone leaves *AB* let us suppose the resultant attraction of the two opposite elements to be no longer zero, but to act, say, towards the centre side of *APB*. Then it will either continue towards that side as the cone moves all the way round from *APB* to *BPA*, or it will vanish at some position, and then act in the opposite direction. In the first case we should have a number of forces all acting from *P* towards the same side of *APB*, whose resultant is zero; then each separate force must be zero. In the second case the resultant attraction of the opposite sections vanishes somewhere between *AP* and *EP*; then for at least one position of the cone, besides the position *APB*, the resultant attraction of the opposite sections vanishes.

Since this is true for any position of *P*, we can show that the law of the force must be that of the inverse square.

In the position where the two opposite sections exert equal attractions, two sections of the same thickness perpendicular to the axis of the cone would also exert equal attractions; for they would bear to each other the same ratio as the two oblique sections made by the sphere, since these two oblique sections make equal angles with the axis of the cone. Then what we have proved is, that for every position of *P* there are two different distances for which the attractions of the sections of a small cone of equal thickness on a point at its vertex are equal.

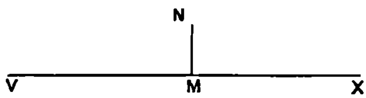


Fig. 2.

Let us take *VMX* to represent the axis of a cone of very small angle of which *V* is the vertex.

At any point *M* draw an ordinate *MN* to represent the attraction of a section of the cone at *M* of small given thickness on a point at the vertex. Then *N* will trace out a curve as *M* moves along *VX*.

Now take a spherical shell of thickness equal to the thickness of the sections of the cone, and of radius nearly equal to *VM*, where *M* is any arbitrary point in *VX*. Take a point near the centre of this sphere. As a cone moves round with this as vertex, its sections by the sphere must be always at distances very nearly equal to the radius from the vertex;

and, by what we have proved above, for some position of the cone the attractions of the opposite sections must be equal. Therefore (in Fig. 2), for two distances very nearly equal to VM the ordinates must be equal to one another. Then the tangent to the curve near N must be parallel to VX. But M is arbitrary, for we can take the sphere of any size. Therefore at all points the tangent to the curve is parallel to VX; and therefore the curve must be a straight line parallel to VX; or, the attractions by sections of the cone of equal thickness are constant, wherever the sections be taken. But the sections are proportional to the direct square of the distance; and therefore the law of the attraction must be that of the inverse square of the distance.

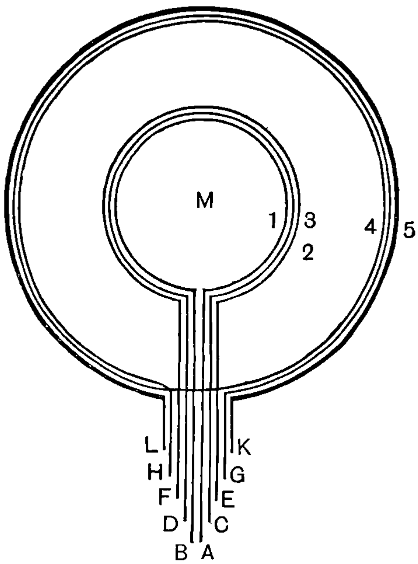
8.

ARRANGEMENT OF A TANGENT GALVANOMETER FOR LECTURE ROOM PURPOSES TO ILLUSTRATE THE LAWS OF THE ACTION OF CURRENTS ON MAGNETS, AND OF THE RESISTANCE OF WIRES.

[*Manchester Lit. Phil. Soc. Proc.* **18**, 1879, pp. 85–88.]

[*Read* April 1, 1879.]

Three coils of similar wire are arranged round the circumference of a circle with a compass-needle at the centre, each wire going only once round the circle. The six ends of the three wires are connected by thick copper wires with the six binding screws *A*, *B*, *C*, *D*, *E*, *F*.



On a concentric circle of twice the radius is arranged a coil of the same wire going twice round the circle and having its ends connected by thick wires with the binding screws *G*, *H*. On the same circle is a single wire of twice the diameter, making only one turn round the circle, and having its ends connected with the binding screws *K*, *L*.

The coils are denoted respectively by the numbers 1–5.

The null method is adopted in each case. That is two forces acting on the needle and arising from different arrangements of the circuit are shown to balance each other, and from the arrangements necessary to produce this equilibrium the desired laws are deduced.

I. *If the current be reversed the force is reversed.* Introduce the current at *A*. Join *BD* and lead away at *C*. Then the same current goes in opposite directions round the coils (1) and (2), and since the needle is not deflected the reversal of the force when the current is reversed is proved.

II. *The force is proportional to the length of current acting.* This is proved by the last, for the two coils (1) and (2) exert equal forces on the needle. If the current went round them in the same direction we should have twice the force which each exerted singly, with twice the length of current. This assumes that the current in different parts of the circuit is the same, which might be shown by slightly modifying I, thus: introduce between B and D various resistances and the equilibrium is not disturbed.

III. *The force is proportional to the strength of the current.* Introduce at A, and connect A with C, and B with D; and connect D with F, and lead off from E. There will be two equal currents in the same directions in coils (1) and (2), for they are exactly similar to each other and similarly situated. These two currents unite to give a double current in the opposite direction in coil (3), and the double current in a single wire exerts twice the force exerted by each single current, since there is no deflection.

IV. *The force is inversely proportional to the square of the distance.* Introduce at A, connect B with H, and lead off at G. Then since we have two turns to the coil (4) we have a current of four times the length at twice the distance acting in opposition to the same current through (1). Since there is no deflection, the two exert equal forces, and therefore the force is inversely proportional to the square of the distance.

RESISTANCE.

I. *The resistance is proportional to the length.* Connect B with C, D with E, A with F. Introduce at A and lead off at E. We have then a divided circuit joining A and E, one branch consisting of the two coils (1) and (2), and the other the coil (3) only one-half the length and going in the opposite direction. But as there is no deflection the current through the first circuit of twice the length must be only one-half that through the other to exert an equal force, i.e., the resistance is doubled when the length is doubled.

II. *The resistance is inversely proportional to the cross-section.* Introduce at A, connect A with L, connect B with C and D with K, and lead off at K. Then we have two circuits connecting A with K, the first consisting of the two coils (1) and (2), the second of the coil (5) of the same length but of four times the cross-section and going round in the opposite direction. Since the needle is not deflected the two currents exert equal forces. But the coil (5 is at twice the distance and must therefore have four times as great a current through it as that through (1) and (2). That is, with equal lengths of wire of the same material connecting two points the currents conveyed are proportional to the cross-sections.

9.

ON THE GRADUATION OF THE SONOMETER.

[*Phil. Mag.* **9**, 1880, pp. 59–64.]

Read before the Physical Society, December 13, 1879.]

It seems likely that such valuable results will be obtained by means of Professor Hughes's sonometer, that it is desirable that some method should be employed to turn its at present arbitrary readings into absolute measure, so that, for instance, the induced currents caused by different metals in the induction-balance may be measured and compared with each other.

In Maxwell's *Electricity*, vol. 2, chap. XIV, the general formula is given for the coefficient of induction of one circular circuit on another. Adapting this to the case where two equal circular circuits are on the same axis at a distance apart greater than the radius of the coils, the following formula is obtained.

Let a = distance between centres,

b = radius of either circle,

c = distance of either circumference from centre of other,

M = coefficient of induction.

Then $$M = -\frac{4\pi^2 b^4}{c^3}\left\{\frac{1}{2} - \frac{3}{4}\frac{b^2}{c^2} + \frac{15}{8}\frac{b^4}{c^4} - \frac{35}{8}\frac{b^6}{c^6} + \frac{2835}{256}\frac{b^8}{c^8} - \text{etc.}\right\} \quad \ldots\ldots(1)$$

or $$M = -\frac{4\pi^2 b^4}{a^3}\left\{\frac{1}{2} - \frac{3}{2}\frac{b^2}{a^2} + \frac{75}{16}\frac{b^4}{a^4} - \frac{490}{32}\frac{b^6}{a^6} + \frac{24570}{256}\frac{b^8}{a^8} - \text{etc.}\right\} \ldots\ldots(2)$$

Of these the latter uses directly the distance between the centres, the observed quantity—but is not nearly so convergent as the former, in which c may be at once deduced from $c = \sqrt{a^2 + b^2}$.

To obtain formulae which might be strictly applied to the sonometer, we should have to consider the more general case of two coils of unequal radii b and β, for which I have found the formula corresponding to (2), viz.

$$M = \frac{4\pi b^2\beta^2}{a^3}\left\{\frac{1}{2} - \frac{3}{4}\frac{b^2+\beta^2}{a^2} + \frac{15}{16}\frac{b^4 + 3b^2\beta^2 + \beta^4}{a^4} - \frac{35}{32}\frac{b^6 + 6b^4\beta^2 + 6b^2\beta^4 + \beta^6}{a^6} + \text{etc.}\right\}. \ldots\ldots(3)$$

We should then have to take the finite integrals of each term between the limiting values of b and β. But this would be exceedingly complicated and would require a knowledge of all the details of construction; and we may at least get a first approximation to the true result by replacing the coils by a single one of a radius intermediate between the greatest and least radii.

In Prof. Hughes's paper (*Phil. Mag.* July 1879) he gives the internal and external radii of his coils as 15 millims. and 27·5 millims. respectively. I have considered, then, that 25 millims. will give results not very far from the truth; and as it makes the calculations considerably easier, I have taken that as the value of b and applied the formulae to the numbers given in the paper. The resultant current in the middle coil was zero when it was distant 47 millims. from one end and 200 from the other. This enables us to find the ratio between the number of turns in the two ends at least sufficiently nearly to apply to some of the results.

Let M_1 be the coefficient of induction of the larger coil on the moveable one, M_2 that of the smaller, the former having m turns, the latter n. When the moveable coil was 200 millims. from the large and 47 millims. from the small coil, since there was no induced current,

$$mM_1 = nM_2.$$

Applying formula (1), we have for the larger coil

$$c - \sqrt{200^2 + 25^2} = 201\cdot5,$$

and for the smaller coil

$$c = \sqrt{47^2 + 25^2} = 53\cdot2,$$

b being the same for both. Then

$$\frac{m}{(201\cdot5)^3}\left\{\frac{1}{2} - \frac{3}{4}\left(\frac{25}{201\cdot5}\right)^2 + \frac{15}{8}\left(\frac{25}{201\cdot5}\right)^4 - \text{etc.}\right\}$$
$$= \frac{n}{(53\cdot2)^3}\left\{\frac{1}{2} - \frac{3}{4}\left(\frac{25}{53\cdot2}\right)^2 + \frac{15}{8}\left(\frac{25}{53\cdot2}\right)^4 - \frac{35}{8}\left(\frac{25}{53\cdot2}\right)^6 + \frac{2835}{256}\left(\frac{25}{53\cdot2}\right)^8 - \text{etc.}\right\}.$$

Multiplying each side by 2 and finding the successive terms,

$$m \times \frac{122}{10^9}\{1 - \cdot02308 + \cdot00088 - \text{etc.}\}$$
$$= n \times \frac{6645}{10^9}\{1 - \cdot33123 + \cdot18286 - \cdot09422 + \cdot02633 - \text{etc.}\},$$

or

$$\frac{m}{n} = 43\cdot6.$$

I have applied the formula to the results for various metals given by Prof. Hughes in a table in his paper. In the table below, in the second column are Prof. Hughes's numbers, i.e. distances from the point of no induction. In the third are numbers proportional to $mM_1 - nM_2$; where M_1, M_2 are the coefficients of induction of two simple coils calculated on the above hypothesis, m and n the number of turns in the two respectively. In the fourth column are the resistances for bars of the metal 100 millims. long and 1 millim. in diameter (Jenkin, p. 249). In the last column are the products of the numbers in the two preceding columns.

Metal	Distance from point of no induction	$mM_1 - nM_2$, proportional to	R	$(mM_1 - nM_2)\,R$
Silver	125	178	·21	37·4
Gold	117	135	·27	36·5
Aluminium ...	112	116	·375	43·5
Copper ...	100	84	·21	17·6
Zinc	80	50·1	·72	36·1
Tin	74	44·6	1·70	75·8
Iron	45	22·46	1·25	28·1
Lead	38	18·87	2·5	47·2
Antimony ...	35	17·35	4·5	78·1
Bismuth ...	10	5·75	16·8	96·6

Mercury has been omitted, as it gives a very much higher value than any of the others. Were the induced currents in the induction-balance proportional to the resistances given in the table, the numbers in the last column would of course be all the same. The deviations from equality are far greater than could be accounted for by errors in the approximations I have adopted, especially for the metals not at the beginning or end of the list. Hence we are driven to conclude, either that the resistances of the metals given in the tables are not the same as the resistances of the metals used by Prof. Hughes, or that the induced current is not proportional to the conductivity of the metal.

It should be noticed that the method of measuring currents by the sonometer assumes that the telephone integrates, as it were, the current; i.e. the loudness of the sound depends only on the total current, not on the time during which the current is passing, provided that the time be very short. I do not know whether this point has been investigated; but if not, it would probably be easy to examine it by means of the sonometer. It would be advisable to modify the instrument in such a way that the formulae might be more easily employed, and that the approximations might be nearer to the truth.

The formulae used in this paper may be obtained as follows, the method being adapted from that given by Maxwell.

The potential of a circular unit current at any point is the same as that of a magnetic shell of unit strength bounded by the circuit. This, again, is the same as the attraction of a thin plate of matter of unit surface-density in a direction perpendicular to the plane of the plate. If ω be the attraction of a plate of radius b, at a point distant c from the plate along its axis,

$$\omega = 2\pi\left(1 - \frac{1}{\sqrt{1 + \frac{b^2}{c^3}}}\right)$$

$$= 2\pi\left\{\frac{1}{2}\frac{b^2}{c^2} - \frac{1\,.\,3}{2\,.\,4}\frac{b^4}{c^4} + \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\frac{b^6}{c^6} - \text{etc.}\right\}.$$

If we introduce zonal harmonics as coefficients, this becomes

$$\omega = 2\pi\left\{\frac{1}{2}\frac{b^2}{c^2}P_1 - \frac{1\,.\,3}{2\,.\,4}\frac{b^4}{c^4}P_3 + \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\frac{b^6}{c^6}P_5 - \text{etc.}\right\}.$$

This is now the potential at any point in space where $b < c$.

If there be a second circular circuit of radius β on the same axis, we may suppose it replaced by a magnetic shell bounded by the current and lying on the sphere, with centre at the centre of the first current, the radius of the sphere being c.

This shell may be considered to consist of two layers of matter of equal and opposite densities, μ and $-\mu$, at distances c and $c \quad dc$ from the centre. The potential on the second layer is

$$\iint \mu\omega\, dS,$$

where the integration is taken over the shell. The potential on the first layer is

$$-\iint \mu\left(\omega + \frac{d\omega}{dc}dc\right)dS,$$

the sum being $$-\iint \mu\frac{d\omega}{dc}dc\,dS;$$

but since the strength $= 1$, $\mu dc - 1$, and we have the mutual potential

$$M - -\iint \frac{d\omega}{dc}dS.$$

Replacing the element dS by $c^2 d\mu d\phi$, the limits will be for ϕ from 0 to 2π, and for μ from 1 to μ.

Integrating with respect to ϕ, and remembering that c is constant in integrating for μ, we have

$$M = 2\pi c^2\int_\mu^1 \frac{d\omega}{dc}\,d\mu = -4\pi^2 c^2\left\{\frac{b^2}{c^3}\int_\mu^1 P_1 d\mu - \frac{1\,.\,3}{2\,.\,4}\,.\,4\,\frac{b^4}{c^5}\int_\mu^1 P_3 d\mu + \text{etc.}\right\}.$$

But we have the relation for zonal harmonics,

$$\int_\mu^1 P_n d\mu = \frac{1-\mu^2}{n(n+1)}\frac{dP_n}{d\mu}.$$

Substituting, we obtain

$$M = -4\pi^2(1-\mu^2)\left\{\frac{b^2}{2c}\frac{dP_1}{d\mu} - \frac{1.3}{2.4}\frac{b^4}{3c^3}\frac{dP_3}{d\mu} + \frac{1.3.5}{2.4.6}\frac{b^6}{5c^5}\frac{dP_5}{d\mu} - \text{etc.}\right\}.$$

The following are the values for the coefficients (Ferrers's *Spherical Harmonics*. p. 23), both in terms of μ and when we substitute $\mu^2 = 1 - \frac{\beta^2}{c^2}$:

$$\frac{dP_1}{d\mu} = 1,$$

$$\frac{dP_3}{d\mu} = \frac{3}{2}(5\mu^2 - 1) = \frac{3}{2}\left(4 - 5\frac{\beta^2}{c^2}\right),$$

$$\frac{dP_5}{d\mu} = \frac{15}{8}(21\mu^4 - 14\mu^2 + 1) = \frac{15}{8}\left(8 - 28\frac{\beta^2}{c^2} + 21\frac{\beta^4}{c^4}\right),$$

$$\frac{dP_7}{d\mu} = \frac{3003\mu^6 - 3465\mu^4 + 945\mu^2 - 35}{16} = \frac{448 - 3024\frac{\beta^2}{c^2} + \text{etc.}}{16},$$

$$\frac{dP_9}{d\mu} = \frac{109395\mu^8 - 180180\mu^6 + 90090\mu^4 - 13860\mu^2 + 315}{128}$$

$$= \frac{5760 + \text{etc.}}{128}.$$

Substituting these values and putting $c^2 = a^2 + \beta^2$, we obtain

$$M = -4\pi\frac{b^2\beta^2}{a^3}\left\{\frac{1}{2} - \frac{3}{4}\frac{b^2+\beta^2}{a^2} + \frac{15}{16}\frac{b^4+3b^2\beta^2+\beta^4}{a^4} - \frac{35}{32}\frac{b^6+6b^4\beta^2+6b^2\beta^4+\beta^6}{a^6} + \text{etc.}\right\}.$$

The more useful form is obtained by retaining c. If we take the two circles of equal radius (i.e. $b = \beta$), we obtain

$$M = -4\pi^2\frac{b^4}{c^3}\left\{\frac{1}{2} - \frac{3}{4}\frac{b^2}{c^2} + \frac{15}{8}\frac{b^4}{c^4} - \frac{35}{8}\frac{b^6}{c^6} + \frac{2835}{256}\frac{b^8}{c^8} - \text{etc.}\right\}.$$

10.

ON THE TRANSFER OF ENERGY IN THE ELECTROMAGNETIC FIELD.

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A space containing electric currents may be regarded as a field where energy is transformed at certain points into the electric and magnetic kinds by means of batteries, dynamos, thermoelectric actions, and so on, while in other parts of the field this energy is again transformed into heat, work done by electromagnetic forces, or any form of energy yielded by currents. Formerly a current was regarded as something travelling along a conductor, attention being chiefly directed to the conductor, and the energy which appeared at any part of the circuit, if considered at all, was supposed to be conveyed thither through the conductor by the current. But the existence of induced currents and of electromagnetic actions at a distance from a primary circuit from which they draw their energy has led us, under the guidance of Faraday and Maxwell, to look upon the medium surrounding the conductor as playing a very important part in the development of the phenomena. If we believe in the continuity of the motion of energy, that is, if we believe that when it disappears at one point and reappears at another it must have passed through the intervening space, we are forced to conclude that the surrounding medium contains at least a part of the energy, and that it is capable of transferring it from point to point.

Upon this basis Maxwell has investigated what energy is contained in the medium, and he has given expressions which assign to each part of the field a quantity of energy depending on the electromotive and magnetic intensities and on the nature of the matter at that part in regard to its specific inductive capacity and magnetic permeability. These expressions account, as far as we know, for the whole energy. According to Maxwell's theory, currents consist essentially in a certain distribution of energy in and around a conductor, accompanied by transformation and consequent movement of energy through the field.

Starting with Maxwell's theory, we are naturally led to consider the problem: How does the energy about an electric current pass from point to point—that is, by what paths and according to what law does it travel from the part of the circuit where it is first recognisable as electric and magnetic to the parts where it is changed into heat or other forms?

The aim of this paper is to prove that there is a general law for the transfer of energy, according to which it moves at any point perpendicularly to the plane containing the lines of electric force and magnetic force, and that the amount crossing unit of area per second of this plane is equal to the product of the intensities of the two forces, multiplied by the sine of the angle between them, divided by 4π; while the direction of flow of energy is that in which a right-handed screw would move if turned round from the positive direction of the electromotive to the positive direction of the magnetic intensity. After the investigation of the general law several applications will be given to show how the energy moves in the neighbourhood of various current-bearing circuits.

The following is a general account of the method by which the law is obtained.

If we denote the electromotive intensity at a point (that is, the force per unit of positive electrification which would act upon a small charged body placed at the point) by $\mathfrak{E}$, and the specific inductive capacity of the medium at that point by K, the magnetic intensity (that is, the force per unit pole which would act on a small north-seeking pole placed at the point) by $\mathfrak{H}$ and the magnetic permeability by μ, Maxwell's expression for the electric and magnetic energies per unit volume of the field is

$$K\mathfrak{E}^2/8\pi + \mu\mathfrak{H}^2/8\pi. \quad \text{.............................}(1)$$

If any change is going on in the supply or distribution of energy the change in this quantity per second will be

$$K\mathfrak{E}\frac{d\mathfrak{E}}{dt}\Big/4\pi + \mu\mathfrak{H}\frac{d\mathfrak{H}}{dt}\Big/4\pi. \text{.......................}\ \text{....}(2)$$

According to Maxwell the true electric current is in general made up of two parts, one the conduction-current $\mathfrak{K}$, and the other due to change of electric displacement in the dielectric, this latter being called the displacement-current. Now, the displacement is proportional to the electromotive intensity, and is represented by $K\mathfrak{E}/4\pi$, so that when change of displacement takes place, due to change in the electromotive intensity, the rate of change, that is, the displacement-current, is $K\frac{d\mathfrak{E}}{dt}\Big/4\pi$, and this is equal to the difference between the true current $\mathfrak{C}$ and the conduction-current $\mathfrak{K}$. Multiplying this difference by the electromotive intensity $\mathfrak{E}$ the first term in (2) becomes

$$\frac{K\mathfrak{E}}{4\pi}\frac{d\mathfrak{E}}{dt} = \mathfrak{C}\mathfrak{E} - \mathfrak{K}\mathfrak{E}. \quad \text{..............................}(3)$$

The first term of the right side of (3) may be transformed by substituting for the components of the total current their values in terms of the components of the magnetic intensity, while the second term, the product of the conduction-current and the electromotive intensity, by Ohm's law, which states that $\mathfrak{K} = C\mathfrak{E}$, becomes $\mathfrak{K}^2 C$, where C is the specific conductivity. But this is the energy appearing as heat in the circuit per unit volume according to Joule's law. If we sum up the quantity in (3) thus transformed, *for the whole space within a closed surface,* the integral of the first term can be integrated by parts, and we find that it consists of two terms—one an expression depending on the surface alone to which each part of the surface contributes a share depending on the values of the electromotive and magnetic intensities at that part, the other term being the change per second in the magnetic energy (that is, the second term of (2)) with a negative sign. The integral of the second term of (3) is the total amount of heat developed in the conductors within the surface per second. We have then the following result.

The change per second in the electric energy within a surface = (a quantity depending on the surface) − (the change per second in the magnetic energy) − (the heat developed in the circuit).

Or rearranging:

The change per second in the sum of the electric and magnetic energies within a surface together with the heat developed by currents is equal to a quantity to which each element of the surface contributes a share depending on the values of the electric and magnetic intensities at the element. That is, the total change in the energy is accounted for by supposing that the energy passes in through the surface according to the law given by this expression.

On interpreting the expression it is found that it implies that the energy flows as stated before, that is, perpendicularly to the plane containing the lines of electric and magnetic force, that the amount crossing unit area per second of this plane is equal to the product

$$\frac{\text{electromotive intensity} \times \text{magnetic intensity} \quad \text{sine included angle}}{4\pi},$$

while the direction of flow is given by the three quantities, electromotive intensity, magnetic intensity, flow of energy, being in right-handed order.

It follows at once that the energy flows perpendicularly to the lines of electric force, and so along the equipotential surfaces where these exist. It also flows perpendicularly to the lines of magnetic force, and so along the magnetic equipotential surfaces where these exist. If both sets of surfaces exist their lines of intersection are the lines of flow of energy.

The following is the full mathematical proof of the law:

The energy of the field may be expressed in the form (Maxwell's *Electricity*, vol. 2, 2nd ed., p. 253)

$$\tfrac{1}{2}\iiint (Pf + Qg + Rh)\, dx\,dy\,dz + \frac{1}{8\pi}\iiint (a\alpha + b\beta + c\gamma)\, dx\,dy\,dz,$$

the first term the electrostatic, the second the electromagnetic energy.

But since $f = \frac{K}{4\pi} P$, with corresponding values for g and h, and $a = \mu\alpha$, $b = \mu\beta$, $c = \mu\gamma$, substituting, the energy becomes

$$\frac{K}{8\pi}\iiint (P^2 + Q^2 + R^2)\, dx\,dy\,dz + \frac{\mu}{8\pi}\iiint (\alpha^2 + \beta^2 + \gamma^2)\, dx\,dy\,dz. \quad \ldots(1)$$

Let us consider the space within any fixed closed surface. The energy within this surface will be found by taking the triple integrals throughout the space.

If any changes are taking place the rate of increase of energy of the electric and magnetic kinds per second is

$$\frac{K}{4\pi}\iiint \left(P\frac{dP}{dt} + Q\frac{dQ}{dt} + R\frac{dR}{dt}\right) dx\,dy\,dz + \frac{\mu}{4\pi}\iiint \left(\alpha\frac{d\alpha}{dt} + \beta\frac{d\beta}{dt} + \gamma\frac{d\gamma}{dt}\right) dx\,dy\,dz. \quad (2)$$

Now Maxwell's equations for the components of the true current are

$$u = p + \frac{df}{dt}, \qquad v = q + \frac{dg}{dt}, \qquad w = r + \frac{dh}{dt},$$

where p, q, r are components of the conduction-current.

But we may substitute for $\frac{df}{dt}$ its value $\frac{K}{4\pi}\frac{dP}{dt}$, and so for the other two, and we obtain

$$\left.\begin{aligned} \frac{K}{4\pi}\frac{dP}{dt} &= u - p \\ \frac{K}{4\pi}\frac{dQ}{dt} &= v - q \\ \frac{K}{4\pi}\frac{dR}{dt} &= w - r \end{aligned}\right\}. \quad \ldots\ldots\ldots\ldots(3)$$

Taking the first term in (2) and substituting from (3) we obtain

$$\begin{aligned} &\frac{K}{4\pi}\iiint \left(P\frac{dP}{dt} + Q\frac{dQ}{dt} + R\frac{dR}{dt}\right) dx\,dy\,dz \\ &= \iiint \{P(u-p) + Q(v-q) + R(w-r)\}\, dx\,dy\,dz \\ &= \iiint (Pu + Qv + Rw)\, dx\,dy\,dz - \iiint (Pp + Qq + Rr)\, dx\,dy\,dz. \ldots(4) \end{aligned}$$

Now the equations for the components of electromotive force are (Maxwell, vol. 2, p. 222)

$$\left.\begin{aligned} P &= c\dot{y} - b\dot{z} - \frac{dF}{dt} - \frac{d\psi}{dx} = c\dot{y} - b\dot{z} + P' \\ Q &= a\dot{z} - c\dot{x} - \frac{dG}{dt} - \frac{d\psi}{dy} = a\dot{z} - c\dot{x} + Q' \\ R &= b\dot{x} - a\dot{y} - \frac{dH}{dt} - \frac{d\psi}{dz} - b\dot{x} - a\dot{y} + R' \end{aligned}\right\}, \quad \ldots\ldots\ldots\ldots\ldots(5)$$

where P', Q', R' are put for the parts of P, Q, R which do not contain the velocities.

Then

$$\begin{aligned} Pu + Qv + Rw &= (c\dot{y} - b\dot{z})\, u + (a\dot{z} - c\dot{x})\, v + (b\dot{x} - a\dot{y})\, w + P'u + Q'v + R'w \\ &= -\{(vc - wb)\,\dot{x} + (wa - uc)\,\dot{y} + (ub - va)\,\dot{z}\} + P'u + Q'v + R'w \\ &= -(X\dot{x} + Y\dot{y} + Z\dot{z}) + P'u + Q'v + R'w, \end{aligned}$$

where X, Y, Z are the components of the electromagnetic force per unit of volume (Maxwell, vol. 2, p. 227).

Now substituting in (4) and putting for u, v, w their values in terms of the magnetic force (Maxwell, vol. 2, p. 233) and transposing we obtain

$$\begin{aligned} &\frac{K}{4\pi}\iiint\left(P\frac{dP}{dt} + Q\frac{dQ}{dt} + R\frac{dR}{dt}\right) dxdydz \\ &\qquad + \iiint\{(X\dot{x} + Y\dot{y} + Z\dot{z}) + (Pp + Qq + Rr)\}\, dxdydz \\ &= \iiint(P'u + Q'v + R'w)\, dxdydz \\ &= \frac{1}{4\pi}\iiint\left\{P'\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right) + Q'\left(\frac{d\alpha}{dz} - \frac{d\gamma}{dx}\right) + R'\left(\frac{d\beta}{dx} - \frac{d\alpha}{dy}\right)\right\} dxdydz \\ &= \frac{1}{4\pi}\iiint\left(R'\frac{d\beta}{dx} - Q'\frac{d\gamma}{dx}\right) dxdydz \\ &\qquad + \frac{1}{4\pi}\iiint\left(P'\frac{d\gamma}{dy} - R'\frac{d\alpha}{dy}\right) dxdydz \\ &\qquad + \frac{1}{4\pi}\iiint\left(Q'\frac{d\alpha}{dz} - P'\frac{d\beta}{dz}\right) dxdydz \end{aligned}$$

(Integrating each term by parts)

$$\begin{aligned} &= \frac{1}{4\pi}\iint(R'\beta - Q'\gamma)\, dydz + \frac{1}{4\pi}\iint(P'\gamma - R'\alpha)\, dzdx + \frac{1}{4\pi}\iint(Q'\alpha - P'\beta)\, dxdy \\ &\quad - \frac{1}{4\pi}\iiint\left\{\beta\frac{dR'}{dx} - \gamma\frac{dQ'}{dx} + \gamma\frac{dP'}{dy} - \alpha\frac{dR'}{dy} + \alpha\frac{dQ'}{dz} - \beta\frac{dP'}{dz}\right\} dxdydz \end{aligned}$$

(The double integral being taken over the surface)

$$\begin{aligned} &= \frac{1}{4\pi}\iint\{l\,(R'\beta - Q'\gamma) + m\,(P'\gamma - R'\alpha) + n\,(Q'\alpha - P'\beta)\}\, dS \\ &\quad - \frac{1}{4\pi}\iiint\left\{\alpha\left(\frac{dQ'}{dz} - \frac{dR'}{dy}\right) + \beta\left(\frac{dR'}{dx} - \frac{dP'}{dz}\right) + \gamma\left(\frac{dP'}{dy} - \frac{dQ'}{dx}\right)\right\} dxdydz, \ldots(6) \end{aligned}$$

where l, m, n are the direction-cosines of the normal to the surface outwards.

12—2

But from the values of P', Q', R' in (5) we see that

$$\frac{dQ'}{dz} - \frac{dR'}{dy} = -\frac{d^2G}{dt\,dz} - \frac{d^2\psi}{dx\,dz} + \frac{d^2H}{dt\,dy} + \frac{d^2\psi}{dz\,dx}$$

$$= \frac{d}{dt}\left(\frac{dH}{dy} - \frac{dG}{dz}\right)$$

$$= \frac{da}{dt} = \mu\frac{d\alpha}{dt} \text{ (Maxwell, vol. 2, p. 216),}$$

similarly

$$\frac{dR'}{dx} - \frac{dP'}{dz} = \frac{db}{dt} = \mu\frac{d\beta}{dt},$$

$$\frac{dP'}{dy} - \frac{dQ'}{dx} = \frac{dc}{dt} = \mu\frac{d\gamma}{dt}.$$

Whence the triple integral in (6) becomes

$$-\frac{\mu}{4\pi}\iiint\left(\alpha\frac{d\alpha}{dt} + \beta\frac{d\beta}{dt} + \gamma\frac{d\gamma}{dt}\right)dx\,dy\,dz.$$

Transposing it to the other side we obtain

$$\frac{K}{4\pi}\iiint\left(P\frac{dP}{dt} + Q\frac{dQ}{dt} + R\frac{dR}{dt}\right)dx\,dy\,dz + \frac{\mu}{4\pi}\iiint\left(\alpha\frac{d\alpha}{dt} + \beta\frac{d\beta}{dt} + \gamma\frac{d\gamma}{dt}\right)dx\,dy\,dz$$

$$+\iiint(X\dot{x} + Y\dot{y} + Z\dot{z})\,dx\,dy\,dz + \iiint(Pp + Qq + Rr)\,dx\,dy\,dz$$

$$= \frac{1}{4\pi}\iint\{l\,(R'\beta - Q'\gamma) + m\,(P'\gamma - R'\alpha) + n\,(Q'\alpha - P'\beta)\}\,dS. \quad \ldots(7)$$

The first two terms of this express the gain per second in electric and magnetic energies as in (2). The third term expresses the work done per second by the electromagnetic forces, that is, the energy transformed by the motion of the matter in which currents exist. The fourth term expresses the energy transformed by the conductor into heat, chemical energy, and so on; for P, Q, R are by definition the components of the force acting at a point per unit of positive electricity, so that $Pp\,dx\,dy\,dz$ or $Pdx\,p\,dy\,dz$ is the work done per second by the current flowing parallel to the axis of x through the element of volume $dx\,dy\,dz$. So for the other two components. This is in general transformed into other forms of energy, heat due to resistance, thermal effects at thermoelectric surfaces, and so on.

The left side of (7) thus expresses the total gain in energy per second within the closed surface, and the equation asserts that this energy comes through the bounding surface, each element contributing the amount expressed by the right side.

This may be put in another form, for if $\mathfrak{E}'$ be the resultant of P', Q', R', and θ the angle between its direction and that of $\mathfrak{H}$, the magnetic intensity, the direction-cosines L, M, N of the line perpendicular to the plane containing $\mathfrak{E}'$ and $\mathfrak{H}$ are given by

$$L = \frac{R'\beta - Q'\gamma}{\mathfrak{E}'\mathfrak{H}\sin\theta}; \qquad M = \frac{P'\gamma - R'\alpha}{\mathfrak{E}'\mathfrak{H}\sin\theta}; \qquad N = \frac{Q'\alpha - P'\beta}{\mathfrak{E}'\mathfrak{H}\sin\theta};$$

so that the surface-integral becomes

$$\frac{1}{4\pi}\iint \mathfrak{E}'\mathfrak{H}\sin\theta\,(Ll + Mm + Nn)\,dS.$$

If at a given point dS be drawn to coincide with the plane containing $\mathfrak{E}'$ and $\mathfrak{H}$, it then contributes the greatest amount of energy to the space; or in other words the energy flows perpendicularly to the plane containing $\mathfrak{E}'$ and $\mathfrak{H}$, the amount crossing unit area per second being $\mathfrak{E}'\mathfrak{H}\sin\theta\ 4\pi$. To determine in which way it crosses the plane take $\mathfrak{E}'$ along Oz, $\mathfrak{H}$ along Oy. Then

$$P' = 0, \qquad Q' = 0, \qquad \frac{R'}{\mathfrak{E}'} = 1,$$

$$\alpha = 0, \qquad \frac{\beta}{\mathfrak{H}} = 1, \qquad \gamma = 0,$$

and if $\sin\theta = 1$

$$L = 1, \qquad M = 0, \qquad N = 0.$$

If now the axis Ox be the normal to the surface outwards, $l = 1$, $m = 0$, $n = 0$, so that this element of the integral contributes a positive term to the energy within the surface on the negative side of the yz plane; that is, the energy moves along xO, or in the direction in which a screw would move if its head were turned round from the positive direction of the electromotive to the positive direction of the magnetic intensity. If the surface be taken where the matter has no velocity, $\mathfrak{E}'$ becomes equal to $\mathfrak{E}$, and the amount of energy crossing unit area perpendicular to the flow per second is

$$\frac{\text{electromotive intensity} \times \text{magnetic intensity} \times \text{sine included angle}}{4\pi}.$$

Since the surface may be drawn anywhere we please, then wherever there is both magnetic and electromotive intensity there is flow of energy.

Since the energy flows perpendicularly to the plane containing the two intensities, it must flow along the electric and magnetic level surfaces, when these exist, so that the lines of flow are the intersections of the two surfaces.

We shall now consider the applications of this law in several cases.

Applications of the Law of Transfer of Energy.

(1) *A straight wire conveying a current.*

In this case very near the wire, and within it, the lines of magnetic force are circles round the axis of the wire. The lines of electric force are along the wire, if we take it as proved that the flow across equal areas of the cross-section is the same at all parts of the section. If AB, Fig. 1, represents the wire, and the current is from A to B, then a tangent plane to the surface at any point contains the directions of both the electromotive and magnetic intensities (we shall write E.M.I. and M.I. for these respectively in what follows), and energy is therefore flowing in perpendicularly through the surface, that is, along the radius towards the axis. Let us take a portion of the wire bounded by two plane sections perpendicular to the axis. Across the ends no energy is flowing, for they contain no component of the E.M.I. The whole of the energy then enters in through the external surface of the wire, and by the general theorem the amount entering in must just account for the heat developed owing to the resistance, since if the current is steady there is no other alteration of energy. It is, perhaps, worth while to show independently in this case that the energy moving in, in accordance with the general law, will just account for the heat developed.

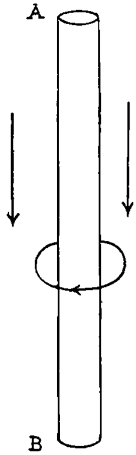


Fig. 1.

Let r be the radius of the wire, i the current along it, a the magnetic intensity at the surface, P the electromotive intensity at any point within the wire, and V the difference of potential between the two ends. Then the area of a length l of the wire is $2\pi rl$, and the energy entering from the outside per second is

$$\begin{aligned}\frac{\text{area} \times \text{E.M.I.} \times \text{M.I.}}{4\pi} &= \frac{2\pi rl \,.\, P \,.\, a}{4\pi} \\ &= \frac{2\pi ra \,.\, Pl}{4\pi} \\ &= \frac{4\pi iV}{4\pi} \\ &= iV,\end{aligned}$$

for the line-integral of the magnetic intensity $2\pi ra$ round the wire is $4\pi \times$ current through it, and $Pl = V$.

But by Ohm's law $V = iR$ and $iV = i^2R$, or the heat developed according to Joule's law.

It seems then that none of the energy of a current travels along the wire, but that it comes in from the non-conducting medium surrounding the wire, that as soon as it enters it begins to be transformed into heat, the amount crossing successive layers of the wire decreasing till by the time the centre is reached, where there is no magnetic force, and therefore no energy passing, it has all been transformed into heat. A conduction-current then may be said to consist of this inward flow of energy with its accompanying magnetic and electromotive forces, and the transformation of the energy into heat within the conductor.

We have now to inquire how the energy travels through the medium on its way to the wire.

(2) *Discharge of a condenser through a wire.*

We shall first consider the case of the slow discharge of a simple condenser consisting of two charged parallel plates when connected by a wire of very great resistance, as in this case we can form an approximate idea of the actual path of the energy.

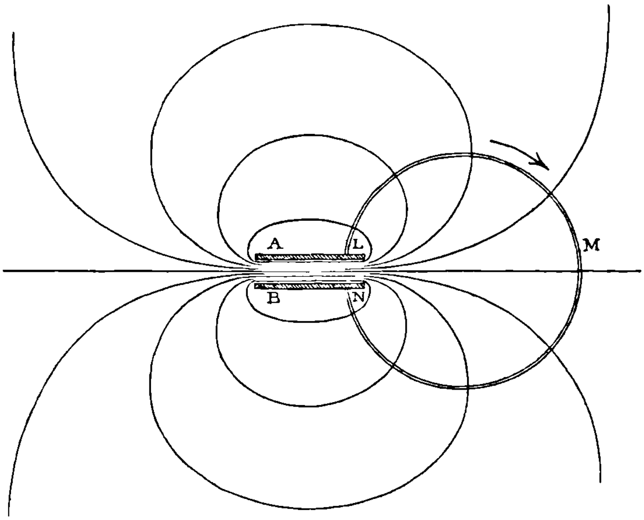


Fig. 2.

Let A and B, Fig. 2, be the two plates of the condenser, A being positively and B negatively electrified. Then before discharge the sections of the equipotential surfaces will be somewhat as sketched. The chief part of the energy resides in the part of the dielectric between the two plates, but there will be some energy wherever there is electromotive intensity. Between A and B the E.M.I. will be from A to B, and everywhere it is perpendicular to the level surfaces. Now connect A and B by a fine wire LMN of very great resistance, following a line of force and with the resistance so adjusted that it is the same for the same fall of potential throughout. We have supposed this arrangement of the resistance so that the level surfaces shall not be disturbed by the flow of the current. The wire is to be supposed so fine that the discharge takes place very slowly.

While the discharge goes on a current flows round LMN in the direction indicated by the arrow, and there is also an equal displacement-current from B to A due to the yielding of the displacement there. The current will be encircled by lines of magnetic force, which will in general form closed curves embracing the circuit. The direction of these round the wire will be from right to left in front, and round the space between A and B from left to right in front. The E.M.I. is always from the higher level surfaces—those nearer A, to the lower—those nearer B, both near the wire and in the space between A and B.

Now, since the energy always moves perpendicularly to the lines of E.M.I. it must travel along the equipotential surfaces. Since it also moves perpendicularly to the lines of M.I. it moves, as we have seen in case No. (1), inwards on all sides to the wire, and is there all converted into heat—if we suppose the discharge so slow that the current is steady during the time considered. But between A and B the E.M.I. is opposed to the current, being downwards, while the M.I. bears the same relation to the current as in the wire. Remembering that E.M.I., M.I., and direction of flow of energy are connected by the right-handed screw relation, we see that the energy moves outwards from the space between A and B. As then the strain of the dielectric between A and B is gradually released by what we call a discharge current along the wire LMN, the energy thus given up travels outwards through the dielectric, following always the equipotential surfaces, and gradually converges once more on the circuit where the surfaces are cut by the wire. There the energy is transformed into heat. It is to be noticed that if the current may be considered steady the energy moves along at the same level throughout.

(3) *A circuit containing a voltaic cell.*

When a circuit contains a voltaic cell we do not know with certainty what is the distribution of potential, but most probably it is somewhat as follows*:—Suppose we have a simple copper, zinc, and acid cell producing a steady current. There is probably a considerable sudden rise in passing from the zinc to the acid, the place where the chemical energy is given up, a fall through the acid depending on the resistance, a sudden fall on passing from the acid to the copper, where some energy is absorbed with evolution of hydrogen,

* It seems probable that the only legitimate mode of measuring the difference of potential between two points in a circuit consisting of dissimilar conductors carrying a steady current, consists in finding the total quantity of energy given out in the part of the circuit between the two points while unit quantity of electricity passes either point. If this is the case, it seems impossible that the surface of contact of dissimilar metals can be the chief seat of the electromotive force, for we have only the very slight evolution or absorption of energy there due to the Peltier effect. I have therefore adopted the theory of the voltaic circuit in which the seat of at least the chief part of the electromotive force is at the contact of the acid and metals. The large differences of potential found by electrometer methods between the air near two different metals in contact are, in this theory, to be accounted for by the supposition that the air acts in a similar manner to an oxidising electrolyte. A short statement of the theory is given in a letter by Professor Maxwell in the *Electrician* for April 26th, 1879, quoted in a note on page 149 of his *Elementary Treatise on Electricity*. (See also § 249, vol. 1, Maxwell's *Electricity and Magnetism.*) June 19, 1884.

and then a gradual fall through the wire of the circuit round to the zinc again. There will be a slight change of potential in passing from copper to zinc, but this we shall neglect for simplicity. The equipotential surfaces will probably then be somewhat as sketched in Fig. 3*, all the surfaces starting from where the acid comes in contact with the zinc, some of the highest potential passing through the acid, others passing between the acid and copper, and crowding in there, the rest lower than these cutting the circuit at right angles in points at intervals representing equal falls of potential.

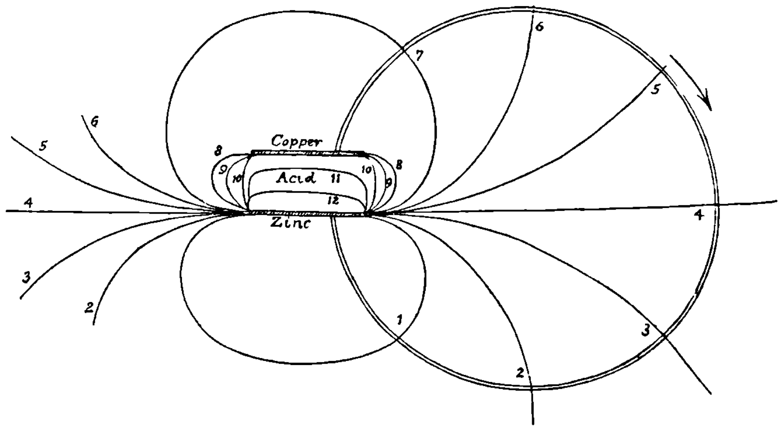


Fig. 3.

If this be the actual arrangement, then it is seen that the current, which travels round the circuit from zinc through acid to copper, is in opposition to the E.M.I. between the zinc and acid, while the M.I. is related to the current in the ordinary way. The energy will therefore pass outwards from there along the level surfaces. In fact, the medium between the zinc and acid behaves like the medium between the plates of the condenser in case No. (2), and it seems possible that the chemical action produces continually fresh 'electric displacement' from acid towards zinc which yields as rapidly as it is formed, the energy of the displacement moving out sideways.

Some of this energy which travels along the highest level surfaces will converge on the acid, and there be, at any rate ultimately, converted into heat. Some of it will move along those surfaces which crowd in between the acid and copper and there converge to supply the energy taken up by the escaping hydrogen. The rest spreads out to converge at last at different parts of the circuit. and there to be transformed into heat according to Joule's law.

It may be noticed that if the level surfaces be drawn with equal differences

* In this and the succeeding cases the circuit is alone supposed to cause the distribution of potential. In actual cases the surfaces would probably be very much deflected from their normal positions in the dielectric through the presence of conductors, electrified matter, and so on.

of potential, equal amounts of energy travel out per second between successive pairs of surfaces. For the amount transformed in the circuit in a length having a given difference of potential V between its ends will be $V \times$ current, and therefore the amount transformed between each pair of surfaces drawn with the same potential difference will be the same. But since the current and the field are steady, the energy transformed will be equal to the energy moving out from the cell between the same surfaces—the energy never crossing level surfaces. This admits of a very easy direct proof, but the above seems quite sufficient.

This result has a consequence which, though already well known, is worth mentioning here. Let V_1 be the difference of potential between the zinc and acid, V_2 that between the acid and copper. If i be the current, $V_1 i$ is the total energy travelling out per second from the zinc surface. Of this $V_2 i$ is absorbed at the copper surface, the rest, viz., $(V_1 - V_2)\,i$, being transformed in the circuit. The fraction, therefore, of the whole energy sent out which is transformed in the circuit is $\frac{V_1 - V_2}{V_1}$, a result analogous to the expression for the amount of heat which can be transformed into work in a reversible heat-engine.

One or two interesting illustrations of this movement of energy may be mentioned here in connection with the voltaic circuit.

Suppose that we are sending a current through a submarine cable by a battery with, say, the zinc to earth, and suppose that the sheath is everywhere at zero potential. Then the wire will everywhere be at higher potential than the sheath, and the level surfaces will pass from the battery through the insulating material to the points where they cut the wire. The energy then which maintains the current, and which works the needle at the further end, travels through the insulating material, the core serving as a means to allow the energy to get in motion.

Again, when the only effect in a circuit is the generation of heat, we have energy moving in upon the wire, there undergoing some sort of transformation, and then moving out again as heat or light. If Maxwell's theory of light be true, it moves out again still as electric and magnetic energy, but with a definite velocity and intermittent in type. We have in the electric light, for instance, the curious result that energy moves in upon the arc or filament from the surrounding medium, there to be converted into a form which is sent out again, and which, though still the same in kind, is now able to affect our senses.

(4) *Thermoelectric circuits.*

Let us first take the case of a circuit composed of two metals, neither of which has any Thomson effect. Let us suppose the current at the hot junction flows from the metal A to the metal B, Fig. 4. According to Professor Tait's theory it would appear that the E.M.I. at the hot junction is to that at the cold as the absolute temperature at the hot is to that at the cold junction. If the current is steady there is probably then a sudden rise in potential from A to B at the hot junction, a gradual fall along B, a sudden fall at the cold junction—less, however, than the sudden rise at the other—and a gradual fall along A. The level surfaces will then all start from the hot junction, the higher ones cutting the circuit at successive points along B, several converging at the cold junction, and the rest cutting the circuit at successive points along A. The heat at the hot junction is converted into electric and magnetic energy, which here moves outwards, since the current is against the E.M.I. Some of this energy converges upon B and A, to be converted

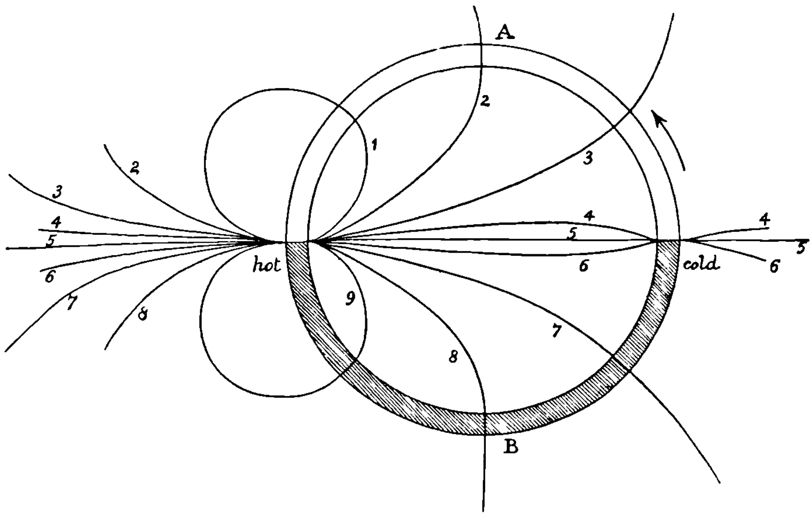


Fig. 4.

into heat, according to Joule's law, and some on the cold junction, there producing the Peltier heating effect.

Let us now suppose that we have a circuit of the same two metals, now all at the same temperature, but with a battery interposed in B, which sends a current in the same direction as before (Fig. 5). Then if C be the junction which was hot, and D that which was cold in the last case, we know that the current will tend to cool C and to heat D. In going from A to B at C there will be a sudden rise of potential, and in going from B to A at D there will be a sudden fall. Then, since the potential falls, as we go with the current along A, there will be a point on A near C which has the same potential as B at the junction. From this point to C, A will have lower potentials, and points with the same potentials will exist on B between C and the battery. Then either the level surfaces passing through C are closed surfaces, cutting A or B, and not passing through the battery at all, or, as seems much more probable, the surfaces from the battery which pass through C cut the circuit in three points in all outside the battery: once somewhere along A, once at C, and once somewhere along B. I have drawn and numbered the surfaces in the figure on this supposition. The heat developed in the parts of the circuit near C will thus be partly supplied from the junction C, where the

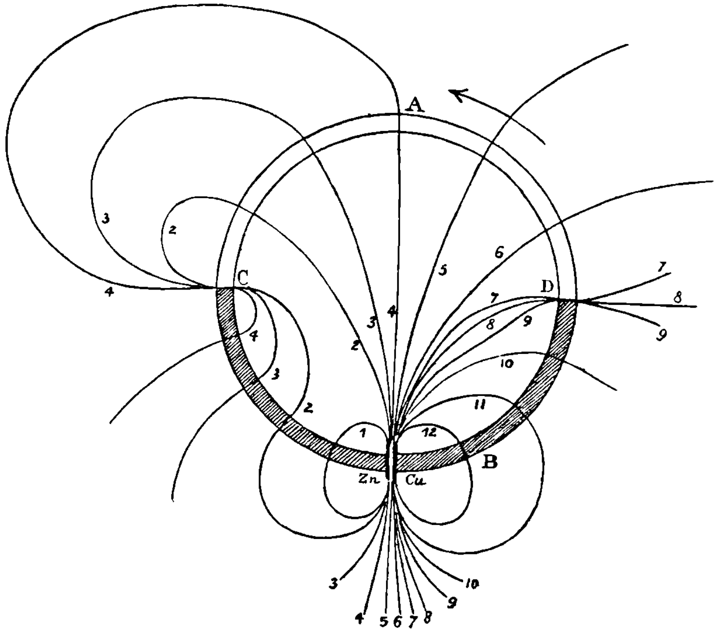


Fig. 5.

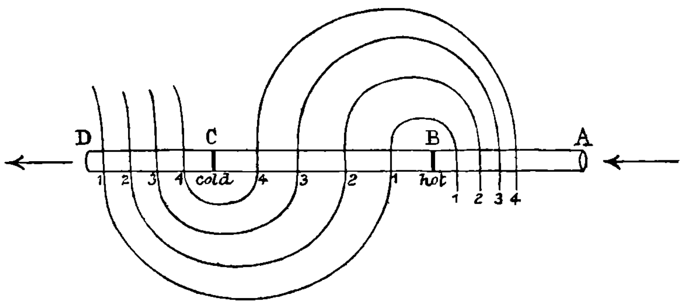


Fig. 6.

current is against the E.M.I. The energy therefore moves out thence, giving a cooling effect.

The Thomson effect may be considered in somewhat the same way. Let us suppose that a metal BC of the iron type, and with temperature falling from B to C, forms part of a circuit between two neutral metals of the lead type AB and CD, Fig. 6, and let us further, for simplicity, suppose that these metals are each at the neutral temperatures with respect to BC, so that there is no E.M.I. at the junction. If we drive a current from A to D by means of some external E.M.I., say at a junction elsewhere in the circuit, the potential will tend to fall from A to D. But a current in iron from hot to cold cools the metal, that is, the E.M.I. appears to be in opposition to the current, so that the energy moves outwards. The potential, therefore, tends to rise from B to C, and actually will do so if the resistance of BC is negligible compared with that of the rest of the circuit. In this case the level surfaces will probably be somewhat as indicated in Fig. 6, where they are numbered in order, each surface which cuts BC also cutting AB and CD, and the energy moving outwards will come into the circuit again at the parts of AB and CD near the junctions, where it will be transformed once more into heat. If the resistance of BC be gradually increased the fall of potential, according to Ohm's law, will tend to lessen the rise, and fewer surfaces will cut BC. It would seem possible so to adjust matters that the two exactly neutralised each other so that no energy either entered or left BC. In this case we should only have lines of magnetic force round BC, and no other characteristic of a current in that part of the circuit*.

If this is the true account of the Thomson effect it would appear that it should be described not as an absorption of heat or development of heat by the current but rather as a movement of energy outwards or inwards, according as the E.M.I. in the unequally heated metal opposes or agrees with the direction of the current.

(5) *A circuit containing a motor.*

This case closely resembles the third case of a circuit containing a copper-zinc cell, the motor playing a part analogous to that of the surface of contact of the acid with the copper. Let us, for simplicity, suppose that the motor has no internal resistance. When it has no velocity all the level surfaces cut the circuit, and the energy leaving the dynamo or battery is all transformed into heat due to resistance. But if the motor is being worked the current diminishes, the level surfaces begin to converge on the motor and fewer cut the circuit. Some of the energy therefore passes into the motor, and is there transformed into work. As the velocity increases the number cutting the rest of the circuit decreases, for the current diminishes, and. therefore, by Ohm's law, the fall of potential along the circuit is less; and ultimately when the velocity of the motor becomes very great the current becomes very small. In the limit no level surface cuts the circuit, all converging on the motor.

* Perhaps this is only true of the wire as a whole. If we could study the effects in minute portions it is possible that we should find the seat of the E.M.I. due to difference of temperature not the same as that which neutralises it, which is according to Ohm's law. One, for instance, might be between the molecules, the other in their interior, so that there might be an interchange of energy still going on, though no balance remained over to pass out of the wire.

That is, all the energy passes into the motor when it is transformed into work, and the efficiency of the arrangement is perfect, though the rate of doing work is infinitely slow.

(6) *Induced currents.*

It is not so easy to form a mental picture of the movement of energy which takes place when the field is changing and induced currents are created. But we can see in a general way how these currents are accounted for. When there is a steady current in a field there is corresponding to it a definite distribution of energy. If there is a secondary circuit present, so long as the primary current is constant, there is no E.M.I. in the secondary circuit for it is all at the same potential. The energy neither moves into nor out of it, but streams round it somewhat as a current of liquid would stream round a solid obstacle. But if the primary current changes there is a redistribution of the energy in the field. While this takes place there will be a temporary E.M.I. set up in the conducting matter of the secondary circuit, energy will move through it, and some of the energy will there be transformed into heat or work, that is, a current will be induced in the secondary circuit.

(7) *The electromagnetic theory of light.*

The velocity of plane waves of polarised light on the electromagnetic theory may be deduced from the consideration of the flow of energy. If the waves pass on unchanged in form with uniform velocity the energy in any part of the system due to the disturbance also passes on unchanged in amount with the same velocity. If this velocity be v, then the energy contained in unit volume of cubical form with one face in a wave-front will all pass out through that face in $1/v$th of a second. Let us suppose that the direction of propagation is straightforward, while the displacements are up and down; then the magnetic intensity will be right and left. If $\mathfrak{E}$ be the E.M.I. and $\mathfrak{H}$ the M.I. within the volume, supposed so small that the intensities may be taken as uniform through the cube, then the energy within it is $K\mathfrak{E}^2/8\pi + \mu\mathfrak{H}^2/8\pi$. The rate at which energy crosses the face in the wave-front is $\mathfrak{E}\mathfrak{H}/4\pi$ per second, while it takes $1/v$th of a second for the energy in the cube to pass out.

Then
$$\frac{\mathfrak{E}\mathfrak{H}}{4\pi v} = \frac{K\mathfrak{E}^2}{8\pi} + \frac{\mu\mathfrak{H}^2}{8\pi}. \quad\text{..............................}(1)$$

Now, if we take a face of the cube perpendicular to the direction of displacement, and therefore containing the M.I., the line-integral of the M.I. round this face is equal to $4\pi \times$ current through the face. If we denote distance in the direction of propagation from some fixed plane by z, the line-integral of the M.I. is $-\dfrac{d\mathfrak{H}}{dz}$, while the current, being an alteration of displacement, is $\dfrac{K}{4\pi}\dfrac{d\mathfrak{E}}{dt}$.

Therefore $$-\frac{d\mathfrak{H}}{dz} = K\frac{d\mathfrak{E}}{dt}. \qquad \text{.................................(2)}$$

But since the displacement is propagated unchanged with velocity v, the displacement now at a given point will alter in time dt to the displacement now a distance dz behind, where $dz - v dt$.

Therefore $$\frac{d\mathfrak{E}}{dt} = -v\frac{d\mathfrak{E}}{dz}. \qquad \text{.................................(3)}$$

Substituting in (2) $$\frac{d\mathfrak{H}}{dz} = Kv\frac{d\mathfrak{E}}{dz},$$

whence $$\mathfrak{H} = Kv\mathfrak{E}, \qquad \text{...................................(4)}$$

the function of the time being zero, since $\mathfrak{H}$ and $\mathfrak{E}$ are zero together in the parts which the wave has not yet reached.

If we take the line-integral of the E.M.I. round a face perpendicular to the M.I. and equate this to the decrease of magnetic induction through the face, we obtain similarly

$$\mathfrak{E} = \mu v\mathfrak{H}. \qquad \text{...................................(5)}$$

It may be noticed that the product of (4) and (5) at once gives the value of v, for dividing out $\mathfrak{E}\mathfrak{H}$ we obtain

$$1 = \mu K v^2$$

or $$v \quad \frac{1}{\sqrt{\mu K}}.$$

But using one of these equations alone, say (4), and substituting in (1) K for $\mathfrak{H}$ and dividing by $\mathfrak{E}^2$, we have

$$\frac{K}{4\pi} = \frac{K}{8\pi} + \frac{\mu K^2 v^2}{8\pi}$$

or $$1 - \mu K v^2,$$

whence $$v = \frac{1}{\sqrt{\mu K}}.$$

This at once gives us the magnetic energy equal to the electric energy, for

$$\frac{\mu\mathfrak{H}^2}{8\pi} = \frac{\mu K^2 v^2 \mathfrak{E}^2}{8\pi} = \frac{K\mathfrak{E}^2}{8\pi}.$$

It may be noted that the velocity $\frac{1}{\sqrt{\mu K}}$ is the greatest velocity with which the two energies can be propagated together, and that they must be equal when travelling with this velocity. For if v be the velocity of propagation and θ the angle between the two intensities, we have

$$\frac{\mathfrak{E}\mathfrak{H}\sin\theta}{4\pi v} \quad \frac{K\mathfrak{E}^2}{8\pi} + \frac{\mu\mathfrak{H}^2}{8\pi},$$

or $$v = \frac{2\sin\theta}{\dfrac{K\mathfrak{E}}{\mathfrak{H}} \quad \dfrac{\mu\mathfrak{H}}{\mathfrak{E}}}.$$

The greatest value of the numerator is 2 when θ is a right angle, and the least value of the denominator is $2\sqrt{\mu K}$, when the two terms are equal to each other and to $\sqrt{\mu K}$.

The maximum value of v therefore is $\frac{1}{\sqrt{\mu K}}$, and occurs when $\theta = \frac{\pi}{2}$ and $K\mathfrak{E}^2 = \mu\mathfrak{H}^2$.

The preceding examples will suffice to show that it is easy to arrange some of the known experimental facts in accordance with the general law of the flow of energy. I am not sure that there has hitherto been any distinct theory of the way in which the energy developed in various parts of the circuit has found its way thither, but there is, I believe, a prevailing and somewhat vague opinion that in some way it has been carried along the conductor by the current. Probably Maxwell's use of the term 'displacement' to describe one of the factors of the electric energy of the medium has tended to support this notion. It is very difficult to keep clearly in mind that this 'displacement' is, as far as we are yet warranted in describing it, merely a something with direction which has some of the properties of an actual displacement in incompressible fluids or solids. When we learn that the 'displacement' in a conductor having a current in it increases continually with the time, it is almost impossible to avoid picturing something moving along the conductor, and it then seems only natural to endow this something with energy-carrying power. Of course it may turn out that there is an actual displacement along the lines of electromotive intensity. But it is quite as likely that the electric 'displacement' is only a function of the true displacement, and it is conceivable that many theories may be formed in which this is the case, while they may all account for the observed facts. Mr Glazebrook has already worked out one such theory in which the component of the electric displacement at any point in the direction of x is $\frac{1}{8\pi}\nabla^2\xi$, where ξ is the component of the true displacement (*Phil. Mag.* June 1881). It seems to me then that our use of the term is somewhat unfortunate, as suggesting to our minds so much that is unverified or false, while it is so difficult to bear in mind how little it really means.

I have therefore given several cases in considerable detail of the application of the mode of transfer of energy in current-bearing circuits according to the law given above, as I think it is necessary that we should realise thoroughly that if we accept Maxwell's theory of energy residing in the medium, we must no longer consider a current as something conveying energy along the conductor. A current in a conductor is rather to be regarded as consisting essentially of a convergence of electric and magnetic energy from the medium upon the conductor and its transformation there into other forms. The current through a seat of so-called electromotive force consists essentially of a divergence of energy from the conductor into the medium. The magnetic lines of force are related to the circuit in the same way throughout, while the lines of electric force are in opposite directions in the two parts of the circuit—with the so-called current in the conductor, against it in the seat of electromotive force. It follows that the total E.M.I. round the circuit with a steady current is zero, or the work done in carrying a unit of positive electricity round the circuit with the current is zero. For work is required to move it against the E.M.I. in the seat of energy, this work sending energy out into the medium, while an equal amount of energy comes in in the rest of the circuit where it is moving with the E.M.I. This mode of regarding the relations of the various parts of the circuit is, I am aware, very different from that usually given, but it seems to me to give us a better account of the known facts.

It may seem at first sight that we ought to have new experimental indications of this sort of movement of energy, if it really takes place. We should look for proofs at points where the energy is transformed into other modifications, that is, in conductors. Now in a conductor, when the field is in a steady state, there is no electromotive intensity, and therefore no motion and no transformation of energy. The energy merely streams round the outside of the conductor, if in motion at all in its neighbourhood. If the field is changing, energy can pass into the conductor, as there may be temporary E.M.I. set up within it, and there will be transformation. But we already know the nature of this transformation, for it constitutes the induced current. Indeed, the fundamental equation describing the motion of energy is only a deduction from Maxwell's equations, which are formed so as to express the experimental facts as far as yet known. Among these are the laws of induction in secondary circuits, and they must therefore agree with the law of transfer. We can hardly hope, then, for any further proof of the law beyond its agreement with the experiments already known until some method is discovered of testing what goes on in the dielectric independently of the secondary circuit.

11.

ON THE CONNECTION BETWEEN ELECTRIC CURRENT AND THE ELECTRIC AND MAGNETIC INDUCTIONS IN THE SURROUNDING FIELD*.

[*Phil. Trans.* **176**, 1885, pp. 277–306.]

[*Received* January 31. *Read* February 12, 1885.]

In a paper published in the *Philosophical Transactions* for 1884 (Part II, pp. 343–361)†, I have deduced from Maxwell's equations for the electromagnetic field the mode in which the energy moves in the field. The result there obtained is that the energy moves at any point perpendicularly to the plane containing the directions of the electric and magnetic intensities, and in the direction in which a right-handed screw would move if turned round from the positive direction of the electric intensity to the positive direction of the magnetic intensity. The quantity crossing the plane per unit area per second is equal to the product of the two intensities, multiplied by the sine of the included angle, divided by 4π ‡.

Hence it follows that the energy moves along the intersections of the two sets of level surfaces, electric and magnetic, where they both exist, their intersections giving, as it were, the lines of flow. In the particular case of a steady current in a wire where the electrical level surfaces cut the wire

* [Added July 15. Since the reading of the paper I have found a remarkable passage in Faraday's *Experimental Researches*, vol. 1, p. 529, § 1659, which I give below. The words I have put in *italics* might be regarded as the starting-point of the views which I have attempted to develop in this paper. '§ 1659. According to the beautiful theory of Ampère, the transverse force of a current may be represented by its attraction for a similar current and its repulsion of a contrary current. May not then the equivalent transverse force of static electricity be represented by that lateral tension or repulsion which the lines of inductive action appear to possess (1304)? Then, again, *when current or discharge occurs between two bodies, previously under inductrical relations to each other, the lines of inductive force will weaken and fade away, and, as their lateral repulsive tension diminishes, will contract and ultimately disappear in the line of discharge.* May not this be an effect identical with the attractions of similar currents, i.e., may not the passage of static electricity into current electricity, and that of the lateral tension of the lines of inductive force into the lateral attraction of lines of similar discharge, have the same relation and dependence, and run parallel to each other?']

† [*Collected Papers*, Art. 10.]

‡ I here adopt the simpler term 'Electric Intensity,' denoted by E.I., instead of 'Electromotive Intensity,' for the force which would act on a small body charged with unit of positive electrification. The magnetic intensity, i.e., the force which would act on a unit north-seeking Pole, will be denoted by M.I.

perpendicularly to the axis, it appears that the energy dissipated in the wire as heat comes in from the surrounding medium, entering perpendicularly to the surface.

In that paper I made no assumption as to the transfer of the electric and magnetic inductions—the electric and magnetic conditions through the medium, merely considering the movement of energy. I now propose to develop a hypothesis as to the transfer of the inductive condition in the medium, and its movement inwards upon current-bearing wires.

The value of the electric induction at any point in an isotropic medium is equal to $K \times$ E.I./4π, and the direction of the induction coincides with that of the intensity. Maxwell terms this electric induction 'displacement,' but I think that 'induction' is preferable, as it implies no hypothesis beyond that of some alteration in the medium, which can be described by a vector. The value of the magnetic induction is equal to μ M.I., and its direction coincides with that of the magnetic intensity.

If we symbolise the electric and magnetic conditions of the field by induction-tubes running in the directions of the intensities, the tubes being supposed drawn in each case so that the total induction over a cross section is unity, then we have reason to suppose that the electric tubes are continuous except where there are electric charges, while the magnetic tubes are probably in all cases continuous and re-entrant.

In the neighbourhood of a wire containing a current, the electric tubes may in general be taken as parallel to the wire while the magnetic tubes encircle it. The hypothesis I propose is that the tubes move in upon the wire, their places being supplied by fresh tubes sent out from the seat of the so-called electromotive force. The change in the point of view involved in this hypothesis consists chiefly in this, that induction is regarded as being propagated sideways rather than along the tubes or lines of induction. This seems natural if we are correct in supposing that the energy is so propagated, and if we therefore cease to look upon current as merely something travelling along the conductor carrying it, and in its passage affecting the surrounding medium. As we have no means of examining the medium, to observe what goes on there, but have to be content with studying what takes place in conductors bounded by the medium, the hypothesis is at present incapable of verification. Its use, then, can only be justified if it accounts for known facts better than any other hypothesis.

The basis of Maxwell's Electromagnetic Theory.

Maxwell's Electromagnetic Theory rests on three general principles.

I. The first principle consists in the assumption that energy has position, i.e., that it occupies space. The electric and magnetic energies of an electro magnetic system reside therefore somewhere in the field. It is an inevitable conclusion that they are present wherever the electric and magnetic intensities can be shown to exist. For instance, suppose a small electrified body placed in a field where there is electric intensity; then the body will be acted on by force and will receive energy which appears as the energy of motion, the electric energy at the same time decreasing. If energy has position, that which is now in the body must have come into it through the surrounding space, or it was present in that space before the body took it up. The alternative that it appeared in the body without passing through the space immediately surrounding the body need not be discussed. Hence the existence of electric intensity implies the existence of electric energy in the place where the electric intensity is capable of manifestation. Similarly magnetic energy accompanies magnetic intensity. The inductive condition of the medium imagined by Faraday is due then to its modification when containing energy. Maxwell has shown that all the energy is accounted for on the supposition that the electric energy per unit volume at any point is $K(\text{E.I.})^2/8\pi$, and that the magnetic energy is $\mu(\text{M.I.})^2/8\pi$. He has given in his *Elementary Treatise on Electricity*, p. 47, another way of describing the distribution of energy which will be more useful for my purpose. If the field be mapped out by unit induction-tubes—either electric or magnetic—i.e., tubes drawn so that the total induction over every cross-section of a tube is unity, and if these tubes be divided into cells of length such that the difference of potential or the line-integral of the intensity between the two ends of each cell is unity, then each cell contains, if electric, half a unit of energy, if magnetic $\frac{1}{8\pi}$ of a unit, the divisor 4π being introduced by the difference in definition of the two inductions. Maxwell terms these unit cells.

II. The second principle is in part experimental, viz.:—that the line-integral of the electric intensity round any closed curve is equal to the rate of decrease of the total magnetic induction through the curve. This is verified by experiment when the curve is drawn through conducting material. Maxwell supposes it to be true in all cases, that is, he supposes that electric induction can be produced in insulators by means of magnetic changes, without the presence of charges on conductors, and is therefore led to identify the growth and decrease of electric induction with current.

III. The third principle is also in part experimental, viz.:—that the line-integral of the magnetic intensity round any closed curve is equal to $4\pi \times$ current through the curve. This is verified by experiment when the current is in a wire, and Maxwell supposes it to be also true in the case where there is change of electric induction in an insulator. The supposition is justified by Prof. Rowland's well-known experiment.

From these three principles Maxwell deduces his general equations of the

Electromagnetic Field. I have stated them in full as I propose to modify the second and third principles, and I wish to make quite clear the nature of the proposed changes.

Modification of the Second Principle.

I propose to replace the second principle by the following: *Whenever electromotive force is produced by change in the magnetic field, or by motion of matter through the field, the* E.M.F. *per unit length or the electric intensity is equal to the number of tubes of magnetic induction cutting or cut by the unit length per second, the* E.M.F. *tending to produce induction in the direction in which a right-handed screw would move if turned round from the direction of motion relatively to the tubes towards the direction of the magnetic induction**.

In order that the results obtained from this should agree with those obtained from Maxwell's statement of the principle, it is necessary that change in the total quantity of magnetic induction passing through a closed curve should always be produced by the passage of induction tubes through the curve inwards or outwards. In some instances this is undoubtedly the case, as, for instance, where a part of a circuit moves so as to cut a fixed magnetic field, or where a magnet moves in the neighbourhood of a circuit. Here the E.M.F. is equal to the number of tubes cut by the wire per second, and its seat is that part of the wire cutting the tubes. In other cases, as, for instance, where the wire is between the poles of an electromagnet whose magnetising current is changing, we have no direct experimental evidence of the movement of the induction in or out. But the induction-tubes are closed, and to make them thread a circuit we might expect that they would have to cut through the boundary. The alternative seems to be that they should grow or diminish from within, the change in intensity being propagated *along* the tubes. This would be inconsistent with their closed nature, unless the energy were instantaneously propagated along the whole length, and is further negatived by the theory of the transfer of energy, which implies that the energy flows transversely to the direction of the tubes. I shall suppose, then, that alteration in the quantity of magnetic induction through a closed curve is always produced by motion of induction-tubes inwards or outwards through the bounding curve.

* Taking the electric intensity as always perpendicular to the plane of motion of the magnetic tubes through a point, and equal to the number cut per second by unit length of the normal to the plane of motion, we can easily show that the component of the intensity in any other direction will be equal to the number of tubes cut by a line of unit length in that direction. For let OA represent a small length drawn perpendicular to the plane of motion, and let OP represent a line drawn in any direction making an angle θ with OA. Draw AP perpendicular to OA, and meeting OP in P. Then the same number of tubes will cut both OA and OP, since AP is parallel to their plane of motion. If the number cutting OA be $E \times OA$, where E is the number cutting unit length, and therefore equal to the resulting intensity, the number cutting unit length of OP will be $E \cdot \frac{OA}{OP} = E \cos \theta$, or the component of the intensity along OP.

Modification of the Third Principle.

The third principle admits of similar analysis, according to which we may regard the magnetic intensity along a closed curve as due to the cutting of the curve by tubes of electric induction. If we regard the line-integral of the magnetic intensity round a tube of induction as measuring the magnetomotive force—employing a useful term suggested by Mr. Bosanquet—we may put the modification in the following form:

Whenever magnetomotive force is produced by change in the electric field, or by motion of matter through the field, the magnetomotive force per unit length is equal to $4\pi \times$ the number of tubes of electric induction cutting or cut by unit length per second, the magnetomotive force tending to produce induction in the direction in which a right-handed screw would move if turned round from the direction of the electric induction towards the direction of motion of the unit length relatively to the tubes of induction.

This is the most general form of the principle, but we shall only require the more special statement which immediately follows from it: that the line-integral of the M.I. round any curve is equal to $4\pi \times$ the number of tubes passing in or out through the curve per second.

We have reasons exactly similar to those given in the last case for supposing that any change in the total electric induction through a curve is caused by the passage of induction-tubes in or out across the boundary. The alternative, that change takes place by propagation from the ends, seems inconsistent with the theory of the transverse flow of energy.

I shall postpone the discussion of the modifications of the general equations of the electromagnetic field following from these changes in the fundamental principles, and proceed to discuss the bearing which they have upon the nature of currents in conductors.

A straight wire carrying a steady current.

Let AB represent a wire in which is a steady current from A to B. The direction of the electric induction in the surrounding field near the wire, if the field be homogeneous, is parallel to AB.

Let E be the value of the electric intensity, or the difference of potential per unit length perpendicular to the level surfaces, and let R be the resistance of the wire per unit length. Then $C = \frac{E}{R}$ where C is the current, and C is uniform throughout the circuit. The magnetic intensity in the immediate neighbourhood of the wire at a distance r from the axis of the wire is $\frac{2C}{r}$.

The hypothesis proposed as to the nature of the current is that C electric induction-tubes close in upon the wire per second. The wire is not capable of bearing a continually-increasing induction, and breaks the tubes up, as it were, their energy appearing finally as heat*.

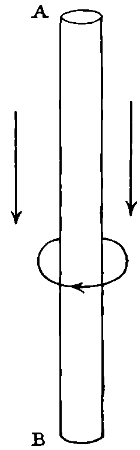


Fig. 1.

Let us see how this hypothesis accounts for known facts, when aided by the two principles just laid down.

It accounts at once for the constancy of the current at all parts of the wire in the steady state, in so far as it reduces this constancy to a particular case of the law according to which there is the same total induction over all cross-sections of a tube. If, for instance, there were more induction entering at A than at B, then more tubes must be entering at A, and so there would be an increase in the number of tubes left in the medium about B, or the field would not be steady.

Further, if we draw any closed curve embracing the wire once, we may apply the third principle to give us the line-integral of the magnetic intensity round the curve. For this is a case where change is certainly going on in the electric field. and the magnetomotive force is due to this change. The field being steady, if C tubes enter the wire and are there broken up, C tubes must cross through any encircling curve to supply their place, or the line-integral of the magnetic intensity round the curve is equal to 4π number of tubes passing through the boundary per second, i.e., $4\pi C$. If the curve be a circle of radius r, with its centre in the axis and plane perpendicular thereto, the intensity at any point of this circle will be tangential to it, and equal to

$$\frac{4\pi C}{2\pi r} \quad \frac{2C}{r}.$$

The known constancy of the line-integral of the magnetic intensity round the wire, which the hypothesis thus accounts for, almost seems to force the hypothesis upon us, if we regard the field as caused by the inward flowing of the energy rather than by something propagated out from the wire.

Assuming that the induction-tubes bring in their energy the quantity is easily found. The number of unit cells per unit length is equal to the difference of potential per unit length, or E. Hence the energy per unit length of each tube is $\frac{E}{2}$, since each cell contains a half unit. If C tubes disappear in the wire per second, they yield up $\frac{CE}{2}$ of energy per unit length. Now the total energy dissipated per unit length is CE per second. Or the movement

* May we not say that the tubes are dissolved? The term seems to suggest that the induction is not destroyed, but only loses its continuity. Probably this is the case; for on the electromagnetic theory of radiant energy, when the wire is heated, it sends out the energy it received, again in the electromagnetic form.

inwards of the electric induction will only account for half of the energy. The other half must be accounted for by the movement inwards of the magnetic induction. This movement of the magnetic induction is suggested by the existence of electric induction, which cannot be ascribed to statical charges.

The electric intensity is E. Hence E tubes of magnetic induction must move in per second, cutting unit length parallel to the axis of the wire, in accordance with the second principle, and it will easily be seen that the inward motion gives the right direction of the electric intensity. The line-integral of the magnetic intensity round a tube is $4\pi C$, the tubes being closed rings. Hence there are $4\pi C$ unit cells in the length. Since each of these contains $\frac{1}{8\pi}$ of energy, the quantity per tube $= \frac{4\pi C}{8\pi} = \frac{C}{2}$. E tubes entering the wire per second will carry in $\frac{CE}{2}$ of energy, the other half to be accounted for.

We can in a similar manner trace the dissipation of the energy, which we must suppose taking place within the wire. The line-integral of the magnetic intensity round a circle, with its centre in the axis of the wire, is constant up to the wire, and equal to $4\pi C$. Within the wire it gradually diminishes as the circle contracts. At a distance r from the centre it is $4\pi C \frac{r^2}{a^2}$ where a is the radius of the wire. If we assume this intensity to be still due to the passage inwards of the tubes of electric induction only, $\frac{Cr^2}{a^2}$ cross inwards per second at a distance r, the difference between this number and the C tubes entering the outer boundary being destroyed and their energy dissipated. The energy thus dissipated per unit length between the outer boundary and a coaxal cylinder of radius r will be $\frac{EC}{2}\left(1 - \frac{r^2}{a^2}\right)$ per second. If $r = 0$ the whole of the electric energy is dissipated. It would appear, then, that we may represent the dissipation of the electric energy by the total destruction of the tubes all through their length.

The value of the electric intensity being E throughout the wire the number of tubes of magnetic induction cutting unit length parallel to the axis is the same at all parts, viz., E per second. Hence the magnetic tubes are not destroyed as the electric tubes are. But the line-integral of the magnetic intensity round the tubes diminishes as they approach the axis, being $4\pi \frac{Cr^2}{a^2}$ round that at distance r. The number of unit cells diminishes, and, therefore, the energy per tube is less, the decrease being due to that dissipated. Thus the energy entering in the E tubes at the outer boundary is $\frac{4\pi CE}{8\pi}$ or $\frac{CE}{2}$.

That crossing in E tubes at a distance r is $\frac{4\pi}{8\pi}\frac{Cr^2}{a^2} E = \frac{CE}{2}\frac{r^2}{a^2}$. The difference $\frac{CE}{2}\left(1 - \frac{r^2}{a^2}\right)$ has been dissipated.

Hence it appears that the energy dissipated per second may be represented as half electric half magnetic, the electric energy being dissipated by the breaking up of the tubes and their disappearance, while the magnetic energy is dissipated by the shortening of the tubes and their final disappearance by contraction to infinitely small dimensions of the diameters of the rings by which we may represent them. At all points therefore outside and inside the energy crossing any surface may be represented as equally divided between the two kinds.

As we know the value of the induction at any point, or the number of tubes passing through unit area, and as we also know the number of tubes cutting the boundary it is easy, on the assumption that the tubes move on unchanged, to calculate their velocity. Of course this velocity is purely hypothetical, as we cannot examine minutely into the medium and observe what goes on there. Probably, if we could observe with sufficient minuteness we should find unevennesses in the induction. If the velocity of the tubes has any physical meaning it is that these unevennesses are carried forward with that velocity. To illustrate this let us suppose that we have water flowing through a glass tube at a steady rate. We have nothing to show that the water is moving past any point in the tube beyond its disappearance at the entrance and its appearance at the exit, but knowing the cross-section of the tube, i.e., the quantity of water in any part of it, and the quantity entering and leaving, it is easy to assign a velocity to the water in the tube which shall account for the observed amount entering and leaving. This velocity is to a certain extent hypothetical. But if we examine the tube with a sufficient magnifying power to show particles of dust in the water the existence of the velocity receives a more direct proof. I do not know whether we should have any right to expect a similar proof of the motion of induction even if we had the means of observation.

To find the hypothetical velocity of the electric induction-tubes let us calculate the number of tubes passing through a circular band with radii r and $r + dr$ and centre in the axis of the wire, and lying in a plane perpendicular to the axis. The intensity being E the induction is $\frac{KE}{4\pi}$, and therefore the area of cross-section of each tube is $\frac{4\pi}{KE}$, since area $\times$ induction is unity The number passing through the circular band is therefore

$$2\pi r\,dr \, . \, \frac{KE}{4\pi} = \frac{KEr\,dr}{2}.$$

Since C tubes move in through the inner circle per second, $\frac{KEr\,dr}{2}$ tubes move in in $\frac{KEr\,dr}{2C}$ of a second, i.e., all the tubes passing through the band will have just moved in in this time. The outermost tubes therefore describe the space dr in time $\frac{KEr\,dr}{2C}$, or the velocity is $\frac{2C}{KEr}$. Now we know that if R be the resistance per unit length, $C = \frac{E}{R}$. Hence we may put the velocity in the form

$$\frac{2}{KR} \cdot \frac{1}{r},$$

which is independent of the current.

To take a special case, let us calculate the velocity just outside the boundary of a copper wire, the specific resistance of copper being 1642 in electromagnetic measure. Then if a be the radius of the wire

$$R = \frac{1642}{\pi a^2}$$

and $K = \frac{1}{v^2}$ where v is the ratio of the units, which in air may be taken as 3×10^{10}.

Then the velocity

$$\begin{aligned} &= \frac{2v^2\pi a^2}{1642a} \\ &= \frac{2 \times 9 \times 10^{20}\pi a}{1642} \\ &= 345 \times 10^{16}a. \end{aligned}$$

At greater distances the velocity will be less, diminishing according to the inverse distance.

The hypothetical velocity of propagation of the magnetic induction may be calculated in a similar manner. The intensity at a distance r from the axis is $\frac{2C}{r}$ and the induction is $\frac{2\mu C}{r}$. The area of each tube is therefore $\frac{r}{2\mu C}$, and the number lying in a ring of rectangular section with depth unity and internal and external radii r and $r + dr$, will be $1 \times dr \div \frac{r}{2\mu C} = \frac{2\mu C\,dr}{r}$.

But E tubes move in per second through the inner face of the ring, so that $\frac{2\mu C\,dr}{r}$ tubes move in in time $\frac{2\mu C\,dr}{Er}$, or this is the time taken by the outermost tubes to move across the ring describing a distance dr. The velocity is therefore

$$\frac{Er}{2\mu C} = \frac{Rr}{2\mu},$$

which is again independent of the current.

If the current-bearing wire is copper, $R = \frac{1642}{\pi a^2}$, and with $\mu = 1$ the velocity becomes

$$\frac{1642r}{2\pi a^2}.$$

We cannot assign a velocity to the electric tubes within the wire since the number is diminishing as their energy dissipates. But the magnetic tubes crossing unit length parallel to the axis are still unchanged in number, so that we may assign a velocity to them. This velocity means that with the known value of the magnetic induction this velocity will give the number crossing inwards required to produce electric intensity E.

The velocity will be found equal to $\frac{Ea^2}{2\mu Cr}$ or $\frac{Ra^2}{2\mu r}$.

In the case of a copper wire this becomes

$$\frac{1642}{2\mu\pi r}.$$

Discharge of a condenser through a fine wire.

Let us suppose that we have a condenser consisting of two parallel plates A and B and charged with equal and opposite charges. Then we know that there will be electric induction between the two plates, and that according to Maxwell's theory the energy of the system is stored there. We may form an idea of the distribution of the energy by drawing the unit induction-tubes, each starting from and ending in unit quantity of electricity, and dividing

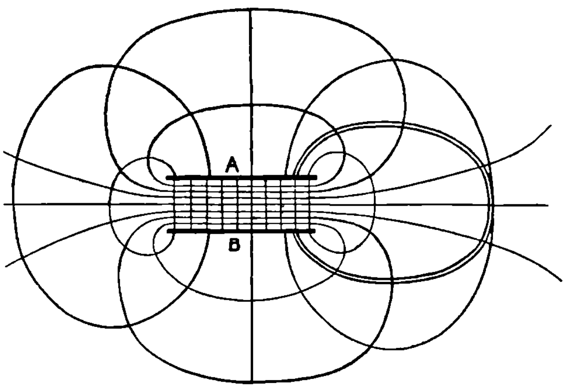


Fig. 2.

these into unit cells by the level surfaces, drawn at unit difference of potential (Fig. 2). If the dimensions of the plates be great compared with their distance apart, then nearly all the cells will be between the two plates, and since each cell contains half a unit of energy, nearly all the energy is there. There will, however, be slight induction, and therefore some small quantity of energy in the surrounding space.

Now let the two plates be connected by a wire. Discharge takes place, and we are fairly justified, from the heat in the wire and the transient magnetic effects, in saying that a current has been in the wire from the positive to the negative plate, or the wire was for the time being in the same relation to the surrounding medium as the wire in the case just considered, the condition of affairs, however, not being steady.

Let us suppose the wire to have a very great resistance, in order that, at least in imagination, we may lengthen out the time of discharge. On the ordinary current-theory, combined with Maxwell's 'displacement' theory, the medium between the plates has returned from the strained condition, denoted by 'displacement' from the positive to the negative plate, causing displacement through the plates and along the wire, the displacement being in the same direction all round the circuit. This is generally, I think, supposed to take place by the recovery of the medium between the plates causing displacement in the metal immediately in front of it, the displacement being analogous to the forcing of water along a pipe corresponding to the plates and wire, by the recovery from strain of some substance placed in a chamber corresponding to the space between the plates.

According to the hypothesis here advanced we must suppose the lessening of the induction between the plates—induction being used with the same physical meaning as Maxwell's displacement—to take place by the divergence outwards of the induction-tubes. We may picture them as taking up the positions of successive lines of induction further and further away from the space between the plates, their ends always remaining on the plates. They finally converge on the wire, and are then broken up and their energy dissipated as heat. At the same time some of the energy becomes magnetic, this occurring as the difference of potential between the plates lowers, so that the tubes contain fewer unit cells.

The magnetic energy will be contained in ring-shaped tubes which will expand from between the plates and then contract upon some other part of the circuit. To illustrate the movement of the electric induction-tubes let us suppose them to be represented by elastic strings stretched between the two plates. Then the motion of the tubes outwards would be roughly represented by pulling the elastic strings outwards and doubling them back close against the wire, their ends being still attached to the plates. It is evident that if any ring surround the wire each of the strings must break through it in order to reach the wire. Hence the total number of strings cutting any ring surrounding the wire is the same wherever the ring be placed. Similarly the total number of tubes of electric induction cutting any curve encircling the wire is the same, and therefore the line-integral of the magnetic intensity round the curve integrated throughout the time of discharge is the same, or the total magnetic effect is the same at all parts of the circuit. It is not necessary to suppose that a tube enters the wire at the same moment throughout its whole length; indeed, the experiments of Wheatstone on the so-called velocity of electricity prove clearly that this is not the case, for in those experiments the tubes reached air-breaks near the two ends of the wire before they reached a break in the middle.

We cannot by this general reasoning show that the energy entering any length of the wire will be proportional to the resistance of that length the result obtained by Riess. Indeed, this cannot always be the case. For instance, imagine a condenser discharged by two wires connected to the two plates of another condenser of greater capacity, whose plates are again connected by a fine wire of enormous resistance, through which the discharge can only take place slowly. Then the energy dissipated in the wires will not to a first approximation depend on their resistances but on the ratios of the capacities, that in the wire of high resistance bearing to that in the other wires the ratio of the less capacity to the greater. Probably Riess's results only hold when the discharge takes place in such a way that it may be looked upon at any one moment as approximately in the steady state.

We have shown that the magnetic measure of the total current is the same all along the wire. Probably also the chemical measure is the same meaning by the chemical measure whatever interchanging or turning round of molecules may occur when induction takes place in a conductor. For even if a tube does not enter the wire at the same time throughout its length, an end part, say, entering first, the point of attachment of the tube to the conductor being transferred from the plate to somewhere along the wire, this transference of the point of attachment from molecule to molecule implies the same amount of chemical change within the wire as if the tube entered all at the same moment. It will not, however, take place equally throughout the cross-section as it does in the steady state.

Probably we only have the simultaneous disappearance of all parts of a tube when the wire follows a line of electric induction, and has its resistance per unit length proportional to the intensity which would exist there if the wire were removed.

The hypothesis here advanced is in accordance with Maxwell's doctrine of closed currents. For the induction dissipated at one part of the circuit has come there from another part where relatively to the circuit it ran in the opposite direction. The total result is equivalent to the addition of so many closed induction-tubes to the circuit, the induction running the same way relatively to the circuit throughout.

If the two plates of the condenser are not connected by a wire but are discharged gradually by the imperfect insulation of the dielectric, then we must suppose that the tubes of induction in this case are dissipated *in situ*, the induction simply decaying at a rate depending on its amount and upon the conductivity of the dielectric. We may still represent this process by a closed current by regarding the loss of induction $\left(\text{Maxwell's} -\frac{df}{dt}\right)$ and the quantity of induction dissipated (Maxwell's p) as two different quantities. We have then $p + \frac{df}{dt} = 0$ or we have two equal and opposite currents. But this seems artificial. It is more natural to look upon the process merely as a decay of electric induction without movement inwards of fresh induction-tubes, and therefore without the formation of magnetic induction.

I have discussed the case of discharge of a condenser at some length, as we can here realise more easily what goes on at the source of energy. The results obtained suggest that a similar action occurs at the source of energy or seat of the electromotive force in other cases where we do not know the distribution of induction, and are obliged to guess at the action.

A circuit containing a voltaic cell.

We may pass on from the discharge of a condenser to the consideration of the current in a circuit containing a voltaic cell. The chemical theory of the cell will be here adopted in fact, the hypothesis I am endeavouring to set forth has no meaning on the voltaic metal-contact theory.

Let us suppose the cell to consist of zinc and copper plates, a vessel of dilute sulphuric acid, and copper wires attached to each plate which on junction complete the circuit. For simplicity I shall disregard the effect of the air and suppose that it is a neutral gas causing no induction.

We shall begin by supposing the circuit open. Then we know that on immersion there will be temporary currents in the wires, the quantities of these currents depending on the electrostatic capacity of the system composed of the wires. The currents last till the wires have received charges such that they are, say, at difference of potential V. If the terminals are connected to a condenser the temporary currents may be easily detected by a galvanometer in the circuit. They are in no way to be distinguished in kind from the permanent current which will be established when the circuit is complete, except that they are of short duration and in general very small. There is no reason then to suppose that the action in the cell is different from that which takes place when the current is permanent, and I think we may safely assume that Faraday's law of electrolysis holds according to which the quantity of electricity flowing along either wire is proportional to the quantity of chemical action—or, in the form appropriate here, the number of tubes of induction produced is proportional to the quantity of chemical action.

Let Q be the total quantity of electricity upon the positive terminal; then $\frac{QV}{2}$ is the total energy thrown out into the dielectric.

Let z be the quantity of zinc consumed per unit of electricity, then Qz is the total quantity consumed in the charging of the terminals. Let E be the energy set free by each quantity z of zinc consumed, after all actions in the cell have been provided for. E then is the E.M.F. which the cell will have on the closure of the circuit, as long as the chemical actions remain the same, for z corresponds to the passage of a unit of electricity or the production of one tube, and we know that the energy set free by C units is CE.

Now while the charges are gathering and while the potential difference of the terminals is gradually increasing, the energy required to add equal increments of charge will also increase, and the charging will cease when the amount of energy given up by a given amount of chemical action in the cell is equal to the amount required to add the corresponding charge to the terminals. For to suppose the action to go beyond this is to suppose that the energy thrown out into the space between the terminals is greater than that yielded by the battery.

Let dQ be the last quantity of charge added to the terminals. This requires energy VdQ.

The corresponding quantity of zinc consumed is zdQ, giving up energy EdQ.

The condition of equilibrium is that

$$VdQ - EdQ$$

or

$$V = E,$$

which agrees with the result of experiment that the difference of potential of the terminals in open circuit is equal to the E.M.F. of the cell immediately after closure.

It may be noticed that the total quantity of energy extracted from the battery is

$$QE = QV,$$

while the electric energy left in the medium is

$$\frac{QV}{2},$$

or half the energy has been converted into heat in the wires.

We will now consider the distribution of level surfaces in the field while the circuit is still open. There will be $V - 1$ surfaces between the terminals, dividing each tube into V cells. None of these will cut the homogeneous parts of the circuit, since the whole of each of these must be at one and the same potential.

They can only cut the circuit by passing through the regions where there is contact of dissimilar bodies. We will neglect the contact of the zinc and copper, as the difference of potential there is insignificant compared with that at the two surfaces, zinc-acid and copper-acid.

Now we know that the energy of the cell is put out at the zinc-acid contact, but the amount is greater than that obtained from a consideration of the E.M.F. of the cell, for some energy is absorbed again, probably, at the copper-acid contact in the evolution of hydrogen. There is probably, then, induction between the acid and the zinc, and between the acid and the copper, these resembling the spaces between the plates of two condensers, the acid being at a higher potential than either. But if a given amount of induction disappears from the zinc-acid contact and appears at the terminals, more energy is lost at the former than appears at the latter. Hence all the cells have not been transferred from one to the other, or the difference of potential zinc-acid

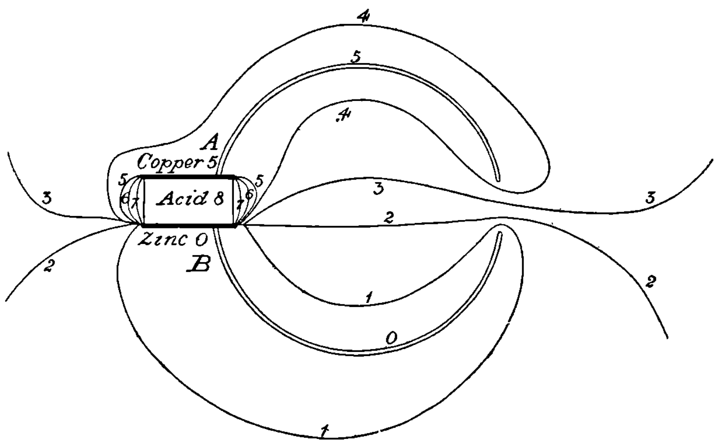


Fig. 3.

is greater than V. Then more than $V-1$ level surfaces pass between the zinc and the acid, the excess over $V-1$ going round and passing between the copper and the acid, somewhat as in Fig. 3, where A, B are the metal plates. The surfaces are roughly sketched and numbered, on the supposition that the zinc terminal is at 0, the copper at 5, and the acid at 8. They have probably the same shape as those which would be produced by condensers at A and B with the wires attached, respectively, to one terminal of each, the other terminals being connected together and the charges adjusted so that the difference of potential of the two terminals at A was 3, while that at B was 8.

Let us now suppose the circuit closed. Then the level surface will cut the circuit at various points, somewhat as in Fig. 4.

The energy being dissipated in the wire, the cell will continually send out fresh energy, the induction-tubes, which proceed from the acid to the zinc, diverging outwards in the same way as described in the discharge of a condenser. They bend round, and finally go into the circuit, the energy they carry being used for the necessary molecular changes, and finally appearing as heat in the circuit—except at the copper-acid contact where there is a crowding in of level surfaces, and therefore a convergence of more energy, which is required to set the hydrogen free.

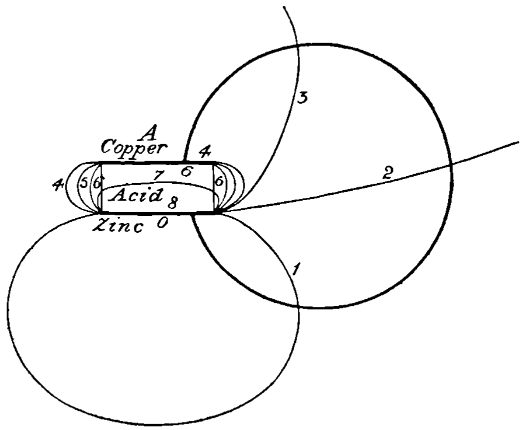


Fig. 4.

At the same time magnetic ring shaped tubes will be continually sent out from the zinc-acid contact, expanding for a time and then contracting again on various parts of the circuit and also giving up their energy.

There is, therefore, a convergence of tubes of electric induction on the circuit, running in the same direction throughout, viz., from copper to zinc outside the cell, and from zinc to copper inside, except between the zinc and acid, where there is a divergence of tubes in which the induction runs in the opposite way. But a divergence of negative tubes causes magnetic intensity in the same direction as, and may therefore be considered as equivalent to, a convergence of positive tubes. The current may therefore be said to go round the circuit in the same way throughout.

The tendency to a steady state in which the current or the number of induction-tubes broken up per second is the same at all parts of the circuit, admits of simple explanation. We know, as the result of experiment given by Ohm's law, that $C = \frac{E}{R}$ where R is the resistance per unit length and E the electric intensity. Until we can explain the molecular working of the current, i.e., the mode in which the induction-tubes are broken up, we must accept Ohm's law as a simple fact. Let us suppose that we have not yet arrived at the steady state, so that in some part of the circuit the electric intensity is less than in the steady state, while in another part it is equal to it or greater. Let the steady value of the intensity be E, the actual value in the former part E', and in the latter E''. By Ohm's law the number of tubes absorbed by the wire per second is given by $C' = E'/R$, and $C'' = E''/R$, in the two parts respectively, so that $C' < C''$ since $E' < E''$ or less tubes are being destroyed in the first than in the second part. But all the tubes are sent out from the source of the energy, and are only destroyed in the circuit, being otherwise continuous and with their two ends in the circuit. Hence, if more tubes are destroyed at one part than another, the parts of the tubes not yet destroyed will gather in the medium surrounding the part where fewer are destroyed, increasing the induction there, and so raising the intensity in the wire and therefore the number of tubes destroyed. The field can evidently only be steady when the number of tubes destroyed in all parts of the circuit is the same.

But it does not follow that in the steady state each tube enters the wire along its whole length at the same moment. This would imply that the axis of the wire is a line of electric induction perpendicular everywhere to the level surfaces. If we draw the level surfaces due to the seats of induction at the contacts of acid and metal, they will probably be somewhat as drawn in Fig. 4. If now the wire is not so arranged as to follow with properly adjusted resistances a line of induction for these surfaces, but pursues an irregular course, then the level surfaces will be much distorted, and the distribution of the induction will be greatly altered.

We may ascribe this alteration to a distribution of electricity along the wire, the quantity in any small area on the surface of the wire being equal to the difference between the number of tubes which have entered and the number which have left that area since the beginning of the system. We have a familiar example of this in the charging of deep-sea cables. Another example is afforded by a condenser with terminals connected to two points in the circuit. The plates of the condenser are then virtually parts of the circuit.

The effect of a junction of two wires, say of the same diameter, but of different specific resistances, upon the level surface will resemble that of a charge upon the separating surface. This can be seen in a general way from the fact that the level surfaces must cut the wire with the higher specific resistance at intervals shorter than those at which it cuts the other wire.

If there be an insulated conducting body, say a metal sphere, near the circuit, we know that in the steady state there is no electric intensity, and therefore no current within it; consequently there is no movement of energy and no movement of induction through it. We can see how this condition is arrived at. As the first tubes of electric and magnetic induction come up to the sphere they will enter it, and the parts of the electric induction-tubes thus entering will be broken up, causing a transient current in the sphere. The parts of the tubes left in the medium will end on the sphere giving a negative charge on the end nearer the regions of higher potential, and a positive charge on the end nearer the regions of lower potential. This will go on until such charges have accumulated that the sphere becomes itself a level surface. When this point is reached no more energy can enter the sphere, and the parts of the magnetic tubes within it cease to move.

The charges formed on the wire or on neighbouring conductors are to be distinguished from ordinary statical charges in this: that their existence depends on the existence of the current, and therefore on the motion of magnetic induction. If the current is stopped by a break in the circuit, so that the motion of the magnetic induction ceases, the electric induction ceases and the charges are all lost. We should expect, therefore, to find that these charges can be described in terms of the magnetic motions which have occurred and are occurring in the system.

Current produced by motion of a conductor in a magnetic field.

We may explain by general reasoning the production of a current by motion of a part of a circuit so as to cut the tubes of magnetic induction. We will consider the simple case of a slider AB, Fig. 5, running on two parallel rails, AC, BD, with a fixed cross-piece CD, the tubes of magnetic induction running from above downwards through the paper. Let AB move so as to enlarge the circuit. We know from experiment that this tends to cause a current in the direction $ACDB$.

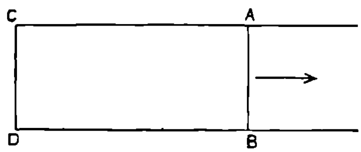


Fig. 5.

As AB moves through the field its motion tends to cause electric intensity in the direction BA. At the same time its kinetic energy is being continually converted into electric and magnetic energy which travels to the rest of the circuit there to be dissipated, that is, there must be a divergence of energy from AB. Instead then of a convergence of positive tubes running from B to A, we shall have what is magnetically equivalent a divergence of negative tubes or tubes running from A to B, their motion outwards being accompanied by tubes of magnetic induction running round in the same way as if there were an ordinary current from B to A. These magnetic tubes must be supposed to move outwards in order to account for the direction of the electric intensity*.

When these electric and magnetic tubes converge upon the rest of the circuit they will evidently form a current running in the direction $ACDB$.

* Note added July 15: The above must not be regarded as an attempt to explain the production of electric induction by the motion of a conductor in a magnetic field, but merely as an attempt to show how the induction arising in the moving part of a circuit finds its way into the rest of the circuit.

We have here taken, just as in the case of the condenser and the voltaic cell, the lessening of negative induction by its motion outwards, as equivalent to the increase of positive induction by its motion inwards, and we have considered both of them to indicate the application of electric intensity in the same direction in the conductor.

If instead of considering AB as a whole we break it up into elements, each element will be a source of diverging negative tubes, and the remainder of AB will, to that element, be a part of the rest of the circuit. Hence some of the energy sent out from the element will converge on and be dissipated in AB, or AB will be heated just as the rest of the circuit.

The general equations of the electromagnetic field.

We can easily obtain equations corresponding to and closely resembling those of Maxwell by means of the principles upon which this paper is founded.

The assumption that if we take any closed curve the number of tubes of magnetic induction passing through it is equal to the excess of the number which have moved in over the number which have moved out through the boundary since the beginning of the formation of the field, suggests a historical mode of describing the state of the field at any moment.

Let a, b, c be the components of magnetic induction at any point O. Consider a small area $dy\,dz$ close to the point, then the number of tubes passing through the area $dy\,dz$ will be $a\,dy\,dz$. This will be equal to the difference between those which have come in and those which have gone out.

Let Ldx, Mdy, Ndz denote the numbers of tubes which have cut the lengths dx, dy, dz since the beginning of the system, those being positive which have tended to produce electric intensity in the positive direction along the axes, and those being negative, and therefore subtracted, which have tended to produce intensity in the opposite direction.

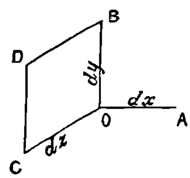


Fig. 6.

Let us consider the number which has come into the area $OBDC = dy\,dz$ (Fig. 6). The number which has come in across OB is $-Mdy$ ($-$ because the movement of tubes passing through $dydz$ in the positive direction must be outwards to produce E.I. along OB). The number which has passed out across CD is $-\left(M + \frac{dM}{dz}dz\right)dy$. The difference is $\frac{dM}{dz}dydz$. The number which has come in across OC is $+Ndz$ ($+$ because the movement of tubes passing through $dydz$ in the positive direction must be inwards to produce E.I. along OC). The number which has passed out across BD is $\left(N + \frac{dN}{dy}dy\right)dz$. The difference is $-\frac{dN}{dy}dydz$.

The number still passing through $dydz$ is therefore $\left(\frac{dM}{dz} \quad \frac{dN}{dy}\right) dydz$.

Equating this to the actual induction through the area, viz.,

$$a\,dy\,dz$$

and performing the same process for the corresponding areas $dzdx$, $dxdy$, we obtain

$$\left.\begin{aligned} a &= \frac{dM}{dz} - \frac{dN}{dy} \\ b &= \frac{dN}{dx} - \frac{dL}{dz} \\ c &= \frac{dL}{dy} - \frac{dM}{dx} \end{aligned}\right\} . \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

Comparing these with Maxwell's equations (vol. 2, p. 216) we see that

$$\frac{dM}{dz} - \frac{dN}{dy} = \frac{dH}{dy} - \frac{dG}{dz},$$

with two similar equations, F, G, H being the components of the vector-potential. We should obtain Maxwell's equations if we defined F, G, H to be the number of tubes which would cut the axes per unit length if the system were to be allowed to return to its original unmagnetic condition, the tubes now moving in the opposite direction. According to Maxwell, the vector whose components are F, G, and H 'represents the time-integral of the electromotive force which a particle placed at the point (x, y, z) would experience if the primary current were suddenly stopped' (vol. 2, 2nd ed., p. 215). If the electric intensity is produced by the motion of magnetic induction, then our definition of F, G, H will by the second fundamental principle agree with Maxwell's statement.

If u, v, w be the components of current—including, of course, under currents, growth of induction—we have from the third principle Maxwell's equations E (vol. 2, p. 233), which on multiplying by μ become when μ is constant

$$\left.\begin{aligned} 4\pi\mu u &= \frac{dc}{dy} - \frac{db}{dz} \\ 4\pi\mu v &= \frac{da}{dz} - \frac{dc}{dx} \\ 4\pi\mu w &= \frac{db}{dx} - \frac{da}{dy} \end{aligned}\right\} . \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$$

Combining these with equations (1) (as in Maxwell, vol. 2, pp. 236 7), and writing $-\nabla^2$ for $\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}$, we obtain

$$\left.\begin{aligned}4\pi\mu u &= -\nabla^2 L - \frac{d}{dx}\left(\frac{dL}{dx}+\frac{dM}{dy}+\frac{dN}{dz}\right)\\ 4\pi\mu v &= -\nabla^2 M - \frac{d}{dy}\left(\frac{dL}{dx}+\frac{dM}{dy}+\frac{dN}{dz}\right)\\ 4\pi\mu w &= -\nabla^2 N - \frac{d}{dz}\left(\frac{dL}{dx}+\frac{dM}{dy}+\frac{dN}{dz}\right)\end{aligned}\right\}. \quad\text{...............(3)}$$

These equations only differ in sign from Maxwell's, and are therefore to be solved in the same way.

It is easy to see by substitution that if we assume

$$\left.\begin{aligned}L' &= -\mu\iiint\frac{u}{r}\,dxdydz\\ M' &= -\mu\iiint\frac{v}{r}\,dxdydz\\ N' &= -\mu\iiint\frac{w}{r}\,dxdydz\\ H &= \frac{1}{4\pi}\iiint\left(\frac{dL}{dx}+\frac{dM}{dy}+\frac{dN}{dz}\right)\frac{1}{r}\,dxdydz\end{aligned}\right\}, \quad\text{............(4)}$$

then the following will be solutions

$$\left.\begin{aligned}L &= L' - \frac{dH}{dx}\\ M &= M' - \frac{dH}{dy}\\ N &= N' - \frac{dH}{dz}\end{aligned}\right\}. \quad\text{..............................(5)}$$

It is evident that we may add to the right-hand side of equations (5) $\frac{d\phi}{dx}, \frac{d\phi}{dy}, \frac{d\phi}{dz}$ respectively, where ϕ is any function of x, y, z, since these will disappear from (3) and also from (1).

The electric intensity, in so far as it depends upon magnetic motions, will consist of two terms, one depending upon the motion of the material at the point (its components being found as in Maxwell, vol. 2, p. 227, note), the other upon the motion of magnetic induction about the point. We may add a third term, arising from any electrical distribution with a potential ψ.

If there is no material motion we shall have

$$\left.\begin{aligned}P &= \frac{dL}{dt} - \frac{d\psi}{dx}\\ Q &= \frac{dM}{dt} - \frac{d\psi}{dy}\\ R &= \frac{dN}{dt} - \frac{d\psi}{dz}\end{aligned}\right\}. \quad\text{..............................(6)}$$

Substituting from (4) and (5) we get

$$P = -\mu\iiint\frac{du}{dt}\cdot\frac{1}{r}\,dxdydz - \frac{1}{4\pi}\frac{d}{dx}\iiint\left(\frac{d}{dx}\frac{dL}{dt} + \frac{d}{dy}\frac{dM}{dt} + \frac{d}{dz}\frac{dN}{dt}\right)\frac{1}{r}\,dxdydz - \frac{d\psi}{dx}$$

$$= -\mu\iiint\frac{du}{dt}\cdot\frac{1}{r}\,dxdydz - \frac{1}{4\pi}\frac{d}{dx}\iiint\left(\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz}\right)\frac{1}{r}\,dxdydz$$

$$- \frac{1}{4\pi}\frac{d}{dx}\iiint\left(\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} + \frac{d^2\psi}{dz^2}\right)\frac{1}{r}\,dxdydz - \frac{d\psi}{dx},$$

substituting for $\frac{dL}{dt}$, etc., from (6).

The last two terms cancel each other, and we get

$$P = -\mu\iiint\frac{du}{dt}\cdot\frac{1}{r}\,dxdydz - \frac{1}{4\pi}\frac{d}{dx}\iiint\left(\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz}\right)\frac{1}{r}\,dxdydz, \quad \ldots(7)$$

or if we put $$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = 4\pi\rho,$$

and $$V = \iiint\frac{\rho}{r}\,dxdydz,$$

then $$P = -\mu\iiint\frac{du}{dt}\cdot\frac{1}{r}\,dxdydz - \frac{dV}{dx}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(8)$$

with similar equations for Q and R.

If the system is steady $\frac{du}{dt}, \frac{dv}{dt}, \frac{dw}{dt}$ are all zero, and then

$$P = -\frac{dV}{dx}, \qquad Q = -\frac{dV}{dy}, \qquad R = -\frac{dV}{dz}.$$

The quantity ρ, of which V is the potential, will be zero within non-conducting homogeneous parts of the field, for there

$$f = \frac{KP}{4\pi}, \qquad g = \frac{KQ}{4\pi}, \qquad h = \frac{KR}{4\pi},$$

and $$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = \frac{4\pi}{K}\left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz}\right) = 0,$$

since no charges can reside within a homogeneous non-conducting medium. Or, stating it in another way, all the induction-tubes brought into any part of such a medium remain there without dissipation, a charge in a non-homogeneous medium being due to unequal amounts of dissipation of induction in different parts of the medium.

But ρ will have value at surfaces separating dissimilar substances either in the insulating or conducting parts of the medium. For in the former the induction is continuous, while the intensity is discontinuous, and in the latter the current or rate of destruction of induction may be continuous, but the relation between intensity and current changes discontinuously with the conductivity. At surfaces separating insulators from conductors ρ may have value, as, for instance, at the surfaces of the plates of a condenser with its terminals connected with two points in a circuit, or at the surface of an insulated conductor near the circuit. It is also to be noted that ρ will have value at the seat of electromotive force.

The values of the components of magnetic induction a, b, c are not in any way dependent on ρ. For taking the first of equations (1) and substituting from (5) we have

$$a = \frac{dM}{dz} - \frac{dN}{dy} = \frac{dM'}{dz} - \frac{dN'}{dy} - \frac{d^2H}{dy\,dz} + \frac{d^2H}{dz\,dy} = \frac{dM'}{dz} - \frac{dN'}{dy}, \quad \ldots(9)$$

where M' and N' depend on the currents in the system and not on the charges.

Comparing our equations with Maxwell's we see that the important point of difference is that we can no longer put the quantity corresponding to his J equal to zero, J being given by $\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz}$.

This does not affect the determination of velocity of propagation of disturbance in a homogeneous non-conducting medium.

For in such a medium we shall have

$$u = \frac{df}{dt} = \frac{K}{4\pi}\frac{dP}{dt},$$

with corresponding values for v and w.

Substituting in (3) the first equation becomes

$$K\mu\frac{dP}{dt} = -\nabla^2 L - \frac{d}{dx}\left(\frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dz}\right);$$

differentiating with respect to t

$$K\mu\frac{d^2P}{dt^2} = -\nabla^2\frac{dL}{dt} - \frac{d}{dx}\left(\frac{d}{dx}\frac{dL}{dt} + \frac{d}{dy}\frac{dM}{dt} + \frac{d}{dz}\frac{dN}{dt}\right),$$

and putting $\frac{dL}{dt} = P + \frac{d\psi}{dx}$,

$$K\mu\frac{d^2P}{dt^2} = -\nabla^2 P - \frac{d}{dx}\nabla^2\psi - \frac{d}{dx}\left(\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz}\right) + \frac{d}{dx}\nabla^2\psi = -\nabla^2 P, \ldots(10)$$

since $$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = \frac{4\pi}{K}\left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz}\right) = 0$$

within a homogeneous non-conductor.

This gives the velocity of propagation of electric induction equal to

$$1/\sqrt{K\mu}.$$

We can also obtain the corresponding equation for the magnetic induction.

Substituting in (3) for u, v, and w in terms of P, Q, and R, as above, differentiating the second with respect to z, and the third with respect to y, and subtracting

$$K\mu \frac{d}{dt}\left(\frac{dQ}{dz} - \frac{dR}{dy}\right) = -\nabla^2\left(\frac{dM}{dz} - \frac{dN}{dy}\right),$$

then from (6)

$$K\mu \frac{d}{dt}\left(\frac{d}{dt}\frac{dM}{dz} - \frac{d^2\psi}{dz\,dy} - \frac{d}{dt}\frac{dN}{dy} + \frac{d^2\psi}{dy\,dz}\right) = -\nabla^2\left(\frac{dM}{dz} - \frac{dN}{dy}\right),$$

or

$$K\mu \frac{d^2}{dt^2}\left(\frac{dM}{dz} - \frac{dN}{dy}\right) = -\nabla^2\left(\frac{dM}{dz} - \frac{dN}{dy}\right),$$

or from (1)

$$K\mu \frac{d^2 a}{dt^2} = -\nabla^2 a, \quad \text{.............................(11)}$$

whence the velocity of propagation of magnetic induction is also equal to

$$1/\sqrt{K\mu}.$$

It would seem that in some cases, such as that of the field surrounding a straight wire with a steady current, the electric intensity may be regarded as entirely due to the motion of magnetic induction, and its components will therefore be $\frac{dL}{dt}, \frac{dM}{dt}, \frac{dN}{dt}$.

But in other cases it would seem that the electric induction cannot be wholly due to the motion of magnetic induction, and we must therefore introduce the terms involving ψ. If, for instance, the electric and magnetic intensities were inclined at an angle θ, we should have to suppose the electric intensity E to be produced by the motion of the component of magnetic induction I perpendicular to E, viz., $\mu I \sin\theta$, the other component $\mu I \cos\theta$ being at rest. To produce intensity E, E tubes must cut unit length in the direction of E per second; and since the value of the magnetic induction is $\mu I \sin\theta$, this requires a velocity v, given by $v \cdot \mu I \sin\theta = E$ or $v = E\ \mu I \sin\theta$. Now we can easily imagine a case where E and I coincide, as, for instance, a condenser with its planes parallel to the axis of a wire carrying a current, and its terminals connected with two points in the wire. Here $I \sin\theta = 0$, and v is infinite. Or we have to suppose the electric intensity to be produced by the movement of tubes of induction of no intensity with infinite velocity, a statement without physical meaning.

But it is, perhaps, worth noting that if we suppose that the electric intensity is produced by the motion of magnetic induction, and that the magnetic intensity is produced by the motion of the electric induction, each carrying its energy with it, the right quantity of energy crosses the unit area.

For E magnetic tubes, with $I \sin\theta$ unit cells per unit length, will carry across unit area in the plane of E and I a quantity $\frac{EI\sin\theta}{8\pi}$, or half the energy which actually crosses the plane. If $I\sin\theta$ is due to the motion of electric tubes, then $I\sin\theta/4\pi$ tubes must cut unit length in the direction of $I\sin\theta$ per second. The number of unit cells per unit length is E, and therefore the motion of the tubes will carry a quantity of energy $\frac{EI\sin\theta}{8\pi}$, or the other half actually crossing.

The equations which have been obtained in the foregoing manner by the aid of the hypothesis of movement of magnetic induction may also be obtained without any special hypothesis as to the motion of the induction-tubes, merely assuming that growth of induction through a curve is accompanied by electric intensity round the curve. Instead of connecting L, M, N with the number of tubes which have cut the axes, we start with the following definitions:

Let L, M, N denote the time-integrals of the components of the electric intensity parallel to the axes since the origin of the system, so that

$$L = \int P dt, \qquad M = \int Q dt, \qquad N = \int R dt,$$

then
$$P = \frac{dL}{dt}, \qquad Q = \frac{dM}{dt}, \qquad R = \frac{dN}{dt}.$$

If a b, c be components of magnetic induction, since the growth of induction through a curve is equal to the line-integral of the electric intensity round a curve in the negative direction, we have

$$\frac{da}{dt} = \frac{dQ}{dz} - \frac{dR}{dy} = \frac{d}{dt}\left(\frac{dM}{dz} - \frac{dN}{dy}\right)$$

with corresponding equations for $\frac{db}{dt}$ and $\frac{dc}{dt}$.

Integrating with respect to t from the origin of the system, when all the quantities were zero

$$\left.\begin{aligned} a &= \frac{dM}{dz} - \frac{dN}{dy} \\ b &= \frac{dN}{dx} - \frac{dL}{dz} \\ c &= \frac{dL}{dy} - \frac{dM}{dx} \end{aligned}\right\}, \qquad \text{..............................(1')}$$

equations the same in form as equations (1).

As before we obtain equations (3), (4), and (5), while instead of (6) we have the simple equations $P = \frac{dL}{dt}$ and the two others.

Substituting for $\frac{dL}{dt}$ we obtain an equation of the same form as (7), which may also be put into the form (8). Equations (9) and (10) will also follow.

Just as we have obtained equations by considering the growth of the magnetic induction to its present state so we may obtain corresponding equations by considering the growth of the electric induction.

Let $\frac{A\,dx}{4\pi}, \frac{B\,dy}{4\pi}, \frac{C\,dz}{4\pi}$ be the algebraic sum of the number of electric induction-tubes which have cut dx, dy, dz drawn from a point in such a way as to create magnetic intensities in the positive direction along dx, dy, dz.

The excess of the number of tubes which have passed in over those which have passed out through the boundary of any area will be equal to the time-integral of the total current through the area.

The components of the total current are

$$u = p + \frac{df}{dt}, \qquad v = q + \frac{dg}{dt}, \qquad w \quad r \quad \frac{dh}{dt},$$

p, q, and r being the components of the conduction-current or the number of tubes dissipated per second, and f, g, h the components of the induction actually existing.

As in the last case, if we put $f' = \int u\,dt$, etc., we at once obtain the equations

$$\left.\begin{aligned} 4\pi f' &= \frac{dC}{dy} - \frac{dB}{dz} \\ 4\pi g' &= \frac{dA}{dz} - \frac{dC}{dx} \\ 4\pi h' &= \frac{dB}{dx} - \frac{dA}{dy} \end{aligned}\right\}. \qquad \text{..........................(12)}$$

Corresponding to the current-equations (2) we have three equations obtained from the condition that the rate of increase of magnetic induction through an area is equal to the integral of the electric intensity round it in the negative direction. These are

$$\left.\begin{aligned} \frac{da}{dt} &= \frac{dQ}{dz} - \frac{dR}{dy} \\ \frac{db}{dt} &= \frac{dR}{dx} - \frac{dP}{dz} \\ \frac{dc}{dt} &= \frac{dP}{dy} - \frac{dQ}{dx} \end{aligned}\right\}. \qquad \text{............................(13)}$$

If $C_{\prime}$ is the specific conductivity we may by Ohm's law put the current-equations after integrating in the form

$$f' = C_{\prime}\int P dt + \frac{KP}{4\pi},$$

$$g' = C_{\prime}\int Q dt + \frac{KQ}{4\pi},$$

$$h' = C_{\prime}\int R dt + \frac{KR}{4\pi},$$

whence in media where K is constant

$$\frac{dg'}{dz} - \frac{dh'}{dy} = C_{\prime}\int\left(\frac{dQ}{dz} - \frac{dR}{dy}\right)dt + \frac{K}{4\pi}\left(\frac{dQ}{dz} - \frac{dR}{dy}\right)$$

$$= C_{\prime}\int\frac{da}{dt}dt + \frac{K}{4\pi}\frac{da}{dt} \text{ from (13)}$$

$$= C_{\prime}a + \frac{K}{4\pi}\frac{da}{dt},$$

with two similar equations.

Finding the values of the left-hand side from (12) we obtain

$$\left.\begin{aligned} 4\pi C_{\prime}a + K\frac{da}{dt} &= -\nabla^2 A - \frac{d}{dx}\left(\frac{dA}{dx} + \frac{dB}{dy} + \frac{dC}{dz}\right)\\ 4\pi C_{\prime}b + K\frac{db}{dt} &= -\nabla^2 B - \frac{d}{dy}\left(\frac{dA}{dx} + \frac{dB}{dy} + \frac{dC}{dz}\right)\\ 4\pi C_{\prime}c + K\frac{dc}{dt} &= -\nabla^2 C - \frac{d}{dz}\left(\frac{dA}{dx} + \frac{dB}{dy} + \frac{dC}{dz}\right)\end{aligned}\right\} \dots\dots\dots(14)$$

If we assume

$$A' = -\frac{1}{4\pi}\iiint\left(4\pi C_{\prime}a + K\frac{da}{dt}\right)\frac{1}{r}\,dx\,dy\,dz,$$

with corresponding values for B' and C' and

$$M = \frac{1}{4\pi}\iiint\left(\frac{dA}{dx} + \frac{dB}{dy} + \frac{dC}{dz}\right)\frac{1}{r}\,dx\,dy\,dz,$$

then

$$\left.\begin{aligned} A &= A' - \frac{dL}{dx}\\ B &= B' - \frac{dM}{dy}\\ C &= C' - \frac{dN}{dz}\end{aligned}\right\} \dots\dots\dots\dots\dots\dots\dots\dots\dots(15)$$

are solutions of (13).

We may obtain by substitution from (15) in (12) values for f', g', h' corresponding to the values of the magnetic induction in (9), viz.:

$$4\pi f' = \frac{dC'}{dy} - \frac{dB'}{dz},$$

and two others; where A', B', C' are given in terms of the magnetic induction as above.

It is only in special cases, such as that of a straight wire with a steady current, that the magnetic intensity will be equal to 4π times the number of electric induction-tubes passing through unit length per second. In all cases the line-integral of the magnetic intensity round a closed curve is equal to 4π times the number of electric tubes passing through the boundary, but the electric tubes may be more crowded in some parts than in others, while the magnetic intensity is not altered in a corresponding manner. For instance, the magnetic tubes will be continued through an insulated conductor in the field, while in the steady state no electric tubes pass through it. But each element adds to the line-integral the quantity which, after Mr. Bosanquet, I have called the magnetomotive force, this being equal to 4π times the number of electric tubes passing through the element. But it only adds it on integrating round the whole of the closed curve.

The intensity at any point will therefore be the resultant of the intensities produced by the magnetomotive forces in the various elements. Perhaps the simplest mode of finding it is as follows.

The components of the magnetomotive force produced in a cube dx, dy, dz parallel to the three edges will be

$$\frac{dA}{dt}dx, \qquad \frac{dB}{dt}dy, \qquad \frac{dC}{dt}dz,$$

for $\frac{1}{4\pi}\frac{dA}{dt}$, $\frac{1}{4\pi}\frac{dB}{dt}$, $\frac{1}{4\pi}\frac{dC}{dt}$ are by definition the rates at which electric tubes are cutting unit lengths parallel to the axes.

But these magnetomotive forces would be produced by currents round the cube in planes perpendicular to the axes respectively, and equal to

$$\frac{1}{4\pi}\frac{dA}{dt}dx, \qquad \frac{1}{4\pi}\frac{dB}{dt}dy, \qquad \frac{1}{4\pi}\frac{dC}{dt}dz,$$

for the line-integral of the intensity round a curve threading a current is $4\pi \times$ current. But the magnetic intensity at any point due to a current is equal to that of a magnetic shell of strength (i.e., intensity thickness) equal numerically to the current bounding the shell.

If we suppose the thickness of the shell equal to that of the cube, the effect is the same as if the cube were magnetised with intensity having components

$$\frac{1}{4\pi}\frac{dA}{dt}, \qquad \frac{1}{4\pi}\frac{dB}{dt}, \qquad \frac{1}{4\pi}\frac{dC}{dt}.$$

The potential of such a distribution of magnetisation is (Maxwell, vol. 2, p. 29, equation (23))

$$V = \frac{1}{4\pi}\iiint\left(\frac{dA}{dt}\frac{dp}{dx} + \frac{dB}{dt}\frac{dp}{dy} + \frac{dC}{dt}\frac{dp}{dz}\right)dx\,dy\,dz,$$

where $p = \frac{1}{r}$, and the magnetic intensity is given by

$$\alpha = -\frac{dV}{dx}, \qquad \beta = -\frac{dV}{dy}, \qquad \gamma = -\frac{dV}{dz}.$$

It may be noticed that in a steady field $\frac{dA'}{dt}, \frac{dB'}{dt}, \frac{dC'}{dt}$ are all zero, so that

$$V = -\frac{1}{4\pi}\iiint\left(\frac{d}{dx}\frac{dM}{dt}\frac{dp}{dx} + \frac{d}{dy}\frac{dM}{dt}\frac{dp}{dy} + \frac{d}{dz}\frac{dM}{dt}\frac{dp}{dz}\right)dx\,dy\,dz,$$

where
$$M = \frac{1}{4\pi}\iiint\left(\frac{dA}{dx} + \frac{dB}{dy} + \frac{dC}{dz}\right)\frac{1}{r}\,dx\,dy\,dz.$$

We may obtain equations of the same form as those given in (14) without any hypothesis as to the movement of electric induction-tubes, merely assuming that the total current through a curve is equal to $4\pi \times$ line-integral of magnetic intensity round the curve.

We start with the following definitions. Let A, B, C be the time-integrals of the components of magnetic intensity since the origin of the system.

Then
$$A = \int \alpha\,dt, \qquad B = \int \beta\,dt, \qquad C = \int \gamma\,dt,$$

and
$$\alpha = \frac{dA}{dt}, \qquad \beta = \frac{dB}{dt}, \qquad \gamma = \frac{dC}{dt}.$$

We have the equation
$$4\pi u = \frac{d\gamma}{dy} - \frac{d\beta}{dz}$$
and two others.

Integrating with respect to t we have

$$\left.\begin{aligned} 4\pi\int u\,dt = 4\pi f' &= \frac{dC}{dy} - \frac{dB}{dz} \\ 4\pi g' &= \frac{dA}{dz} - \frac{dC}{dx} \\ 4\pi h' &= \frac{dB}{dx} - \frac{dA}{dy} \end{aligned}\right\},$$

also

which are of the same form as (12).

Hence exactly as before we obtain equations (14) and their solutions (15).

The equations for the magnetic intensity are now

$$\alpha = \frac{dA}{dt}, \qquad \beta = \frac{dB}{dt}, \qquad \gamma = \frac{dC}{dt}.$$

If we differentiate (14) with respect to t, and substitute from these equations for magnetic intensity, we obtain

$$4\pi\mu C, \frac{d\alpha}{dt} + K\mu \frac{d^2\alpha}{dt^2} = -\nabla^2\alpha - \frac{d}{dx}\left(\frac{d\alpha}{dx} + \frac{d\beta}{dy} \quad \frac{d\gamma}{dz}\right),$$

with corresponding equations for β and γ.

Differentiating the second of these with respect to z, and the third with respect to y, and subtracting, we obtain

$$4\pi\mu C, \frac{du}{dt} + K\mu \frac{d^2u}{dt^2} \quad -\nabla^2 u,$$

with corresponding equations for v and w.

These correspond to Maxwell's equations (7), p. 395.

In conclusion it may be remarked that the equations found in this paper give the same expression for the rate of Transfer of Energy as that in my previous paper derived from Maxwell's equations involving F, G, and H.

12.

DISCHARGE OF ELECTRICITY IN AN IMPERFECT INSULATOR.

[*Birmingham Phil. Soc. Proc.* **5**, 1885, pp. 68–82.]

[*Read* December 10, 1885.]

Maxwell has shown that the phenomenon known as the Residual Discharge may be accounted for on the supposition that the dielectric is an imperfect insulator in which the conductivity varies in different parts. His theory is really quite simple and straightforward and free from any hypothesis beyond the fundamental one of electric displacement. But its very generality makes it, I believe, difficult to grasp. The idea of a yielding of displacement in the dielectric, accompanied by a conduction-current in the opposite direction, gives us no help in forming a mental picture of the process actually going on in the dielectric. A hypothesis as to the nature of electric current, which will shortly be published in the *Philosophical Transactions*, seems to me to render the theory easier to follow, and I propose in this paper to arrange Maxwell's account of the Residual Discharge in accordance with it.

I shall first give some account of the hypothesis referred to in the special case of the discharge of a condenser. Let us imagine that we have two conductors, A and B, which we may suppose to be the two plates of a condenser, charged with equal and opposite amounts of electricity, that of A being positive. Then the lines of force will run from A to B through the medium, the condition of the medium being described by saying that there is 'electric displacement' from A to B. Or we may describe it without introducing the confusing term 'displacement' by returning to Faraday's term 'induction.' We may then say that tubes of electric induction pass through the medium, each tube starting from $+ 1$ of electricity on A, and ending in $- 1$ on B. The total induction across any section of a tube is then always equal to 1. If we draw the level surfaces at unit differences of potential the tubes will be divided up into cells, and if we suppose each cell to contain half a unit of energy then the whole energy of the electrified system is accounted for. Maxwell has called these *unit cells* (*Elementary Treatise on Electricity*, p. 47). According to the views of Faraday and Maxwell, the charges on the conductors bounding the dielectric are to be regarded as the surface-manifestations of the altered state of the dielectric corresponding to the energy put into it, somewhat as the pressure on a piston in the wall of a closed vessel of compressed water might be regarded as the surface-manifestation of the strained condition of the water.

In order to follow out the process of discharge in the medium, i.e., the mode in which it is relieved from its strained condition, we will first take a simpler case in which we connect the two plates, A and B (Fig. 1), of one condenser to the two plates, C and D, of another condenser previously uncharged, and so far from A and B that there is no appreciable direct inductive action on C and D. When equilibrium is again restored the charge is shared between A and C, the $-$ charge between B and D, while the difference of level has decreased. There is the same total number of tubes of induction, but each contains fewer unit cells than before, the energy corresponding to the decrease having been transferred to the wires, where it has been dissipated as heat. I shall use the term energy-length to indicate the line-integral of the electric intensity along its axis, this being the same as the difference of

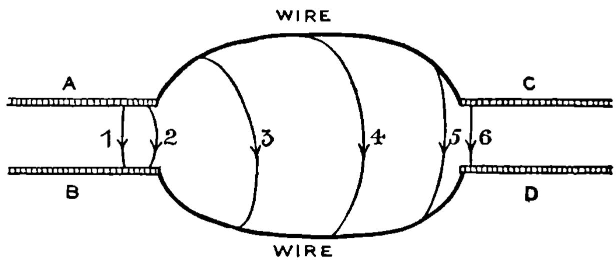


Fig. 1.

potential when there is equilibrium. We may say then that the energy-length of the tubes has decreased. During the change some of the electric energy was converted into magnetic energy in the medium. This might be observed if sufficiently delicate means were used.

If we confine our attention to the charges on the conductors we must say that equal quantities of $+$ and $-$ have moved respectively from A to C and from B to D along the wires.

But taking into account the condition of induction in the medium, described by the induction-tubes, we must say that the induction-tubes move sideways out from the space between A and B into the space between C and D, the motion of the charges along the wires being really the motion of the ends of the induction-tubes. (See Fig. 1, where 1 6 may be taken as successive positions of a tube.) During the motion of the tubes some of their energy was converted into the magnetic form, the co-existence of the two forms, electric and magnetic, being a necessary condition of motion. We may illustrate this from the analogous case of a strained incompressible solid which can be sheared. If there is any mode of escape given to the strain-energy by a slipping of the surface against the constraint, then the state of strain will be propagated outwards from the interior of the solid, but some of the strain-energy will be converted into kinetic energy, and the presence of the two is a necessary condition for the propagation of the strain.

Since the energy-length of a tube diminishes as its ends move along the connecting wires, we may represent this by supposing that parts of the tubes move into the wire. If a similar motion of electric induction took place into a dielectric it would remain, and the dielectric would become electrically strained, but in the wire the strain breaks down rapidly, the energy being converted into heat. I think there is good reason to suppose that it is the electric energy which thus breaks down, the magnetic only being dissipated after it has been reconverted into the electric form.

We may now consider the case in which total discharge of a condenser takes place through a connecting wire. Considering merely the conducting plates and the wire, we say that the charges move along them towards each other and finally unite, neutralising each other and producing heat in the wire. Regarding the medium we must suppose the tubes of induction to move sideways towards the wire, shortening as their ends, which are represented by the charges, approach each other, and finally disappearing into the wire. Faraday describes the process by saying that 'when current or discharge occurs between two bodies, previously under inductrical relations to each other, the lines of inductive force will weaken and fade away, and, as their lateral repulsive tension diminishes, will contract and ultimately disappear in the line of discharge.' (*Exp. Res.* vol. 1, p. 529, § 1659.) The so-called velocity of electricity is merely the velocity of the ends of the tubes, and this may evidently vary according to the nature of the circuit. It is quite conceivable that if the wire be in a neutral medium, i.e., one in which there is no surface-difference of potential, say gold in air, and if it follow the direction of a tube of induction, then a tube may move into the wire throughout its whole length at once. In this case the 'velocity of electricity' would be infinite.

We know from experiment that if a galvanometer be inserted in the connecting wire then the same magnetic impulse is observed wherever in the circuit the galvanometer be placed, the impulse depending on the galvanometer-constant and on the total discharge. The same experimental result may be stated in an equivalent form, viz., that the line-integral of the magnetic intensity round a closed curve encircling the wire if integrated for the time of discharge is the same for all positions of the curve. On the hypothesis here described all the electric induction-tubes of the system finally pass sideways from the medium into the wire. They must, therefore, on their way pass inwards across any curve encircling the wire, so that the total number of induction-tubes cutting such a curve is the same for all positions of the curve. In the paper above referred to I have sought to connect these two constants by supposing that the magnetic effect is due to, or more correctly accompanies, the motion inwards of the condition of electric induction. As soon as motion commences some of the electric energy is converted into magnetic, and the magnetic induction may be represented by ring-shaped closed tubes surrounding the wire. The two inductions, electric and magnetic, co-existing, will propagate the energy onwards till it finally arrives in the wire and is dissipated as heat, the induction there losing its directed condition.

The flowing of electric charges along the wire, which is usually considered as the essential part of the phenomenon, or at least that to which attention is to be chiefly directed, becomes on this hypothesis merely the last stage in the process, which consists of a propagation from the surrounding dielectric towards the wire of electric and magnetic induction, which we may symbolise by the motion inwards of two sets of tubes, the electric tubes being, on the whole, more or less in the direction of the wire, the magnetic tubes being closed rings surrounding it. The wire plays the part of the refrigerator in a heat-engine, turning the energy it receives into heat a necessary condition for the working of the machinery.

Let us now take the case of a condenser in which the dielectric, though homogeneous, is imperfectly insulating, so that the charge gradually disappears. According to Maxwell, in this case 'induction and conduction are going on at the same time.' Though Maxwell gave no precise account of the process of discharge, his theory and the mechanical illustration accompanying it are based on the supposition that two processes are going on at the same time in every part of the medium, viz.: (1) a yielding of the electric strain or 'displacement' in the dielectric, equivalent to a displacement-current from the negative towards the positive plate, and (2) a conduction-current from the positive plate to the negative equal to (1) in amount. This latter is accompanied by dissipation of energy. The two equal and opposite currents being superposed have no external magnetic effect.

But it seems to me that we may equally well and more simply represent the facts by considering the first process alone, viz., the yielding of the electric strain, the medium being incapable of bearing it permanently. The electric energy is gradually converted into heat in the same part of the dielectric where it was previously electric, i.e., there is here no transfer of energy. The decrease of induction in the medium is accompanied by a corresponding decrease of charge on the plates, not by conduction of + or − electricity either way through the medium, but simply because there is a decrease of the induction in the medium of which the charges on the plates are the surface-manifestations. The induction decreases equally through the whole length of a tube, so that the tube 'weakens' at the same rate throughout its length.

There will be no magnetic effect in the surrounding space for there is no movement inwards of electric induction-tubes to supply the place of those which decay.

Perhaps we may take the following as illustrating the two modes of regarding the process. Suppose that a solid is submitted to some strain and kept in the strained position, but that the energy of the strain gradually dissipates; then we may confine ourselves simply to the statement that owing to some rearrangement of the molecules they cease to have molecular strain-energy, the energy in each portion of the mass being transformed into heat in that portion, or we may imagine that there is a continual return from the strained towards the original position, accompanied by an equal reverse flow of the matter towards the strained position, this latter not storing up energy but dissipating the energy given up by the yielding of the strain. The ultimate result according to each is the same, but the latter account is purely hypothetical.

We may at once obtain the equation giving the value of the charge at any time in terms of the initial charge when the condenser is left insulated.

Let σ be the charge per unit area, this being equal to the electric induction across unit area in the dielectric.

Let K be the specific inductive capacity.

Let X be the electric intensity in the dielectric, i.e., force per unit electricity on a small electrified body.

We have
$$X = \frac{4\pi\sigma}{K}. \qquad \text{.........(1)}$$

Now we know that the rate of decrease of charge on the ends is proportional to the charge σ and therefore to X.

The decrease of charge or of induction in the medium is therefore
$$\frac{d\sigma}{dt} = -\frac{X}{r}, \qquad \text{.........(2)}$$

where r is a constant, which we may term the specific resistance.

Hence from (1)
$$\frac{d\sigma}{dt} + \frac{4\pi\sigma}{Kr} = 0, \qquad \text{.........(3)}$$

or
$$\sigma = \sigma_0 e^{-\frac{4\pi t}{Kr}}. \qquad \text{.........(4)}$$

If we use p to denote the decrease of induction per second,
$$p = -\frac{d\sigma}{dt} = -\frac{K}{4\pi}\frac{dX}{dt}. \qquad \text{.........(5)}$$

The energy per unit volume is $\frac{KX^2}{8\pi}$; its rate of decrease is therefore
$$-\frac{KX}{4\pi}\frac{dX}{dt}. \qquad \text{.........(6)}$$

Substituting from (2) and (5) we get the expression which here corresponds to Joule's law for the heating effect, viz., rate of decrease of electric energy per unit volume $= p^2r$.

If at any moment the two end-plates be connected by a wire, transfer of induction will at once take place into the wire, and the whole system will be completely discharged. During this discharge there will be magnetic energy accompanying the motion of electric induction.

We will now investigate the more complicated case of a stratified dielectric in which the different layers have different specific resistances. Before proceeding to the mathematical account we shall consider the process generally, taking the simple case in which K is the same throughout. Let the condenser be charged very rapidly and then insulated. At the first moment there will be equal and opposite charges on the two end plates, and the number of induction-tubes running through unit area parallel to the plates will be the same in each layer. But decay of induction, and dissipation of energy, at once sets in, the rate of decay varying in different layers, so that after a time the number of induction-tubes in contiguous layers will differ and there will be charges on the separating surfaces. In those layers where the rate of decay is most rapid there will be negative charges on the surface nearer the + plate, and + charges on the surface nearer the − plate. But still the induction in all is in the same direction.

Now let the two end-plates be connected by a wire. At once induction is propagated into the wire and transference takes place from the space between the plates until they are at the same potential, i.e., until the line-integral of the electric intensity, or, since K is constant, that of the induction, from plate to plate is zero. The same number of tubes must have entered all parts of the wire, otherwise there would be charges at points along its length. Hence the same number of tubes running in the positive direction must have passed out from each of the layers. The result must be a reversal of the induction in some of the layers, viz., in those in which the induction decayed most rapidly. This, of course, means that after their positive induction has all flowed out and they are quite discharged, tubes from the other layers have bent round and entered them, now charging them in the opposite direction. We may imagine the process to be somewhat as in Figs. 2 and 3, representing a condenser with three layers, A, B, C, the decay having been most rapid in the middle one, so that it has become completely discharged, while there is still positive induction in A and C. 1, 2, 3, 4 (Fig. 2) represent successive positions of a tube moving out from A towards the wire; 1′, 2′, 3′, 4′, successive positions of a tube moving out from C. When they have taken up the positions 4, 4′ they come in contact, and where they overlap they will neutralise each other and break up into two portions, the outer part of each forming one positive tube, as 5, Fig. 3, which will move off to the wire, inner parts uniting to form a negative tube 6 in B.

When the difference of potential between the end-plates is zero, suppose the wire to be removed. The induction still remaining decays. If it decayed in the same proportion throughout, the difference of potential would always remain zero. But it decays in greater proportion in the negative layers, since in these the dissipation is, by hypothesis, most rapid. Hence in the line-integral of the induction from plate to plate the negative terms decrease more rapidly than the positive, and so the total value becomes positive. Then on

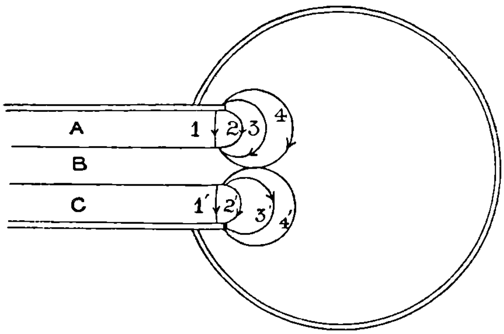


Fig. 2.

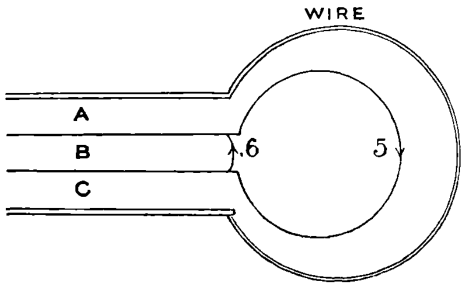


Fig. 3.

again connecting with a wire another positive discharge occurs. The process may evidently be repeated, the discharge always being positive, until finally it becomes insensible.

The analogy between the residual discharge and the phenomenon of elastic recovery in strained solids, pointed out by Kohlrausch, suggests a simple illustration.

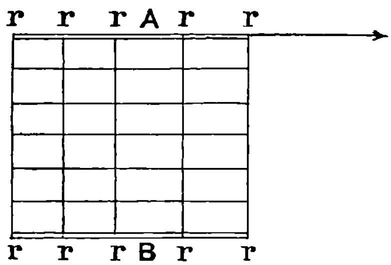


Fig. 4.

Suppose that we build up a cube with successive layers of substances with the same instantaneous rigidity but with different viscosities. Let this be placed between two plates, A, B, Fig. 4, the lower plate being fixed. Let rigid transverse partitions, r, r, be passed through the layers and attached by hinges to the two plates, and then let the upper plate be acted on by a force in a direction perpendicular to the partitions, so that a shearing strain is given to the whole cube. The partitions, r, r, are merely put so that the distortion from the original position shall always be the same throughout. When a given strain has been produced let the upper plate be also fixed. Now if the rate of dissipation of strain-energy were the same throughout the layers the stress would also be the same throughout, though gradually decreasing, and on removing the constraint the upper plate would return by a certain amount and then remain in its new position. But the dissipation is not uniform and after a time the stress in some of the layers is greater than in others. Hence, on removing the constraint from A and allowing it to return, when those in which dissipation has been most rapid have become entirely free from strain-energy, there is still some remaining in the other layers. These latter will, therefore, strain the former, and we shall have a reverse stress in some of the layers. Thus A will come to a new position of equilibrium, not so far, however, as its first position. Suppose that it is now again fixed. At first no force is necessary to keep it in position, but the stress exerted by the negative layers decays more rapidly than that exerted by the positive, and soon, on being released, A will return still further towards its original position. The process may be repeated, the successive discharges of momentum imparted to A being always in the same direction.

(*Added April* 15*th*, 1886. The supposition of stratification made by Maxwell is, no doubt, very artificial, and was made for the sake of simplicity in the mathematical treatment. He states that 'an investigation of the cases in which materials are arranged otherwise than in strata would lead to similar results, though the calculations would be more complicated, so that we may conclude that the phenomena of electric absorption may be expected in the case of substances composed of parts of different kinds, even though these individual parts should be microscopically small.

'It by no means follows that every substance which exhibits this phenomenon is so composed...' (*Electricity and Magnetism*, 2nd ed., vol. 1, p. 419).

Probably in the case of blown glass or any dielectric made up of heterogeneous parts, which has been flattened by rolling, there is more or less approach to the stratified condition, but in other cases, such as shellac or paraffin, we might fairly expect the dielectric to be similarly constituted in all directions. We can only, therefore, take Maxwell's investigation as showing in a general way that heterogeneity would introduce absorption phenomena, and we cannot expect the results obtained on the supposition of such a special arrangement to agree with those of experiment. We may regard the stratified arrangement as giving a superior limit, as it were, this being the constitution most favourable to the production of the phenomena in the way supposed. The inferior limit would be given by an arrangement in which each portion of the substance of the same kind stretched from plate to plate with the same cross-section throughout. In this case there would be no residual discharge produced. Using Maxwell's notation (see below), the resistance per unit cross-section may be shown to be

$$R' = \frac{(a_1 + a_2 + \ldots)^2}{\frac{a_1}{r_1} + \frac{a_2}{r_2} + \ldots}$$

instead of $$R = a_1r_1 + a_2r_2 + \ldots$$

and R' is always less than R. In any intermediate composition in which portions of more conducting matter are insulated from each other by less conducting matter we shall have residual discharge.

It appears probable from experiments of Dr. Schulze-Berge (*Nature*, March 4th, 1886, p. 432) that the resistance of certain dielectrics is not proportional to the thickness, but is much less for thin layers than might be expected. May this not possibly arise from the size of the heterogeneous portions being comparable with the thickness of the dielectric, so that the more easily conducting portions may stretch in some parts from plate to plate? If so, we approximate more nearly to the inferior limit.)

The mathematical account of the residual discharge on this hypothesis is practically the same as Maxwell's, but it may, perhaps, be worth while to give it with the necessary alterations, as these seem to make it somewhat more straightforward and evident.

We shall suppose with Maxwell, 'for the sake of simplicity, that the dielectric consists of a number of plane strata of different materials and of area unity,' and that the induction is in the direction of the normal to the strata.

Let a_1, a_2, etc. be the thicknesses of the different strata.

Let X_1, X_2, etc. be the electric intensity within each stratum.

Let p_1, p_2, etc. be the amount of decay of induction per second in each stratum.

Let f_1, f_2, etc. be the induction in each stratum.

Let u_1, u_2, etc. be the total number of tubes of electric induction entering each layer sideways, i.e., crossing in through its boundary, per second.

Let r_1, r_2, etc. be the specific resistance referred to unit of volume.

Let K_1, K_2, etc. be the specific inductive capacity.

Let k_1, k_2, etc. be the reciprocal of the specific inductive capacity.

Let E be the electromotive force due to a voltaic battery placed in the part of the circuit leading from the last stratum towards the first, which we shall suppose good conductors.

Let Q be the total number of induction-tubes which have left the battery and entered the wires and dielectric up to the time t.

Then since the same number of tubes enter all parts of the circuit in a given time,

$$u_1 = u_2 = u_3 = \ldots = u \text{ say.} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

These tubes tend to increase the induction in the layers. But at the same time decay is going on so that we have

$$u_1 = p_1 + \frac{df_1}{dt}, \qquad u_2 = p_2 + \frac{df_2}{dt}, \text{ etc.,} \quad \ldots\ldots\ldots\ldots\ldots(2)$$

whence
$$p_1 + \frac{df_1}{dt} = p_2 + \frac{df_2}{dt} = \text{etc.} \quad\text{.........................(3)}$$

We also have by Ohm's law
$$p_1 = \frac{X_1}{r_1}, \qquad p_2 = \frac{X_2}{r_2}, \text{ etc.,} \quad\text{.......................(4)}$$

and by the relation between induction and intensity
$$X_1 = 4\pi k_1 f_1, \quad\text{...............................(5)}$$

whence
$$u = \frac{X_1}{r_1} + \frac{1}{4\pi k_1}\frac{dX_1}{dt}. \quad\text{.............................(6)}$$

Let us suppose that at first there is no charge and that suddenly the E.M.F. E is made to act. Then if at once Q tubes enter the dielectric,
$$X_1 = 4\pi k_1 Q, \text{ etc.,} \quad\text{.............................(7)}$$

and since
$$E = a_1 X_1 + a_2 X_2 + \ldots, \quad\text{..........................(8)}$$
$$E = 4\pi (k_1 a_1 + k_2 a_2 + \ldots) Q.$$

The instantaneous capacity C which is equal to $\frac{Q}{E}$ is given by
$$C = \frac{1}{4\pi (k_1 a_1 + k_2 a_2 + \ldots)}. \quad\text{.......................(9)}$$

But dissipation at once sets in, and if the electromotive force E be continued uniform a steady state will ultimately be reached in which the dissipation in each layer is equal to the number of fresh tubes reaching that layer. The number of tubes entering being the same throughout, the dissipation p is also the same throughout.

We have then
$$p = \frac{X_1}{r_1} = \frac{X_2}{r_2} = \text{etc.,} \quad\text{..........................(10)}$$

and substituting in (8) $\quad E = (r_1 a_1 + r_2 a_2 + \ldots) p.$

Hence if
$$R = r_1 a_1 + \ldots,$$
$$p = \frac{E}{R}. \quad\text{....................................(11)}$$

In this state we have the induction given by
$$f_1 = \frac{X_1}{4\pi k_1} = \frac{r_1}{4\pi k_1} p = \frac{E r_1}{4\pi k_1 R}. \quad\text{....................(12)}$$

If we now suddenly connect the extreme strata by means of a conductor of small resistance, E will be suddenly changed from the value E_0 to zero and Q' tubes will pass out from each layer of the dielectric into the wire. If then X' be the new value of the intensity,
$$Q' = \frac{X_1}{4\pi k_1} - \frac{X_1'}{4\pi k_1},$$

whence
$$X_1' = X_1 - 4\pi k_1 Q'. \quad\text{..........................(13)}$$

Since then the difference of potential is zero

$$a_1 X_1' + a_2 X_2' + \ldots = 0,$$

substituting from (13) we get

$$a_1 X_1 + a_2 X_2 + \ldots = 4\pi (a_1 k_1 + a_2 k_2 + \ldots) Q'$$

or
$$Q' = \frac{a_1 X_1 + a_2 X_2 + \ldots}{4\pi (a_1 k_1 + a_2 k_2 + \ldots)} = CE = Q \quad \ldots\ldots\ldots\ldots\ldots(14)$$

from (8) and (9).

Hence the instantaneous discharge is equal to the instantaneous charge.

By (10) and (11) we may put (13) in the form

$$X_1' = pr_1 - 4\pi k_1 Q$$
$$= \frac{Er_1}{R} - 4\pi k_1 Q. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(15)$$

Let us next suppose the connection broken immediately after the discharge. No fresh tubes enter any layer, so that putting $u = 0$ we have from (6)

$$0 = \frac{X_1}{r_1} + \frac{1}{4\pi k_1}\frac{dX_1}{dt},$$

or
$$X_1 = X_1' e^{-\frac{4\pi k_1}{r_1}t},$$

where X_1 is now the value of the electric intensity at any time t after the connection is broken.

Substituting from (15) and putting E_0 for the initial value of E,

$$X_1 = E_0 \left(\frac{r_1}{R} - 4\pi k_1 C\right) e^{-\frac{4\pi k_1}{r_1}}. \quad \ldots\ldots\ldots\ldots\ldots\ldots(16)$$

The value of E at any time is

$$E = a_1 X_1 + a_2 X_2 + \ldots$$
$$= E_0 \left\{\left(\frac{a_1 r_1}{R} - 4\pi a_1 k_1 C\right) e^{-\frac{4\pi k_1}{r_1}t} + \left(\frac{a_2 r_2}{R} - 4\pi a_2 k_2 C\right) e^{-\frac{4\pi k_2}{r_2}t} + \ldots\right\}$$
$$= E_0 \left\{\left(\frac{r_1}{R} - 4\pi k_1 C\right) a_1 e^{-\frac{4\pi k_1}{r_1}t} + \left(\frac{r_2}{R} - 4\pi k_2 C\right) a_2 e^{-\frac{4\pi k_2}{r_2}t} + \ldots\right\}$$
$$= E_0 \left\{\left(\frac{r_1}{k_1} - 4\pi CR\right) \frac{a_1 k_1}{R} e^{-\frac{4\pi k_1}{r_1}t} + \ldots\right\}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(17)$$

The instantaneous discharge obtained at any time t will be, as before, CE.

If the terms be arranged in descending order of magnitude of $\frac{r_1}{k_1}$, then the exponentials are also in descending order of magnitude, or the negative terms decrease more rapidly than the positive, and E is positive.

13.

ON THE PROOF BY CAVENDISH'S METHOD THAT ELECTRICAL ACTION VARIES INVERSELY AS THE SQUARE OF THE DISTANCE.

[*British Association Report*, 1886, pp. 523–524.]

The proof of the law of electrical action depending on the fact that there is no electrification within a charged conductor was first given by Cavendish. His proof was made more general by Laplace, who has been followed by other writers, including Maxwell. Maxwell and MacAlister have also verified the experimental fact, repeating an investigation of Cavendish only recently published in Maxwell's edition of the Cavendish papers. The proof may be analysed in the following way: Take the case of a uniformly charged sphere. The action at a point within it may be considered as the resultant of the actions of the pairs of sections of the surface by all the elementary cones, with the point as vertex. If, then, the resultant action is zero for all points and for all sizes of the sphere, it follows that the action of the pair of sections by each elementary cone is zero; and, since the sections of the surfaces are directly as the squares of the distances, the two sections neutralising each other, the force per unit area must be inversely as the squares of the distances. There appear to be two objections to this proof. (1) That it takes no account of the always existing opposite charges. When the sphere, for instance, is positively charged, an equal and opposite negative charge is on the walls of the room, and the action of this should be considered. Probably this objection could be removed. (2) There is a solution still simpler than the inverse square law—viz., that no element of the surface has any action within the closed conductor. If we suppose that a conductor is a complete screen to electrical action, then, whatever the law of the force exerted across an insulator, there will be no action within the conductor. In any null proof it is not sufficient merely to show that there is no action in the null arrangement, but it is also necessary to show that on disturbing the null arrangement some action is manifested. Now, in the case here considered it is impossible to obtain any action within the conductor in any statical arrangement; it is only during changes of the system while charging or discharging that we can get a disturbance of the null arrangement. But here new phenomena come in, for we have currents, and therefore electromagnetic action. But, even disregarding the different kind of action occurring, the only experiment which I know of on this point was that of Faraday with his electrified cube. While the most violent charges and discharges were taking place on the outside of the cube, so that the null arrangement was probably disturbed, he found no action on his electroscope within. Possibly the actions were alternating, and so rapid that no electroscope of ordinary construction would reveal them. But he himself went into the cube, and he would probably be sensitive to rapidly alternating electromotive forces. It appears to me, then, that we cannot accept this proof, and must fall back upon the more direct proof of Coulomb*. I do not know whether Maxwell was aware of this objection; but it is worthy of note that in the remarkable fragment published since his death, as *An Elementary Treatise on Electricity*, he returned to Coulomb's proof, and was apparently building up the mathematical theory of electricity in a way quite different from that followed in his larger work.

* [In his lectures on electrostatics Poynting used to give an experimental proof somewhat differing from that of Coulomb, and simpler. A brief account of Poynting's apparatus will be found in *Electricity and Magnetism* by Poynting and Thomson, vol. 1, pp. 65 and 66. Ed.]

14.

ON A FORM OF SOLENOID-GALVANOMETER.

[*Birmingham Phil. Soc. Proc.* **6**, (1888), pp. 162 167.]

[*Read* May 10, 1888.]

The instrument described in this paper is a form of solenoid galvanometer in which the iron core is still far from saturation, so that the attraction of the core by the coil is nearly proportional to the square of the current. The peculiarity consists in an arrangement by which a pointer moves over a scale a distance not very far from proportional to the current.

The moveable core of the solenoid consists of an iron rod or bundle of wires, and is suspended by a silk fibre, which is wrapped on to the circumference of a small wheel with a horizontal axis turning in bearings as free from friction as possible. The wheel has an arm (Fig. 1) with a moveable bob on it, and the bob is so adjusted that the weight of the iron core just balances it when the arm is horizontal. The equilibrium is of course unstable, and a stop S is necessary just above the arm when in the horizontal position. The arm ends in a pointer moving over a divided quadrant.

The solenoid is placed above the iron core so as to act against its weight, and in the position of maximum pull. The coil is moveable up and down by means of a screw, so that it may always be put in this position of maximum pull. The current passing through the coil does not saturate the iron, and the upward attraction is therefore nearly proportional to the square of the current. Let it be equal to KC^2 where K is a constant for the particular instrument.

If W is the weight of the core, a the radius of the wheel, w the weight of the wheel and bob, and b the distance of its centre of gravity from the axis, the condition for equilibrium when no current passes is

$$Wa - wb. \quad \text{.................................(1)}$$

If now a current C passes, the down pull of the core is lessened and the arm falls into a position in which the bob has a less moment. If it moves through an angle θ,

$$(W - KC^2)\, a - wb \cos\theta. \quad \text{........................(2)}$$

Substituting from (1) for Wa,

$$KC^2a = wb\,(1 - \cos\theta) = 2wb\sin^2\frac{\theta}{2},$$

or
$$C = \sqrt{\frac{2wb}{Ka}}\sin\frac{\theta}{2}.$$

From this
$$d\theta = 2\sqrt{\frac{Ka}{2wb}}\,\frac{dC}{\cos\frac{\theta}{2}},$$

which only very gradually increases with θ, and when $\theta = 90°$ it has a value $\sqrt{2}$ or 1·41 times its value at 0°.

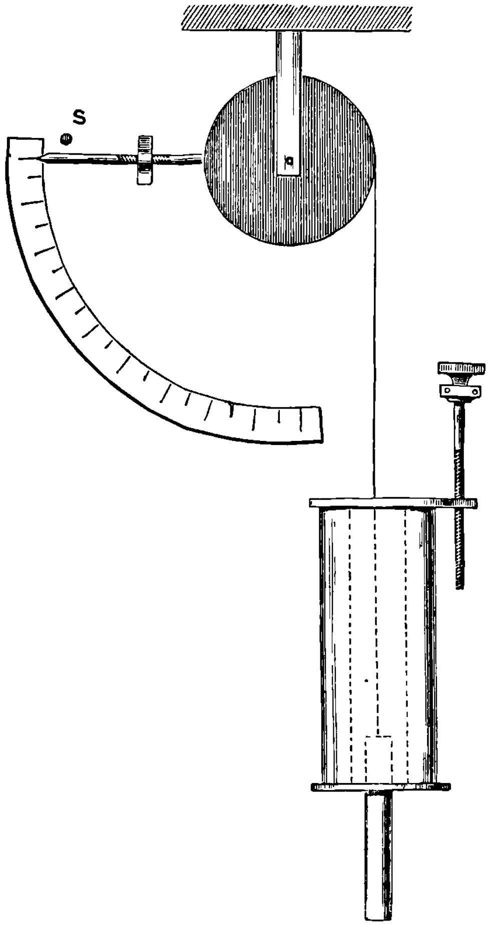


Fig. 1.

It is very easy to construct an arc divided to give readings proportional to $\sin\frac{\theta}{2}$ as follows :—Describe a quadrant, and mark off points on it with ordinates increasing by equal amounts. On the radius from which these ordinates are measured describe a semicircle. Drawing the radii of the quadrant to the successive points marked, they will intersect the semicircle in points with equally increasing values of $\sin\frac{\theta}{2}$, θ being the angle subtended at the centre of the semicircle.

In practice it would no doubt be better to graduate by trial, having a standard instrument in the circuit.

The instrument shown, though faulty in several points and far from frictionless, works fairly well. The range is limited by the fact that unless the adjustment is very perfect, the readings cannot be trusted below 10 or 20 , but I think the principle might be usefully adopted for voltmeters of small range, or for ammeters, to give a correct value for a current within a small range. It might be useful to extend the range by a counterpoise to part of the weight of the core, on the other side of the wheel. The instrument has the advantage that, when the current is passing, the pointer very rapidly comes to rest.

A Suggestion for a Wattmeter.

The above instrument has suggested to me a possible form of wattmeter which I have not seen described before. I have not yet constructed an instrument on this plan.

A soft iron core is fixed vertically at one end of a steelyard, with a moveable counterpoise as usual on the arm beyond the knife-edge. Two co-axial solenoids, one of high and the other of low resistance, are fixed in the position of maximum pull on the core when the arm is horizontal. For stability they should be above the core. The ends of the high-resistance-coil are connected to the two ends of the circuit in which the rate of working is to be measured, a commutator being interposed so as to reverse the current in the coil. The low-resistance-coil forms part of the main circuit. When the currents pass in the same way through the two coils, the pull on the core will be

$$(aC + bE)^2,$$

where a and b are constants for the solenoids. The counterpoise is to be adjusted for equilibrium. The current now being reversed in the high-resistance-coil, and the counterpoise being again adjusted, the pull on the core will be

$$(aC - bE)^2.$$

The distance through which the counterpoise has been moved will be proportional to the difference between these two pulls, or to

$$4abCE,$$

i.e., to the rate of working CE.

A Square-Root Steelyard.

Some years since another arrangement occurred to me for obtaining an equally divided scale, giving directly the square root of the pull on a soft iron core or on a moveable coil. After recently constructing a model, I found it was only a particular case of the very remarkable machine for solving equations, devised and constructed by Mr. Boys (*Philosophical Magazine*, vol. 21, 1886, p. 241). Being, however, a very special case, it is less complicated than the general instrument, and as the model works easily and correctly, it may be worth while to describe it.

ABC (Fig. 2) is a lever balancing on a knife-edge at B, and the pull W, of which the square root is to be measured, is applied at the end A. GE is

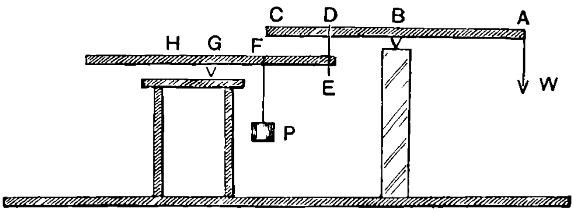


Fig. 2.

another lever balancing on a knife-edge at G, the arm GE being about equal to the arm BC. The plane upon which G rests is, in the model, a plate of glass, about equal in length to BC, and so arranged that GE may be moved until E is under any point of BC. E is connected by a link DE with BC, and from F, exactly under C, hangs a fixed weight P.

If the down pull of the link at D is T, and w is its weight, the up pull at E is $T - w$.

The equations of equilibrium of the two levers are

$$W \,.\, AB = T \,.\, BD \quad \text{.................................(1)}$$

and

$$(T - w)\, GE = P \,.\, GF,$$

or

$$\begin{aligned} T \,.\, GE &= P \,.\, GF + w \,.\, GE \\ &= P\left(GF + \frac{w}{P} GE\right) \\ &= P \,.\, HF \quad \text{................................(2)} \end{aligned}$$

if GH be made equal to $\frac{w}{P} GE$.

Multiplying (1) and (2) together, T is eliminated and

$$W \cdot AB \cdot GE = P \cdot BD \cdot HF.$$

Making HE equal to BC, and keeping P always exactly under C, BD is equal to HF, and

$$W \cdot AB \cdot GE = P \cdot BD^2;$$

$$\therefore\ BD = \sqrt{\frac{AB \cdot GE}{P}}\sqrt{W}.$$

If then BC is equally divided, the equilibrium position of D gives a reading proportional to the square root of W.

In the model a lever, not shown in the figure, fixes ABC, and at the same time lifts P up so as to release GE. GE and the link DE can then be moved along to a new position. On moving back the lever, ABC is released and P is dropped again into position on GE, exactly under C.

15.

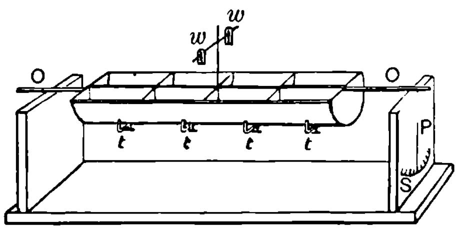
ON A MECHANICAL MODEL, ILLUSTRATING THE RESIDUAL CHARGE IN A DIELECTRIC.

[*Birmingham Phil. Soc. Proc.* **6**, 1888, pp. 314–317.]

[*Read* November 8, 1888.]

The model is designed to exhibit a phenomenon analogous to the residual charge which gathers in a condenser after it has been charged and then discharged, when the dielectric is not a perfect insulator. Its mode of action is similar to that which Maxwell supposes to occur in the dielectric. According to his theory the residual charge is due to the breaking down of the state of strain (or, perhaps, more correctly, of the stress) in the dielectric corresponding to the original charge, but in an uneven manner in different parts of the dielectric, so that just before the discharge the stress is greater in some parts than in others. On discharging, it is impossible, from the nature of electric discharge, to remove all the strain by connecting the two plates of the condenser, and the condition of equilibrium which is arrived at consists in an actual reversal of the strain in the parts where the breaking down has been most rapid, the reversed stress in these parts balancing the remnant of the original stress in the other parts. On insulation, the strain breaks down again, and at the greatest rate in the same layers, now reversed. Consequently the reversed stress is no longer able to balance the direct stress, and, on the whole, there is a preponderance of strain in the original direction, or a gathering of charge the same in kind as the original charge.

The model consists of a trough (see figure) of semicircular cross-section, 24 ins. long, 6 ins. diameter, and divided into eight equal compartments by a middle partition along the axis and three cross-partitions. It is supported at the two ends, so that it can rotate about its axis OO, a pointer P attached to one end moving in front of a scale S. Four pipes, with taps t, t, t, t, connect the opposite compartments when the taps are turned on. The trough is balanced by the weights w, w, so that when empty it is in neutral equilibrium. Turning the taps off, and pouring in water to the same depth in all the compartments, the equilibrium at once becomes stable, and the trough, if displaced, stores up energy. It may be considered as analogous to a 'tube of force,' connecting charges $\pm q$ on the surfaces of two opposite conductors, the axis of the trough representing the axis of the tube of force, the angle of displacement the charge at either end, or the induction along the tube. A clockwise rotation at the pointer-end might signify a positive charge at that end. As long as the taps are off, the trough represents a perfect insulator, a displacement through a given angle, and fixture at that angle, corresponding to the communication of a charge and subsequent insulation. The energy remains in the trough undissipated. Discharge, of course, corresponds to release of the trough, and we have oscillations corresponding to the electrical oscillations brought recently into such prominence. It may be noted that a decrease in the quantity of water corresponds to an increase in specific inductive capacity, while a decrease in the weight of the trough corresponds to an increase in magnetic permeability. We might, perhaps, obtain an analogy to the spark-discharge by completing the cylinder, of which the trough forms half, and carrying the partitions up through the added half. On turning the trough through anything more than a right angle it would fall over and oscillate about a new position 180° from the original one, the discharge of



energy occurring now with an increase of strain, not with a return to the unstrained condition. If the taps are turned on, but all to the same extent, the trough corresponds to a 'leaky' dielectric in which the conductivity is uniform. Turning the trough through a given angle and holding it, the water begins to flow back from the higher to the lower compartments, thus dissipating the energy, and if after a short time the trough is released it returns to a position short of the original position and remains there, the level of the water in the two sides of the middle partition being the same. But if the taps are turned on by different amounts—if, for example, the two end-taps are turned off while the two middle ones are turned on,—then on turning the trough through a given angle and holding it, the energy of the two middle pairs of compartments is gradually lessened, and on release the trough moves part way back. But now it is only the mean level which is the same on the two sides. In the two pairs of compartments with no communication there is still a positive difference of level, while in the other two there is now a negative difference. Holding the trough in its new position for a short time, the negative difference is reduced by leakage from one side to the other, and on release the trough returns by another amount towards its original position

—and this may be repeated several times, until finally the original position is sensibly regained.

The first model I made, for ease of construction and without sufficient consideration, with rectangular instead of circular cross-section; and with this the phenomenon of residual charge is obtained, even though all the taps are turned on equally. For consider what happens if the trough is turned through an angle and held. The water comes to a level after a time, but still its centre of gravity is not in the lowest possible position, and on release the trough returns part way, making a negative difference of level between the two sides. Again holding it, this difference is reduced, and on release there is another return, and so on. This suggests that possibly residual charge may occur not only when the substance is heterogeneous, but also when it is homogeneous, if with electric induction or strain there is both energy of the molecules as a whole and internal energy between the parts of each molecule. If the latter dies away after the bounding conductors are charged, the former may still remain, and on discharge it is possible that it may not all be dissipated, but may partly go to renew the internal energy. If this renewal accompanies a reversal of the direction of electric strain, we shall have the phenomena of residual charge.

It is hardly necessary to point out that the model serves equally as an illustration of a possible explanation of elastic after-action. It is evident that the phenomenon of residual charge will always occur when a body strained is such that the stress dies away unequally in different parts, while at the same time its constitution is such that on release from strain an equal amount of strain is taken from each part.

From this illustration of residual charge we may pass to a possible analogue of conduction in a metal wire. Let us suppose the trough replaced by a hollow cylinder, with its axis horizontal, ends closed, and without partitions. If the cylinder is only partly filled with water, a small couple applied to it will produce continuous rotation, but with a limiting angular velocity, attained when the water is dragged up in one side so far that the moment of its weight about the axis is equal to that of the applied couple. The energy put in by the couple is all ultimately converted into heat in the water. Thus the angular displacement increases indefinitely, though the stress always remains small. Similarly, as I believe, the 'electric strain,' or 'induction,' or 'displacement' in a wire carrying a current increases indefinitely, as induction is continually coming into it from the outside, although the stress always remains small.

16.

ELECTRICAL THEORY. LETTERS TO DR. LODGE.

[*Electrician*, **21**, 1888, pp. 829-831.]

TO THE EDITOR OF 'THE ELECTRICIAN.'

SIR:

I have prevailed on Prof. Poynting to let me send you the enclosed two letters, wherein he continues the discussion of electrical theory begun in Section A at Bath. It must be understood that the letters are merely hasty epistles, not intended for publicity; but Prof. Poynting's ideas are so original and weighty that one is glad to extract from him, when possible, a casual contribution to a discussion, as well as one of his sledge-hammer communications to the Royal Society. I hope that this may be the means of extracting a reply or a criticism more competent than anything of mine would be.

Yours, etc.

OLIVER J. LODGE.

DEAR LODGE:

I thank you very much for the copy of your exceedingly interesting account of Electrical A. Perhaps my gratitude would be best shown by silence, but I am tempted to show my appreciation by asking you to help me with some difficulties.

My first difficulty is as to the interpretation of Hertz. You say though, I think, FitzGerald is responsible for the statement—that ether is a demonstrated fact. I do not see how Hertz adds to our certainty. Is not our belief in ether due to the fact that light takes time to travel in interplanetary spaces, where we cannot put enough matter for it to travel by, so that we have to imagine something else for it to use. The fact that the velocity of light is nearly the same in vacuo and in gases, and not widely different in denser substances, of course supports the view that on the earth it also uses ether. Hertz shows that there is an 'interference' in electromagnetic disturbance which we can only (at least, with our present knowledge) put down to wave-motion travelling with a definite velocity which he finds equal to that of light. Hence these disturbances probably make use of the same ether. Does this prove its existence any more? I should expect a sceptic to ask why may not electromagnetic disturbance make use of air, since Hertz carried out his experiments in air. I could only reply to the sceptic that he was a very disagreeable person.

Secondly, Thomson's [Kelvin's] 'Simple Hypothesis' Paper* appears to be a very serious attack on Maxwell's theory; in fact, on reading it over carefully, I can only come to the conclusion that it would lop off not only Maxwell's excrescences but his whole theory. According to the concluding sentence of § 4, ('each component of electric current at any point is equal to the electric conductivity multiplied into the sum of the corresponding component of electrostatic force and the rate of decrease per unit of time of the corresponding component of velocity of liquid in our primary') the current

$$u = C\left(-\frac{d\Psi}{dx} - \frac{du_1}{dt}\right);$$

which $= 0$ if C the conductivity $= 0$, so that Maxwell's f (his 'displacement-current') goes altogether. The x component of Maxwell's E.M.F. will contain a term $-\frac{\mu K}{4\pi}\iiint \frac{d^2P}{dt^2}\,dx\,dy\,dz$, since Maxwell's $u =$ Thomson's $u + \frac{K}{4\pi}\frac{dP}{dt}$, where $P =$ E.M.F. along x. This has no representative in Thomson. Thus with a homogeneous but leaky condenser with no connecting wire, we have, according to Maxwell, total current $= 0$, for leak is made up for by yield of displacement;

$$\therefore\ P = -\frac{dV}{dx},$$

where V is potential due to electrification on the plates.

According to Thomson, u is to be taken as rate of leak, and is positive;

$$\therefore\ P = -\iiint \frac{\frac{du}{dt}}{r}\,dx\,dy\,dz - \frac{dV}{dx}.$$

According to Maxwell there is no magnetic effect, since total current $= 0$.

According to Thomson—using his notation—x component of magnetic force $u_2 = \frac{dw_1}{dy} - \frac{dv_1}{dz} = -4\pi\nabla^{-2}\left(\frac{dw}{dy} - \frac{dv}{dz}\right)$ from Thomson's equation (7), i.e., if ∇^{-2} and $\frac{d}{dy}$, etc., are transposable.

But $$u = KP,\quad v = KQ,\quad w = KR;$$

$$\therefore\ u_2 = -4\pi\nabla^{-2}\left(\frac{dR}{dy} - \frac{dQ}{dz}\right)$$

$$= -4\pi\nabla^{-2}\left\{\frac{d}{dy}\iiint\frac{\frac{dw}{dt}}{r}\,dx\,dy\,dz - \frac{d}{dz}\iiint\frac{\frac{dv}{dt}}{r}\,dx\,dy\,dz\right\},$$

* Reprinted in *The Electrician*, vol. 21, Sept. 14, 1888, p. 605.

the term in V disappearing. This is awful; but I see no reason to suppose that it vanishes. If it does not vanish, then the existence of magnetic effect would decide against Maxwell.

Thirdly, I note on p. 10 of your sketch that Rowland and FitzGerald consider that electrostatic potential is not propagated by end-thrust, and you remark that it is the magnetic potential which travels, generating the electrostatic potential as it goes along. I think I remember that you have expressed the view that potential energy must undergo a kind of conversion, and that unless it be born again as kinetic energy it can in no wise go forward on its journey*. With strained solid waves it looks as if it were so, though it is, I think, possible to regard both energies as going forward linked together, yet retaining their individuality. And if potential energy is, after all, kinetic, but of another kind, it is conceivable that they should keep their separate identities. But with electrostatic and electromagnetic strains, which is potential and which kinetic? I know it is usual to call the magnetic kinetic: but if we had started with permanent magnets, and travelled by means of magnetoelectric machines to our present knowledge of electric phenomena, I expect we should now be discussing the propagation of magnetostatic potential and magnetoelectric potential, and we should, perhaps, consider the former generated by the latter. This is really the view I take, or, rather, I think both are true. It seems to me that the sideway propagation of electric induction is accompanied by (let us drop 'generated by') magnetic induction, and equally the sideway propagation of magnetic induction is accompanied by electric induction. The two go together when a disturbance is propagated. In a steady state they do not, i.e., if we can separate them from each other; at least I do not see how otherwise to interpret the results I have obtained (see pp. 284–5 of paper referred to below)†. This brings us back to the old point whereon we have differed before. It would be better to give in like the unjust judge for the sake of peace and quietness, and to ward off any more such letters as this.

I have been going again through a paper 'On Connection between Electric Current and Electric and Magnetic Induction' (*Phil. Trans.* 1885)‡, in which I tried to work out the equations to the magnetic field on the supposition of this sideway propagation. Ψ ceases to be troublesome, and both electric and magnetic inductions are propagated at the same rate; indeed, they are by Maxwell's equations, though I do not think he ever definitely worked this out. I cannot see any point, in the assumption to begin with, or in the subsequent reasoning, where I have gone astray. If you have any time to

* *Phil. Mag.* October, 1879, p. 281, § 11; June, 1881, p. 534; and June, 1885, p. 486. I do not regard this as a 'view,' however, but as a proved truth.—O. J. L.

† [*Collected Papers*, pp. 201–2.] ‡ [*Collected Papers*, Art. 11.]

spare, would you look at the Paper, pp. 277–281, and 294–300? The rest is not essential, though on 301 I show that Maxwell, pure and simple, gives the same results*.

Yours, etc.,

J. H. Poynting.

Mason College, Birmingham,
October 12, 1888.

[The following extract from my note in answer may be inserted, in order to make the next letter clear:

'As regards the proof of the ether, I confess I did not quite see FitzGerald's point as to why Hertz's experiments rendered the existence of ether any more certain; but knowing that I had felt it thoroughly established long ago, I supposed I was not a good judge. Of course he must appreciate all the stock arguments about air, etc., not transmitting transverse disturbances, and about neither it nor glass transmitting anything at the speed 3×10^{10}, etc. And Hertz's experiments only seemed to me to *prove* that electromagnetic waves existed and travelled at the same speed, thus practically proving that light is electromagnetic waves, and establishing Maxwell's theory.

'This seems to me far more important than proving once more the existence of ether. At the same time I feel sure FitzGerald has some point. It may be only that an electromagnetic ether has been proved, and thus the action-at-a-distance-Germans confounded. He spoke as if he meant more than this. Perhaps I have to that extent misrepresented him in my "sketch." How does it strike you?

'With regard to Thomson's Paper, it is certainly very anti-Maxwellian. but I believe it only represents a transition stage through which he was somewhat rapidly passing, and through which he may now have almost passed. I certainly do not know now where he is.

'Is it not that, finding that displacement-currents have no magnetic effect, therefore he ignores them? But, then, have they no magnetic effect? In some cases they cannot have, for electrostatic displacement is of the nature of an "expansion," as Clifford called it; there is no "spin" about it.

'I cannot find Thomson's Paper this minute to refer to, but I have it somewhere. I will look it up again in the light of your remarks.' O. J. L.

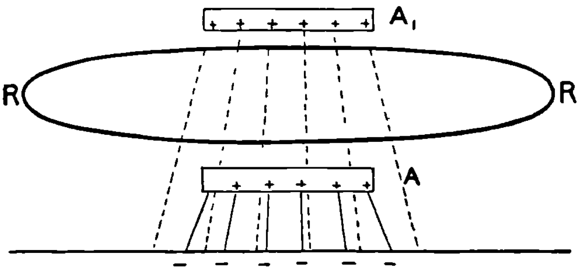
After referring to my reply, Prof. Poynting writes, in a second letter:]

About the ether, I should entirely accept your interpretation of Hertz. I should think, as you say, that FitzGerald was aiming at believers in action-at-a-distance, and probably he knew where to have them in showing that there is an electromagnetic medium.

You say, with regard to Thomson's Paper, 'Is it not that, finding that displacement-currents have no magnetic effect, therefore he ignores them? But, then, have they no magnetic effect? In some cases they cannot have.'

* [*Collected Papers*, pp. 194–198, 212–217, and 218 respectively.]

It is just here that I find the supposition of transverse propagation of electric strain accompanied by magnetic induction so clarifying to my ideas. If the magnetic effect is the whirling of the machinery which is sending electric strain energy onwards, the machinery cannot know whether the



energy is going to be dissipated in a Prony-brake-like wire, or whether it is going to increase the electric strain, so producing a 'displacement-current.' To use an illustration, I regard a re-entrant line of magnetic force as a kind of ring of Custom House officers registering the amount of electric strain sent in per second (they are porters as well), and they do not know what will happen to the energy. They will register just the same whether the imports are for immediate consumption or go to add to stock. Of course, they will take no account of shooting stars, balloons, or destruction of stock already within the ring; which, being interpreted, is that the line-integral of the magnetic force ($4\pi i$, isn't it?) will not be affected by lines of electric force other than those coming in or going out through the boundary. For instance, if A moves through the ring RR to A_1 (see figure) conveying a charge, hardly any lines of electric force will cut the ring RR, yet the number of lines through it is increased. And it will not be affected by induction which dissipates itself *in situ* as in a leaky Leyden jar, where the energy changes to heat without moving. I do not know of any other case, but, being utterly ignorant of pyro-electricity, I imagine it might supply a case of establishment of electric induction without motion of the energy and without magnetic effect.

Yours, etc.,

J. H. Poynting.

October 18, 1888.

17.

AN EXAMINATION OF PROF. LODGE'S ELECTRO-MAGNETIC HYPOTHESIS.

[*Electrician*, **31**, 1893, pp. 575–577, 606–608, 635–636.]

The leaders in Physical Science, impressed perhaps with the responsibility of their position, and fearing that their weaker followers will distort their views, are, as a rule, very cautious in giving us their vaguer speculations as distinguished from the more exact hypotheses which can at once be put into working shape. Yet these less-formed speculations are often helpful, even if they only arouse our minds to attempt to disprove them. Still more are they helpful if they aid us in thinking of facts in a more connected way until the finished working hypothesis is ready to take their place.

We owe a special debt of gratitude on this ground to Dr. Lodge for his well-known book on the *Modern Views of Electricity*, in which he describes not only the more definite beliefs which he firmly holds, but gives us also any suggestions rising in his mind which seem to give promise of light to guide us in the dark ways of the science. He talks as it were confidentially to us, and though the speculative character of the book makes it by no means the easy reading which the absence of mathematical treatment might lead us to expect, and even perhaps unfits it for beginners, the bold attempts at explanation give it great value to more advanced students. Such students will be brought face to face with many difficulties which they may hitherto not have recognised through haziness of thought. And even where they are unconvinced by Dr. Lodge's attempts to solve the difficulties they will be gainers by the orderly review of their knowledge necessary before they can form a judgment.

The book is built round a central hypothesis of the nature of electric action, which I propose to examine. I shall first give an account of the hypothesis as it appears to me to stand, freed from the details and the wealth of illustration, which, though appropriate, and even necessary, in the original work, make the argument at times rather difficult to follow. I shall then examine the evidence for or against the hypothesis.

We start with that which everyone accepts as the result of experiment and observation, that there are two kinds of electrification with oppositely-directed qualities, and that they make their appearance always in equal amounts. Hence, on their union, the net electrification is zero. Compare this with the case of momentum. According to our experience, as summed up in the third law of motion, we may regard the mutual stress between two bodies as consisting of a *transfer* of momentum from one to the other. A gun at rest is fired. The momentum gained by the bullet and powder may conveniently be regarded as derived from the gun, which, having none to begin with, now has an amount of negative momentum equal to the positive possessed by the charge. In other words, positive momentum is transferred from gun to charge. Or compare with a case of material transfer, as when A lends B a sum of money. Then A and B, after the transfer, are oppositely affected, so that if they both assign their share in the transaction to a third person C the effect on C is zero; or if B re-transfers the sum borrowed to A the net result is zero.

Such cases as these suggest that positive and negative electrification are merely the creditor and debtor sides of a single transaction, the sending out and the reception of something transferred (p. 9). What kind of transfer we must imagine is best gathered from the 'ice-pail' experiment, a particular case of the general principle that when induction occurs + and − always *face* each other in equal quantities with an insulating medium between. We may suppose that we have a nearly closed hollow insulated conductor, and that through an orifice we introduce a body having on it a charge $+Q$. Immediately $-Q$ gathers opposite to it on the inside surface, and $+Q$ is on the outside surface, facing $-Q$ on the walls of the room in which the experiment is made. If, instead of carrying out this electrical experiment, we imagine an indefinitely extended incompressible liquid, and think of merely mathematical surfaces occupying the positions of the surfaces of the conductors, the introduction of Q of liquid within the inner surface would force Q of liquid through each of the two surfaces, and relative to the space between the surfaces, inwards towards the inner surface, and outwards from the outer.

Now, suppose that such an incompressible liquid, to which for the present we need not ascribe gravitation, has an actual existence, that it fills all space with which we are concerned, and that it permeates matter. Let us suppose that in bodies which we term electrical conductors it is free to flow with nothing worse than frictional loss of energy, but that in insulators it has some kind of attachment to the matter, so that in the displacement of one relative to the other—that is, in the strain—energy is stored, and in such a way that it can be regained when the strain is relaxed. We shall call this liquid Electricity. When a displacement occurs, so that some of the liquid is pushed into or out of any conductor, the quantity flowing through the surface from or to the insulator is that which we have hitherto called the charge of electricity on the surface. We must now term it Electrification to distinguish it from the general body of the liquid, which is, according to Lodge, all Electricity. It is important to notice this distinction in studying Lodge's hypothesis. Electricity is the generic name for the fluid all through space, Electrification the specific name for that part of it which happens to have flowed in any given disturbance through the surface of a conductor. Thus, if we imagine a conducting sphere A within a hollow conducting sphere B, B having an orifice through which we can introduce a wire to charge the inner sphere, say, positively, the gathering of a charge Q on A is to be regarded as a flowing of some of the all-extensive electricity along the wire into the sphere, which is, however, already full of electricity. Hence Q must be pushed through the surface of the sphere out into the insulator or dielectric. This outward displacement manifests itself as the + electrification of A. As the dielectric is also initially full of electricity, Q must be pushed out through every surface in it completely enclosing A. The displacement relative to the air-particles stores energy—the energy of the charge. When we come to the inner surface of B, Q is pushed into the substance of B, an inward displacement which we term a negative charge. It is also pushed through the substance of B, but as this substance is conducting, no energy is stored, and only a little is dissipated by the frictional rub, or, perhaps better, the viscous cling. At the outer surface of B there is another displacement of Q outwards into the dielectric—i.e., another positive charge, and energy-storing begins again. The pushing out will take place through the second dielectric till we come to the walls of the room in which the action is occurring. Here it will probably end, for the generator of the original charge has probably sucked in the fluid from the walls. There is therefore a confined circulation, and not an infinitely extended pushing-out.

Sources of electrification with their connecting wires are evidently to be regarded as turbines working in pipes or channels laid in space, incompressible and fluid electricity filling both the pipes and the space outside them. When the turbines work, the fluid runs along the pipes, forming what we call an electric current. When a pipe ends in a reservoir bounded by a dielectric, the fluid presses out into the dielectric, and there stores the energy put into it by the turbine, *minus* that dissipated by the viscous resistance in the pipes and conducting channels.

Prof. Lodge illustrates the connection between the electric incompressible fluid (why not the electric liquid?) and the molecules of matter by a series of ingenious and suggestive models either with cord running through beads or with liquid to represent the electricity. It is hardly necessary here to describe these. Any reader who is not yet acquainted with them should study the original account. It is enough to say that Prof. Lodge can make his models behave like Leyden jars and give the phenomena of charge, residual charge, and oscillating discharge as perfectly as if they were thorough believers in his hypothesis.

I find it somewhat easier to form a picture of the hypothesis by supposing matter to have a sponge-like constitution, i.e., to be permeated in every direction by passages, the pores so dear at one time to the writers on elementary science. These passages are filled with liquid electricity. In every passage or tube, however short, we must imagine a little turbine turning round with the flow through it and never letting any fluid pass without duly turning. In dielectrics there is a spring, like the mainspring of a watch, attached to the spindle of each turbine so that when the wheel turns energy is stored. In conductors this spring is wanting, and there is only a viscous resistance to rotation. We may think of the source of electrification, machine battery or induction coil, as a large turbine somewhere in the system with a supply of energy behind it dealt out by a motor of some kind or other. When the large turbine works, a flow takes place in the system dissipating energy in the conductors and storing it in the dielectrics. When the charging turbine is removed or disconnected from the motor, so that the way is clear for a return, all the wound-up mainsprings return and drive the liquid back through the sponge. We have only to make the turbines with different moments of inertia, with different qualities of lubricator in the conductors, and with different strengths of spring, and different firmness of attachment in the dielectrics, to get varying permeability, electric resistance, specific inductive capacity, and residual charge. We may. perhaps, simplify the arrangement of affairs by supposing that the molecules themselves are the turbines, and then we get a kind of inversion of the hypothesis described hereafter, which Dr. Lodge develops to account for electromagnetism.

In the displacement-hypothesis, with a single electric liquid flowing past matter, there is a very serious difficulty. The energy being stored by the flow past the molecules of matter, we might reasonably expect the electricity to pull, or to tend to pull, the molecules with it, and there should, therefore, be a motion of matter along the lines of force in one direction. But instead of this we have a tension, both ways as it were, along the lines of force or flow. There is displacement of matter only in the case of electrolytes, and here it is both ways along the lines of flow, one set of atoms going one way and another set the opposite way. A modification of the hypothesis is suggested by Dr. Lodge to meet this difficulty and to account for the double electrolytic procession. He supposes that there are two constituents of the electric fluid, each in general filling half any space, intimately mixed and evenly distributed. The molecules of matter are made up each of two constituents in accordance with the usual view, and one of these is attached to one kind of electric fluid the other to the other. When electric displacement occurs it is really a double flow, the two constituents of the fluid travelling equal distances in opposite directions past each other. The atomic constituents of each molecule move in opposite directions, but not in general very far, so that there is in dielectrics no displacement of the matter as a whole. In electrolytes the displacement continues till separation and re-pairing occur, and thus we get a double procession.

We see that in dielectrics, the pulls of the two constituents of electricity in the molecules, one on one atom, the other on the other, balance each other. These electric pulls lead to internal stresses within the molecule about which Dr. Lodge does not say anything very definite, but it seems to me that we have to introduce chemical forces here to account for the pull of the atoms on each other, distinct from the electric forces or the pulls of electricity on matter. This dualism is hardly in accordance with the late exposition of Dr. Lodge's ideas, where he appears to identify electrical and chemical forces (p. 84). Perhaps we might as well, while we are inventing a constitution for the ether, make a third or neutral constituent to which all the atoms of matter are attached. We will suppose this neutral electricity to resist extension and compression. We then have electro-positive atoms attached to positive electricity, electro-negative atoms to negative electricity, and all of them to neutral electricity. When an electric displacement occurs, positive ether tugs at one set of atoms, negative ether at the other set, and neutral ether prevents their separation, so that all our forces are of one kind, insomuch as they are forces between atoms and ether. I rather like this neutral ether, but I am afraid Dr. Lodge will not adopt a strange infant into a family already sufficiently large.

But taking the hypothesis as set forth by its author, the mere duality does not much affect the general notion of the nature of electric charge. We must remember that motion of negative fluid inwards equally with that of positive charge outwards gives a positive electrification, so that the explanation of the electrification of a sphere within a conductor already given has only to be amplified by supposing that there is another ethereal fluid displaced in the opposite direction at the same time and throughout the system.

I can imagine the agnostic in ethereal matters protesting here against the multiplication of unknowns and unknowables. I can imagine him saying that his sense-organs are only excited by material motions and affections, that his instruments are all material, and only appear to undergo changes of shape, colour, sound—i.e., affections of matter, and that he cannot with any certainty get beyond these material affections. He will argue that, as we have no sense affected by ether alone, we can form no adequate conception of the ether; we can only suppose it endowed with material properties, and conceive of it as some form of matter. And it would appear possible to imagine various material ethereal constitutions or connecting machineries between the different portions of matter evident to our senses, all equally accounting for all known facts. When a new fact turned up he would own that probably some of the ethereal machinery would fail to account for it, and so would have to be taken off to the lumber-room for worn-out hypotheses;

but the new fact, he would argue, would very likely enable us to imagine new types of machinery to replace some, at least, of the old rejected ones. And probably, till the whole range of physical phenomena was known, it would always be possible to imagine more than one kind of machinery to account for the phenomena known. Probably only when nothing remained to be discovered would there be a single solution, and only then would it be possible to give a single answer to the question, What is ether? And even then we might be wrong, for the ether might have properties in its action on matter quite different from any of which we have material types.

Though our agnostic, when following this train of thought, may be unconvinced by Dr. Lodge's preface, and may urge that the ether is and will probably remain a hypothetical medium, he will, no doubt, adopt some form of hypothesis for working purposes. If he is an ordinary human being, when he studies such actions as we term actions-at-a-distance, he will prefer to think of the different parts of the acting matter as connected by something continuous, with material properties. Following Boscovich and Faraday, he may extend the atoms throughout space, and give this extension material properties (a special case of this type of hypothesis is presented to us in the ring-vortex theory of the Universe); or he may limit the atoms and put in some new connecting machinery to fill up the vacuum he abhors, and this he may as well call 'ether.' While, therefore, he may protest against Dr. Lodge's 'cocksureness' about any particular constitution for the ether, he is bound to examine any hypothesis reasonably presented to see if it is likely to form a good working hypothesis to account provisionally for the observed facts. He would, no doubt, admit that Dr. Lodge's hypothesis is reasonably presented, and it would only be a question with him whether so complicated a constitution for the ether enables him to think sufficiently easily of the phenomena for which it is to account. Leaving him to consider this, we may pass on to the further development of the hypothesis.

So far, we have only been thinking of the properties of electricity at rest. It is true that we have thought of the electricity as being pumped along conductors and as wasting energy in the passage, but this was only a step onwards to a final statical distribution. We are now to concentrate our attention on the pumping stage.

When electricity is in motion in sufficient quantity and for sufficient time, a new set of phenomena come into prominence. Among these are the heating of the conductor, the heating or cooling of junctions, the opposite ionic processions in electrolytes, and the creation of a magnetic field. The heating of the conductor is to be explained, according to Dr. Lodge, as something analogous to frictional, or rather viscous, dissipation of energy. We may think of the two streams of electricity flowing past the atoms of matter, and continually catching hold of them and letting them go again, as a fiddle-bow catches hold of and lets go a fiddle-string. Thus, some of the energy of flow is converted into vibrational energy of the atoms, that is into heat. The junctional heat-phenomena still wait for complete explanation. We may consider that the facts imply that at a junction of dissimilar metals there is a tendency for positive electricity to move more easily in one direction than the other, and, of course, the reverse with negative electricity. A compound bar with free ends will thus tend to be positively electrified at one end and negatively at the other. Suppose, further, that the tendency to separation of electricities varies with the temperature, and we have at once the thermo-electric current in a closed circuit. Returning to the fiddle-bow and string used to illustrate the development of heat by conduction, we can see how it ought to work to illustrate thermo-electricity. Suppose a set of parallel strings in a horizontal plane, one half tuned to one note and the other half to another. The one set may represent one metal with its atoms vibrating in given modes, and the other set another metal in contact with it and with its atoms vibrating in other given modes. Now, laying the fiddle-bow across the strings after they are set in vibration, if the motion of the bow is always in one direction it illustrates the pushing of one of the electricities from one metal to the other. I have found that when light bits of paper are laid across two vibrating strings of different pitch there is frequently a movement of translation. Unfortunately for the illustration it is sometimes in one direction, sometimes in the other. The paper was not part of Prof. Lodge's book, and knew nothing of the hypothesis it was expected to support. The opposite procession of ions we may think of as the transport of the atoms by the positive and negative electric streams respectively, the connections between the pairs of atoms being broken down and renewed with fresh partners all along the line and continually.

The most evident phenomenon characterising the electric current is the magnetic field around it. This we may regard as manifesting the existence of so much magnetic energy in the neighbourhood of the conducting wire. Let us see how the hypothesis will account for this energy. The first step is to reduce permanent magnetism and current-magnetism to one species by adopting Ampère's theory. If we consider a small closed current-bearing circuit, observation tells us that, at a distance from the circuit, the field is indistinguishable from that due to a small steel magnet, with centre in the plane of the circuit and axis perpendicular to it. Starting from this, we know that we can deduce an arrangement of permanent magnets, equivalent, as regards the outside field, to any current-bearing circuit. The circuit differs magnetically from the steel in two respects only, viz., that it requires a continual supply of energy to maintain it, and that we can get into its inside. This last difference may be merely due to the large size of any apparatus at our command, and it is quite thinkable that, if we could make ourselves or our apparatus smaller than molecules, we could explore the inside of the steel molecules. The other difference is possibly not one of the kind; for suppose the resistance to be diminished till it disappears, the rate of energy-supply, C^2R, disappears also, and we have a current-circuit—never mind how the current was started—which is as permanent a magnet as a steel bar. It is a short and inevitable step from this to Ampère's hypothesis that a magnetic molecule—a molecule of steel, say—*is* essentially a small closed perfectly-conducting circuit with a current of electricity in it; or, in terms of Dr. Lodge's hypothesis, either two equal and opposite currents whirling round at equal speeds in opposite directions in each molecule, or a positive whirl in one direction in one molecule, accompanied by a negative whirl in the other direction in the next molecule. We may dismiss this duality for the present, on condition that it comes up for sentence when called upon, and return to the single circuit. Such a circuit, when placed in a magnetic field, behaves, doubtless, like finite circuits, and tends to set itself perpendicular to the lines of force, and with its own lines parallel to the lines of the field, and in the same direction through the circuit. It tends to move from weaker to stronger parts of the field—tends, in fact, to include as many positive and exclude as many negative lines as possible.

But the disappearance of resistance has a peculiar effect. Even a circuit of the resisting kind, with which alone we have practical acquaintance, would protest against the inclusion of foreign lines of force in addition to its own, and the current would diminish while they were being included. An Amperean circuit would not merely protest, but would absolutely prevent any change in the total number included, for any increase would be accompanied by a finite negative E.M.F. proportional to the rate of increase, and therefore by an infinite current, since R is zero. This is not to be accepted as possible, so that all that can happen is that some of the current's own lines of force shall be replaced by those of the field, and there is a consequent weakening of the current.

When a mass of iron consisting, we suppose, of such Amperean currents is brought into a magnetic field, all the circuits tend to turn round to include the lines of the field, and at the same time the Amperean currents decrease in strength. The circuits thread themselves like beads on to the lines of force of the external field, their currents falling as they thread on. But their own surviving lines of force are added to those of the field, so that the total field is greater, and the iron has greater permeability than a vacuum; or perhaps it will be better here to say that the iron *conducts* the lines of force better than a vacuum would, for permeability has an exact significance, not quite describing the property now under discussion. As new lines of force are added to the field the iron will conduct them better than a vacuum, until every molecule has all its lines brought into service, and its current, therefore, reduced to zero. After this point is reached, since the molecules either will not take any more lines of force, or if they take any more will establish negative currents to neutralise them, they are worse conductors than a vacuum, for they fill up part of the space uselessly or injuriously. They will therefore tend to move from stronger to weaker parts of the field, as if diamagnetic. Some indication of such a change in conducting power has been detected by Ewing in the magnetisation of iron in exceedingly strong fields, for though the permeability or *induction produced ÷ the field producing it*—both reckoned from zero—was always greater than 1, the value of *increase in induction ÷ increase in field producing it*, or the conducting power, as I have called it, fell ultimately below 1.

For some reason, not yet explicable, the magnetic chains in iron become unstable, and break up when the temperature is raised to the neighbourhood of 800 C. The iron then above this temperature is practically equivalent magnetically to any other substance.

Assuming then that we have some notion of what we mean by currents of electricity whirling in channels of no resistance, Ampère's hypothesis gives us a fair explanation of iron and steel magnetism. If we accept the view that the interior of a magnet does not differ from the exterior in kind but only in degree and in permanence, we may attempt to extend the hypothesis to explain the magnetic qualities of all other substances, i.e., their power of carrying the lines of force, and of carrying them in slightly different degrees.

Let us think of the lines of force in air circling round a current or passing from pole to pole of a magnet. We may think of these as passing through a number of electric whirls, or at any rate through perfectly conducting rings ready to exist as whirls. Before the passage of the lines of force these rings are turned in all directions. After the passage they tend to set perpendicular to the lines of force. If the medium is paramagnetic we may suppose the rings to have initial currents in them; if it is diamagnetic we may with Weber suppose that they have no currents initially, and when no lines of force pass through. On the establishment of the field negative currents are excited of such value as to make negative lines of force thread each ring equal in number to the positive lines sent through by the field. Thus each ring acts as a part of the field through which no lines of force pass, and the permeability is thereby diminished. At the same time the diamagnetic substance will tend to weaker parts of the field, and we have the main facts of diamagnetism explained. There is a serious difficulty in the nearly constant permeability, differing only by a very small amount for a diamagnetic solid like bismuth and a magnetic gas like oxygen, the one with its molecules crowded together, the other with its molecules comparatively wide apart. Perhaps we can strengthen this weak point by supposing the conducting rings of very different diameters in the two substances, or we may think of the electric channels as different altogether from the molecules and the same in number per c.c. in bismuth and in oxygen. Another difficulty lies in the non-existence of permanent magnetism except in iron, nickel, and perhaps cobalt, but it is no greater than the difficulty with iron above 800 C. Perhaps both will ultimately find the same explanation, and until this is forthcoming we need only say that Ampère's hypothesis is merely silent on the point and is not necessarily unable to explain it.

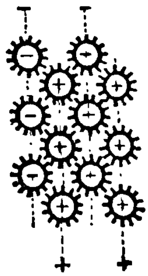
Now as to magnetic energy. Dr. Lodge points out a number of facts which suggest that magnetic disturbance is of the nature of spin round the lines of force. The Amperean circuits at once present themselves as being the seat of this spinning, and the electric fluid or fluids in the channels as the spinning material. These whirlings of electricity, either in themselves or in the accompanying motion of the entangled matter (Maxwell, by the way, thought it was the matter), possess, according to Dr. Lodge, the magnetic energy of the system. In fact, they are themselves magnetism.

So far we have been dealing with magnetism and its relation to electricity, the space-filling fluid, and we have come to the conclusion that magnetism consists of vortices in this fluid. It remains to explain the nature of the ordinary electric current, and the way in which its accompanying magnetic field is maintained. Dr. Lodge uses for the purpose a mechanical analogue or mechanical model, a modification of Maxwell's well-known model, which is described in his *Scientific Papers*, vol. 1, page 451, and I believe by Dr. Garnett in *Maxwell's Life.*

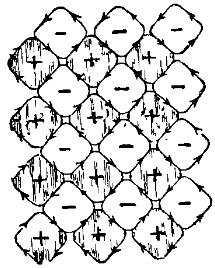
Let us imagine the two fluids, which are to be regarded as jointly filling space, to have a cellular construction, each consisting of spheres or little india-rubber bags, or what you like, in contact with each other. In dielectrics we think of contiguous cells as gearing in some way. Let these cells, when in a magnetic field, be spinning round the lines of force, the positive in one direction, the negative in the opposite; and let positive and negative be alternated so that the opposite motions may be possible without slip of gearing.

Fig. 37 from Dr. Lodge's book illustrates this idea when the lines of force pass into the paper from above, the axis of spin, therefore, being perpendicular to the paper. We may further materialise the conception by inserting teeth round the edges of the cells; and here the family likeness to the parent (Maxwell's model) becomes stronger. We then have Fig. 36. If we suppose Fig. 37 to represent the unstrained state in a dielectric, then electrostatic strain will be presented by some such deformation as that represented in Fig. A; not, I think, in the manner represented by Fig. 46 in Dr. Lodge's book.

If we take Fig. 36 as our type, it is easier to think of the wheels as being attached to some kind of framework. We may think of all the positive wheels in Fig. 36 as arranged flat against one series of parallel rods going down the page, and the negative wheels on another intermediate and parallel series. We may think of the wheels, or their teeth, as not quite rigid, so that when the positive rods are pulled down and the negative up, there is a slight



Dr. Lodge's Fig. 36. Rows of cells alternately positive and negative, geared together, and free to turn about fixed axes.



Dr. Lodge's Fig. 37. Section of a magnetic field, perpendicular to the lines of force; alternate cells rotating oppositely. (Another mode of drawing Fig. 36.)

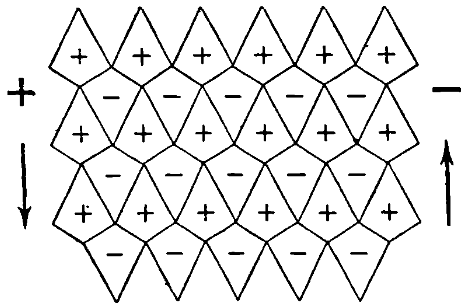
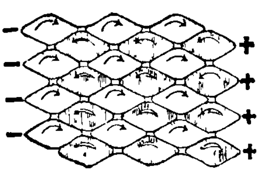


Fig. A. Electrostatic strain. The cells displaced slightly in opposite directions.



Dr. Lodge's Fig. 46.

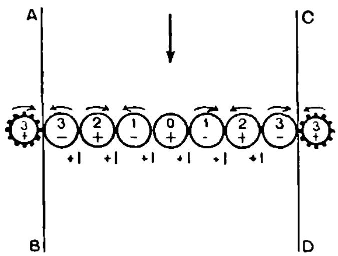


Fig. B. Cells slipping in a wire carrying a uniformly distributed current.

relative displacement and energy is stored, and this will represent an electrostatic strain. Probably, if we arranged all the forces properly, we should be able to do without the framework of rods. Meanwhile I find I cannot think of the model clearly without it.

The distinction between a dielectric and a conductor is to be represented by the abolition of the teeth on the wheels in the conductor. The surfaces of the wheels are in contact, and are only more or less rough—viscously rough, rather than frictionally rough. It appears rather difficult to think of the layer of wheels separating a dielectric and a conductor, for on the dielectric-side they must have toothed gearing, and on the conductor-side they must only rub; but we may get over the difficulty by supposing these wheels double, one toothed to gear on the dielectric-side, and the other untoothed to rub on the conducting side, and both keyed on the same axis.

Dr. Lodge regards a current as represented by a slip of one row of wheels against the next. He says (p. 206), 'Notice that in a medium so constituted and magnetised—that is, with all the wheel-work revolving properly there is nothing of the nature of an electric current proceeding in any direction whatever. For, at every point of contact of two wheels, the positive and negative electricities are going at the same rate in the same direction; and this is no current at all....A current is nevertheless easily able to be represented by mechanism such as that of Fig. 36 or 37; for it only needs the wheels to gear imperfectly and to work with slip. At any such slipping-place the positive is going faster than the negative, or *vice versa*, and so there is current there. A line of slip among the wheels corresponds therefore to a linear current....Understand: one is not here thinking of a current as analogous to a *locomotion* of the wheels—their axes may be quite stationary. The slip contemplated is that of one *rim* on another.'

Thus in Fig. B, let the row of wheels represent the cells across the diameter of a wire carrying a steady current, *ABCD* representing the section of the wire. Let the speeds of rotation of the wheels be as marked on each. The resultant positive slip is $+ 1$ at each surface of contact, a total of 6.

It is perhaps presumptuous to quarrel with a parent such as Dr. Lodge as to his mode of developing the faculties of his own offspring, but I must venture here to differ from him entirely in the way in which he seeks to make his model represent current. It appears to me that he has grafted on to his own model the representation of current in the entirely different model of FitzGerald, and so obtains something quite inconsistent with his previous ideas. It is only by stopping short at the centres of the bounding wheels that he can obtain a resultant flow in one direction. Obviously, if he took in a whole number of wheels, each entire, he could get no resultant flow, for the flow on the opposite sides of each wheel is equal and opposite. Thus, in the figure, if he took into account the outer sides of the two last wheels in the wire, he would have $- 6$ neutralising the previously obtained $+ 6$. Or, to put it in another way, if we draw a plane, say, above the line of centres, a tangent to all the wheels, there is evidently no resultant flow across that plane. And we can think of cases of slip when we have no reason to suppose there is current. Thus, if a bar of soft iron is placed axially in a magnetic field, near the centre of the bar the lines of force are parallel to its length within and without the bar. According to Lodge, the electric whirls are very rapid within the iron, and comparatively slow in the air outside. There must, therefore, be slip at the boundary, and yet we have no dissipation of energy to indicate the existence of the current.

Another objection is that the model worked thus would make a difference in kind between the process of displacing in a dielectric, the equal and opposite motions of the two fluids which are leading to an electrostatic strain, and the current in a conductor. This is rather setting back the clock. Perhaps we have gone on too fast, and the difference *may* exist, but I think we should hardly accept the evidence of the model on the point.

What Prof. Lodge calls current appears to me then to be merely sudden change of magnetic intensity. If this is just criticism let us see if the model can be made to represent a true current. I shall take the case of a steady current in which the condition of affairs is not altering. It is always better to begin with such a case, just as it is better to begin with statics, hydrostatics, electrostatics, than with dynamics, hydrodynamics, and electrodynamics. When matters, or ethers, get into changing motion they are, like the celebrated pig of the Irishman, difficult to count.

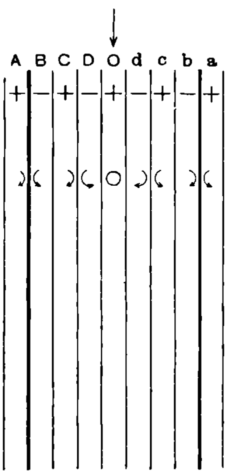


Fig. C. Rods on which the positive and negative wheels are supposed to be fixed in the model of a wire carrying a current.

In the model we shall suppose that the current in the wire is represented by two equal and opposite processions of cells or wheels along the wire, and to help us in thinking of these processions we shall, as before, suppose two sets of rods parallel to the axis of the wire, the positive wheels on one set with their spindles perpendicular to the rods, and the negative wheels on the other set.

In Fig. C the rods alone are represented, directions of motion of the wheels on each being shown by arrows. The + rods are to be regarded as moving down the page, and the − rods up. The wheels on the bounding rods, B, b, gear with the wheels on A, a in the dielectric, but slip on those on C, c in the conductor. Now, if the conductor extended through all space I do not see that the motions of the rods would involve rotation of the wheels. For any rod would be as it were surrounded by a symmetrical system, its wheels would not know which way to turn, and so would merely rub against their neighbours. But here the conductor is bounded by an insulating medium in which the rods can only move a very little way, and that only by straining the teeth of the wheels.

The result is that as B moves up past A its wheels behave as pinions moving on a rack in the place of A. They tend to turn the wheels of C in the opposite direction. These in turn tend to send round the wheels of D, and so on to O, the middle rod. Now, starting from the other side, a is at rest, and as b moves past it its wheels must rotate. This rotation is transmitted inwards, as before, to O. The result is that the wheels on O are urged in opposite directions from the two sides, and so remain at rest, while those of D and d rub against them. Those of C and c rotate faster than those of D and d, the friction due to the slip balancing that due to the slip of D and d on O, and so on, the rotation increasing outwards. When we come to B, b, their wheels have the frictional resistance against the C, c wheels on the inside, which must be balanced—since the motion is steady by pressure on the outside against the A, a wheels. Hence the B, b wheels are not merely rolling on the A, a wheels, but are pressing against them, and so turning them round. In other words, the rotation extends out into the dielectric. Thus, at the middle of the wire, there is no rotation no magnetic intensity. It increases as we go outwards to the boundary, being in opposite directions on the two sides, and it exists in the surrounding medium. The rubbing of the surfaces, perhaps of the rods, perhaps of the wheels, dissipates energy which represents the heat appearing in the wire.

The model is probably indeterminate as to the way in which the energy finds its way into the various parts of the conductor, where it appears as heat. We might imagine the rods moved by end-thrusts along the wire, their wheels setting the outside wheels spinning, or we might imagine a spin propagated along the outside wheels to the wire, and there setting the rods in longitudinal motion.

I have no doubt that the model would be able to represent the phenomena of the current-induction when the motion is àccelerating or unsteady, for Maxwell's model does this, and the differences of construction would hardly affect the point.

I have now given an account of the main features of Dr. Lodge's hypothesis as to the nature of electric charge, magnetism, and current, and I have done this at considerable length because I propose to discuss the foundations for the hypothesis, and to see if they are sufficiently firm to bear the superstructure. To recapitulate the main features, we have a double ether consisting of equal + and − portions attached in some sort of way to matter as a framework elastically in dielectrics—loosely, with a kind of viscous connection in conductors. The two ethers always move in opposite directions along the lines of electric force, and in dielectrics they store energy by their displacement past the material framework, while in conductors they only dissipate it during the displacement. The ethers are cellular in construction, and the cells are capable of rotation, this rotation constituting magnetism, and the accompanying rotational energy being magnetic energy. The axes of rotation are along the lines of magnetic force. The cells roll on one another as if quite rough in dielectrics, and as if lubricated with viscous oil in conductors, and they are alternately + and −, and moving in opposite directions, a clockwise moving + cell giving the same direction to the magnetic force as a counter-clockwise − cell. Current is, according to Lodge, slip of these cells against each other, but according to the interpretation I have given of his theory, current is a double and continuous procession of the cells in a conductor, with the consequent rotations.

The hypothesis is avowed by its author to be an attempt to obtain a mechanical explanation of electric and magnetic phenomena. He uses the main idea of Maxwell's well-known model, but replaces Maxwell's duality of magnetic wheels and electric 'idle' wheels by a duality of electric wheels. It is, perhaps, open to question whether this is really a simplification, but the attempt was very well worth making, for it is only by variation and natural selection that the mechanical model will be suited to its environment in the electric world.

It is, I suppose, useless to look for any other than a mechanical hypothesis as final. Probably because we are able to picture mechanical processes, able to think of ourselves as seeing what goes on, seeing kinetic energy manifested in the moving parts, able to think of ourselves as part of the connecting machinery, feeling the stresses, and helping to make the strains, we have come to regard mechanical explanations as the inevitable and ultimate ones.

Thus, though the old mechanical hypothesis of light is for the present discarded in favour of an electromagnetic one, I suppose no one is content with the present position; but we are all looking forward to the time when, by mechanical explanation of electromagnetism, light shall once more become mechanical. Nevertheless, it is well to bear in mind that all such explanations are merely hypothetical, and may at any time have to be discarded, as the solid-ether light-hypothesis has been discarded. Indeed, they are solely of value as a scaffolding enabling us to build up a permanent structure of facts, i.e., of phenomena affecting our senses. And inasmuch as we may at any time have to replace the old scaffolding by new, more suitable for new parts of the building, it is a mistake to make the scaffolding too solid, and to regard it as permanent and of equal value with the building itself. It is on this point that I find most cause for disagreement with Prof. Lodge. He appears to me to regard his hypothesis as inevitable and permanent, or at least as approximating to the permanent and inevitable. The scaffolding, in fact, is made as important as the building. It behoves us, therefore, to examine into the security of its foundations.

We may take the hypothesis as based on two statements:

I. Regarding electricity at rest, 'Whenever we perceive that a thing is produced in precisely equal and opposite amounts, so that what one body gains another loses, it is convenient and most simple to consider the thing not as generated in the one body and destroyed in the other, but as simply *transferred*' (p. 9).

II. Regarding electricity in motion: 'Magnetism is nothing more nor less than a whirl of electricity' (p. 171).

Let us examine some cases which should come under the first statement.

We may regard the upper and under surfaces of a vessel of liquid as respectively positive and negative, and if we measure them by their projections on a horizontal plane they are equal and opposite element by element. We clearly cannot create a positive without an equal negative. Bring the two elements together, and they neutralise each other, for both cease to exist. We have no idea here of something transferred.

Or, take the case of a rotating cord. Its two ends, as viewed from the outside, have equal and opposite properties, as may be seen at once if we think of them as brought close beside each other. If the rotations were transferred to one and the same body the net result both to themselves and to the body would be no motion. Or if the cord were cut away in slices, bit by bit, when it is all cut away the two ends have come together, and have, in a sense, neutralised each other. Here again we think of nothing transferred.

As a third case take a magnet. We always create equal and opposite poles at the same time with mutually neutralising properties, and to bring this within Dr. Lodge's statement we have only to consider the magnet as made up of two bodies joined together in the middle. Or perhaps we might take the case of the creation of N. and S. poles by the breaking of a magnet. No one has yet made a displacement- or transfer-hypothesis for magnetism except for the purpose of illustration. This, by the way, is rather remarkable, for magnetism with its re-entrant tubes of induction is so much more ready to lend itself to such a hypothesis. We seem to have finally made up our minds that electric is strain-energy, and that magnetic is rotational energy, and we do not even consider the alternative of magnetic as strain and electric as kinetic energy. No doubt the existence of magnetic rotation of the plane of polarisation of light strongly suggests the rotational nature of magnetism, but we can hardly claim any explanation of the phenomenon yet put forth as complete, and certainly none is exclusive. The other facts would possibly be equally well explained if we thought of electric tubes of induction as strings of wheels with their spindles along the axis of the tubes, and magnetic tubes as lines of flow of ether or of two ethers, with a certain displacement-coefficient, greater in diamagnetics, less in paramagnetics. I do not put this forward as worth following out, but merely to illustrate the contention that we need not necessarily think of + and − electrification as due to a longitudinal transfer of something. It appears to me quite possible then to symbolise the positive and negative charges facing each other across a dielectric in other ways than by the transfer of something from one towards the other.

Turning now to the second fundamental idea, Ampère's hypothesis, that a magnetic molecule is a small closed electric current, this is really closely connected with the idea that there is something moving in the direction of an electric current and constituting the current. It is in fact dependent on our acceptance of the first idea. For consider what an Amperean current is phenomenally. There is no resistance, and therefore no heat developed, and no fall of potential; there is no junction, and therefore no Peltier effect; there is no break, and therefore no chemical effect. The one effect left is the magnetic one. If, then, we take away the idea of some substance whirling round and round in a channel, all that we have left is a little permanent magnet. In other words, we explain the constitution of a magnetic molecule by supposing that it is a molecule having a magnetic constitution.

It is true that if we get a bundle of lines of magnetic force and tie them together with a perfectly conducting cord with its ends joined up we have a permanent magnet, for no more and no less lines can pass through the ring of perfectly conducting stuff. But this does not really explain. It does not show that the unknown is a case of the known. For a perfect conductor is far more difficult to think of than a permanent magnet.

It is getting time that these so-called 'perfects' were abolished from Physics. We have to deal with matter as it is and not as we should have had it if we had been consulted as to how it should be made in order to simplify our equations. The perfect gas has nearly gone and I shall be glad to see the perfect conductor preparing also to depart.

If, however, we grant substantiality to the electric current, we are met by a difficulty in connection with Ampère's hypothesis which, at any rate, requires examination.

Let us suppose that we have a circuit of self-induction L and resistance R, and let N be the number of outside lines passing through the circuit. If E is the E.M.F. of the battery kind in the circuit the current-equation is

$$E - \frac{d}{dt}(LC) - \frac{dN}{dt} = CR.$$

If $E = 0$ and $R = 0$ we have an Amperean circuit for which

$$\frac{d}{dt}(LC) + \frac{dN}{dt} = 0,$$

or

$$LC + N = \text{constant}.$$

In words, the total number of lines of force threading the circuit is constant.

If, now, N increases, either L or C must diminish. In order that L may diminish the circuit must contract and pinch in, as it were, the lines of force. But meanwhile, since the total is constant, the number of lines passing through the same area is increasing, and we should expect the spin, therefore, to increase and tend to make the tubes of force widen out. That is, there would be resistance to the contraction. The contraction is contrary, too, to our ordinary experience of a circuit with a current in it, for such a circuit tends to expand. If, on the other hand, the circuit is rigid, L is constant and C must diminish. If, as I gather from the general tenor of Dr. Lodge's work, the Amperean circuit is one of the ether cells peculiarly constructed or at least of the same order of magnitude, a diminution of C means a diminution of spin of this cell. But the total number of lines of force through it is constant, which seems to imply that the spin remains the same. This seems to force us to suppose that the Amperean circuit is *not* of the order of the ether cells, but larger, so that it may include within its contour a number of these cells. Then when N increases, C round the boundary may diminish, and still $LC + N$ be constant, for the internal wheels may be spinning at the same rate as before, but the external wheels more slowly, the current being a function of their difference. It would require further examination to see whether the total spinning energy could thus be accounted for. At first sight it would appear that that energy is diminished by the lessening of C just when we want it to increase.

If we are to suspend our judgment as to the substantiality of electricity, and are not as yet to conclude that a current is a rushing round of a thing of some kind, we are bound also to suspend our judgment as to Ampère's hypothesis, and if we are in this state of suspense, and, for my own part, I must confess to being so, Dr. Lodge's hypothesis and its accompanying models are to be used rather as illustrations as analogies—than as ultimate solutions. For the purpose of illustration they are of the greatest value. Like a bank-reserve, ready in case of emergency to cash the symbolic bank-note, they are in the background in the mind ready to turn into mechanical reality the ordinary electromagnetic symbols, such as lines of force, which are, I think, much more easy to deal with, but which are, doubtless, wanting in reality.

Perhaps Dr. Lodge may consider that my criticism is somewhat carping, in that I have nothing constructive to put in the place of the hypothesis to which I object. But I believe that the time has hardly come for ultimate mechanical construction, and that, at present, progress is more likely to be made if we are content with an electromagnetic explanation—if we merely carry down to the molecules and their interspaces the electric and magnetic relations which we find between large masses and round large circuits, and leave the ether out of account. I believe that we may symbolise electric and magnetic actions by means of lines of force and their motions in a way which allows us to think clearly of the phenomena, and though the ultimate nature of the lines of force is unknown, we can only say the same of the ether. In the application of these lines of force to the molecules, and their constituent atoms, there appears to be hope for some kind of explanation of the chemical phenomena of the circuit and of the distinction between electrolytic and metallic conduction. The difficulties in the way are by no means small, as everyone knows. I may, at some future time, attempt to set forth some of the difficulties as they occur to me, in the hope that a plain statement of them may lead some reader to help in their solution.

18.

MOLECULAR ELECTRICITY.

[*Electrician*, **35**, 1895, pp. 644–647, 668–671, 708 712, 741–743.]

In a Paper in *The Electrician* of October 13, 1893*, after some criticism of the 'wheelwork' hypothesis of electromagnetic actions, I concluded by saying that I believed that 'the time has hardly come for ultimate mechanical construction, and that, at present, progress is more likely to be made if we are content with an electromagnetic explanation if we merely carry down to the molecules and their interspaces the electric and magnetic relations which we find between large masses and round large circuits, and leave the ether out of account.'

I propose to follow out this idea, to see where it leads us, and what difficulties we have to face. The idea is in many minds, and we have examples of its use in the papers of Prof. J. J. Thomson and in the articles by Mr. Chattock in Dr. Lodge's *Modern Views of Electricity* and the *Philosophical Magazine*. The fullest account yet published is, I think, in Prof. Thomson's *Recent Researches*, Chapter I, but there are some difficulties not there brought to the front. I believe there will be some advantage in a new statement, starting with the very alphabet of the subject, and following it up by full examination of the consequences. In the belief that some way may yet be found to remove the difficulties, and that even now it gives us a valuable picture of some electrical actions, I venture here on a full description of the shape it has taken in my own mind.

The hypothesis with which we start is that electrical and chemical forces are identical; that electrification is a manifestation of unsatisfied chemical affinities, and that chemical union is a binding together of oppositely-charged atoms or groups of atoms.

Before descending to the atoms, let us briefly consider what we observe on the large scale coming within the range of experiment. The foundation-stone of our electrical knowledge is the experimental result that the two kinds of charge are always found in equal amounts with opposite or neutralising mechanical properties, and that they always face each other on the two sides of the insulating matter between them. We add to this the idea that the

* [*Collected Papers*, Art. 17.]

energy of the charges accompanies some kind of strain—alteration, perhaps, of atomic or molecular configuration—in the insulating medium. The two charges, in fact, always join hands through the dielectric; or, putting the same idea in another form, the charges are manifestations on the bounding surface of a state of strain within the dielectric, somewhat as hydrostatic pressure is a manifestation on the surface of the vessel containing a liquid of a strain to which the liquid is subjected. When this idea of strain was enunciated by Faraday and rendered precise by Maxwell we had only the variation of inductive capacity to support it. But now the double refraction of dielectrics in the field discovered by Kerr, and the wave phenomena discovered by Hertz, give as near a positive proof of the existence of the electric strain as we can ever hope to get.

In a system at rest the electric strain ends at the surface of a conductor and the electric stress or pull on the surface $2\pi\sigma^2/K$ per sq. cm. is resisted on the other side of the surface by ordinary mechanical stress accompanying ordinary mechanical strain in the matter of the conductor. Some day, perhaps, we may be able to manufacture a hypothesis identifying mechanical and electrical strain; but at present we are obliged to separate them and think of them as different in kind, since mechanical strain in moving off or dying away does not give rise to the magnetic or chemical actions which characterise the moving off or dying away of electric strain.

We symbolise the relative value of the strain at different points of the field by unit tubes of induction each beginning at $+1$ and ending at -1 of electrification. The strain varies inversely as the cross-section of a tube, and we regard $1/K$, the reciprocal of the specific inductive capacity, as a kind of modulus of electric elasticity. Though tubes of induction give us a better description of the imagined physical condition of the dielectric, it is easier to use lines of induction, one for each tube, when we have to draw figures.

These ideas of duality of charge, and of strain between them, which we owe to Faraday and Maxwell, though now accepted by everyone, have hardly yet so saturated our minds that we instinctively use them in every case. We still too often find descriptions of elementary phenomena which entirely leave them out of account. Like charges are still described as repelling each other, as when the gold leaves of an electroscope diverge, without a hint that the apparent repulsion is really a pull on each body along the lines of induction stretching out to the opposite charges induced on the surrounding conductors. The earth is still looked upon as a big conductor which will hold any reasonable amount of electricity without showing signs of it, and we speak of discharging a body into it instead of saying that the opposite charge is usually on the earth or the walls of the room in which we work, and that when a conducting bridge is made the two charges can come together so that the inductive strain is relieved, and its energy is dissipated. Or to take another example where common language and thought lag behind the more precise ideas given to us by science: the earth, as the 'return' circuit of a telegraph wire, is still described as a reservoir from which, say, + can be pumped up at the sending end and into which it can be emptied out at the receiving end, just as if a telegraph wire corresponded to a pipe running above a water reservoir with a pump at one end and a spout at the other. Sometimes even we find the process described as if the charge going to earth at the receiving end knew its way back through the earth to the particular battery from which it started. Whereas the earth is really not a return circuit at all, but a parallel out-going circuit for the opposite charge, enabling the two electrifications to travel in company and to face each other—one on the wire, the other on the earth—whether the earth be the bare ground under an aerial line or the sheath of a cable in the bed of the ocean.

This travelling of opposite charges to meet each other is to be regarded, I am convinced, as the essential electrical part of the ordinary current. The lines of induction connecting the charges sweep through the air or other surrounding insulator, and this motion of the induction, or travelling onwards of the electric strain, is accompanied by indeed, is rendered possible by—the magnetic induction which surrounds the conducting wire. The only case of current without motion of charge is the gradual cessation of charge in an imperfectly insulating condenser. Here the charges simply die away *in situ* —their strain decays and their energy is dissipated; and as there is no motion of induction there is no magnetic field.

To realise how ordinary current may be described in terms of motion of lines of induction let us suppose that, instead of the more usual dynamo or battery as source, we have a charged condenser with the – terminal to earth and the + terminal insulated, but capable of connection by a key with a wire earthed at the farther end. Up to the instant when the key is put down the two charges are almost entirely in the condenser, joining hands in the dielectric between the plates. The still prevalent mode of describing the current on contact at the key is equivalent to supposing that, as soon as the found a door open to it, it freed itself from the embrace of its –, put its hands in its pockets, ran along the wire, setting all the neighbouring magnetic machinery spinning as it rushed past, and finally plunged into the earth at the farther end. The –, no longer kept up or 'bound' by the +, sank into the earth at the condenser end, and both were lost in that vast electrical abyss, the globe.

But let us see what is really implied in the process of discharge if we keep to our fundamental principle that + and – always have induction tubes or lines between them. Let us try the various suppositions which seem open to us, and let us first imagine as a possibility that the charges are able to start on their travels by breaking their induction-tubes somewhere in the dielectric between the condenser-plates, as in Fig. 1. At the broken ends we must have opposite charges (just as in a broken magnet we have opposite poles), for a charge is the end on matter of an induction-tube. If each + unit setting out along the wire drags the broken half of its tube after it into the wire, we shall have on the whole equal + and − travelling in the same direction along the wire and no external magnetic effect. Indeed, as soon as the leading + has drawn its piece of tube completely into the wire there does not appear to be any reason against the union of the + and − at opposite ends of the piece, and then nothing need occur in or around the rest of the wire. But as we have magnetic and other effects in and around the whole length of wire, and everything is against the passage of + and − in the same direction, we are bound to reject this supposition of breaking tubes. As another supposition let us imagine the tubes as remaining in the dielectric of the condenser but growing longer where they stand, and as it were pushing their end-charges before them, the one through the wire, the other into the earth. But this

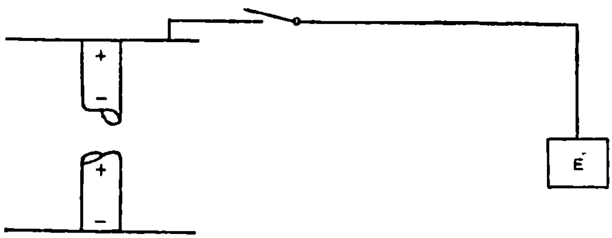


Fig. 1.

will leave the tubes still in full strength in the dielectric, and when the disturbance in the wire has died away the condenser will still be fully charged, which is absurdly at variance with experience. The only possible supposition still left to us is that the tubes of induction spread out sideways from the dielectric of the condenser into the surrounding air, connecting the + and − charges as these move respectively along the wire and the earth. The induction—that is, the condition we call electric strain—is propagated by the working machinery from point to point through the air, and the motion of this machinery is manifested as magnetic induction, symbolised by lines of magnetic force which form closed circuits round the wire. We cannot explain this mode of transfer, but the mechanical models of Maxwell and Lodge and FitzGerald help us to accept it as possible.

We must now introduce the idea that conductors are dielectrics—of a special kind, no doubt—admitting of electric strain, though allowing it to decay rapidly and dissipating the energy they receive in the form of heat. We may take as a helpful analogue to this behaviour of a conductor the behaviour of a liquid under ordinary elastic shear-strain. A liquid can receive shear-strain just as much as a solid, but the strain decays, that is, rapidly loses its energy, and the lost energy is transformed into heat. But the still undecayed strain remaining at any instant gives a tangential resistance to shear, which we recognise as viscous resistance to the motion of one layer relative to the next. So in a conductor such as a wire, electric strain may be produced and electric energy may be taken in; but the strain decays almost as fast as it is received and the energy is changed to heat. The E.M.F. along the wire is proportional to the strain remaining still undecayed at any instant, and this E.M.F. corresponds to the elastic stress—the viscous resistance of a sheared liquid.

In the case of the condenser which we are considering, as the induction-tubes spread out from it their end-portions are continually moving sideways into the wire, and to a less extent into the earth, and there melting away and dissipating their energy as heat. New ends are continually being formed at the junctions of the decayed and undecayed portions, and these ends travel further and further along the circuit until the tubes are entirely propagated into the conductors, when the discharge is complete.

A full account of this mode of regarding current, and the relations involved between electric and magnetic induction, with an attempt to show that it applies to the voltaic circuit as well as to such a circuit as we have here considered, was given in the *Philosophical Transactions* for 1885*. Prof. J. J. Thomson has modified and elaborated the theory in the *Philosophical Magazine* for March, 1891, and in his *Recent Researches in Electricity and Magnetism*, Chapter I. The full theory shows that if we assume that the magnetomotive force round a closed curve is equal to $4\pi \times$ number of electric tubes cutting the curve per second, then the magnetic field is accounted for. If, then, we make the electric tubes move so as to account for the right electrical quantities, the magnetic properties will follow as a matter of course, and we may therefore concentrate our attention on the electric tubes, as we have done in the preceding account of one case of current. I have given this case at length, by way of introduction, to show that here at least the only way in which we can think consistently of current in a wire is in terms of motion of $+$ and of ,
and of tubes of electric induction connecting them and sweeping through the surrounding medium into the wire. We shall see later how such tubes may be supposed to be furnished by the recombinations which occur in the voltaic circuit, and I have no doubt that in all other cases of current the same kind of explanation may be given.

There is another reason, perhaps, for giving here the foregoing account of a condenser-discharge, in that the hypothesis which I am going to describe may not inaptly be termed a condenser-hypothesis of electricity. The properties which we find in condensers will be carried down to the molecules, which we shall suppose to be small condensers with equally and oppositely charged atoms forming the two plates. Of course, this will not explain

* [*Collected Papers*, Art. 11.]

electricity. It is a purely electrical hypothesis, and it shifts all responsibility of further explanation on to the shoulders of whatever atomic hypothesis we adopt, just as Weber's magnetic hypothesis gives no explanation of magnetism, but assumes the molecules to be ready-made magnets, and leaves to them the burden of accounting for themselves.

We are naturally led to make the hypothesis from the consideration of the chemical action of current. When a voltameter is included in a circuit a perfectly definite quantity of electrolyte is decomposed for a given number of induction-tubes moving sideways into it. All parts of the liquid between anode and cathode are concerned in what goes on, and there are two opposite processions of ions. This is all experimental fact. Representing the action atomically we may obtain the net result by the old Grotthus chain method of picturing the process, and each atom set free requires the supply of a definite constant amount of electric induction, calculable if we know the number of atoms per gramme of the substance.

Let us now consider how electrolysis may occur. We shall imagine a somewhat abstract kind of electrolyte, one consisting of molecules, each with a pair of atoms, an abstraction, a simplification, which we shall have to discard later. To account for the exact chemical equivalence of electrolysis in different cases we shall have to suppose each atom to be supplied with the same amount of electric induction, to have the same minute fraction of an induction-tube starting from it if it is of one kind, ending on it if it is of the other kind. To bring the induction up to a thinkable size we must choose a new unit—I suppose millions of billions of times less than the ordinary unit, and it will be convenient to picture the atom as supplied with two, four, or some such small number of tubes in terms of this new unit.

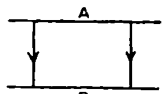


Fig. 2.

Here, then, is our molecule, represented in Fig. 2. A is the positive and B the negative atom. The molecule is, in fact, a little condenser with fixed charges, and the distance between A and B is usually small compared with their distances from neighbouring molecules. But we must suppose that in general A and B do not come in contact, perhaps through motion of vibration or of rotation relative to their centre of gravity. There is therefore persistence or conservation of their tubes. The electrical attractions symbolised by these tubes are absolutely identical with the chemical attractions of the molecules, and the electrical energy they contain with the chemical energy of the molecule. It is easy to see, however, that two molecules colliding may become connected, and form a more complex group, and may even effect an exchange of atoms.

Thus, in Fig. 3, (a) to (f) represent successive stages in the process of exchange. Just as, when two condensers are brought near with their tubes of induction running in opposite directions, cross-connections are formed by the coalescence and neutralisation of parts of the tubes, so here we may suppose that in a molecular collision the tubes straying out coalesce, and the parts which run in opposite directions destroy each other. In (*c*) we have a more complex molecular group. If the process is continued we may arrive at (*f*), which gives the original molecular configuration but with exchange of partners. If we are ever to attempt an electrical hypothesis of elasticity and cohesion we shall have to suppose that straying of tubes is always going on in solids and liquids, and that complex groups like (*c*) are always being formed, so that each molecule is attached to its surroundings. In gases not in an electric field the molecules may be regarded as much more self-contained.

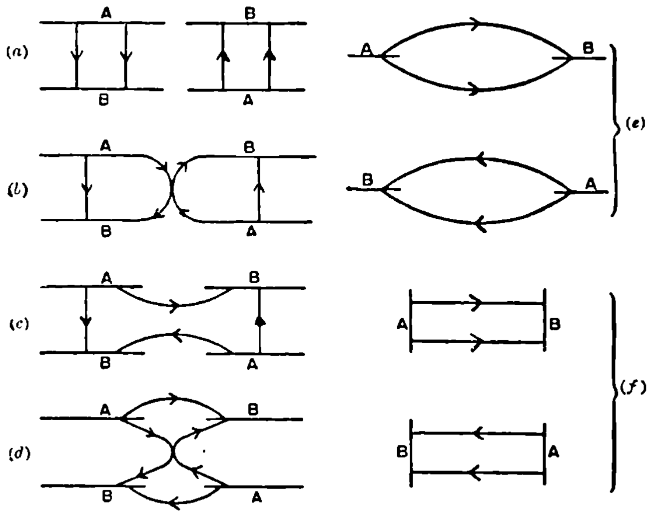


Fig. 3.

When a line or tube of induction moves sideways into an electrolyte composed of such condenser-molecules it finds the molecules with their axes distributed equally in all directions. It will pick out those already facing in the direction suitable for it—those requiring the least energy for its purpose, and those alone need be drawn.

In Fig. 4 (*a*) two lines of induction, XX, YY, are represented as ready to move into an electrolyte, in which AB, AB represent the suitable molecules.

The successive stages are represented by (*b*), (*c*), (*d*), (*e*), and the final result is that the highest B atom is delivered up to the + electrode, and the lowest A atom to the $-$ electrode, while all the intermediate atoms in the chain change partners, merely forming molecules like the original ones, but with their axes reversed.

If more lines come in they will find new molecules directed as they require, and soon also the collisions occurring in the liquid will distribute the axes of the molecules of the first chain into various directions, so that some of these, too, will be ready for later lines of induction.

The coalescence and destruction of the parts of two tubes which run alongside each other in opposite directions, upon which the whole process

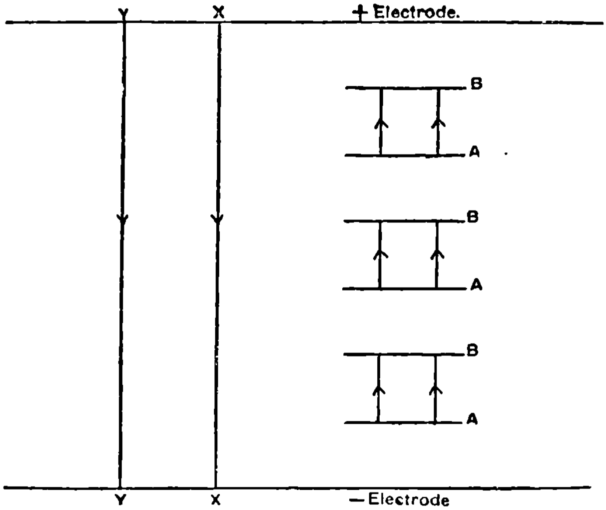


Fig. 4 (*a*).

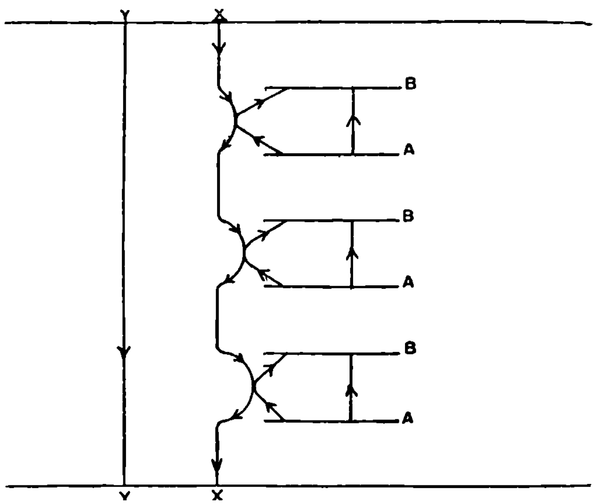


Fig. 4 (*b*).

depends, will naturally tend to take place, as less potential energy will be needed for the coalesced tubes than for the two side by side. The energy of the final configuration is the same as that of the initial configuration, except at the two ends, so that if XX and YY bring in enough energy to provide for the new configurations at the electrodes the process will continue. If XX

and YY bring in an excess of energy we may regard the change of partners as occurring with more or less of a rush, and the excess is converted into internal molecular energy, partly kinetic, partly potential; in fact, the chain

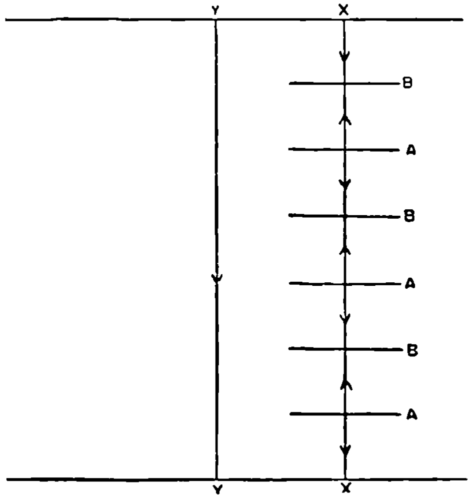


Fig. 4 (*c*).

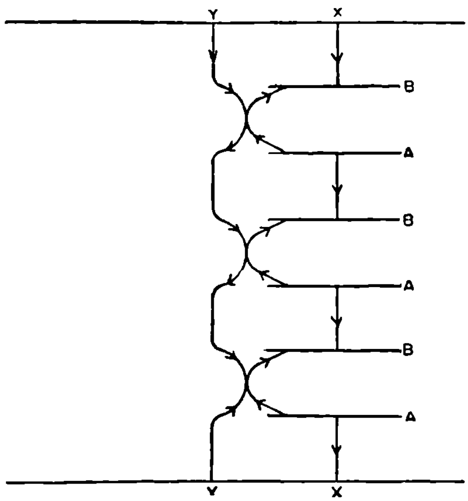


Fig. 4 (*d*).

of recombined molecules is warmer than it was, and we have the Joule C^2R effect.

For simplicity we have considered the tubes XX, YY as stretched through free space till they come to the chain of molecules which they are to rearrange. But it is easy to see how a tube may be handed on from

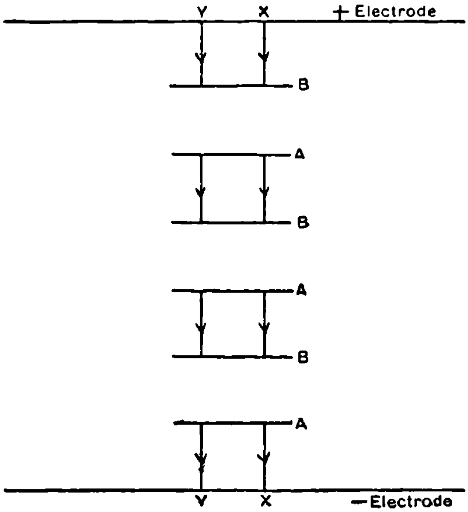


Fig. 4 (*e*).

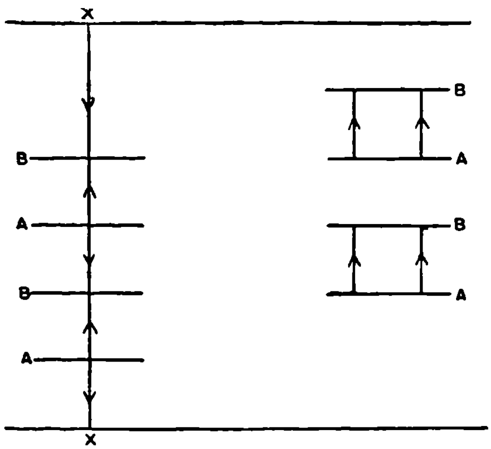


Fig. 5 (*a*).

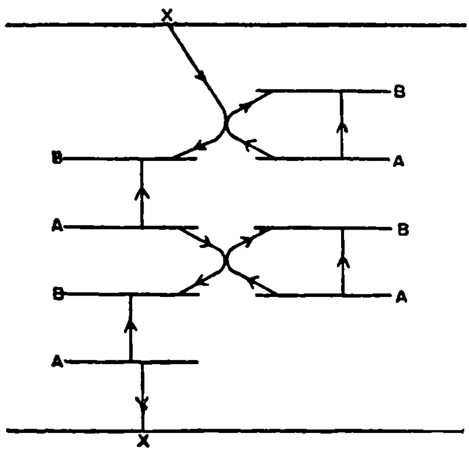


Fig. 5 (*b*).

chain to chain without any free existence. In Fig. 5 (*a*) a tube XX already threads a chain of AB molecules and in (*b*), (*c*), (*d*) are shown the successive stages of its transfer to a neighbouring chain. The process is rendered more capable of representation by supposing each member of the second set of molecules lifted its own depth upwards. The derivation of each stage from the preceding is evident. This is doubtless the process which we must

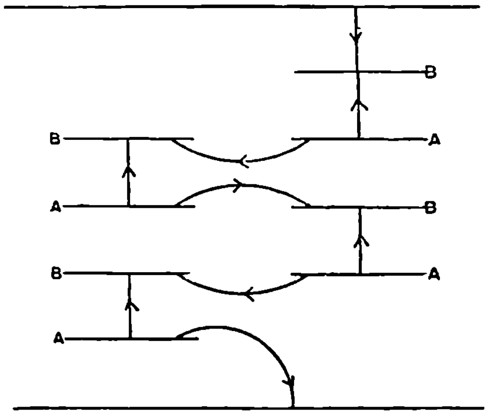


Fig. 5 (*c*).

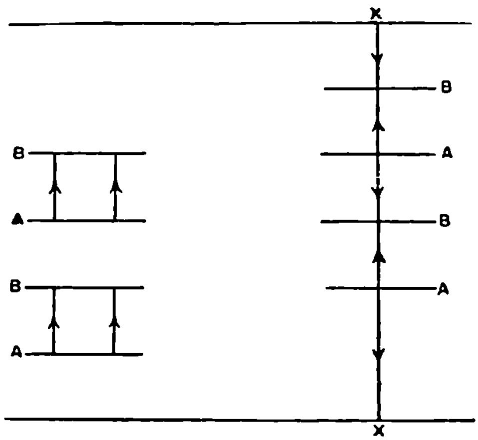


Fig. 5 (*d*).

suppose to occur in all cases, and the tubes must be regarded as threading such chains in the dielectric, even before they enter the electrolytic cell.

Let us now consider what must happen at the electrodes. If the ions unite chemically with the electrodes, the positive and negative atoms find respectively negative and positive atoms ready to combine with them, and no difficulty is introduced at this point. But if the ions are set free, as are hydrogen and oxygen at platinum electrodes in dilute acid, we are brought face to face with the great, and, it is to be confessed, the yet unsolved difficulty in the hypothesis. Take the case of the hydrogen atoms released at the cathode, the atoms such as the lowest A in Fig. 4 (e). We may think of them as connected at first by tubes of induction going from them to platinum atoms on the surface of the cathode. But soon the gas bubbles up, and there is no manifestation of the charge all of one kind which we have ascribed to the separate hydrogen atoms. We may attempt several explanations. The common notion appears to be that the hydrogen gives up its charge to the platinum, and then rises up quite deprived of electricity. This might occur by the breaking away of the tubes from the hydrogen atoms and the withdrawal of the positive ends into the platinum, the positive charge moving across the separating space, like a disembodied soul, from its hydrogen habitation into the platinum. The platinum having two neutralising souls, becomes as merely material as the deserted hydrogen. But this is strongly against our experimental knowledge of charge, which is always, so far as we know, on matter, whereas here charge crosses over a separating space. We might suppose this got over by thinking of the hydrogen and platinum atoms as coming absolutely in contact, shortening the connecting tubes to nothing, and putting them out of existence. If the tubes are not re-created when the hydrogen rebounds the gas rises up without electricity.

But, according to the current ideas of chemistry, the hydrogen issuing from the cell is not atomic, but molecular, and consists of paired atoms at least; that is to say, it is a chemical compound, differing from ordinary compounds it is true, in that the two members of each molecule are the same in kind, but the atoms are held together by chemical attraction, and satisfy each other's chemical affinity just as much as if they were different elements. If our hypothesis has any truth in it, this means that electric induction exists between the atoms and holds them together, a view supported, I think, by the phenomena of electric discharge in gases and the electrolysis which appears to take place in that discharge. To be consistent, then, we must suppose that the hydrogen, when it rises up, consists of pairs of + and − atoms, and for us the difficulty is to explain, not what has become of the hydrogen charge, but how half the atoms have succeeded in exactly reversing their charges, so that they are able to combine with the other half to form neutralising pairs. And, of course, we must extend our supposition of the constitution of an element as being made up of + and − atoms to the platinum also. When the lines of induction move into the platinum, the − atoms of the surface pairs are freed from their + partners, and are free to form pairs with the + hydrogen atoms. But when half the hydrogen reverses its charge half the platinum must do the same in order that it too may effect the neutral combination which we find when the electrolysis has ceased. Perhaps the best course is to say that if our main hypothesis of identity of chemical attraction and electric induction is real, then the reversal of charge, such as occurs on our supposition at the electrodes, is up to the present an unexplained, an ultimate fact. Without attempting explanation, but rather as a crude mode of picturing a process which would lead to the result, we might think of a + H and a − Pt as coming absolutely in contact, so that the tube-ends can slide round on the surface of the atoms and across the bridge of contact until + is on the Pt and − on the H. Possibly Fig. 6 will make this clearer.

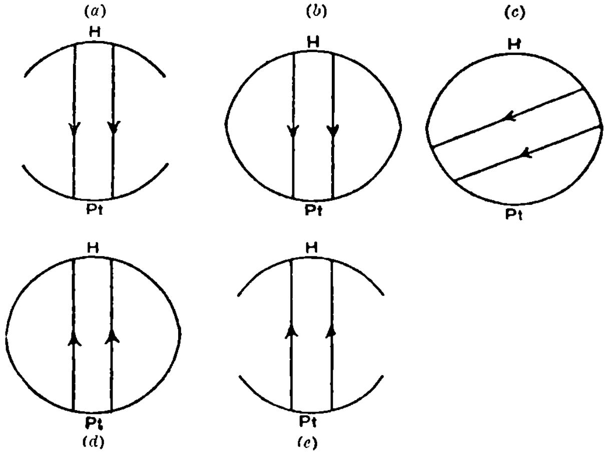


Fig. 6.

In (*b*) two atoms have come in contact, in (*c*) the tubes are beginning to slide round, in (*d*) the ends have changed places, and in (*e*) the atoms have drawn apart with reversed charges. Having thus, or otherwise, reversed half the pairs, combination with the other half may ensue as in Fig. 7, the result being hydrogen molecules and platinum molecules each consisting of + and − pairs.

At first sight it seems as though the reversal occurring in the spark-discharge of a Leyden jar or a Hertz vibrator would give us the key to the reversal pictured in Fig. 6. But further examination takes away this hope. The jar-reversal is never complete and exact such as we have to suppose the atomic reversal. Indeed, the induction may have any value, + or −, less numerically than the initial value, as it gradually dies down to zero through the successive vibrations. We shall see later how some account may be given of the reversal in vibrators, an account which quite destroys any analogy with atomic reversal.

Perhaps a better analogy is afforded by two vortex rings *A* and *B* when they are playing at leap-frog. The first widens out and lets the second go through it. Before the passage the liquid is streaming through *A* towards *B*. After the passage it is streaming from *B* towards *A*, so that if we think of induction as corresponding to direction of flow we have here a reversal.

But I suspect that this liquid-analogy is much more appropriate to a magnetic than to an electric hypothesis.

Were it not for the magnetic difficulties involved there would be some temptation to attempt a theory in which the sign of charge is merely a statement of the kind of atom concerned, and that the tubes, like gravitation-tubes, have nothing but alignment and neither + nor − direction. But this would, I fear, be getting over one difficulty only by introducing much greater difficulties hereafter.

We shall assume, then, that in some way or other reversal can take place in certain cases, and we may apply it at once to explain the apparently neutral condition of the ions in the electrolytic cell. We suppose that half the ionic atoms in Fig. 4 reverse or change charges with the electrode atoms, as in Fig. 6, and that then they go through the process of Fig. 7 with the other half, and so form + and − pairs, exhibiting no external electrification.

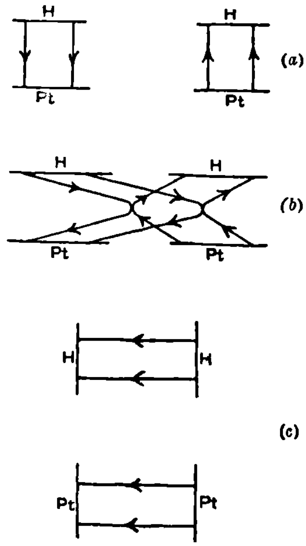


Fig. 7.

A similar reversal will enable us to give an electrolytic account of metallic conduction. The great distinction between electrolytes and metals is that in the former there are opposite atomic processions, while in the latter there is apparently no motion in either direction. Electrolysis without procession means, as we have just seen with the electrodes, where the procession stops, reversal of charge. Suppose, then, that such reversal is possible with every metallic molecule. Imagine a copper wire carrying a current to be made up of molecules, as represented in Fig. 8 (*a*), and let tubes of induction XX, YY be just moving into the wire. Before the current is established the molecular axes are evenly distributed in all directions, but the tubes entering in select those most suitable, as shown in (*a*), turning the configuration to that shown in (*b*); if now reversal takes place we get (*c*). Let two more tubes come in, and we get (*d*), a reversal of which gives the same arrangement as (*a*) as far as the copper is concerned, but the four entering tubes of induction have entirely disappeared.

We must now attempt to give some account of the action at the source of a voltaic current. As we simplified the electrolyte so we shall simplify the active liquid of the voltaic cell by imagining that we have merely sulphuric acid, that is, we shall neglect the solvent, water. We shall take as the two metals zinc and copper.

We know that the result of putting the copper and zinc in the acid is that the air above the acid tends to become the seat of tubes of induction running from copper to zinc. This will be neutralised by the oxidising tendency of the air on the zinc—at any rate that is the 'chemical theory' of voltaic action. To eliminate this action of the air we shall suppose the zinc to have

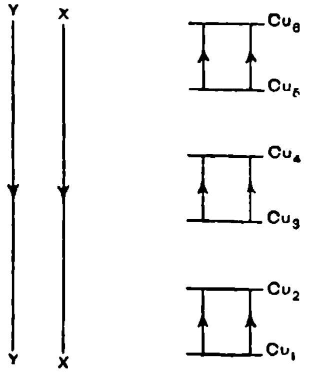


Fig. 8 (*a*).

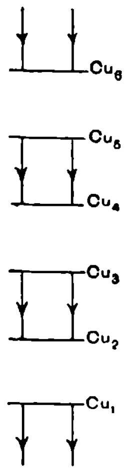


Fig. 8 (*b*).

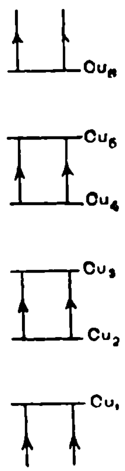


Fig. 8 (*c*).

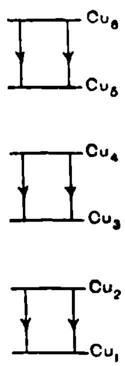


Fig. 8 (*d*).

a copper terminal in the air, as it always has in practice. Experiment shows that these tubes of induction always run from copper terminal to zinc terminal with a fall of potential which may be as much, say, as a volt and a half. To represent the establishment of this induction let us imagine the cell initially to consist of such molecules as are shown in Fig. 9 (*a*), where we represent the acid molecules as consisting of $+ H_2$ paired with $- SO_4$, and the copper and zinc plates as $\pm$ Cu and $\pm$ Zn.

Of course, the molecules in each substance are evenly distributed in all directions, but only those are shown in the figure which are suitably directed for the action which is going to occur. The first stage is represented in Fig. 9 (*b*), where change of partners has occurred between metal and acid both with copper and zinc. The H_2Zn molecules do not reverse, for it is an experimental fact that the hydrogen does not rise up where it is first turned out. The reversal may perhaps be prevented by the presence of the $ZnSO_4$

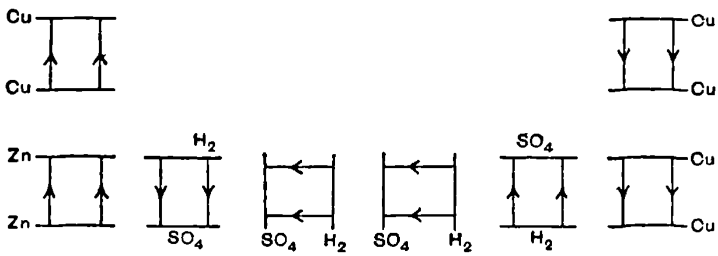


Fig. 9 (*a*).

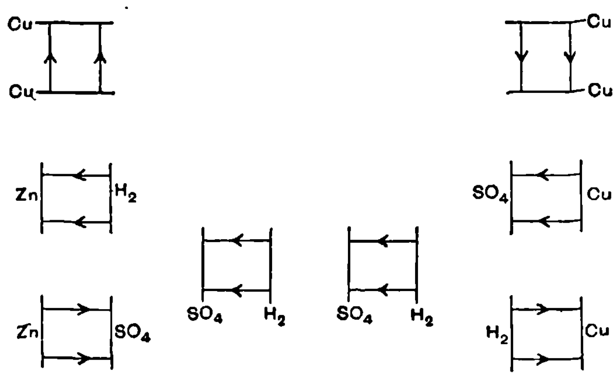


Fig. 9 (*b*).

molecules, but it is more probably due to the electrical energy put into the H_2Zn. For the $ZnSO_4$ contains much less energy than the H_2SO_4 it replaces; so that the H_2Zn contains much more than the ZnZn it replaces. The electrical energy put in makes the atoms separate too widely, we may imagine, to allow of the contact needed for reversal. Probably at first there is no reversal in the H_2Cu molecules at the other plate, and for similar reasons.

The electrical energy in the H_2Zn and H_2Cu molecules will imply that the acid is at a higher potential than the metals, and tubes of induction will spread out from the H_2 atoms. Since the H_2Zn molecules contain the most energy, we may represent their tubes as spreading rather than those of the

H_2Cu, and in Fig. 9 (*c*) we have the tubes shown as going out towards the copper and then doubling back again. In (*d*) the tubes have entered into the neighbouring molecules by coalescence with oppositely-directed tubes, and we are left with two tubes running from the + to the − terminal with a $ZnSO_4$ molecule and an H_2Cu molecule, the $CuSO_4$ molecule having been dissociated in the process. The molecules of acid have changed partners, but still have the same constitution.

If the action is not continuous, but merely goes on till the terminals are charged, we must suppose that H_2Zn and H_2Cu pairs are left against each

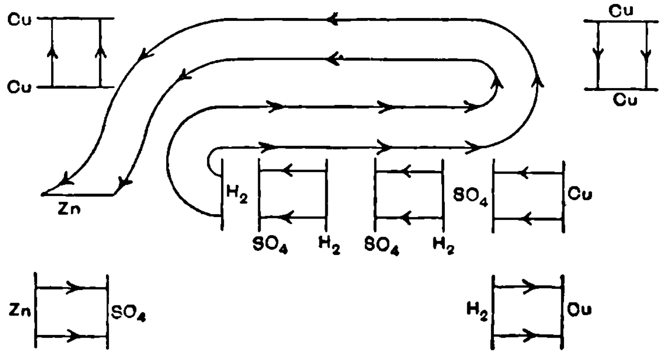


Fig. 9 (*c*).

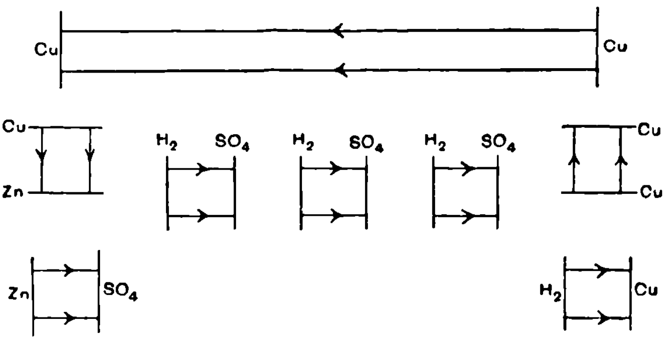


Fig. 9 (*d*).

plate with an average fall from H to metal of, say, $1\frac{1}{2}$ volts. The pairs against the copper plate are no doubt the agents in polarisation. The reverse current which we get on replacing the zinc by a fresh copper is to be set down to the straying-out of the tubes of induction of these H_2Cu pairs. If the action is made continuous by connecting the terminals with a wire, the hydrogen rises up from the copper plate, and we must suppose that half the H_2Cu pairs have reversed and have then changed partners with the other half. Perhaps this reversal is rendered possible by the resolving of the $CuSO_4$ molecules, perhaps by the outlet provided for the energy of the H_2Cu pairs in the external circuit.

In a very similar way we may account for the contact-difference of potential of copper and zinc in air. We know that if the two metals are brought into contact a fall of potential occurs from the air near the zinc to the air near the copper; that is, electric induction passes from the neighbourhood of one to the neighbourhood of the other. Both metals can be oxidised, but zinc by far the more readily. Let us suppose the oxygen molecules in the air to be made up of pairs of opposite atoms, and that *before contact* we have a state of affairs, represented by Fig. 10 (*a*), developing into (*b*) by the actions at the metals. On each metal we have the normal oxides ZnO, CuO, where the positive atom comes first, and the unstable molecules OZn and OCu. If the tubes of induction of these unstable molecules stray out they have to double

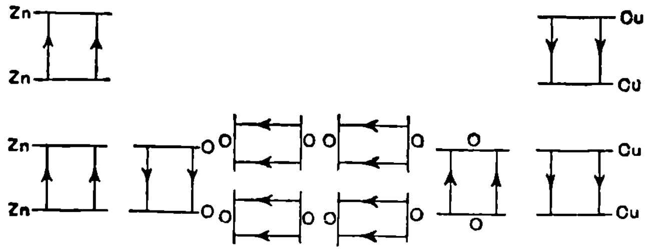


Fig. 10 (*a*).

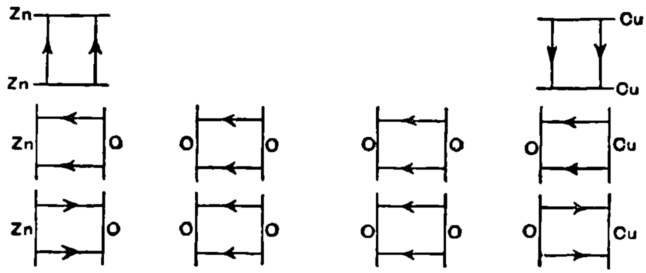


Fig. 10 (*b*).

back on themselves, so that as long as there is no contact there are equal numbers in the two directions passing through the air, and no fall of potential except from the surface-layer of the air to the surface-layer of the metal close to it. This fall will be greater at the zinc than at the copper, since there is presumably more energy given up at the zinc surface. Probably reversal is prevented either by the presence of the normal oxides or by the want of outlet for the energy. If, however, it does occur in some of the unstable molecules there will be re-pairing and the formation of new metal molecules and new oxygen molecules, with the net result that each metal is left slightly oxidised. But let us make the two metals touch as at *J* in Fig. 11 (*a*). If the tubes of induction of the OZn molecule now move out like those of the H_2Zn molecule in Fig. 9 (*c*), the upper returning part can enter into the continuous metal bridge and there be dissipated; while the lower outgoing part will thread the oxygen molecules, as shown in Fig. 11 (*b*), and decompose the CuO molecule already found. We shall as a net result have ZnO on the surface of the zinc, a fall of potential from O on the zinc surface to O on the copper surface, and the unstable OCu, which possibly reverses and re-pairs, leaving the copper surface unacted on. If it remains then the copper is polarised, and if the zinc were suddenly removed and replaced by a new copper it would appear that the old copper should show a fall of potential towards the new.

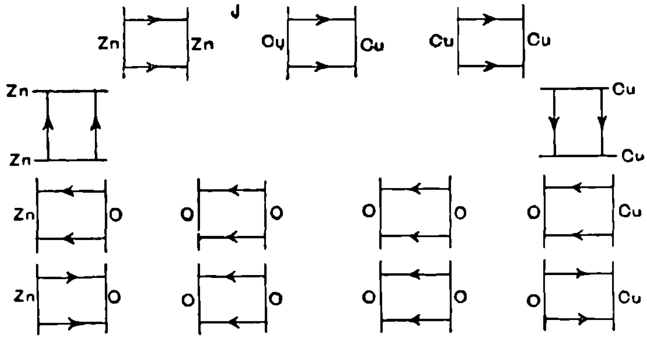


Fig. 11 (*a*).

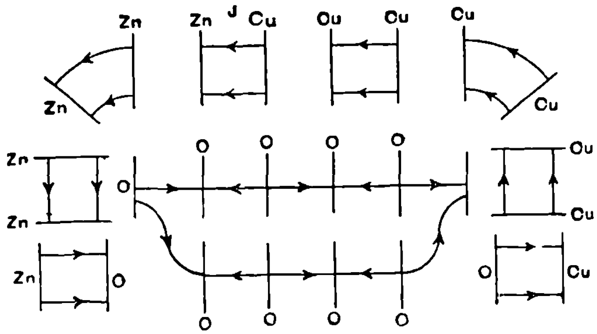


Fig. 11 (*b*).

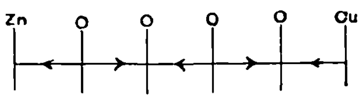


Fig. 11 (*c*).

But any such fall would probably be disguised by the induction ending in the O atoms next to the copper. We have probably gone too far in this account in supposing that *all* the induction of the OZn molecules goes out in this way. If, for example, we suppose that only half goes out, we get a chain extending from Zn to Cu, as in Fig. 11 (*c*). Probably some such supposition must be made in order to explain how the + O atoms remain at the zinc surface and the − O atoms at the copper surface after the break of contact. That they do remain is shown by the + electrification of the zinc and the

– of the copper when tested by an electrometer. But our account must be regarded as a first attempt and not as a complete explanation.

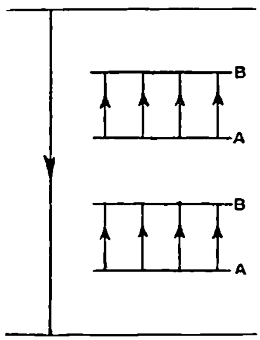


Fig. 12 (*a*).

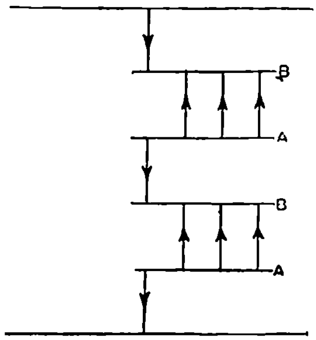


Fig. 12 (*b*).

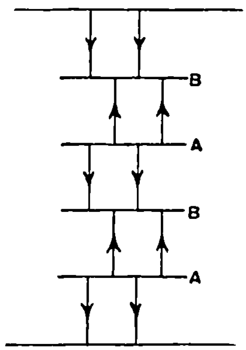


Fig. 12 (*c*).

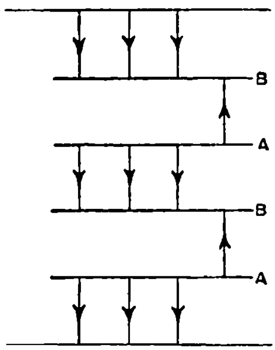


Fig. 12 (*d*).

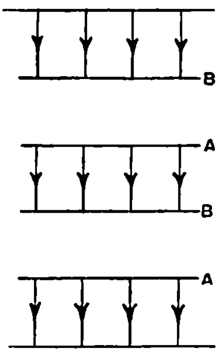


Fig. 12 (*e*).

Leaving electrolysis, let us consider how an ordinary insulator may be affected by induction, and how we may represent the condition of affairs leading up to spark-discharge. We shall thus get some useful ideas which will supplement our account of electrolysis and make it somewhat easier to understand the true nature of the process.

We shall suppose that we are dealing with a dielectric consisting of paired + and − atoms, and we shall suppose, further, that these molecules are initially self-contained—that is to say, that their tubes do not stray out to surrounding molecules. This is, no doubt, a simplification not existing in nature; but, as with electrolysis, so here also we must be content to begin with an abstract case. When there is no apparent electrification in the system the axes of the molecules will be equally distributed in all directions. But if electrification is communicated to a pair of conductors bounding the dielectric, tubes of induction move into the dielectric, and, selecting the suitably arranged molecules, connect these in chains. It will be convenient now to suppose at least four tubes of induction to pass from atom to atom in a molecule. Let Fig. 12 (*a*) represent a number of molecules ready for a tube of induction to affect them. In (*b*) it has moved in and formed the molecules into a chain stretching right through the dielectric. If we suppose another tube to move in, as in (*c*), we get a condition of instability, for now we are just half-way to a change of partners all along the line. A third tube will give us (*d*), a configuration with the same amount of energy as (*b*), since the molecules in the two cases are similar, the axes only being reversed. Hence in passing from (*c*) to (*d*) energy is given up, another way of saying that (*c*) is unstable. A fourth line entering will change (*d*) to (*e*), where the change of partners is complete, and where electrolysis has occurred.

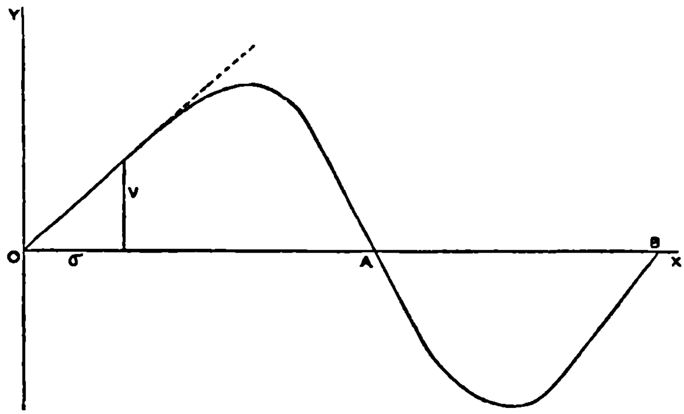


Fig. 13.

We may usefully follow out the process by the aid of a diagram representing the relation between energy put in and induction. Beginning with an ordinary condenser, let distances along OX (Fig. 13) represent induction put in per unit area cross-section, distances along OY difference of potential between the end-plates.

At first we have $$V = \frac{4\pi\sigma d}{K},$$

where V is the potential difference, d the thickness, σ the surface-density, and K the specific inductive capacity.

But since the induction $D = \sigma$,

$$V = \frac{4\pi d}{K} D.$$

If then K is constant, the relation between V and D is represented by a straight line, which makes with OX an angle θ given by

$$\tan \theta = \frac{4\pi d}{K}.$$

The energy stored per unit cross-section is equal to $\frac{\sigma V}{2}$, or equal to the area from the origin up to the ordinate V, and bounded by the line representing the relation between V and D. We may indeed regard the diagram as showing the relation between induction and energy stored, and this is probably a better point of view, since we cannot attach much idea to potential in the later parts of the process now to be considered. From this point of view the abscissa is the induction through unit area and the ordinate is the energy added per unit addition of induction.

All experiments hitherto made appear to show that K is practically constant so long as the medium can continue to store up energy, though the Kerr effect shows that K does alter slightly as D increases, apparently sometimes increasing and sometimes diminishing, or, perhaps, always increasing, but sometimes most along the lines of induction, at other times most at right angles to them.

Sooner or later, however, a point of instability is reached—sooner in gases, later in liquids and solids; and discharge occurs along one track and all the energy is dissipated. We can see how the curve in Fig. 13 must run in order to represent this instability. Making the unit area small enough to represent the cross-section of a molecule, the curve expresses the relation between induction going right through a chain of molecules from plate to plate and energy put in. At first, while only a small part of the induction is continuous, as in Fig. 12 (*b*), we know that the energy stored is proportional to the square of the induction, and the curve is, as we have seen, a straight line. But as more induction becomes continuous from plate to plate, and as the atoms are pulled in both ways, the force resisting separation does not go on increasing so rapidly, and the curve falls below a straight line.

At some point on the way to instability and subsequent change of partners the force will reach a maximum, and after that the curve will turn down, successive equal additions of induction requiring diminishing additions of energy. At last, when the lines of induction from each atom run half one way, half the other, as in Fig. 12 (*c*), the energy put in is a maximum, and the curve crosses the X axis, as at A (Fig. 13). The configuration is now unstable, and will of itself pass through (*d*) and (*e*), absorbing two more positive lines or extruding two negative lines till we arrive at (*e*), represented by the point B in Fig. 13, when the condition is again stable, like that at the beginning. The energy put in between O and A is given out again between A and B in part, no doubt, as the light, heat, and so on of the discharge. The curve OAB may be termed, perhaps, a molecular characteristic. It should be noted that the passage from (*c*) to (*d*) (Fig. 12), or past A (Fig. 13), may be effected

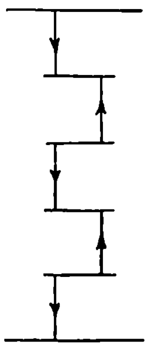


Fig. 14 (*a*).

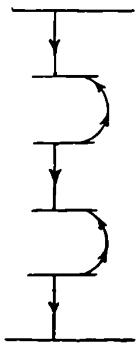


Fig. 14 (*b*).

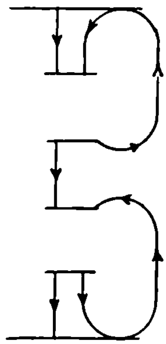


Fig. 14 (*c*).

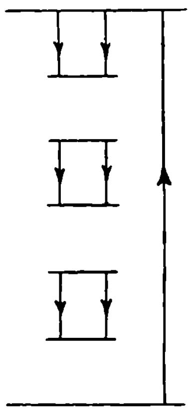


Fig. 14 (*d*).

either by absorbing more positive tubes, or by sending out negative tubes. If the passage through the position A occurs with a rush, then we may regard the chain as sending out negative tubes by some such process as is illustrated in Fig. 14, (*a*) to (*d*). Though the instability of a single chain will not be reached till its condition is represented by A (Fig. 13), the instability for a great number of parallel chains is reached as soon as they are all at or near the highest point of the characteristic. In the ascending part of the curve all the parallel chains will tend to have the same amount of induction through them, for if any one has more than another, energy will be yielded up by a redistribution between them. Thus let one chain have induction OM (Fig. 15), another near it induction ON, the energies stored being OMP, ONQ. If L bisect MN the second chain may give up induction $LN = LM$ to the first, and at the same time there will be a yield of energy equal to the difference between the areas PL and QL. Probably the induction is really distributed about an average, some of the chains having more, others less, for no doubt the condition is kinetic, and when there appears to be equilibrium it is not static but 'mobile.'

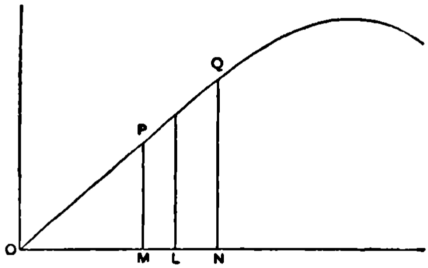


Fig. 15.

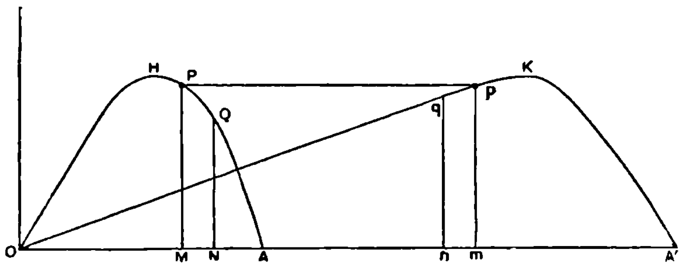


Fig. 16.

If the average condition is represented by a point far up the slope and near the crest, and if any one of the chains gets past the crest, as far below it on the other side as the average is below on the first slope, then this advanced chain will at once receive induction from the others, and continue to move down the second slope towards the point of instability. Thus, let there be n chains in all. Let OHA (Fig. 16) be the characteristic of that with the greatest induction, OKA' the sum of the other $n - 1$ characteristics. If the first chain is at P while the others are at p on the same level, by a transfer of induction $mn = MN$ from the general body in the first chain, there is a yield of energy, since the area of $pmnq$ is greater than the area of $PMNQ$, for the two slips are of the same breadth, but the slope of PQ is steeper than that of pq. The transfer will therefore take place, and the chain moves from P to Q. At Q, *a fortiori*, a new transfer will take place, and so on, and the chain will move towards the position of instability. This will all probably occur even when the average is far below the crest if the induction is widely distributed about that average, for as soon as one chain gets over the crest it will probably find others between it and the average ready to hand on their induction and energy to it, and send it down the second slope.

As, then, the general average rises towards the crest, the most advanced chains, as soon as they pass the crest, tend to discharge the rest, and there will at a certain point be a rush in sideways on to these advanced tubes. They will move down to the condition of instability represented by A on the diagram. They can of themselves move past this point, extruding negative tubes, till they arrive at the point B, where the change of partners is complete.

It appears at least probable that this yield of negative tubes gives the opposite charging of the medium which takes place in the second quarter-period of the oscillation accompanying rapid discharge, now made so familiar by the work of Hertz. We may, perhaps, think of the process somewhat as follows: If the inrush of positive tubes during the first stage is rapid they will concentrate on the chains in the neighbourhood of that which began the breakdown, and carry them past the unstable point. Then will begin the outrush of negative tubes, and when this is complete the central chains will be discharged and in the condition of Fig. 12 (e), while the surrounding medium will contain negative induction. There will be now an inrush of negative induction into the *locus* of the first discharge, for not only is this free from induction, but also its molecules are suitably arranged to take up negative tubes, and during the second half-period of swing from negative to positive there will be a second change of partners along the same line. And so on with the successive alternations of charge, and there is a tendency, evidently, to keep the same line of discharge. Each change will give atoms at the two end-plates, which will combine either with each other or with the electrodes, or perhaps be taken again into the chains in the following changes. In the sudden changes of partners some of the energy goes to atomic vibration, and we have evidence of this in the atomic radiation which we call spark. There is also energy of translation of the atoms from one partner to another, which appears as heat in the molecules. This heat possibly produces the sound of the spark through the sudden expansion. We have then dissipation of energy as well as the radiation out to space in the Hertzian waves, and the two gradually reduce the electrical energy of the system.

We can see, too, how the amplitude of charge may lessen in the process. For if the negative tubes begin to move out at the middle of each oscillation before the positive tubes have all moved in, or *vice versa*, then the first of the issuing kind will destroy the last of the incoming kind, and the final charge will be diminished by the amount of this overlap. In the extreme case of slow discharge, as through a wet thread, the issuing tubes are neutralised as they come out by the incoming ones, and the motion is dead-beat.

These negative tubes turned out may also supply the negative tubes required in the theory of Prof. J. J. Thomson (*Recent Researches*, Chapter I).

Now let us take a conducting dielectric such as water. As guiding us to an account of what goes on we have the facts (1) that, however small the E.M.F., conduction and presumably electrolysis take place, and no finite difference of potential can be maintained between the electrodes unless we continually supply fresh energy, and (2) that a solute such as sulphuric acid, which appears to combine with the solvent in some way, enormously increases the conductivity. From (1) we gather that some of the molecules must be just ready for change of partners, and from (2) we may at least guess that molecular groups are formed much more complex than the atomic pairs we have hitherto dealt with. We can see how such molecular groups might arise by the straying out and coalescence of induction-tubes of neighbouring molecules, and the consequent formation of new connections. Thus, if two

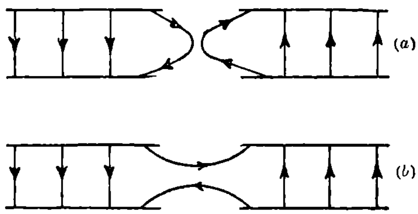


Fig. 17.

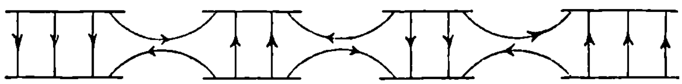


Fig. 18.

pairs come together, as in Fig. 17 (*a*), we may have them connected into a single group, such as (*b*), and any number of molecules may be brought into circuit in the same way, so that we may have a parallel arrangement as in Fig. 18. Or if the coalescence occurs by the approach of two or more groups, such as Fig. 17 (*b*), we may have a series arrangement as in Fig. 19, the kind which we shall suppose to be effective in conduction. We must further suppose that small quantities of acids or salts in solution enormously increase the number of complex molecules. Some of these groups would appear to be more energetic than the initial pairs. Probably on that account they are continually breaking up and re-forming, the energy of translation being no doubt converted at each collision and reformation into energy of electrical separation. There are no doubt all degrees of connection from those of Fig. 17 (*b*) and Fig. 19 (where the result may be obtained practically by adding one closed ring-tube of induction to a series of pair-molecules) to the case where the tubes from each atom go half one way and half the other.

But I imagine that at any given instant only a small fraction of the molecules are thus connected into groups or circuits.

If a tube of induction running from above downwards moves sideways into such a group as that in Fig. 19 or Fig. 20 (*a*), it finds the right-hand side made ready for it, and we may possibly have in succession Fig. 20 (*a*), (*b*) and (*c*), where we suppose that the left-hand side splits up into pairs, while the right-hand side remains threaded on the incoming line. When a liquid contains many such groups, some of the chains of molecules are almost ready-made, and the chains are very easily completed from plate to plate, since the entering tubes have only to furnish a link, as it were, here and there. The 'electric elasticity'

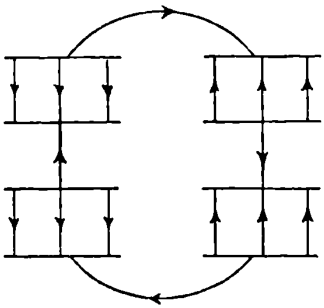


Fig. 19.

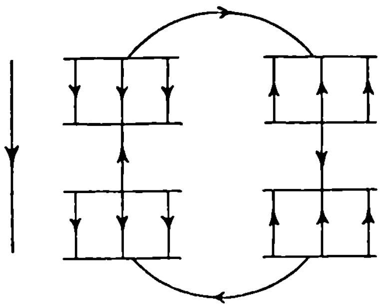


Fig. 20 (*a*).

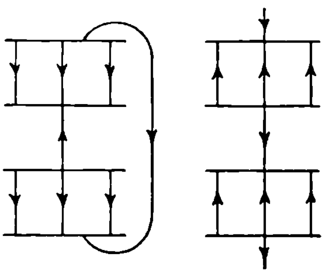


Fig. 20 (*b*).

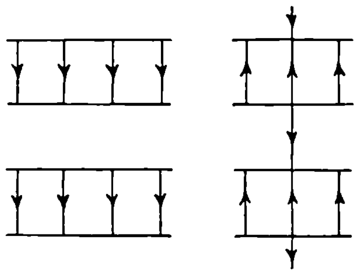


Fig. 20 (*c*).

1 K will be a sort of average of the elasticities or of the difficulties of forming chains in all the various kinds of molecules present. To give a numerical illustration, let us have two condenser-plates in a liquid in which we suppose that in the simply paired molecules, such as those on the left in Fig. 20 (*c*), the value of K is 1·77. Let us suppose also that these occupy 99 per cent. of the paths from plate to plate, while the other 1 per cent. is occupied by very-much-connected molecules with K equal to 7500. Then if the capacity of the condenser in air were C, it would be with this arrangement of molecular paths

$$\frac{99 \times 1{\cdot}77C + 7500C}{100} = 76{\cdot}76C,$$

or the resultant specific inductive capacity is 76·76.

But this is only true in the mass. If we have very minute electromagnetic waves going through the liquid they will, for 99 per cent. of the molecules, use $K = 1{\cdot}77$. For a very large fraction of the remaining 1 per cent. they will probably also use this K, for they are not concerned with the group as a whole, but only with the individual members to which they add or from which they subtract small quantities of induction; and as long as the points representing these members of groups are on the straight part of the characteristic their K for waves is still 1·77. We may therefore expect the value for small waves generally to be very little more than 1·77.

This appears to indicate a possible explanation of the high inductive capacity of such substances as water and alcohol. Accompanying this high value there is generally conductivity and no doubt electrolysis. If we suppose that some of the groups are already so far towards decomposition that half the tubes from each atom run one way and half the other, tubes entering the substance will concentrate on such groups, for a further addition of induction will yield energy instead of requiring it, and the point of instability being passed electrolysis will occur. Perhaps even before the tubes are thus evenly divided, tubes entering the substance may prefer to pass through the groups rather than through the simply paired molecules, for while more energy may be stored in the one half of such a group as that represented in Fig. 20, when a new tube enters in it, less may be required in the discarded half, and so, on the whole, energy may be given up. But it does not seem possible to give any satisfactory account of the process in our ignorance of the real constitution of the complex molecules. All we can say is that some such process probably occurs, inasmuch as conduction does occur even with the smallest external E.M.F.

We may perhaps suppose that in metallic conduction we have a similar process. If the metallic molecular structure is very complicated, with many groups having unstable, or nearly unstable, construction, then tubes of induction entering a metal will select such groups in preference to the more simple stable molecules and electrolyse them. To account for the absence of transfer of atoms along the line of current we must, I think, introduce the supposition of change of charge as already explained on p. 282, and to account for the continuance of conduction we must suppose that there is a continuous formation of new groups as the old ones are broken up. Perhaps we have here some key to the rise of resistance with rise of temperature. As the molecules become more energetic we may expect that the groups will be more broken up by the motions of vibration and translation, and that the number of unstable groups is diminished and their rate of formation is decreased. Hence the entering tubes will find fewer groups ready for them, and if the external E.M.F. remains constant, the rate at which the tubes are dissipated will be decreased. The difficulty of such explanation consists in understanding its inapplicability to electrolytes. Perhaps, too, we have here a hint as to the superior conductivity for heat of metals. If a metal consists largely of many-atomed groups entangled together, and continually breaking up and re-forming, energy supplied to one molecule will, we may imagine, be more readily transferred to its neighbours than if each molecule is self-contained and permanent. This will certainly be the case if energy given to an atom in a molecule is more rapidly transferred to its fellows in the same molecule than to atoms at the same distance in the surrounding molecules. To use an illustration which may put the suggestion in a clearer way, news will be transmitted through a population dwelling in villages and towns much more rapidly than through an agricultural population of the same average density scattered over a country in isolated homesteads.

The theory which I have been trying to set forth may be regarded as a theory of the conservation of induction-tubes, and of their beginning and ending on atoms. That the atoms always have charges on them, and therefore have tubes proceeding to or from them, appears to be generally held as necessary if we accept the electromagnetic theory of light. If the molecules give rise to waves of electric induction they must necessarily be electric systems, and their parts must almost certainly be bound together by electric forces. Whether it is possible for an induction-tube to exist without atoms is at present merely a matter of speculation. At present we know of no such thing. If such a tube exist, it can only be as a closed ring, like a closed ring-tube of magnetic induction round a current, for an unclosed tube would have opposite charges at its ends in free space, and charges not on matter are so entirely outside experience that we cannot accept their existence. The weight of evidence appears to me rather against the view of matter-free induction, and though at first we might be inclined to think that the passage of light-waves across interstellar space implied such induction, yet even in this case we have possibly quite enough matter to supply atomic ends for the tubes to attach themselves to. If we accept the electric discharge theory of comets' tails we apparently assume the existence of enough matter to carry electric induction from a cometic nucleus outwards. I suppose that the theory implies that the nucleus is charged in one way, say positively, and that it has become separated from the matter bearing the negative, which remains far out in space. The sun is itself to be regarded as charged with the same sign as the nucleus, while the corresponding solar negative is also somewhere in space. When the induction of the comet is added to that of the sun the strain is sufficient to break down the feeble insulation of interplanetary space, and a discharge results straight out, or nearly straight out from the nucleus, and this discharge is through the interplanetary matter. But though this appears to be the only reasonable account of the discharge-theory, after all it is bringing little more than a speculation to bolster up another speculation, viz., the existence of matter sufficient to carry waves of induction in the interatomic form wherever light-waves travel.

At first sight this theory of molecular electricity appears to be very different from the chemical dissociation-theory now generally held; but if the dissociated atoms of that theory have charges, they have also tubes of induction proceeding from the charges. When the tubes are taken into account they must, I believe, lead to some such hypothesis as that of which I have attempted an inadequate and imperfect explanation. I am only too deeply conscious of the difficulties unsurmounted. But in working at the subject I have felt all through that, since so much is nearly but not quite explained, there must be hope of progress on these or similar lines if we can only supply some as yet unrecognised idea. Perhaps the very imperfections of my account may stimulate some reader to take up the subject afresh from some better point of view, and, with new ideas, achieve success.

PART III.

WAVE PROPAGATION—RADIATION—PRESSURE OF LIGHT—AND RELATED SUBJECTS.

19.

NOTE ON AN ELEMENTARY METHOD OF CALCULATING THE VELOCITY OF PROPAGATION OF WAVES OF LONGITUDINAL AND TRANSVERSE DISTURBANCES BY THE RATE OF TRANSFER OF ENERGY.

[*Birmingham Phil. Soc. Proc.* **4**, (1885), pp. 55–60.]

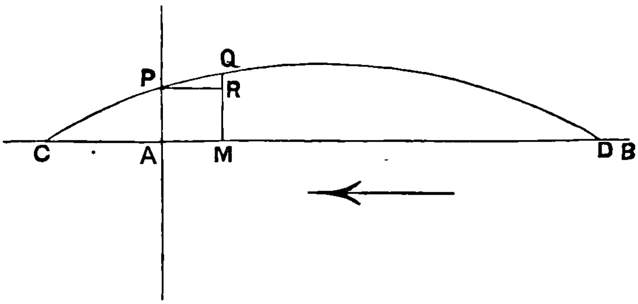
[*Read* Nov. 8, 1883.]

Waves of Longitudinal Disturbance.

A wave of sound may be considered as energy of a particular type, partly potential and partly kinetic, which is being passed on from point to point through the medium, so that the energy which is at any moment occupying a particular portion of space will have passed in a second later to a distance equal to the velocity of sound. Of the two energies the potential is due to the strain of the medium, and, when in this strained condition, each part of the medium exerts force on the neighbouring parts. But it also has kinetic energy, that is, the part considered is in general in motion and there is therefore motion of the point of application of the force which it exerts on the contiguous parts through the existence of its potential energy; that is, it does work and passes on energy to the contiguous parts. If we consider for instance a series of plane waves of sound which move on *unchanged*, the work done in any small time t at any plane perpendicular to the direction in which the sound is moving must be equal to the sound-energy contained in the space immediately behind the plane through which the sound will travel in the time t. This gives us one relation between the various quantities, and we obtain another from the consideration that any condition as to velocity and displacement which is now at a particular point will have travelled on unchanged in the time t to a distance Ut where U is the velocity of sound. From these two relations we can at once find the velocity U*.

Let BA be the direction in which the sound is travelling, and let AP be the trace of a plane perpendicular to AB. Draw a curve $CPQD$ whose height above each point of AB shall represent the displacement of the particle at that point (these displacements are actually of course along AB). Thus AP represents the displacement at A along AC, and MQ the displacement at M along MA.

Consider a small volume V with unit area on the plane through AP as base and height $AM = V$. The volume of the medium which had height V before the disturbance reached A will now be compressed; for the end A has moved forward a distance AP, while the end M has moved forward a greater distance MQ. The compression is therefore the difference between these, viz., $QR = v$, say. But if the sound takes a time t to travel over MA,



after that time the displacement of A will be equal to the present displacement of M. Or if u be the actual velocity of the particle at A,

$$QR = v = ut. \quad \text{.................................(1)}$$

Now, considering the energies, we have the kinetic energy of the volume $V = \rho \frac{Vu^2}{2}$, where ρ is the density of the medium. The potential energy equals the work done in compressing. If P is the original pressure, and $P + p$ the pressure in the compressed state, $P + \frac{p}{2}$ is the average pressure during the compression, and the distance through which this has moved is numerically equal to the diminution of volume v. Then the potential energy is

$$\left(P + \frac{p}{2}\right) v.$$

* This method of treating the subject of wave-propagation is given by Lord Rayleigh in a note in vol. 9, no. 125, of the *Proceedings of the London Mathematical Society* (republished at the end of vol. 2 of his *Theory of Sound*). This paper is merely an application of his method to two particular cases.

The work done in t secs. across unit area at A is equal to the pressure exerted by the medium to the right on the medium to the left, multiplied by the distance which the particle has moved in the time t, or $(P + p)\,ut$. We may equate this to the sum of the two energies, potential and kinetic, contained in V, for this energy is passed across the plane in the time t.

Then $$(P + p)\,ut = \left(P + \frac{p}{2}\right) v + \rho \frac{Vu^2}{2}. \quad \text{.....................(2)}$$

But $ut = v$ by equation (1);

$$\therefore\ (P + p)\,v = \left(P + \frac{p}{2}\right) v + \rho \frac{Vv^2}{2t^2},$$

or $$p = \rho \frac{Vv}{t^2}.$$

But $V = AM =$ (distance travelled by the sound in t) $= Ut$;

$$\therefore\ p = \rho \frac{Vv}{t^2} = \rho \frac{v}{V}\frac{V^2}{t^2} = \rho \frac{v}{V} U^2,$$

or $$U^2 = \frac{1}{\rho}\frac{Vp}{v}.$$

But $\dfrac{Vp}{v}$, or $\dfrac{p}{\frac{v}{V}}$, that is the ratio of a small increase of pressure to the change per unit of volume thereby produced, is the elasticity of the medium. We therefore obtain

$$U^2 = \frac{\text{elasticity}}{\text{density}},$$

or $$\text{velocity} = \sqrt{\text{elasticity}} \div \text{density}.$$

Waves of Transverse Disturbance.

The velocity of propagation of plane waves in which the disturbance is in the plane of the wave can also be easily found by this method. Since the displacements are all perpendicular to the direction of propagation, the force acting across a plane perpendicular to this direction will be entirely tangential and the rigidity of the medium will alone be brought into play.

As before, let the waves be travelling in the direction BA and let the curve $CPQD$ represent the displacement. The curve may now represent the actual displacements since they are perpendicular to AB.

If, as before, u is the velocity of the particle displaced from A, and t the time the wave takes to travel over the small distance MA, then the displacement AP becomes equal to MQ in t, or $QR = ut$.

Now consider the energies in the volume V, with unit area on the plane through AP and height AM numerically equal to V.

If ρ is the density, the kinetic energy is $\rho \frac{Vu^2}{2}$.

The potential energy is equal to the work done in bringing the medium from its normal state into its present state of shear—the angle of shear at A being QPR, and since MA is very small this angle is measured by

$$\frac{QR}{PR} \text{ or } \frac{QR}{V} = \frac{ut}{V}.$$

If G be the modulus of rigidity, the tangential force per unit area is

$$G \times \text{angle of shear} = G\frac{ut}{V}.$$

Now the average value of this force in bringing the shear to its present value is half this, and the distance of displacement of the force is QR. Then the potential energy in the volume V is

$$G\frac{ut}{2V} . QR = G\frac{u^2t^2}{2V}.$$

The work done in t secs. across unit area at A is equal to the tangential force at A multiplied by the distance through which the particle at A moves in t;

or
$$G\frac{ut}{V} . ut = G\frac{u^2t^2}{V}.$$

Equating this to the sum of the two energies, potential and kinetic, contained in V we have

$$G\frac{u^2t^2}{V} = G\frac{u^2t^2}{2V} + \rho\frac{Vu^2}{2},$$

or
$$G\frac{t^2}{V} = \rho V.$$

But $V = AM =$ (distance through which the wave travels in t) $= Ut$;

$$\therefore \frac{G}{\rho} = \frac{V^2}{t^2} = U^2,$$

or
$$U = \sqrt{\text{modulus of rigidity} \div \text{density}}.$$

It follows at once from the above that the energy in any part of the medium is equally divided between the two forms kinetic and potential, for the equation

$$G\frac{u^2t^2}{V} = G\frac{u^2t^2}{2V} + \rho\frac{Vu^2}{2}$$

gives
$$G\frac{u^2t^2}{2V} = \rho\frac{Vu^2}{2},$$

or potential energy in $V =$ kinetic energy in V.

This result also holds for waves of longitudinal disturbance if the initial pressure is zero. That is if we may put $P = 0$ in the equation

$$(P + p)\, v = \left(P + \frac{p}{2}\right) v + \rho\, \frac{Vv^2}{2t^2},$$

for the potential energy is then $\frac{pv}{2}$, and the above equation gives us

$$\frac{pv}{2} = \rho\, \frac{Vv^2}{2t^2},$$

or potential energy in V = kinetic energy in V.

20.

RADIATION IN THE SOLAR SYSTEM: ITS EFFECT ON TEMPERATURE AND ITS PRESSURE ON SMALL BODIES.

[*Phil. Trans.* A, **202**, 1903, pp. 525–552.]

[*Received* June 16. *Read* June 18, 1903.]

PART I.

TEMPERATURE.

When a surface is a full radiator and absorber* its temperature can be determined at once by the fourth-power law if we know the rate at which it is radiating energy. If it is radiating what it receives from the sun, then a knowledge of the solar constant enables us to find the temperature. We can thus make estimates of the highest temperature which a surface can reach when it is only receiving heat from the sun. We can also make more or less approximate estimates of the temperatures of the planetary surfaces by assuming conditions under which the radiation takes place†, and we can determine, fairly exactly, the temperatures of very small bodies in interplanetary space.

These determinations require a knowledge of the constant of radiation and of either the solar constant or the effective temperature of the sun, either of which, as is well known, can be found from the other by means of the radiation-constant. It will be convenient to give here the values of these quantities before proceeding to apply them to our special problems.

* A surface which absorbs, and therefore emits, every kind of radiation is usually described as 'black,' a description which is obviously bad when the surface is luminous. It is much better described as 'a full absorber' or 'a full radiator.'

† This was pointed out by W. Wien in his report on 'Les Lois Théoriques du Rayonnement' (*Congrès International de Physique*, vol. 2, p. 30). He remarks that Stefan's law enables us to calculate the temperatures of celestial bodies which receive their light from the sun, by equating the energy which they radiate to the energy which they receive from the sun, and states that for the earth we obtain nearly the mean temperature, using the reflecting power of Mars, while the temperature of Neptune should be below $-200°$ C.

The Constant of Radiation.

If R is the energy radiated per second per square centimetre by a full radiator at temperature $\theta^\circ A$ (where A stands for the absolute scale), the fourth-power law states that

$$R = \sigma\theta^4,$$

where σ is the constant of radiation.

According to Kurlbaum* the constant is

$$\sigma = 5{\cdot}32 \times 10^{-5} \text{ erg/cm.}^2 \text{ sec. deg.}^4.$$

The Solar Constant.

The solar constant is usually expressed as a number of calories received per minute by a square centimetre held normal to the sun's rays at the distance of the earth. The determinations by different observers differ so widely that it is not necessary for our present purpose to consider whether the constant really exists or whether there are small periodic variations from constancy.

Angström estimated the value as 4 calories per square centimetre per minute, and this value is adopted by Crova as very probable†. When converted to ergs per second this gives

$$S_a = 0{\cdot}28 \times 10^7 \text{ ergs cm.}^2 \text{ sec.},$$

where the suffix denotes that it is Angström's value.

Langley‡ assumed that the atmosphere transmits about 59 per cent. of the energy from a zenith sun, and from his measurement of the heat reaching the earth's surface he estimated the value of the constant at 3 cal./cm.^{2} min. This gives

$$S_l = 0{\cdot}21 \times 10^7 \text{ ergs/cm.}^2 \text{ sec.}$$

Rosetti§ assumed a transmission of 78 per cent. from the zenith sun, but Wilson and Gray|| consider that 71 per cent. represents Rosetti's numbers better than 78 per cent. If in Langley's value we replace 59 per cent. by 71 per cent., we get 2·5 cal./cm.^{2} min. This gives

$$S_r = 0{\cdot}175 \times 10^7 \text{ ergs/cm.}^2 \text{ sec.}$$

* *Wied. Ann.* vol. 65, 1898, p. 748.

† *Congrès International de Physique,* vol. 3, p. 453.

‡ *Phil. Mag.* vol. 15, 1883, p. 153, and *Researches on Solar Heat.*

§ *Phil. Mag.* vol. 8, 1879, p. 547.

|| *Phil. Trans.* A, 1894, p. 383.

The Radiation from the Sun's Surface.

If s is the radius of the sun's surface, R the radiation per square centimetre, then the total rate of emission is $4\pi s^2 R$. This passing through the sphere of radius r, at the distance of the earth and with surface $4\pi r^2$, gives

$$4\pi s^2 R = 4\pi r^2 S,$$

where S is the solar constant.

Hence $$R = \frac{r^2}{s^2} S = \left(\frac{9{\cdot}23 \times 10^7}{4{\cdot}3 \times 10^5}\right)^2 S = 46{,}000S.$$

Corresponding to the three values of S just given we have three values of R, viz.,

$$R_a = 1{\cdot}29 \times 10^{11}; \qquad R_l = 0{\cdot}945 \times 10^{11}; \qquad R_r = 0{\cdot}805 \times 10^{11}.$$

The Effective Temperature of the Sun.

If we equate the sun's radiation to $\sigma\theta^4$, where σ is the radiation-constant, we get θ, the 'effective temperature' of the sun, that is the temperature of a full radiator which is emitting energy at the same rate.

Thus $$5{\cdot}32 \times 10^{-5}\,\theta_a{}^4 = 1{\cdot}29 \times 10^{11},$$

whence $$\theta_a = 7000^\circ\,A \text{ approximately.}$$

Similarly $$\theta_l = 6500^\circ\,A; \qquad \theta_r = 6200^\circ\,A.$$

Wilson* made a direct comparison of the radiation from the sun with that from a full radiator at known temperature. Assuming a zenith transmission of 71 per cent., he obtained $5773^\circ\,A$ as the effective solar temperature. If we put

$$46{,}000S = 5{\cdot}32 \times 10^{-5} \times 5773^4,$$

we get $$S = 0{\cdot}128 \times 10^7.$$

This is no doubt too low a value. Either then Wilson's zenith transmission was less than 71 per cent. or Kurlbaum's constant is too small.

The low value is probably to be accounted for chiefly by the first supposition. Wilson points out that if x is the true value of the transmission, his value of the temperature is to be multiplied by $(71/x)^{\frac{1}{4}}$. If we take $\theta_r = 6200^\circ\,A$ as the true value, then x will be given by

$$x = \left(\tfrac{5773}{6200}\right)^4 \times 71 = 53.$$

This low value is not necessarily inconsistent with the much higher value 71 per cent. used above in finding Rosetti's solar constant, for no doubt the transmission varies widely with time and place, and we have no reason to

* *Roy. Soc. Proc.* vol. 69, 1901–2, p. 312.

assume that 1·77 calories per minute, obtained by Langley, would have been received from the zenith at the time and in the place where Wilson was making his determination.

The Effective Temperature of Space.

In determining the steady temperature of any body as conditioned by the radiation received from the sun, we have to consider whether it is necessary to take into account the radiation from the rest of the sky. If it receives S from the sun, ρ from the rest of the sky, and if its own radiation is R, then in the steady state

$$R = S + \rho \quad \text{or} \quad R - \rho = S.$$

It behaves therefore as if it were receiving S from the sun, but as if it were placed in a fully radiating enclosure of such temperature that the radiation is ρ. This temperature is the 'effective temperature of space.'

The temperature may perhaps be more definitely described as that of a small full absorber placed at a distance from any planet and screened from the sun. Various well-known attempts have been made to estimate this temperature, but the data are very uncertain. The fourth-power law however shows that it is not very much above the absolute zero, if we can assume that the quality of starlight is not very different from that of sunlight.

According to Hermite* starlight is one-tenth full moonlight. Full moonlight is variously estimated in terms of full sunlight. Langley† takes it as $\frac{1}{400000}$. These two values combined give sunlight as 4×10^6 starlight. But starlight comes from the whole hemisphere, while the sun only occupies a small part of it. In comparing temperatures we have to use the brightness of sunlight as if the whole hemisphere were paved with suns.

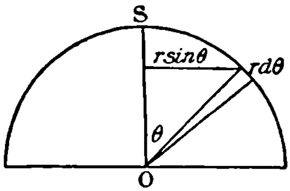


Fig. 1.

If B is the illumination of a surface at O, Fig. 1, lighted by the sun in the zenith at S, and if πs^2 is the area of the sun's diametral plane, then $B/\pi s^2$ is the illumination at O due to each square centimetre. If the hemisphere were all of the same brightness as the sun, the illumination at O due to the ring of sky between θ and $\theta + d\theta$ would be

$$\frac{B}{\pi s^2} 2\pi r^2 \sin\theta \cos\theta \, d\theta,$$

where r is the distance of the sun.

Integrating from $\theta = 0$ to $\theta = \pi/2$, we have

$$\text{Total illumination} = Br^2/s^2 = 46{,}000\, B.$$

* *L'Astronomie*, vol. 5, p. 406.

† 'First Memoir on the Temperature of the Surface of the Moon.' *National Academy of Sciences, Memoirs*, vol. 3, 1884.

The illumination from a hemisphere paved with suns is therefore $46{,}000 \times 4 \times 10^6 = 1{\cdot}84 \times 10^{11}$ times that from the stellar sky.

If we assume that the quality of the radiation is the same in both cases, that is, if we assume that the energy is proportional to the light-part of the spectrum, we have by the fourth-power law

$$\text{Effective temperature of space} = \frac{\text{effective temperature of sun}}{(0{\cdot}184 \times 10^{12})^{\frac{1}{4}}}$$
$$= \frac{\text{effective temperature of sun}}{655}.$$

As the temperature of the sun probably lies between $6000° A$ and $7000° A$, this gives

$$\text{Effective temperature of space} = 10° A.$$

If, then, a body is raised by the sun to even such a small multiple of 10° as, say, 60°, the fourth-power law of radiation implies that it is giving out, and therefore receiving from the sun, more than a thousand times as much energy as it is receiving from the sky.

The sky-radiation may therefore be left out of the account when we are dealing with approximate estimates and not with exact results, and bodies in the solar system may be regarded as being situated in a zero enclosure except in so far as they receive radiation from the sun.

Temperature of a Planet under Certain Assumed Conditions when placed at a Distance from the Sun equal to that of the Earth.

The real earth presents a problem of complexity far too great to deal with. I shall therefore consider an ideal earth for which certain conditions hold, more or less approximating to reality, and determine the temperature of its surface on the assumption that it receives heat from the sun only.

Let us suppose:

1. That the planet is rotating about an axis perpendicular to the plane of its orbit, which is circular.

This will give us too high a temperature at the equator, and the absolute zero, which is too low, at the poles. The mean, however, over the planet will probably be not much affected by the supposition.

2. That the effect of the atmosphere is to keep the temperature in any given latitude the same, day and night.

This is not a great departure from reality. On the sea, which is more than two-thirds of the earth's surface, the daily range is very small, of the order of 1° or 2° C., while even on the land it is, in extreme cases, not more than 15° C., which is not a large fraction of the absolute temperature.

3. That the surface and the atmosphere over it at any one point have one effective temperature as a full radiator. This is no doubt a departure from reality. How wide a departure we have no present means of estimating.

4. That there is no convection of heat from one latitude to another.

This is a very wide departure from reality. But, as we shall see below, the mean temperature of the planet is very little affected by convection, even if we assume that it is so extensive as to make the surface of uniform temperature.

5. That the reflection at each point is $\frac{1}{10}$ of the radiation received.

This is probably of the order of the actual reflection from the earth. According to Langley* the moon reflects about $\frac{1}{8}$ of the radiation received. The earth certainly reflects less. The temperatures determined hereafter are proportional to the 4th root of the coefficient of absorption. Even if this coefficient is as low as 0·9 its 4th root is 0·974. Hence if the actual value is anywhere between 0·9 and 1, the assumed value of 0·9 will not make an error of more than $2\frac{1}{2}$ per cent. in the value of the temperature.

6. That the planet ultimately radiates out all the heat received from the sun, no more and no less.

This again is very near the condition of the real earth, which, on the whole, radiates out rather more than it receives perhaps on the average a calorie per square centimetre in three days.

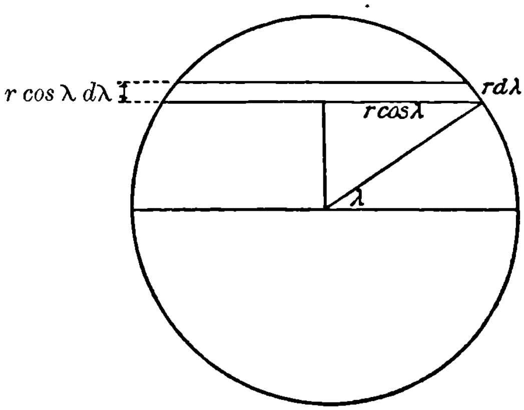


Fig. 2.

Making these six suppositions, let us calculate the temperature of various parts of this ideal planet.

Consider a band between latitudes λ and $\lambda + d\lambda$. The area receiving heat from the sun at any instant, if projected normally to the stream of solar radiation, is (Fig. 2)

$$2r \cos\lambda \, r d\lambda \cos\lambda = 2r^2 \cos^2\lambda d\lambda,$$

where r is the radius of the planet.

* 'Third Memoir on the Temperature of the Moon.' *National Academy of Sc'e ces, Mem irs* vol. 4, part 2, p. 197.

If S is the solar constant, this band is absorbing, with coefficient 0·9,

$$0{\cdot}9S \times 2r^2 \cos^2 \lambda d\lambda.$$

But the band all round the globe is radiating equally, according to the second supposition, and the radiating area is

$$2\pi r \cos \lambda \,.\, r d\lambda = 2\pi r^2 \cos \lambda d\lambda.$$

Hence the radiation emitted per square centimetre per sec. is

$$\frac{0{\cdot}9S \,.\, 2r^2 \cos^2 \lambda d\lambda}{2\pi r^2 \cos \lambda d\lambda} = \frac{0{\cdot}9S \cos \lambda}{\pi}.$$

If the effective temperature in this latitude is θ_λ, we have

$$\frac{0{\cdot}9S \cos \lambda}{\pi} = 5{\cdot}32 \times 10^{-5}\, \theta_\lambda{}^4,$$

or

$$\theta_\lambda = \left(\frac{0{\cdot}9 \times 10^5 S}{5{\cdot}32\pi}\right)^{\frac{1}{4}} \cos^{\frac{1}{4}} \lambda.$$

If we put $\lambda = 0$, we get the equatorial temperature corresponding to each of the different values of S given above, viz.:

$$\begin{array}{lll} \text{Equatorial } \theta_a = 350^\circ A & \text{approximately.} \\ \quad ,, \qquad \theta_l = 325^\circ A & \quad ,, \\ \quad ,, \qquad \theta_r = 312^\circ A & \quad ,, \end{array}$$

The temperature in latitude λ is

$$\theta_\lambda = \text{equatorial temperature} \times \cos^{\frac{1}{4}} \lambda.$$

Thus, in latitude 45°, it is 0·917 × equatorial temperature.

The average temperature over the globe is

$$\frac{2}{4\pi r^2} \int_0^{\frac{\pi}{2}} 2\pi r^2 \cos \lambda\, \theta_E \cos^{\frac{1}{4}} \lambda d\lambda,$$

where θ_E is the equatorial temperature,

$$= \theta_E \int_0^{\frac{\pi}{2}} \cos^{\frac{5}{4}} \lambda d\lambda = \theta_E \frac{\sqrt{\pi}}{2} \frac{\Gamma\left(\frac{9}{8}\right)}{\Gamma\left(\frac{5}{8}\right)} = 0{\cdot}93\theta_E.$$

The average temperature, then, is little more than 1 per cent. above the temperature in latitude 45°.

If we use the three values of θ_E just given, we have

$$\begin{array}{lll} \text{Average } \theta_a = 325^\circ A & \text{approximately.} \\ \quad ,, \qquad \theta_l = 302^\circ A & \quad ,, \\ \quad ,, \qquad \theta_r = 290^\circ A & \quad ,, \end{array}$$

Our fourth supposition was that there is no convection by wind or water from one latitude to another. Let us now go to the other extreme and suppose that the convection is so great that the temperature is practically uniform all over the globe. We then have a receiving surface virtually πr^2, and a radiating surface $4\pi r^2$. Then we get the radiation emitted per square centimetre

$$\frac{0{\cdot}9S\pi r^2}{4\pi r^2} = \frac{9S}{40};$$

and if θ is the temperature required for this,

$$5{\cdot}32 \times 10^{-5}\theta^4 = \frac{9S}{40};$$

whence

$$\begin{aligned} \text{Uniform } \theta_a &= 330^\circ A \text{ approximately,} \\ \text{,, } \quad \theta_l &= 307^\circ A \quad \text{,,} \\ \text{,, } \quad \theta_r &= 293^\circ A \quad \text{,,} \end{aligned}$$

values not more than 5° above those obtained for the average on the supposition of no convection.

Comparing these results with the temperature of the real earth, it is seen at once that they are of the same order.

The average temperature of the earth's surface is usually estimated at about 60° F., say 289° *A*. The temperature of the atmosphere is on the whole decidedly lower than that of the surface below it. We should therefore conclude that the earth's effective temperature is somewhat below 289 *A*.

Again, the earth and the atmosphere, taken as one surface, do not constitute a full absorber, but are to some extent selective. Hence we should expect the earth to be, if anything, of a higher temperature than a full absorber and radiator under the same conditions.

For both these reasons, then, the ideal planet might be expected to have a temperature below rather than above 289 *A*. The lowest estimate obtained above is therefore probably nearest to the truth, and it would appear that even that is somewhat too high. This tends to show that, if we accept Kurlbaum's value of the radiation-constant, we cannot put the solar constant so high as 3 or 4, but must accept a value much nearer to that which I have called Rosetti's value, viz., 2·5.

In what follows I shall therefore take Rosetti's value and the resulting value of the solar temperature, viz., 6200° *A*.

The calculation made above may be turned the other way round, and may be used for a

Determination of the Effective Temperature of the Sun from the Average Temperature of the Earth.

Assuming that the real earth may be replaced by the ideal planet already considered, the radiation per square centimetre from the equatorial band is $\frac{0{\cdot}9S}{\pi}$. But the radiation per square centimetre from the sun's surface is

$46{,}000S$. If then θ_E is the earth's equatorial temperature, and θ_S is the solar temperature,

$$\frac{0{\cdot}9S}{\pi} : 46{,}000S = \theta_E{}^4 : \theta_S{}^4,$$

whence

$$\theta_E = \theta_S/20.$$

The average temperature of the earth is 0·93 of the equatorial temperature. If this average is θ_A, then

$$\theta_A = \theta_S/21{\cdot}5.$$

If we take the temperature of the real earth as 289° A, and as being equal to that of the ideal,

$$\theta_S = 21{\cdot}5 \times 289^\circ = 6200^\circ\ A \text{ approximately.}$$

Upper Limit to the Temperature of a Fully Radiating Surface exposed normally to Solar Radiation at the Distance of the Earth from the Sun.

The highest temperature which a full radiator can attain is that for which its radiation is equal to the energy received. This will only hold when no appreciable quantity of heat is conducted inwards from the surface.

To obtain the upper limit in the case under consideration, we have to equate the radiation to the solar constant, which we shall now take as $S_r = 0{\cdot}175 \times 10^7$. Then

$$5{\cdot}32 \times 10^{-5}\theta^4 = 0{\cdot}175 \times 10^7,$$

whence

$$\theta = 426^\circ\ A.$$

If the surface reflects some of the radiation and absorbs a fraction x of that falling on it, then the effective temperature is

$$x^{\frac{1}{4}} \times 426^\circ\ A.$$

The Limiting Temperature of the Surface of the Moon.

We may apply this result to find an upper limit to the temperature of the moon's surface. This upper limit can only be attained when it is sending out radiation as rapidly as it receives it, and is therefore conducting no appreciable quantity inwards.

We shall take Langley's estimate (*loc. cit.*) of $\frac{\text{reflected radiation}}{\text{emitted radiation}} = \frac{1}{6{\cdot}7}$. This is represented nearly enough by $x = \frac{7}{8}$.

The upper limit of temperature of the surface exposed to a zenith sun is, therefore,

$$\theta = 426 \times (\tfrac{7}{8})^{\frac{1}{4}} = 426 \times 0{\cdot}967 = 412^\circ\ A.$$

This, then, is the upper limit to the temperature of the hottest part of an airless moon.

For a surface with normal at angle λ with the line to the sun,

$$\theta_\lambda = 412 \cos^{\frac{1}{4}} \lambda.$$

If we take this as the law of temperature of the side of the moon exposed to the sun, we can find the effective temperature of the full moon as seen from the earth, i.e., the uniform temperature of a flat disc of radius equal to that of the moon, sending to us the same total radiation.

If $N d\omega$ is the normal stream of radiation from 1 cm.^{2} of surface of the moon immediately under the sun sent out through a cone of angle $d\omega$, that sent out in direction λ to the normal is $N \cos \lambda d\omega$. But 1 cm.^{2} on the moon's surface, with normal inclined at λ to the sun's rays, only receives $\cos \lambda$ of the radiation received by the surface immediately under the sun. It therefore sends in the direction of the earth, also at λ to the normal, only $N \cos^2 \lambda d\omega$. Hence the total radiation to the earth, obtained by integrating, is

$$\int_0^{\frac{\pi}{2}} N \cos^2 \lambda \,.\, \frac{2\pi m^2 \sin \lambda d\lambda}{r^2}$$

where m is the radius of the moon and r is its distance from the earth,

$$= \frac{2\pi}{3} \frac{m^2}{r^2} N.$$

Let N_D be the normal stream from the equivalent flat disc, then

$$\frac{\pi m^2 N_D}{r^2} \quad \frac{2\pi}{3} \frac{m^2}{r^2} N,$$

whence $$N_D - \frac{2}{3} N.$$

The effective temperature of the flat disc is therefore $\sqrt[4]{\frac{2}{3}}$ that of the surface immediately under the sun at the same distance from it.

Then the effective average $= 412 \times \sqrt[4]{\frac{2}{3}} - 412 \times 0{\cdot}9 \quad 371$ A. The upper limit, then, to the average effective temperature of the moon's disc is just below that of boiling water.

This is very considerably above Langley's estimate, that the surface of the full moon is a few degrees above the freezing-point. There can be no doubt that a very appreciable amount of heat is conducted inwards. The observations during eclipses by Langley* and by Boeddicker show that some heat is still received from the moon's surface when it has entered the full shadow, and that it takes time after the eclipse has passed to establish a steady temperature again. It might be possible to make some rough estimate of the amount conducted inwards from the Fourier equation, but the problem is not an easy one. Perhaps we get the best estimate by comparing the actual temperature with that found above.

* 'Third Memoir,' p. 159.

If the actual temperature is taken as about $\frac{4}{5}$ of the upper limit, say $297^\circ A$, then the radiation outwards is of the order $(\frac{4}{5})^4 = 0{\cdot}41$ of that where no conduction exists. Then nearly $\frac{3}{5}$ of the heat is probably conducted inwards.

If the moon always turned the same face to the sun instead of to the earth, the upper limit would be approached.

Temperature of a Spherical Absorbing Solid Body of the Order 1 *cm. in diameter at the Distance of the Earth from the Sun.*

The calculation of the temperature of such a body is interesting for two reasons. Firstly, the body will be at nearly the same temperature throughout, and secondly, as we shall show in the second part of this paper, the mutual repulsion of two such bodies, due to the pressure of their radiation, is of the same order as their gravitative attraction.

If the radius of the body is a, its effective receiving area is πa^2, and it receives

$$\pi a^2 S \text{ ergs/sec.}$$

Its radiating surface is $4\pi a^2$, and therefore its average radiation per square centimetre per sec. in the steady state is

$$\pi a^2 S / 4\pi a^2 = \tfrac{1}{4} S.$$

If we take $S = 2{\cdot}5$ cal./cm.^{2} min. or $0{\cdot}04$ cal./cm.^{2} sec., and if the conductivity is of the order of that of terrestrial rock lying, say, between $0{\cdot}01$ and $0{\cdot}001$, it is evident that a difference of temperature of only a few degrees between the receiving and the dark surfaces will convey heat sufficient to supply radiation, $0{\cdot}01$ cal. cm.^{2} sec., equal to the average. Thus, if the conductivity is $0{\cdot}001$ and the diameter is 1 cm., a difference of temperature of 10° suffices.

We may therefore take the temperature of the surface as approximately uniform when the steady state is reached. Let the temperature be θ, and let the solar temperature be θ_S. Then we have

$$\theta^4 : \theta_S{}^4 = \frac{S}{4} : 46{,}000 S$$

and

$$\theta = \frac{\theta_S}{20{\cdot}7}\,.$$

If

$$\theta_S = 6200^\circ\ A,$$

$$\theta = 300^\circ\ A \text{ approximately.}$$

This will be the temperature of fully absorbing bodies of diameter less than 1 cm., so long as they are not too small to absorb the radiation falling on them.

Variation of Temperature with Distance from the Sun.

Since the radiation received varies inversely as the square of the distance from the sun, that given out varies in the same ratio. The temperature of the radiating surface varies therefore as the fourth-root of the inverse square, that is inversely as the square-root of the distance.

This enables us to deduce at once the temperatures of the various surfaces and bodies which we have considered, if placed at the distances of different planets as well as at the distance of the earth. We have merely to multiply the results hitherto found by $\sqrt{\frac{\text{earth's distance}}{\text{planet's distance}}}$.

The following table contains the values of the temperatures at selected distances, all on the absolute scale:

Table of Temperatures of Surfaces at Different Distances from the Sun. All on the Absolute Scale.

I	II	III	IV	V	VI	VII	VIII	IX
At the distance of the planet	Distance. Earth's distance = 1	Square-root of (distance)$^{-1}$	Equatorial temperature of ideal planet	Average temperature of ideal planet	Upper limit of a surface reflecting one-eighth under zenith sun	Average temperature of equivalent disc	Temperature four-fifths that of equivalent disc	Temperature of small absorbing sphere
Mercury	0·3871	1·61	502	467	664	598	478	483
Venus	0·7233	1·18	368	342	486	438	350	358
Earth	1·0000	1·00	312	290	412	371	297	300
Mars	1·5237	0·81	253	235	337	300	240	243
Neptune	30·0544	0·18	56	52	74	67	53	54

We have omitted the larger planets except Neptune, since in all probability they radiate heat of their own in considerable proportion. Neptune is inserted merely to show how low temperatures would be at his distance if there were no supply of internal heat.

The results given in the table may not be exactly applicable to any of the planets, but they at least indicate the order of temperature which probably prevails.

If, for instance, Mars is to be regarded as having an atmosphere with regulating properties like our own, his equatorial temperature (Column IV) is probably far below the temperature of freezing water, and his average temperature (Column V) must be not very different from that of freezing mercury. If, on the other hand, we suppose that his atmosphere has no regulating power, we get the upper limits not very different from those in

Columns VI and VII. These are the limits for the bright side, and they imply nearly absolute zero on the dark side. If we regard Mars as resembling our moon, and take the moon's effective average temperature as 297° *A*, the corresponding temperature for Mars is 240° *A*, and the highest temperature is $\frac{4}{5} \times 337 = 270°$. But the surface of Mars has probably a higher coefficient of absorption than the surface of the moon—it certainly has for light—so that we may put his effective average temperature on this supposition some few degrees above 240° *A*, and his equatorial temperature some degrees higher still.

It appears exceedingly probable, then, that whether we regard Mars as like the earth, or, going to the other extreme, as like the moon, the temperature of his surface is everywhere below the freezing-point of water. The only escape from this conclusion that I can see is by way of a supposition that an appreciable amount of heat is issuing from beneath his surface.

We cannot draw any definite conclusions as to the temperatures of Mercury and Venus till we know whether they have atmospheres and whether they rotate on their own axes. If we make both these suppositions and further suppose that their conditions approximate to those (given in Columns IV and V) of the ideal planet at their distances, then they may well be surrounded by hot clouds, as is sometimes supposed, entirely screening their solid bodies from us. If, on the other hand, their atmospheres are ineffective as regulators and if they always present the same face to the sun, the hottest part of Mercury is probably not far from 650° *A*, and that of Venus not far from 500° *A*.

If a comet consist of small solid particles of diameter of the order 1 cm. or less, then the temperatures of these particles are given in Column IX. At one-quarter of the earth's distance, say 23 million miles from the sun, the temperature is 600 *A*, about the melting-point of lead. At one-twenty-fifth, say $3\frac{3}{4}$ million miles, it will be about 1500° *A*, say the melting-point of cast-iron. Nearer than this the temperature no doubt increases rapidly, but the law of temperature, deduced from the inverse-square law for the radiation received, requires amendment, as that law was based on the supposition that a hemisphere only is lighted by the sun, and that the whole of his disc is visible from every part of that hemisphere. Both of these suppositions cease to hold when the distance from the sun is only a small multiple of his radius.

PART II.

Radiation-Pressures.

The pressure of radiation against a surface on which it falls, first deduced by Maxwell from the Electromagnetic Theory of Light, is now established on an experimental basis by the work of Lebedew, confirmed by that of Nichols and Hull.

Though this pressure was first deduced as a consequence of the Electromagnetic Theory, Bartoli showed, independently, that a pressure must exist without any theory as to the nature of light beyond a supposition which may perhaps be put in the form that a surface can move through the ether, doing work on the radiation alone and not on the ether in which the radiation exists. Professor Larmor* has given a proof of this pressure and has shown that it has the value assigned to it by Maxwell, viz., that it is numerically equal to the energy-density in the incident wave, whatever may be the nature of the waves, so long as their energy-density for given amplitude is inversely as the square of the wave-length. We may, in fact, regard a pencil of radiation as a stream of momentum, the direction of the momentum being the axis of the pencil. If E is the energy-density of the pencil, U its velocity, the momentum-density may be regarded as $E\ U$.

If the stream of radiation is being emitted by a surface, the surface is losing the momentum carried out with the issuing stream, and is so being pressed backwards. If the stream is being absorbed by the surface, then it is gaining the momentum and is still being pressed backwards, the forces being in the line of propagation.

As the expressions for the radiation-pressure in various cases are probably not very well known, it may be convenient to state them here for use in what follows.

Values of Radiation-Pressure in Different Cases.

If 1 cm.² of a full radiator is emitting energy R per second, and if $N d\omega$ is the energy it is emitting through a cone $d\omega$, with axis along the normal, then in direction θ its projection is $\cos\theta$, and it is emitting $N\cos\theta\, d\omega$ through a cone $d\omega$. Putting $d\omega = 2\pi\sin\theta\, d\theta$, and integrating over the hemisphere, we have

$$R = \int_0^{\frac{\pi}{2}} N\cos\theta \,.\, 2\pi\sin\theta\, d\theta \qquad \pi N.$$

If we draw a hemisphere, radius r, round the source as centre, the energy falling on area $r^2 d\omega$ is $N\cos\theta\, d\omega$ per second, and, since the velocity is U cm. per second, the energy-density just outside the surface on which it falls is $N\cos\theta/Ur^2$, and this is the rate at which the momentum is being received, that is, it is the normal pressure. The total force on area $r^2 d\omega$ is $N\cos\theta\, d\omega\ U$. This is the momentum sent out per second by the radiating square centimetre through the pencil with angle $d\omega$, in the direction θ, and is therefore the force on the square centimetre due to that pencil.

Resolving along the normal and in the surface we have

$$\text{Normal pressure } = N\cos^2\theta\, d\omega/U,$$

$$\text{Tangential stress} = N\cos\theta\sin\theta\, d\omega/U.$$

* *Brit. Assoc. Report*, 1900; *Encyc. Brit.* vol. 32, Art. 'Radiation.'

Putting $d\omega = 2\pi \sin\theta d\theta$ and integrating over the hemisphere, we get

$$\text{Total normal pressure} = \int_0^{\frac{\pi}{2}} (N \cos^2\theta \,.\, 2\pi \sin\theta\, d\theta/U) = 2\pi N/3U = 2R/3U.$$

Total tangential stress = 0, since the radiation is symmetrical about the normal.

If the surface is receiving radiation, let us suppose that the stream is a parallel pencil S ergs per second per square centimetre held normal to the stream, and that it is inclined at an angle θ to the normal to the receiving surface. The momentum received per second is $S\cos\theta/U$. This produces

$$\text{Normal pressure} = S\cos^2\theta/U,$$
$$\text{Tangential stress} = S\cos\theta\sin\theta/U.$$

If the stream is entirely absorbed both these forces exist.

If the stream is entirely reflected, the reflected pencil exerts an equal normal force and an equal and opposite tangential force, and we have only normal pressure of amount $2S\cos^2\theta/U$.

If only a fraction μ is reflected, the incident and reflected streams will give

$$\text{Normal pressure} = (1+\mu)\, S\cos^2\theta/U,$$
$$\text{Tangential stress} = (1-\mu)\, S\cos\theta\sin\theta/U.$$

To the normal pressure must be added the pressure due to the radiation emitted from the surface.

Radiation-Pressure in Full Sunlight.

If a full absorber is exposed normally to the solar radiation at the distance of the earth the pressure on it is S/U, or $\dfrac{0{\cdot}175 \times 10^7}{3 \times 10^{10}} = 5{\cdot}8 \times 10^{-5}$ dyne/cm.^{2}.

The Radiation-Pressures between Small Bodies. Comparison with their mutual Gravitation.

It is well known that the radiation-force on a small body, exposed to solar radiation, does not decrease so rapidly as gravitative pull on the body when its size decreases. If the body is a sphere of radius a and density ρ, and with a fully absorbing surface, and if it is so small that it is practically at one temperature all through, it is receiving a stream of momentum

$$\pi a^2 S/U$$

directed from the sun. Its own radiation outwards being equal in all directions has zero resultant pressure.

The gravitative acceleration towards the sun at the distance of the earth is about 0·59 cm./sec.². Then we have

$$\frac{\text{Radiation-pressure}}{\text{Gravitation-pull}} = \frac{\pi a^2 S}{U \times \frac{4}{3}\pi a^3 \rho \times 0{\cdot}59}.$$

The two will be equal when

$$a = \frac{3}{4}\,\frac{S}{U\rho \times 0{\cdot}59}.$$

If we put $\rho = 1;\quad S = 0{\cdot}175 \times 10^7;\quad U = 3 \times 10^{10};$

we get $a = 74 \times 10^{-6}.$

This is the well-known result that a body of diameter about two wave-lengths of red light would be equally attracted and repelled if we could assume that a surface so small still continued to absorb. But, of course, when we are getting to dimensions comparable with a wave-length that assumption can no longer be made.

It is not, I think, equally well recognised that if the radiating body is diminished in size, the radiation-pressure due to it also decreases less rapidly than the gravitative pull which it exerts. For the radiation decreases as the square of the radius of the emitting body and its gravitative pull as the cube.

We can easily compare the forces due to radiation and gravitation between two bodies, if for simplicity we assume that their distance apart is very great compared with the radius of either.

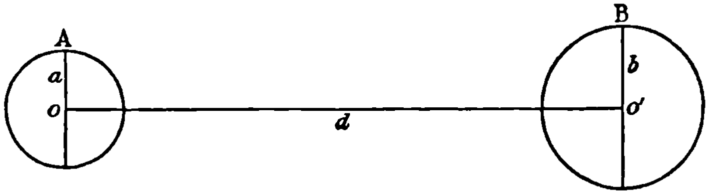


Fig. 3.

Let A, B, Fig. 3, be two spheres with full radiating surfaces. Let their radii be a, b, and let their centres o, o' be d apart. If this distance is great compared with a and b, each may be regarded as receiving a parallel stream from the other.

Let A send out a normal stream $N\,d\omega$ per square centimetre through cone $d\omega$, while B sends out $N'\,d\omega$.

B receives the stream of cross-section πb^2 or the angle of the cone is $\pi b^2\ d^2$, and it issues virtually from area πa^2, for at B, A will appear as a uniformly bright flat disc.

Then the total force on B is

$$F = \frac{\pi a^2 N}{U} \times \frac{\pi b^2}{d^2} = \frac{\pi a^2 b^2 R}{U d^2},$$

where

$$R = \pi N.$$

The force on A due to B is $\pi a^2 b^2 R'/Ud^2$, where $R' = \pi N'$.

These are not equal unless $R = R'$, i.e., unless the two bodies have the same temperature, an illustration of the fact that equality of action and reaction does not hold between the radiating and receiving bodies alone. They no longer constitute the whole of the momentum-system. The ether, or whatever we term the light-bearing medium, is material, and takes its part in the momentum-relations of the system.

If the surfaces are partially or totally reflecting, the forces are easily obtained. Thus if one is totally reflecting, it can be shown that the force is only half as great as when it is fully absorbing. But it will be sufficient to confine ourselves to the case of complete absorption, followed by radiation of the absorbed heat equally in all directions from all parts of the surface. More general assumptions do not alter the order of the forces found.

If G is the constant of gravitation $= 6{\cdot}67 \times 10^{-8}$, and if ρ, ρ' are the densities of A and B, the gravitation-pull, P, is $G \dfrac{16\pi^2 a^3 b^3 \rho\rho'}{9d^2}$.

Then on B

$$\frac{\text{Radiation-push } F}{\text{Gravitation-pull } P} = \frac{9\pi a^2 b^2 R}{16 G U \pi^2 a^3 b^3 \rho\rho'},$$

or

$$\frac{F}{P} = \frac{9R}{16 G U \pi a b \rho\rho'}.$$

If $a - b$; $\rho = \rho'$; $R = 5{\cdot}32 \times 10^{-5}\theta^4$, we have

$$a = \frac{0{\cdot}69 \times 10^{-4}\theta^2}{\rho} \sqrt{\frac{P}{F}}\ *.$$

If we suppose the two bodies to have the temperature of the sun, say 6200 A, and its density, say 1·375, then $F = P$, when $a = 1930$ cm. or 19·3 metres†.

Of course two globes of this size would soon cool far below the temperature of the sun, even if for an instant they could be raised up to it.

If we suppose $\theta = 300°$ A—the approximate temperature of small bodies at the distance of the earth from the sun—and if we take $\rho = 1$, then $F = P$ when $a = 6{\cdot}2$ cm.†

* [The original has 2·18, instead of 0·69, which is a slip due to the omission of $\sqrt{10}$. The density of the sun is also wrongly taken as 0·25 instead of 1·375 (see Art. 65, p. 709). This necessitates some corrections in the succeeding part of the paper, which have in all cases been marked with a †. Ed.]

† [See note above. Ed.]

Thus two globes of water—probably nearly full absorbers at 300° A—will at that temperature neither attract nor repel each other if their radii are about 6 cm.†

If the density of the spheres is 11, about that so often used for masses in the Cavendish experiment, $F = P$ when

$$a = 0{\cdot}564 \text{ cm.}\dagger$$

This does not throw any doubt on the results of Cavendish experiments, for it only holds when the radiators are in an enclosure of very low absolute temperature. In all Cavendish experiments the greatest care is taken to make the attracted body and its enclosure of one uniform temperature.

The really interesting case is that of two small meteorites, in interplanetary space. To judge from the specimens which succeed in penetrating the earth's atmosphere they are very dense. Let us suppose them to have density 5·5 that of the earth—and temperature 300° A, that which they will have at the earth's distance. Then $F = P$ when

$$a = 1{\cdot}13 \text{ cm.}\dagger$$

If the radii of the bodies are less than the values found for equality of F and P in the different cases, the net effect is repulsion.

The ratio of F to P is inversely as the square of the radius, so that, as the radii are decreased from the values giving $F - P$, the radiation-repulsion soon becomes enormously greater than the gravitation-pull, and the latter may be neglected in comparison. Thus for two drops of water at 300 A in a zero enclosure, with radii 0·001 cm., the pressure is nearly 40,000,000 times the pull†.

It is not, however, that the radiation-force is great, or even its acceleration. The force becomes exceedingly minute, but the gravitation much more minute.

Thus consider two drops of water at 300° A placed in a zero enclosure at a distance $d = 10a$ apart. Our assumption of parallel radiation from one to the other is now only a rough approximation, but the result will be of the right order.

The radiation-push is $\pi a^4 R/Ud^2$, and the acceleration is

$$3aR/4Ud^2 = \frac{1}{10^7} \times \frac{1}{a} \text{ approximately.}$$

This only becomes considerable when the drops approach molecular dimensions, and long before this they cease to absorb fully the stream of momentum falling on them. Still, even molecules are selective absorbers, and absorb especially each other's radiations. And we may expect that if two gas-molecules collide and set each other radiating much more violently than before, they will be practically in an enclosure of much lower temperature than their own, and their mutual radiation may result in very rapid repulsion —repulsion of the order of the fourth power of the temperature reached.

† [See note on p. 320. ED.]

Radiation-Pressure between Small Bodies at Different Distances from the Sun.

We have seen above, that if two small spheres of density 5·5 are at the distance of the earth from the sun, their gravitation will be balanced by their radiation-pressure when the radius of each is 1·13 cm.† Now the balancing radius is proportional to the square of the temperature, that is, inversely proportional to the distance, since the temperature (Part I) is inversely as the square-root of the distance. Thus, at the distance of Mercury, the radii would be about 3 cm.†; a million miles from the sun's surface they would be about 100 cm.†; out at Neptune they would be about 0·4 mm.†

We see then that the mutual action between small bodies of density that of the earth, will, at different distances, change sign for different sizes of body, ranging from something of the order of 2 metres diameter† near the sun to the order of 1 mm. diameter† at the distance of Neptune. A ring of small planets, each of radius 1·13 cm.†, and density 5·5, would move round the sun at the distance of the earth without net mutual attraction or repulsion, and each might be regarded as moving independently of the rest. It appears possible that if Saturn is hot enough, considerations of this kind may apply to his rings.

The repulsion between small colliding bodies, even if not heated by the sun, must lead to some delay in their final aggregation. This is obvious when there are only two small bodies, and their temperature is very considerably raised by the collision. But there is also delay if instead of a single pair we suppose two swarms to collide. Near the boundary of the colliding region, a body will experience radiation-pressure chiefly on one side, and will tend to be driven out of the system. Of course, if the swarms are so dense that a member near the outside cannot see through the rest, this effect will be less. A body in front of another entirely screens its radiation, but the gravitation is not screened. Hence, a body near the boundary of a densely-packed region of collision may be repelled only by the colliding bodies just round it, while it will be attracted by all; or, to put the same idea in another way, a body in a spherical swarm of uniform temperature will only be pulled equally in all directions at the centre of the swarm, but it will be equally repelled in all directions as soon as it is sufficiently deep to be surrounded by its fellows wherever, so to speak, it looks.

Inequality of Action and Reaction between Two Mutually Radiating Bodies.

We have seen that two distant spheres push each other with forces $\pi a^2 b^2 R / U d^2$ and $\pi a^2 b^2 R' / U d^2$, and that these, though opposite, are not equal unless $R = R'$.

It would be easy to imagine cases in which the forces were not even opposite or in the same directions. At first sight, then, it would appear that

† [See footnote, p. 320. ED.]

we have two bodies acting upon each other with unequal forces, but of course this statement is inexact. The bodies do not act upon each other at all; each sends out a stream of momentum into the medium surrounding it. Some of this momentum is ultimately intercepted by the other, and in its passage the momentum belongs neither to one body nor to the other. If we assume that the momentum is conserved, and of course everything in the methods of this paper depends on that assumption, the action on one of the bodies is equal and opposite to the reaction on the light-bearing medium contiguous to it. There is no failure of the law of action and reaction, but an extension of our idea of matter to include the medium. There should be no difficulty in this extension; indeed, we have made it long ago in endowing the medium with energy-carrying properties. Whether the momentum in the medium is in the form of mass m moving with velocity v in the direction of propagation is perhaps open to doubt. We may, perhaps, have different forms of momentum just as we may have different forms of energy, and possibly we ought not to separate the momentum in radiation into the factors m and v, but keep it for the present as one quantity M.

An interesting example of inequality of the radiation-forces on two mutually radiating bodies is afforded by two equal spheres, for which, at a given temperature, the radiation-push F balances the gravitation-pull P. Raise one in temperature so that the push on the other becomes F'. Lower the other so that the push on the first becomes F'', but adjust so that

$$F' + F'' = 2F = 2P,$$

then

$$P - F'' = F' - P.$$

There will then be equal accelerations of the two in the same, *not* in opposite directions, and a chase will begin in the line joining the centres, the hotter chasing the colder. If the two temperatures could be maintained, the velocity would go on increasing; but the increase would not be indefinitely great, inasmuch as a Doppler effect would come into play. Each sphere moving forward would crowd up against the radiation it emitted in front, and open out from the radiation it emitted backwards. This would increase the front and decrease the back pressure, and ultimately the excess of front pressure would balance the accelerating force due to mutual radiation.

Let us examine the effect of motion of a radiating surface on the pressure of its radiation against it.

Application of Doppler's Principle to the Radiation-Pressure against a Moving Surface.

If a unit area A, Fig. 4, is moving with velocity u in any direction AB, making angle ψ with its normal AN, the effect on the energy-density in the stream of radiation issuing in any direction AP is two-fold. If the motion is such as to shorten AP, the waves and their energy are crowded up into less space, and if such as to lengthen AP, they are opened out. At the same time, in the one case A is doing work against the radiation-pressure and in the other is having work done on it. We shall assume, as in the thermodynamic theory of radiation, that this work adds to or subtracts from the energy of radiation. Both effects, (1) the crowding, and (2) the work done, or the reverse of each, combine to alter the energy and therefore the radiation-pressure. We have no data by which we can determine whether the motion alters the rate at which the surface is emitting radiation, but it appears worth while to trace consequences on the assumption that the radiation goes on as if the surface were at rest*, but that it is crowded up into less space or spread over more, and that we can superpose on this the energy given out to, or taken from, the stream by the work done by, or on, the moving surface by the radiation-pressure. This work can evidently be calculated to the first order of approximation by supposing the pressure equal to its value when the surface is at rest.

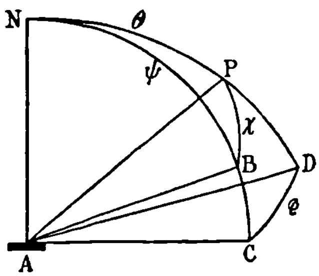


Fig. 4.

Let us draw from A as centre a sphere of radius U, equal to the velocity of radiation. The energy which, in a system at rest, would be radiated into a cone with A as vertex, length U, and solid angle $d\omega$, in the direction AP making an angle χ with the direction of motion AB, will now be crowded up into a cone of length $U - u\cos\chi$, since $u\cos\chi$ is the velocity of A in the direction AP. We shall suppose that u/U is very small. Hence the energy-density in the cone is increased in the ratio $U + u\cos\chi : U$ or by the factor

$$1 + \frac{u\cos\chi}{U}.$$

Considering now the effect of the work done, the force on A due to the stream in $d\omega$ is $N\cos\theta\, d\omega/U$, and the work done in one second is

$$(N\cos\theta\, d\omega/U) \times u\cos\chi.$$

When A is at rest the energy in this cone is

$$N\cos\theta\, d\omega.$$

* *Added August* 20, 1903. Since the above was written Professor Larmor has pointed out to me that the results obtained in the text from this assumption, along with the hypothesis of crowding of the radiation and its increase by an amount equivalent to the work of the radiation-pressure, can be justified by an argument based on the following considerations. A perfect reflector moving with uniform speed in an enclosure. itself also moving at that speed. and so in a steady state, must send back as much radiation of every kind as a full radiator in its place. Now the electrodynamics of perfect reflection are known; hence the effect of motion of a full radiator on the amount of its radiation can be determined. The result is equivalent to the statement that the amplitudes of the excursions of the optical vibrators are the same at the same temperature whether the source to which they belong is moving or not.

When A is moving it is increased to

$$N \cos \theta d\omega + \frac{N \cos \theta d\omega}{U} u \cos \chi,$$

that is
$$N \cos \theta d\omega \left(1 + \frac{u \cos \chi}{U}\right).$$

Thus the effect of the work done is equal to that of the crowding, and the energy-density on the whole is increased in the ratio

$$1 + \frac{2u \cos \chi}{U} : 1.$$

The pressure is increased in the ratio of the energy-density*. Then the force on A due to the radiation through $d\omega$ is increased from

$$\frac{N \cos \theta d\omega}{U} \text{ to } \frac{N \cos \theta d\omega}{U}\left(1 + \frac{2u \cos \chi}{U}\right).$$

If we resolve this along the normal to the surface A and integrate over the hemisphere we obtain the total normal pressure. As we only want to know the change in pressure P we may neglect the first term which gives the pressure on A at rest, and we have

$$P = \int \frac{N \cos^2 \theta}{U} \cdot \frac{2u \cos \chi}{U} d\omega.$$

If ϕ is the angle between the normal planes through B and P we have

$$\cos \chi = \cos \theta \cos \psi + \sin \theta \sin \psi \cos \phi.$$

Putting $d\omega = \sin \theta d\theta d\phi$, we get

$$\begin{aligned} P &= \int_0^{\frac{\pi}{2}} \int_0^{2\pi} \frac{2Nu}{U^2} \cos^2 \theta \sin \theta \left(\cos \theta \cos \psi + \sin \theta \sin \psi \cos \phi\right) d\theta d\phi \\ &= \frac{\pi N u \cos \psi}{U^2} = \frac{Ru \cos \psi}{U^2}. \end{aligned}$$

The change in the tangential stress, T, is evidently in the direction AC, that of the component of u in the plane of A.

We may therefore resolve each element of tangential stress in the direction AC. Omitting the first term again, since in this case it disappears on integration, the element due to $d\omega$ in the direction AP will contribute

$$\frac{N \cos \theta \sin \theta \cos \phi}{U} \cdot \frac{2u \cos \chi}{U} d\omega,$$

and integrating over the hemisphere we have

$$\begin{aligned} T &= \int_0^{\frac{\pi}{2}} \int_0^{2\pi} \frac{2Nu}{U^2} \cos \theta \sin^2 \theta \cos \phi \left(\cos \theta \cos \psi + \sin \theta \sin \psi \cos \phi\right) d\theta\, d\phi \\ &= \frac{\pi N u \sin \psi}{2U^2} = \frac{Ru \sin \psi}{2U^2}. \end{aligned}$$

* [See note, p. 330. Ed.]

Force on a Sphere moving with Velocity 'u' in a Given Direction.

If a sphere, radius a, is moving with velocity u, we may from symmetry resolve the forces on each element in the direction of motion. The resolutes will be $P \cos \psi$ and $T \sin \psi$. Evidently it is sufficient to integrate over the front hemisphere and then double the result. We have the

$$\text{Retarding Force} = 2\int_0^{\frac{\pi}{2}} \left(\frac{Ru \cos^2 \psi}{U^2} + \frac{Ru \sin^2 \psi}{2U^2}\right) 2\pi a^2 \sin \psi d\psi$$
$$= \tfrac{8}{3} \frac{Ru}{U^2} \cdot \pi a^2.$$

It is noteworthy that one half of this is due to the normal, the other half to the tangential stresses.

If the sphere has density ρ the acceleration is obtained by dividing by $\frac{4}{3}\pi a^3 \rho$, then

$$du/dt = -\ 2Ru/U^2 \rho a.$$

Effect on Rotation.

If the sphere radius a is rotating with angular velocity ω, then any element of the surface, λ from the equator, is moving with linear velocity $a\omega \cos \lambda$ in its own plane. This does not affect the normal pressure, but it introduces a tangential stress opposing the motion

$$Ru/2U^2 = Ra\omega \cos \lambda / 2U^2.$$

Taking moments round the axis and integrating over the sphere, we obtain a couple

$$\tfrac{4}{3}\pi a^3 \rho \,.\, \tfrac{2}{5} a^2 \frac{d\omega}{dt} = \frac{Ra\omega}{2U^2} \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} 2\pi a^3 \cos^3 \lambda \, d\lambda,$$

whence $$d\omega/dt = -\ \tfrac{5}{2} R\omega / U^2 \rho a.$$

The rate of diminution of ω is therefore of the same order as that of u.

To obtain an idea of the magnitude of the retardation of a moving sphere, let us suppose that one is moving through a stationary medium. Let its radius be $a = 1$ cm., its density $\rho = 5{\cdot}5$, its temperature 300° A.

Then $$\frac{1}{u}\frac{du}{dt} = -\frac{2 \times 5{\cdot}32 \times 10^{-5} \times 300^4}{9 \times 10^{20} \times 5{\cdot}5}$$
$$= 1{\cdot}75 \times 10^{-16}.$$

This will begin to affect the velocity by the order of 1 in 10,000 in, say, 10^{12} seconds, or taking the year as $3{\cdot}15 \times 10^7$ seconds, in about 30,000 years.

The effect is inversely as the radius, so that a dust-particle 0·001 cm. radius will be equally affected in 30 years.

The effect is as the fourth power of the temperature, so that with rising temperature it becomes rapidly more serious.

Equation to the Orbit of a Small Spherical Absorbing Particle Moving in a Stationary Medium Round the Sun.

It is evident from the above result, that the effect of motion on radiation-pressure may be very considerable in the case of a small absorbing particle moving round the sun.

We shall take the particle as spherical, of radius a and distance r from the sun. We shall suppose the radius so small that the particle is of one temperature throughout, the temperature due to the solar radiation which it receives, but that it is still so large as to be attracted much more than it is repelled by the sun. Both attraction and repulsion are inversely as the square of the distance, so that we shall have a central force which we may put as producing acceleration A/r^2, where A is constant.

We know that at the distance of the earth, putting $r = b$, $A/b^2 \quad 0{\cdot}59$ cm./sec.^{2}, say $0{\cdot}6$ cm./sec.^{2}. Then $A = 0{\cdot}6b^2$. The force acting against the motion produces retardation $-\ 2Ru\ U^2\rho a$.

If S is the solar constant at the distance b, its value at distance r is

$$Sb^2/r^2.$$

Putting
$$4\pi a^2 R = \pi a^2 S b^2\ r^2,$$

$$R = (S/4)\,(b^2\ r^2),$$

then the acceleration in the line of motion is

$$\frac{Sb^2}{2U^2\rho a}\cdot\frac{u}{r^2} \qquad \frac{T\dot{s}}{r^2},$$

where $T = Sb^2/2U^2\rho a$, and $\dot{s}$ is now written for the velocity u.

The accelerations along and perpendicular to the radius-vector give the equations

$$\ddot{r} - r\dot{\theta}^2 \quad -\frac{A}{r^2} - \frac{T\dot{s}}{r^2}\frac{dr}{ds}, \qquad \text{.........................(1)}$$

$$\frac{1}{r}\frac{d}{dt}(r^2\dot{\theta}) = -\frac{T\dot{s}}{r^2}\frac{r\,d\theta}{ds}. \qquad \text{.............................(2)}$$

From (2) we get
$$\frac{d}{dt}(r^2\dot{\theta}) = -\frac{T\,d\theta}{dt},$$

whence
$$r^2\dot{\theta} = C - T\theta, \qquad \text{.............................(3)}$$

where C is the constant of integration.

If θ is 0 when $t = 0$, then C is the initial value of $r^2\dot{\theta}$. Further, as θ increases $r^2\dot{\theta}$ decreases and is 0 when $\theta = C/T$. This gives a limit to the angle described.

Equation (1) may be written

$$\ddot{r} - r\dot{\theta}^2 = -\frac{A}{r^2} - \frac{T\dot{r}}{r^2}. \quad \text{.........................(4)}$$

Putting u for r^{-1} we have

$$\dot{r} = \frac{dr}{d\theta}\dot{\theta} = -\frac{1}{u^2}\frac{du}{d\theta}\dot{\theta} = -(C - T\theta)\frac{du}{d\theta} \quad \text{from (3)},$$

$$\ddot{r} = T\dot{\theta}\frac{du}{d\theta} - (C - T\theta)\frac{d^2u}{d\theta^2}\dot{\theta}$$

$$= T(C - T\theta)u^2\frac{du}{d\theta} - (C - T\theta)^2 u^2\frac{d^2u}{d\theta^2} \quad \text{from (3)}.$$

Substituting in (4) $\quad \dfrac{d^2u}{d\theta^2} + u = \dfrac{A}{(C - T\theta)^2}.$

This can probably only be integrated by approximation. We can see the effect on the motion at the beginning by putting

$$\frac{d^2u}{d\theta^2} + u = \frac{A}{C^2}\left(1 + \frac{2T}{C}\theta\right),$$

since T/C is small if we begin at the distance of the earth and with a particle having the velocity of the earth.

An integral of this is

$$u = \frac{A}{C^2}\left(1 + \frac{2T}{C}\theta\right).$$

The complementary function will be periodic and may be omitted. To the order of approximation adopted

$$r = \frac{C^2}{A}\left(1 - \frac{2T}{C}\theta\right) \text{ and } \dot{r} = -\frac{2CT}{A}\dot{\theta}.$$

Then initially $\quad \dot{r}/r = -(2T/C)\dot{\theta}.$

In applying these results, we may note that $T = Sb^2/2U^2\rho a$ is constant for all distances, and that b, the earth's distance, is 493 U. Inserting the value of the solar constant, $0{\cdot}175 \times 10^7$, and taking $\rho = 5{\cdot}5$, we get

$$T = 3{\cdot}9 \times 10^{10} \,.\, a^{-1}.$$

C will depend on the initial conditions. Assuming that the body considered is initially moving in a circle, then, at the beginning

$$r\dot{\theta}^2 = \frac{A}{r^2} \text{ or } \dot{\theta} = \sqrt{\frac{A}{r^3}} = \sqrt{\frac{0{\cdot}6b^2}{r^3}},$$

since at $r = b$ the acceleration to the centre is $0{\cdot}6$.

Then $\quad C = r^2\dot{\theta} = \sqrt{0{\cdot}6b^2r}.$

Substituting these values in $\dot{r}/r$ we have

$$\frac{\dot{r}}{r} = -\frac{7\cdot8 \times 10^{10}}{r^2 a}.$$

This gives only the initial value of $\frac{\dot{r}}{r}$ and cannot be taken to hold for a time which will make $T^2\theta^2/C^2$ appreciable. But by (3) we see that $r = 0$ if $\theta = C/T$, so that $C/2\pi T$ is a superior limit to the number of revolutions, even if we suppose the way clear right up to the centre.

Putting the numerical values we get

$$C/2\pi T = 46\cdot5 r^{\frac{1}{2}} a *.$$

Suppose, for example, that $r = b - 493 \times 3 \quad 10^{10}$; $a = 1$, then

$$\dot{r}/r = -\, 3\cdot5 \times 10^{-16}.$$

If we multiply by $3\cdot15 \times 10^7$, the seconds in a year, we obtain

$$(\dot{r}/r) \times 3\cdot15 \times 10^7 = 1\cdot1 \times 10^{-8}.$$

This implies that a sphere 1 cm. radius and density 5·5, starting with the velocity of the earth, and at its distance from the sun, will move inwards $\frac{1}{10,000}$ of its distance in about 10,000 years. It cannot in all make so many as $46\cdot5 \times b^{\frac{1}{2}} = 1\cdot79 \times 10^8$ revolutions.

If we put $a = 0\cdot001$ cm., since the effects are inversely as a, then its distance will decrease by about 1 in 10,000 in 10 years, and it cannot make in all so many as $1\cdot79 \times 10^5$ revolutions.

If instead of starting from the distance of the earth, the particlė starts from, say, 0·1 the distance, the effect in the radius is 100 times as great and the number of revolutions is $\surd 10$ times less. Then with radius 1 cm. the distance decreases by $\frac{1}{10,000}$ in 100 years, and there are not so many as 80×10^6 revolutions§, while with radius 0·001 cm. the distance decreases by $\frac{1}{10,000}$ in 0·1 year, and there are not so many as 80,000 revolutions§.

Small particles, therefore, even of the order of 1 cm. radius, would be drawn into the sun, even from the distance of the earth, in times not large compared with geological times, and dust-particles if large enough to absorb solar radiation would be swept in in a time almost comparable with historical times. Near the sun the effects are vastly greater. The application to meteoric dust in the system is obvious.

There should be a similar effect with dust and small particles circulating round the earth. If, for example, any of the Krakatoa dust was blown out so far beyond the appreciable atmosphere, and was given such motion that the particles became satellites to the earth, at no long time the dust will

* [The original has $61 r^{\frac{1}{2}} a$. In what follows, the necessary alterations consequent upon this correction have been made. ED.]

§ [The original has 80,000 and 80 respectively. The correction necessitates a modification in the views expressed in the succeeding paragraphs. ED.]

return. A ring of dust-particles moving round a planet and receiving heat either from the sun or from the planet will tend to draw in to the planet.

[*Note added October* 31. Since the foregoing paper was printed I have re-examined the theory of the pressure on a fully radiating surface when in motion, and have come to the conclusion that the change in pressure due to the motion is only half as great as that obtained on p. 325. In that investigation the pressure was assumed to be equal to the energy-density, whether the surface was at rest or in motion, whereas it appears, if the following mode of treatment is correct, that the pressure on a radiating surface moving forward is only $1 - \frac{u}{U}$ of the energy-density of the radiation emitted.

Let us suppose that a surface A, a full radiator, is moving with velocity u towards a full absorber B, which, with the surroundings, we will suppose at 0° A. Consider for simplicity a parallel pencil issuing normal from A with velocity U towards B. Let the energy-density in the stream from A be E when A is at rest, and E' when it is moving. Let the pressure on A be $p = E$ when it is at rest, and p' when it is moving. When moving, A is emitting a stream of momentum p' per second and this momentum ultimately falls on B. Let A start radiating and moving at the same instant; let it move a distance d towards B, and then let it stop radiating and moving. It emits momentum p' per second for a time d/u and therefore emits total momentum $p'd/u$. Since B is at rest, the pressure on it, the momentum which it receives per second, is E'. But since A is following up the stream sent out, B does not receive through a period as long as d/u, but for a time less by d/U. If we assume that the total momentum received by B is equal to the total sent out by A, we have

$$p'd/u = E'(d/u - d/U),$$

or

$$p' = E'(1 - u/U).$$

To find E' in terms of E we must make some assumption as to the effect of the motion on the radiation emitted. In the paper I have assumed that the emitting surface converts the same amount of its internal energy per second into radiant energy as when it is at rest, but that $p'u$ of the energy of motion of the radiating mass is also converted into radiant energy. Since the radiation emitted in one second is contained in length $U - u$, we have

$$E'(U - u) = EU + p'u = EU + E'\left(\frac{U - u}{U}\right)u,$$

whence

$$E' = E\frac{U^2}{(U - u)^2} = E(1 + 2u/U).$$

The same result is obtained if we assume that the amplitude of the emitted waves is the same whether the surface is moving or not, and that the energy-density is inversely as the square of the wave-length for given amplitude.

We have, therefore, if the above application of the equality of action and reaction is justified,

$$p' = E'\left(1 - \frac{u}{U}\right) \quad E\frac{U}{U - u} \quad p\left(1 + \frac{u}{U}\right).$$

In a similar way we can find the effect of motion of an absorber on the pressure against it due to the incident radiation.

Let a stream of energy-density E be incident on a fully absorbing surface moving towards the source with velocity u. Let the surface be at $0^\circ A$, so as to obtain the effect of the incident radiation only. When the surface is at rest, we may regard the stream as bringing up momentum E per second, or as containing momentum of density E/U brought up with velocity U to it. If the surface is moving towards the source, it takes up in one second the momentum in length $U + u$, or receives $\frac{E}{U}(U + u)$, and the pressure on it is

$$p' = E\left(1 + \frac{u}{U}\right) = p\left(1 + \frac{u}{U}\right).$$

It is easy to show that when a perfect reflector is moving, the pressure upon it is altered from p to $p\left(1 + \frac{2u}{U}\right)$.

In the paper, the case of a full radiator in an enclosure at zero has alone been considered, so that the correcting factor is $1 + \frac{u}{U}$ or $1 + \frac{u \cos \chi}{U}$ when the motion is at an angle χ to the line of radiation. Hence the forces obtained in the paper when the factor was $1 + \frac{2u}{U}$ are all double those obtained with the factor now given. The process of drawing in small particles to the sun is correspondingly lengthened out.

It is, perhaps, worth noting that the motion of a body round the sun produces a small aberration-effect. If the body is a sphere, the sunlight does not fall on the hemisphere directly under the sun, but on one turned round through an angle u/U. The pressure of the radiation, though still straight from the sun, does not act through the centre but through a point $\frac{u^2}{2U^2} \times$ (radius of sphere) in front of the centre. Thus, in the case of the earth, it will tend to stop the rotation. But the effect is so minute that if present conditions as to distance and radiation were maintained, it would take something of the order of 10^{19} years to stop the whole of the rotation. J. H. P.]

21.

NOTE ON THE TANGENTIAL STRESS DUE TO LIGHT INCIDENT OBLIQUELY ON AN ABSORBING SURFACE.

[*Phil. Mag.* **9**, 1905, pp. 169-171.]

[*Read* at Section A, British Association, Cambridge, August, 1904.]

The existence of pressure on a surface due to the incidence of a normal beam of light, first deduced as a consequence of the electromagnetic theory by Maxwell, has been fully confirmed by the experiments of Lebedew, and quite independently by the exact work of Nichols and Hull. These experiments show that the pressure exists and that it is equal to the energy per c.c. or to the energy-density in the incident beam.

In so far as it produces this pressure we may regard the beam as a stream of momentum, the direction of the momentum being along the line of propagation, and the amount of momentum passing per second through unit area cross-section of the beam being equal to the density of the energy in it. Let E denote this energy-density. If the beam is inclined at θ to the normal to a surface on which it falls, the momentum-stream on to unit area of the surface is $E \cos\theta$ per second, and this is the force which the beam will exert in its own direction. If the beam is entirely absorbed, the result is a pressure $E \cos^2\theta$ along the normal and a tangential stress in the plane of incidence $E \sin\theta \cos\theta = \frac{1}{2}E \sin 2\theta$. If μ of the incident beam is reflected, the normal pressure is $(1+\mu) E \cos^2\theta$, and the tangential stress is $\frac{1-\mu}{2} E \sin 2\theta$*. When there is absorption the tangential stress has a maximum value at 45° if μ is constant. When there is no absorption the tangential stress disappears.

The tangential stress is much more easily detected than the normal pressure. For the action of the gas surrounding the surface is normal to it and is with difficulty disentangled from the normal light-pressure. But the gas-action is at right angles to the tangential stress, and it is merely necessary to arrange a surface free to move in its own plane to eliminate the action of the normal forces and to reveal the tangential stress.

* These expressions are given in 'Radiation in the Solar System,' *Phil. Trans.* A, 202, p. 539. [*Collected Papers*, Art. 20.]

With the assistance of my colleague Dr. Guy Barlow, to whom I am much indebted for help in the work, I have made the following experiment to show the existence of the stress.

Two circular glass discs, each 2·75 sq. cm. area, were fixed at the ends of a horizontal light glass rod 5·3 cm. long, the discs being perpendicular to the rod and fixed to it at their highest points. One of the discs was lampblacked, and the other silvered. The rod was placed in a light wire cradle and suspended by a fine quartz fibre about 25 cm. long in a brass case with glazed sides. On the cradle was a mirror by which deflections could be observed with a telescope on a millimetre-scale 1·8 metres distant. The moment of inertia of the system was 2·35 gm. cm.^{2} and the time of vibration was 146 seconds. A deflection of 1 scale-division therefore corresponded to a tangential force on a disc of about one two-millionth of a dyne more exactly $0{\cdot}483 \times 10^{-6}$ dyne.

The air was pumped from the case till the pressure was less than 1 cm. of mercury. At this pressure the irregularity of the disturbances due to the residual gas is very greatly reduced. A parallel beam of light from a Nernst lamp was then directed so as to be incident obliquely on the lampblacked disc. From the arrangement of the discs it is obvious that a uniformly distributed normal force would have no moment tending to twist the system, while a tangential force would have a moment and would twist it. In all cases the disc moved away from the source of light. The deflection was a maximum when the incidence was not very far from 45 , and fell off on each side of the maximum value.

As there are various sources of error not yet removed, we have not made a complete series of measurements but have only made sure that the effect is of the order to be expected from the theory, by finding the deflection for an angle of 45°.

The beam from the Nernst lamp when incident at 45 turned the rod through 16·5 scale-divisions. Assuming total absorption, the tangential force should be $\frac{1}{2}E \sin 2\theta \times$ area of disc $- \frac{1}{2}E \times 2{\cdot}75$.

Equating to the value of the force given by the deflection, viz.,

$$0{\cdot}483 \times 10^{-6} \times 16{\cdot}5,$$

we have
$$E = 5{\cdot}8 \times 10^{-6} \text{ erg/cm.}^3.$$

The same beam was then directed on to a small lampblacked silver disc of known heat-capacity, through a glass plate of thickness equal to that of the side of the case. The initial rise of temperature per second was measured by a thermojunction of constantan wire soldered to the disc. The energy-density of the stream was thus found to be $E = 6{\cdot}5 \times 10^{-6}$ erg/cm.^{3}.

The agreement of the two values is quite as close as could be expected in so rough a determination*.

When the beam was directed on to the silver disc at the other end of the torsion-rod, the deflection was much less, as was to be expected.

We have also made some qualitative experiments with a blackened glass cylinder a ring cut from a test-tube—suspended by a quartz fibre with its axis vertical. When a beam fell on this in any direction not along a diameter, there was always a twist in the direction corresponding to the tangential stress.

* [A redetermination of the various constants and a revision of the calculations gave a still closer agreement. The observed torque was 21×10^{-6} cm. dynes, while the torque calculated from the energy was 22×10^{-6} cm. dynes. This correction is given in *The Pressure of Light*, p. 55. (Romance of Science Series, S.P.C.K. 1910.) ED.]

[NOTE BY G. BARLOW, *July* 1916.

Particular care was taken to make the torsion-system very symmetrical with respect to the axis of suspension. The success of the experiment depended, also, on using a uniform parallel beam of light of cross-section *slightly greater* than the projected area of the disc. During the small oscillations of the system the disc, therefore, remained uniformly illuminated. It was found that when a circular patch of light was focussed centrally on the disc the deflections were irregular. On one occasion we used a beam of sunlight reflected from a heliostat, but owing to the very variable absorption by the town atmosphere the observed pressure showed great fluctuations.

The experiment with the cylinder has since been repeated, and was shown to some members of the British Association at the Birmingham meeting of 1913. The cylinder was of aluminium and was turned very accurately by a watchmaker. The ends were closed, but near the axis two small air-holes were drilled. The surface was blackened with a deposit of asphaltum. The observed torques due to the light-pressure were always of the order of magnitude expected, and variation of the gas-pressure over a considerable range did not greatly affect the results. This method is suitable for lecture demonstration but it does not appear satisfactory for exact measurements.]

22.

RADIATION-PRESSURE*.

[*Phil. Mag.* **9**, 1905, pp. 393–406.]

[Presidential Address, delivered at the Annual General Meeting of the Physical Society, February 10, 1905.]

A hundred years ago, when the corpuscular theory held almost universal sway, it would have been much more easy to account for and explain the pressure of light than it is to-day, when we are all certain that light is a form of wave-motion. Indeed, on the corpuscular theory it was so natural to expect a pressure that numerous attempts were made† in the eighteenth century to detect it. But the early experimenters had a greatly exaggerated idea of the force they looked for. Even on their own theory it would only have double the value which we now know it to possess, and their methods of experiment were utterly inadequate to show so small a quantity. But had these eighteenth-century philosophers been able to command the more refined methods of to-day, and been able to carry out the great experiments of Lebedew and of Nichols and Hull, and had they further known of the emission of corpuscles revealed to us by the cathode-stream and by radioactive bodies, there can be little doubt that Young and Fresnel would have had much greater difficulty in dethroning the corpuscular theory and setting up the wave-theory in its place.

The existence of pressure due to waves, though held by Euler and used by him 160 years ago to explain the formation of comets' tails by repulsion, seems to have dropped out of sight, till Maxwell, in 1872, predicted its existence as a consequence of his Electromagnetic Theory of Light. It is remarkable that it should have been brought to the front through the investigation of such a special type, such an abstruse case, of wave-motion, and that it was not seen that it must follow as a consequence of any wave-motion, whatever the type of wave we suppose to constitute Light. I believe that the first suggestion that it is a general property of waves is due to Mr. S. Tolver Preston, who in 1876‡ pointed out the analogy of the energy-carrying power

* [This address is included here because it contains an account of some original work not elsewhere described. ED.]

† Some account of these methods is given by Nichols and Hull in 'The Pressure due to Radiation,' *Proc. Am. Ac.* vol. 38, no. 20, p. 559. See also Priestley, *On Vision*, p. 385.

‡ *Engineering*, 1876, vol. 21, p. 83.

of a beam of light with the mechanical carriage by belting, and calculated the pressure on the surface of the Sun by the issuing radiation, obtaining a value equal to the energy-density in the issuing stream, without assumption as to the nature of the waves. But though the analogy is valuable, I confess that Mr. Preston's reasoning does not appear to me conclusive, and I think it still remains an analogy. There is, I suspect, some general theorem yet to be discovered, which shall relate directly the energy and the momentum issuing from a radiating source. It seems possible that in all cases of energy-transfer, momentum in the direction of transfer is also passed on, and therefore there is a back pressure on the source. Such pressure certainly exists in material transfer, as in the corpuscular theory. It exists too, as we now know, in all wave-transfer. From the investigation below (p. 338) it appears to exist when energy is transferred along a revolving twisted shaft. In heat-conduction in gases, the kinetic theory requires a carriage of momentum from hotter to colder parts; so that there is some ground for supposing the pressure to exist in all cases.

Though we have not yet a general and direct dynamical theorem accounting for radiation-pressure, Professor Larmor* has given us a simple and most excellent indirect mode of proving the existence of the pressure, which applies to all waves in which the average energy-density for a given amplitude is inversely as the square of the wave-length. Let us suppose that a train of waves is incident normally on a perfectly reflecting surface. Then, whether the reflecting surface is at rest, or is moving to or from the source, the perfect reflection requires that the disturbance at its surface shall be annulled by the superposition of the direct and reflected trains. The two trains must therefore have equal amplitudes. Suppose now that the reflector is moving forwards towards the source. By Doppler's principle, the waves of the reflected train are shortened, and so contain more energy than those of the incident train. This extra energy can only be accounted for by supposing that there is a pressure against the reflector, that work has to be done in pushing it forward. When the velocity of the reflector is small, the pressure is easily found to be equal to $E\left(1+\frac{2u}{U}\right)$, where $\frac{E}{2}$ is the energy-density just outside the reflector in the incident train, U is the wave-velocity, and u the velocity of the reflector. If $u=0$, the pressure is E; but it is altered by the fraction $\frac{2u}{U}$ when the reflector is moving, and the alteration changes sign with u. A similar train of reasoning gives us a pressure on the source, increased when the source is moving forward, decreased when it is receding.

It is essential, I think, to Larmor's proof that we should be able to move the reflecting surface forward without disturbing the medium except by

* *Encyc. Brit.* vol. 32, 'Radiation,' p. 121.

reflecting the waves. In the case of light-waves it is easy to imagine such a reflector. We have to think of it as being, as it were, a semipermeable membrane, freely permeable to ether, but straining back and preventing the passage of the waves. In the case of sound-waves, or of transverse waves in an elastic solid, it is not so easy to picture a possible reflector. But for sound-waves I venture to suggest a reflector which shall freeze the air just in front of it, and so remove it, the frozen surface advancing with constant velocity u. Or perhaps we may imagine an absorbing surface which shall remove the air quietly by solution or chemical combination. In the case of an elastic solid, we may perhaps think of the solid as melted by the advancing reflector, the products of melting being passed through pores in the surface and coming out to solidify at the back.

Though Larmor's proof is quite convincing, it is, I think, more satisfying if we can realise the way in which the pressure is produced in the different types of wave-motion.

In the case of electromagnetic waves, Maxwell's original mode of treatment is the simplest, though it is not, I believe, entirely satisfactory. According to his theory, tubes of electric and of magnetic force alike, produce a tension lengthways and an equal pressure sideways, equal respectively to the electric and magnetic energy-densities in the tubes. We regard a train of waves as a system of electric and magnetic tubes transverse to the direction of propagation, each kind pressing out sideways—that is, in the direction of propagation. They press against the source from which they issue, against each other as they travel, and against any surface upon which they fall. Or we may take Professor J. J. Thomson's point of view*. 'Let us suppose that the reflecting surface is metallic; then, when the light falls on the surface, the variation of the magnetic force induces currents in the metal, and these currents produce opposite effects to the incident light, so that the inductive force is screened off from the interior of the metal plate: thus the currents in the plate, and therefore the intensity of the light, rapidly diminish as we recede from the surface of the plate. The currents in the plate are accompanied by magnetic force at right angles to them; the corresponding mechanical force is at right angles both to the current and the magnetic force, and therefore parallel to the direction of propagation of the light.' In fact, we have in the surface of the reflector a thin current-sheet in a transverse magnetic field, and the ordinary electrodynamic force on the conductor accounts for the pressure.

In sound-waves there is at a reflecting surface a node a point of no motion, but of varying pressure. If the variation of pressure from the undisturbed value were exactly proportional to the displacement of a parallel layer near the surface, and if the displacement were exactly harmonic, then the average pressure would be equal to the normal undisturbed value. But

* Maxwell's *Electricity and Magnetism*, 3rd edition, vol. 2, p. 441, footnote.

consider a layer of air quite close to the surface. If it moves up a distance y towards the surface, the pressure is increased. If it moves an equal distance y away from the surface, the pressure is decreased, but by a slightly smaller quantity. To illustrate this, take an extreme case, and for simplicity suppose that Boyle's law holds. If the layer advances half-way towards the reflecting surface, the pressure is doubled. If it moves an equal distance outwards from its original position, the pressure falls, but only by one-third of its original value; and if we could suppose the layer to be moving harmonically, it is obvious that the mean of the increased and diminished pressures would be largely in excess of the normal value. Though we are not entitled to assume the existence of harmonic vibrations when we take into account the second order of small quantities, yet this illustration gives the right idea. The excess of pressure in the compression-half is greater than its defect during the extension-half, and the net result is an average excess of pressure—a quantity itself of the second order—on the reflecting surface. This excess in the compression-half of a wave-train is connected with the extra speed which exists in that half, and makes the crests of intense sound-waves gain on the troughs.

Lord Rayleigh*, using Boyle's Law, has shown that the average excess on a surface reflecting sound-waves should be equal to the average density of the energy just outside; and I think the same result can be obtained by his method if we use the adiabatic law. But the subject is full of pitfalls, and I am by no means sure that the result is to be obtained so easily as it appears to be. It is perhaps worth while to note one of these pitfalls, of which I have been a victim. It is quite easy to obtain the pressure against a reflecting surface by supposing that the motion just outside it is harmonic. But the result comes out to $(\gamma + 1) \times$ energy-density, where γ is the ratio of the specific heats. Lord Rayleigh kindly pulled me out of the pit into which I fell, pointing out that when we take into account second-order quantities the ordinary sound-equation does not hold. In fact we cannot take the disturbance as harmonic, and the simple mode of treatment is illusory.

The pressure in transverse waves in an elastic solid is, I think, to be accounted for by the fact that when a square, $ABCD$, is sheared into the position $aBCd$ (Fig. 1) through an angle e, the axes of the shear, aC and Bd, no longer make 45° with the planes of shear AD, BC. Since $ACa = \frac{e}{2}$, the pressure-line aC is inclined at $45° - \frac{e}{2}$ to the direction of propagation, and the tension-line at $45° + \frac{e}{2}$ to that line. The result is a small pressure perpendicular to the planes of shear, that is, in the direction of propagation: and this small pressure is just equal to the energy-density of the waves.

* *Phil. Mag.* vol. 3, 1902, p. 338, 'On the Pressure of Vibrations.'

For let PQR (Fig. 2) be a small triangular wedge of the solid, PQ being a plane of shear perpendicular to the direction of propagation. Let this wedge have unit thickness perpendicular to the plane of the figure. Let PR be along a pressure-line and QR along a tension-line, and let pressure and tension each be P. Resolve the forces on PR and QR perpendicular to PQ. Then we have a force from right to left,

$$P \,.\, QR \cos PQR - P \,.\, PR \cos QPR$$
$$= P \,.\, PQ \left\{ \cos^2\left(45^\circ - \frac{e}{2}\right) - \cos^2\left(45 + \frac{e}{2}\right) \right\} = P \,.\, PQ \,.\, e.$$

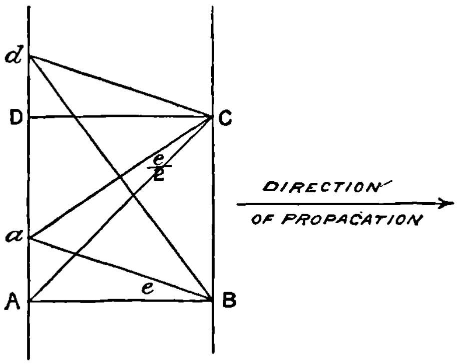


Fig. 1.

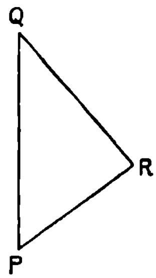


Fig. 2.

Thus, to prevent motion in the direction of propagation there must be a pressure on PQ equal to $Pe = ne^2$, where n is the rigidity modulus. But the strain-energy per unit volume is $\frac{ne^2}{2}$, and the kinetic energy is equal to it. The total energy-density is therefore ne^2, and the pressure is equal to this.

The pressure of elastic-solid waves appears to be beyond experimental verification at present. But that of sound-waves has been demonstrated most successfully by Altberg*, working in Lebedew's laboratory at Moscow.

A small wooden cylinder, 21 mm. diameter, was suspended at one end of a torsion-arm, with its axis horizontal and transverse to the arm. One end of the cylinder occupied a circular hole in the middle of a board, there being just sufficient clearance to allow it to move, and the plane end was flush with the outer surface of the board. When very intense sound-waves 10 cm. in length, from a source 50 cm. distant, impinged on the board, the cylinder was pushed back, the pressure sometimes rising to as much as 0·24 dyne cm.^{2}. The intensity of the sound was measured independently by the vibrations of a telephone-plate, in a manner devised by M. Wien, and through a large range it was found that the pressure on the cylinder was proportional to the intensity indicated by the telephone-manometer.

* *Ann. der Physik*, vol. 11, 1903, p. 405.

Just lately Professor Wood* has devised a strikingly simple experiment to illustrate sound-pressure. The sound-waves from strong induction-sparks are focussed by a concave mirror on a set of vanes like those of a radiometer, and when the focus is on the vanes as they face the waves the mill spins round.

Theory and experiment, then, justify the conclusion that when a source is pouring out waves, it is pouring out with them forward-momentum as well as energy, the momentum being manifested in the reaction, the back-pressure against the source, and in the forward pressure when the waves reach an opposing surface. The wave-train may be regarded as a stream of momentum travelling through space. This view is most clearly brought home, perhaps, by considering a parallel train of waves which issues normally from a source for one second, travels for any length of time through space, and then falls normally on an absorbing surface for one second. During this last second, momentum is given up to the absorbing surface. During the first second, the same amount was given out by the source. If it is conserved in the meanwhile, we must regard it as travelling with the train.

Since the pressure is the momentum given out or received per second, and the pressure is equal to the energy-density in the train, the momentum-density is equal to the energy-density $\div$ wave-velocity.

This idea of momentum in a wave-train enables us to see at once what is the nature of the action of a beam of light on a surface where it is reflected, absorbed, or refracted, without any further appeal to the theory of the wave-motion of which we suppose the light to consist†.

It is convenient to consider the energy per linear centimetre in the beam, and the total pressure-force, equal to this linear energy-density, so as to avoid any necessity for taking into account the cross-section of the beam.

Thus, in total reflection, let a beam AB (Fig. 3) be reflected along BC, and let $AB = BC$ represent the momentum in each in length V equal to the velocity of light.

Produce AB to D, making $BD = AB$.

Then DC represents the change in the momentum per second due to the reflection—the force on the beam, if such language is permissible; and CD is the reaction, the total light-force on the surface.

If there is total absorption, let AB (Fig. 4) represent the momentum of the incident beam. Resolve AB into AE parallel and EB normal to the surface. Then, since the momentum AB disappears as light-momentum, there must be a normal force EB on the surface and a tangential force AE

* *Phys. Zeitschrift*, 1 Jan. 1905, p. 22.

† A discussion, on the electromagnetic theory, of the forces exerted by light is given by Goldhammer, *Ann. der Phys.* vol. 4, 1901, p. 483.

parallel to the surface. I have lately* described an experiment which shows the existence of the tangential force AE.

If there is total refraction, let AB (Fig. 5) be refracted along BC with velocity V'. If E is the energy in unit length of AB, and if E' is the energy in unit length of BC, the equality of energy in the two beams is expressed by

$$VE = V'E'.$$

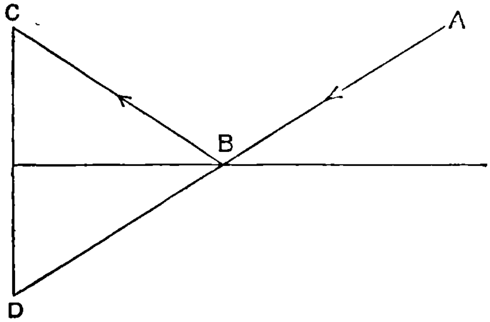


Fig. 3.

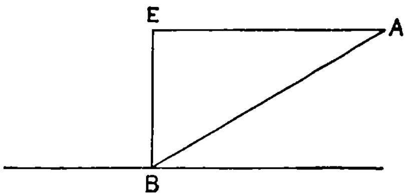


Fig. 4.

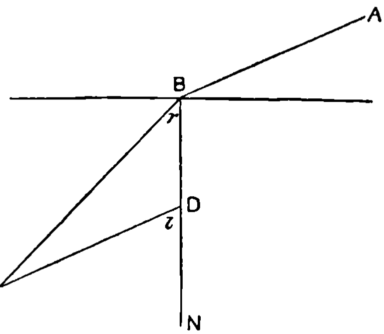


Fig. 5.

But if M is the stream of momentum passing per second along AB, and if M' is that along BC, then

$$M = E \quad \text{and} \quad M' = E'.$$

Whence
$$VM = V'M'$$

and
$$M' = \frac{V}{V'} M = \mu M.$$

Let $AB = M$, and BC along the refracted beam $= M' = \mu M = \mu AB$.

Draw CD parallel to BA, meeting the normal BN in D. Then

$$CD = CB \sin r/\sin i = \frac{CB}{\mu} = AB = M.$$

* *Phil. Mag.* Jan. 1905, p. 169. [*Collected Papers*, Art. 21.]

Hence, by the refraction, momentum DC has been changed to momentum BC, or momentum BD has been imparted to the light. There is therefore a reaction DB on the surface. The force DB may be regarded as a pull-out or a pressure from within, and it is along the normal*.

If the refraction is from a denser to a rarer medium, CB will now represent the incident stream and BA or CD the refracted stream. BD is the stream added to CB to change it to CD, and DB is the force on the surface, again a force outwards along the normal.

In any real refraction with ordinary light, there will be reflection as well as refraction. The reflection always produces a normal pressure, and the refraction a normal pull. But with unpolarised light, a calculation shows that the refraction-pull, for glass at any rate, is always greater than the reflection-push, even at grazing incidence.

The following table has been calculated from Fresnel's formula for unpolarised light by Dr. Barlow:

P = total pull on surface.
M = momentum per second in incident beam.
R = reflection-coefficient for angle i.
μ = 1·5.

i	R	P/M
0	·0400	·4000
20	·0402	·4240
40	·0458	·4925
50	·0572	·5310
60	·0893	·5720
65	·1205	·5771 Maximum
70	·1710	·5683
75	·2531	·5329
80	·3878	·4521
89	·9044	·0738
$90-d\theta$	....	$2\mu^2 d\theta$
90	1·0000	·0000

If a ray of light passes obliquely through a parallel plate, there is a normal pull outwards at incidence and a normal pull outwards at emergence: and if the refraction were total, this would result in a couple. But since some of the light returns into the first medium, it is easy to see that the net result is a normal repulsion and a couple.

An experiment which I have lately made in conjunction with Dr. Barlow will serve as an illustration of the idea of a beam of light regarded as a stream

* It has been pointed out by J. J. Thomson, *Electricity and Matter*, p. 67, 'that even when the incidence of the light is oblique, the momentum communicated to the substance is normal to the refracting surface' The change of momentum of a beam of light is, it may be noted, the same on the wave and on the corpuscular theory.

of momentum. A rectangular block of glass, 3 cm. 1 cm. 1 cm., was suspended by a quartz fibre so that the long axis of the block was horizontal. It hung in a case with glass windows, which was exhausted to about 15 mm. of mercury. A horizontal beam of light, from either a Nernst lamp or an arc, was directed on to one end of the block so that it entered centrally as AB in Fig. 6, and at an angle of incidence about 55°. After two internal reflections it emerged centrally as EF from the other end. Thus a stream of momentum AB was shifted parallel to itself into the line EF, or a counter-clockwise couple acted on the beam. The reaction was a clockwise couple on the block. Using mirror, telescope, and a millimetre-scale about 184 cm. distant, with the strongest light a very small deflection in the right direction could just be detected. But the quartz fibre was rather coarse, indeed needlessly strong; and as the time of vibration was only 39 seconds, the deflection was very minute. To render the effect more evident we used intermittent passage of the beam, sending it in during the half-period of vibration while B was

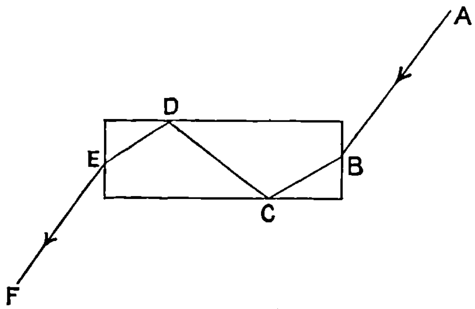


Fig. 6. Plan.

moving from A, and shutting it off while B was moving towards A. The swings then always increased. When the beam was sent in during the approaching half and shut off during the receding half, the swings always decreased, and always rather more rapidly than they increased during the first half. For in the first case the natural damping acted against the light couple, and in the second with it. In one experiment the average increase was ·55 scale-division and the average decrease ·61 per period, and was fairly regular in each case. The mean was ·58. The steady deflection is half this, or 0·29 division, giving a couple 11×10^{-6} cm. dyne. We made a measurement of the energy in the beam by means of the rate of rise of a blackened silver disc; but it was necessarily very inexact, as we had no means of securing constancy in the arc used in this experiment. This energy-measurement gave as the value of the couple $6 \quad 10^{-6}$, and the agreement is sufficient to show that the order of the result is right.

An analysis of this experiment shows that the couple was really due to the pressures at the two internal reflections; for, as we have seen, the forces at incidence at B and emergence at E are normal and produce no twist.

Another experiment which we have made is, I think, more interesting, in that it brings into prominence the pull outwards or push from within occurring on refraction. Two glass prisms, each with refracting angle 34°, another angle being a right angle, and with refracting edge 1·6 cm. long, were arranged as in Fig. 7 (which shows the plan) at the ends of a thin brass torsion-arm suspended at its middle point from a quartz fibre in the same case as that used in the last experiment. The two inner faces were 3 cm. apart, and their width was 1·85 cm. A mirror gave the reflection of a millimetre-scale 171·4 cm. distant. The moment of inertia of the system was 48 gm. cm.^{2}, and the time of vibration was 317 seconds. The air-pressure was reduced as before. When a beam of light from a Nernst lamp was sent through the system, as shown in the figure, it was shifted parallel to itself through a distance about 1·64 cm. The torsion-arm moved round clockwise by an easily measurable amount. In one experiment the deflection was 3·3 scale-divisions, indicating a couple $1{\cdot}84 \times 10^{-5}$ cm. dyne. The same beam directed

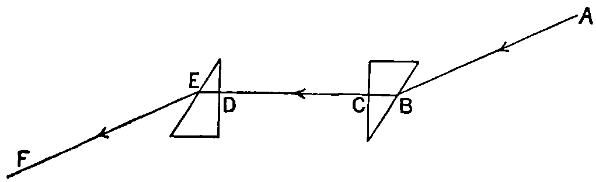


Fig. 7. Plan.

on to the blackened silver disc gave the linear energy-density as $9{\cdot}8 \times 10^{-6}$, which should have given a couple $1{\cdot}6 \times 10^{-5}$. Though the agreement is perhaps accidentally close, yet, as we could use a Nernst lamp, the measurements were much more trustworthy than in the last experiment*.

The interesting point here is that the effect could only be produced by a force outwards at B and E. Whatever forces exist at C and D would be normal to the surfaces and would give no twist.

A very short experience in attempting to measure these light-forces is sufficient to make one realise their extreme minuteness—a minuteness which appears to put them beyond consideration in terrestrial affairs, though I have tried to show† that they may just come into comparison with radiometer-action on very small dust-particles.

In the Solar system, however, where they have freer play and vast times to work in, their effects may mount up into importance. Yet not on the larger bodies; for on the earth, assumed to be absorbing, the whole force of

* [See *The Pressure of Light*, p. 61 (S.P.C.K. 1910), where these values are slightly corrected, and results are given for a similar experiment with smaller prisms. Ed.]

† *Nature*, Dec. 29, 1904, p. 200. [*Collected Papers*, Art. 65.]

the light of the sun is only about a 50 million-millionth of his gravitation-pull. But since the ratio of radiation-pressure to gravitation-pull increases in the same proportion as the radius diminishes if the density is constant, the pressure will balance the pull on a spherical absorbing particle of the density of the earth if its radius is a 50 billionth that of the earth a little over a hundred-thousandth of a centimetre, say, if its diameter is a hundred-thousandth of an inch.

We may illustrate the possible effects of radiation-pressure without proceeding to such fineness as this. Let us imagine a particle of the density of the earth, and a thousandth of an inch in diameter, going round the sun at the earth's distance. There are two effects due to the sun's radiation. In the first place, the radiation-push is $\frac{1}{100}$ of the gravitation-pull; and the result is the same as if the sun's mass were only 99/100 of the value which it has for larger bodies like the earth. Hence the year for such a particle would be longer by $\frac{1}{200}$, or about 367 instead of $365\frac{1}{4}$ days. In the second place, the radiation absorbed from the sun and given out again on all sides is crushed up in front as the particle moves forward and is opened out behind. There is thus a slightly greater pressure due to its own radiation on the advancing hemisphere than on the receding one, and this appears as a small resisting force in the direction of motion. Through this the particle tends to move in a decreasing orbit spiralling in towards the sun, and at first at the rate of about 800 miles per annum.

Further, if there be any variation in the sun's rate of emitting energy, there will be a corresponding variation in the increase of the year and the decrease of the solar distance, and the particle, if we could only observe it, would form a perfect actinometer.

Though, unfortunately, we cannot observe the motion of independent small particles circling round the sun at the distance of the earth, there is good reason to suppose that some comets at least are mere clouds of dust. If we are right in this supposition, they should show some of these effects. Encke's comet at once suggests itself as of this class; for, as everyone knows, it shortens its journey of $3\frac{1}{2}$ years round the sun on every successive return, and on the average by about $2\frac{1}{2}$ hours each revolution. Mr. H. C. Plummer* has lately been investigating this comet's motion; and he finds that if it were composed of dust-particles, each of the earth's density and about $\frac{1}{50}$ mm. or rather less than a thousandth of an inch in diameter, the resisting force due to radiation-pressure would account for its accelerating return. But the sun's effective mass would be reduced by about 1/80; and on certain suppositions he finds that the assumed mean distance as calculated from Kepler's law, without reference to radiation, is greater than the true mean distance

* *Monthly Notices R.A.S.* Jan. 1905, 'On the Possible Effects of Radiation on the Motion of Comets, with special reference to Encke's Comet.'

by something of the order of 1 in 400, and he thinks such a large error is hardly possible. So that radiation-pressure has not yet succeeded in fully explaining the eccentricities of this comet. But comets are vague creatures. As Mr. Plummer suggests, we hardly know that we are looking at the same matter in the comet at its successive returns; and I still have some hope that the want of success is due to the uncertainty of the data.

There is one more effect of this radiation-pressure which is worthy of note: its sorting action on dust-particles. If the particles in a dust-cloud circling round the sun are of different sizes or densities, the radiation-accelerations on them will differ. The larger particles will be less affected than the smaller, will travel faster round a given orbit, and will draw more slowly in towards the sun. Thus a comet of particles of mixed sizes will gradually be degraded from a compact cloud into a diffused trail lengthening and broadening, the finer dust on the inner and the coarser on the outer edge.

Let us imagine, as an illustration of this sorting action, that a planet, while still radiating much energy on its own account, while still in fact a small sun, has somehow captured and attached to itself as satellite a cometary cloud of dust. Then, if the cloud consists of particles of different sizes, while all will tend to draw in to the primary, the larger particles will draw in more slowly. But if the larger particles are of different sizes among themselves, they will have different periods of revolution, and will gradually form a ring all round the planet on the outside. Meanwhile the finer particles will drift in, and again difference in size will correspond to difference in period and they too will spread all round, forming an inner fringe to the ring. If there are several grades of dust with gaps in the scale of size, the different grades will form different rings in course of time. Is it possible that here we have the origin of the rings of Saturn?

The Radiation Theory is only just starting on its journey. Its feet are not yet clogged by any certain data, and all directions are yet open to it. Any suggestion for its future course appears to be permissible, and it is only by trial that we shall find what ways are barred. At least we may be sure that it deals with real effects and that it must be taken into account.

[Compare Rayleigh, 'On the Momentum and Pressure of Gaseous Vibrations,' *Phil. Mag.* vol. 10, 1905, p. 364. Ed.]

23.

ON PROF. LOWELL'S METHOD FOR EVALUATING THE SURFACE-TEMPERATURES OF THE PLANETS; WITH AN ATTEMPT TO REPRESENT THE EFFECT OF DAY AND NIGHT ON THE TEMPERATURE OF THE EARTH*.

[*Phil. Mag.* **14**, 1907, pp. 749–760.]

Prof. Lowell's paper in the July number of the *Philosophical Magazine* marks an important advance in the evaluation of planetary temperatures, inasmuch as he takes into account the effect of planetary atmospheres in a much more detailed way than any previous writer. But he pays hardly any attention to the 'blanketing effect,' or, as I prefer to call it, the 'green-house-effect' of the atmosphere. He assumes in fact that the fourth power of the temperature is proportional to the fraction of solar radiation reaching the surface, and he neglects both the surface-radiation reflected down again and the radiation downwards of the energy absorbed by the atmosphere.

This is brought out clearly in the footnote on p. 172, where he uses a formula of Arrhenius, to which I am unable to refer, but which I think he must misinterpret in making it give his result. The inadequacy of his method is well shown by its application to the cloud-covered half of the earth's surface. He finds that this half only receives 0·2 of the radiation which the clear sky half receives. The surface-temperature under cloud should therefore be only $\sqrt[4]{0{\cdot}2} = 0{\cdot}67$ of that under clear sky. If the latter is 300° A. the former is only about 200° A. Common observation contradicts this flatly, for the difference is at most but a few degrees.

On another point common observation appears, at any rate at first sight, to contradict Professor Lowell. He assumes that the loss in the radiation of the visible spectrum in its passage through the atmosphere is practically all due to reflection, and he puts it down as about 0·7 of the whole in clear sky. If this were true the reflection from the sky opposite to the sun would I think be vastly greater than it is. White cardboard reflects diffusely about 0·7 of sunlight. But when a piece of white cardboard is exposed normally to the sun's rays it is several times brighter than the cloudless sky.

* In *Phil. Trans.* A, vol. 202, p. 525† I attempted an evaluation, in which the atmosphere was taken into account as keeping the temperature at a given point practically the same day and night. I did not then know that Christiansen (*Beiblätter zu den Ann. der Physik und Chemie*, vol. 10, 1886, p. 532) had nearly twenty years earlier applied the fourth-power law to calculate planetary temperatures. His work deserves recognition as the first in which this law was applied.

† [*Collected Papers*, Art. 20.]

The 'greenhouse-effect' of the atmosphere may perhaps be understood more easily if we first consider the case of a greenhouse with horizontal roof of extent so large compared with its height above the ground that the effect of the edges may be neglected. Let us suppose that it is exposed to a vertical sun, and that the ground under the glass is 'black' or a full absorber. We shall neglect the conduction and convection by the air in the greenhouse.

Let S be the stream of solar radiation incident per sq. cm. per sec. on the glass. Of this let rS be reflected, aS be absorbed, and tS be transmitted by the glass. Then $r + a + t = 1$. Let the ground send out radiation R per sq. cm. per sec. and of this let r_1R be reflected, a_1R be absorbed, and t_1R be transmitted by the glass. Here also $r_1 + a_1 + t_1 = 1$. It is to be noted that since the edges are far distant R is incident on each sq. cm. of glass. The glass, then, absorbs $aS + a_1R$, and as it is thin it may be taken as having the same temperature on each side, so that it sends down to the ground $\frac{1}{2}(aS + a_1R)$, the other half going upwards into space. Equating receipt and expenditure of radiation by the ground,

$$R = tS + r_1R + \tfrac{1}{2}(aS + a_1R),$$

whence on putting $r_1 = 1 - a_1 - t_1$ we obtain

$$R = \frac{t + \frac{a}{2}}{t_1 + \frac{a_1}{2}} S.$$

The values of t and a depend upon the glass. By way of illustration let us take $t = 0{\cdot}6$, $a = 0{\cdot}3$. For radiation from a surface under 100° C. Melloni found that even thin glass is quite opaque. We have then $t_1 = 0$, and if we neglect reflection, probably small, $a_1 = 1$.

Then $$R = \tfrac{75}{50}S = 1{\cdot}5\,S.$$

If the glass were removed we should have

$$R = S.$$

The temperature of the ground is therefore $\sqrt[4]{1{\cdot}5} = 1{\cdot}1$ times as high under the glass as it is in the open. If, for instance, it is 27° C. or 300° A. in the open, it is 330° A. or 57° C. under the glass.

If the glass reflects some of the radiation R then a_1 is less and the ground temperature is still higher.

If the ground, instead of being black, reflects a fraction ρ of the incident sunlight, or has total albedo ρ, the formula must be modified. If we take into account merely the first reflection from the ground and assume that the glass has absorption a for it, then we easily find

$$R = S\,\frac{t + \frac{a}{2} + \left(\frac{a}{2} - \rho\right) t}{t_1 + \frac{a_1}{2}}.$$

If we take $\rho = 0{\cdot}1$ the numerator is 0·78 instead of 0·75, and if we assume the fourth-power law for the low-temperature radiation emitted by the surface, the temperature is about 1 per cent. higher*. But the ground will probably reflect a much smaller fraction of the whole spectrum, and the correction for total albedo becomes inconsiderable.

If we replace the sun by cloud the radiation is, on the average, of much lower temperature, and t and a are much nearer to t_1 and a_1. The value of R S is then much nearer to 1, and the covered ground has a temperature much less raised above that of the open ground. This agrees of course with common experience.

A planetary atmosphere no doubt acts in some such way as the greenhouse glass. Let us, for the sake of comparison with Prof. Lowell's results, assume, as he has done, that we have a steady state, with the incident radiation normal to the surface. I do not see how to estimate the distribution of the radiation from the air between the upward stream into space and the downward stream to the surface. Since the lower layers of air are warmer than the upper probably more than half comes down, and the truth probably lies between the assumptions that the atmospheric radiation is $\frac{1}{2}(aS + a_1R)$ as it is with the greenhouse, and that it is $aS + a_1R$ when all the radiation would be downwards. Let us suppose that $\frac{1}{n}(aS + a_1R)$ comes downward.

The albedos of the surfaces of both the Earth and Mars average, according to Lowell, 0·1 for visible radiation. They must be much less for the whole spectrum. Where all the data are uncertain the effect of small albedo may be neglected, and indeed in our ignorance of the dependence of temperature on radiation in the case of a partially reflecting surface, it is safer to neglect it. If θ_s is the actual surface-temperature under a vertical sun, and θ is the temperature which the surface would have without atmosphere, it is easily found that

$$\theta_s = \sqrt[4]{\frac{t + a\ n}{t_1 + (n-1)\,a_{1,}\ n}}\,\theta.$$

Earth. If we use Lowell's figures for the Earth under a clear sky,

$$t = 0{\cdot}42, \quad a = 0{\cdot}5 \times 0{\cdot}65 = 0{\cdot}325,$$

$t_1 = 0{\cdot}5$, since of the invisible radiation half is transmitted,

$a_1 = 0{\cdot}5$, ... very little is reflected.

* [It would appear that in the preceding equation for R the term $\left(\frac{a}{2} - \rho\right)t$ should be $\left(\frac{a}{2}\quad 1\right)\rho t$. This would give the numerator as 0·70, and the temperature about 2 per cent. *lower*. Ed.]

We shall suppose in succession that

(*a*) half of the radiation is downwards or that $n = 2$,
(*b*) two-thirds .... $n = \frac{3}{2}$,
(*c*) all .... $n = 1$.

We then find

$$(a)\ \frac{\theta_s}{\theta} = 0{\cdot}94; \qquad (b)\ \frac{\theta_s}{\theta} = 0{\cdot}99; \qquad (c)\ \frac{\theta_s}{\theta} = 1{\cdot}12.$$

For the case of a cloud-covered earth the data are very uncertain. Lowell takes $t = 0{\cdot}2$ of $0{\cdot}42 = 0{\cdot}084$, assuming that the atmosphere has already reflected and absorbed 0·58 before the cloud is reached, surely an overestimate, since the cloud-surface is in the higher air. Let us guess that $t = 0{\cdot}1$. The absorption without cloud is according to Lowell about 0·3. With cloud much is reflected back without reaching the lower and more absorbing regions. Let us guess that $a = 0{\cdot}2$. Of the radiation from the surface we may suppose perhaps that 0·2 passes through, that 0·7 is reflected, and that 0·1 is absorbed. Of the 0·2 passing we may suppose that 0·1 is absorbed and 0·1 goes into space. Then $t_1 = 0{\cdot}1$ and $a_1 = 0{\cdot}2$.

With these values we get for the different values of n

$$(a)\ \frac{\theta_s}{\theta} = 1; \qquad (b)\ \frac{\theta_s}{\theta} = 1{\cdot}08; \qquad (c)\ \frac{\theta_s}{\theta} = 1{\cdot}31.$$

These guesses, then, make the temperature under a cloudy sky at least as great as under a clear sky. But this is certainly not true in common experience, where, however, we may have clouds accompanied by cold winds and no approach to the steady state here assumed. The results merely serve to show that with certain absorptions and transmissions clouds might actually raise the surface-temperature, and that for the present it is better to neglect them.

Mars. If we apply Lowell's data for Mars we have

$$t - 0{\cdot}64, \text{ and } a = 0{\cdot}40 \times 0{\cdot}65 = 0{\cdot}26,$$
$$t_1 = 0{\cdot}6, \text{ and } a_1 = 0{\cdot}4, \text{ since } R \text{ is dark radiation.}$$

With these values we get for the different values of n

$$(a)\ \frac{\theta_s}{\theta} = 0{\cdot}99; \qquad (b)\ \frac{\theta_s}{\theta} = 1{\cdot}02; \qquad (c)\ \frac{\theta_s}{\theta} = 1{\cdot}10.$$

Comparison of the Earth and Mars. Let us take the temperature of the Earth as 17° C. or 290° A. If it were removed to the distance of Mars its temperature would be inversely as the square-root of the distance, which is 1·524 that of the Earth, or 290/1·235 = 235° A.

With the different values of n the temperature of Mars should be

$$(a)\quad 235 \times \tfrac{99}{94} \;= 247^\circ \text{ A. or } -26^\circ \text{ C.,}$$
$$(b)\quad 235 \times \tfrac{102}{99} = 242^\circ \text{ A. or } -31^\circ \text{ C.,}$$
$$(c)\quad 235 \times \tfrac{110}{112} = 231^\circ \text{ A. or } -42^\circ \text{ C.}$$

Of course the data are very uncertain and the formula used is only an approximation. But with these data it is hard to see how the temperature of Mars can be raised to anything like the value obtained by Professor Lowell. Perhaps the data are quite wrong. It is conceivable that Mars has a quite peculiar atmosphere practically opaque to radiations from the cold surface. Those who believe that there is good evidence for the existence of intelligent beings on that planet, should find no difficulty in supposing that they have been sufficiently intelligent to cover the planet with a glass roof or its equivalent. Then we might easily have $t+\frac{a}{2}$ 0·77 and $t_1+\frac{a_1}{2}$ 0·5, and then the temperature might be raised to 281° A. or 8 C. Indeed, if the glass were of such kind as to transmit solar radiation, and if it were quite opaque to dark radiation while still reflecting a considerable proportion, the temperature might easily be raised far above this.

An Attempt to represent the Effect of Day and Night on the Temperature of the Earth.

The 'greenhouse' formula, which has been used in the foregoing discussion, would hold only if all the conditions were steady. But in reality the alternations of day and night prevent a steady state, and we can only hope that the neglect of these alternations does not greatly affect the ratios of the temperatures found for different planets or for different elevations on the same planet.

I shall now attempt to represent the effect of the diurnal variation in the supply of solar heat to the Earth, or rather to an abstract Earth. For even if we could represent the actual conditions we should obtain differential equations so complicated that they would be useless for practical purposes.

To simplify matters, let us suppose that we are dealing with the equatorial region of the earth at the equinox, that the air is still, that the surface is solid and black, and that the sky is clear.

The temperature of the air except near the surface can change but little during 24 hours. For over each square centimetre at sea-level we have 1000 gms. of air with specific heat 0·2375, and therefore with heat capacity 237·5. Consider a band of the atmosphere 1 cm. wide round the equator. A stream of solar radiation of length equal to the diameter $2r$ of the earth enters a band of air of length equal to half the circumference. If the solar constant is 3 the average energy entering a sq. cm. column is

$$\frac{2rS}{\pi r} = \frac{2S}{\pi} = \frac{6}{\pi} \text{ cal./min.}$$

Then in 12 hours 1375 cal. enter on the average, and if this heat were all absorbed and retained it would raise the temperature on the average about 1375 237·5 = 5°·8 C.

As the absorption is only partial and as radiation takes place from the air, the rise cannot really average nearly as much as this.

Again, consider the radiation during the twelve hours of night. If the air were a black body and of temperature 300° A., and these are absurdly exaggerated estimates of its radiating power and of its average temperature, it would only radiate about 1·2 cal./min. per sq. cm. column from its two surfaces, or 864 calories in the twelve hours, and neglecting the radiation from the ground the temperature would only fall about 864/237·5 or 3°·6 C. Obviously, then, the air as a whole cannot undergo much variation in temperature as day alternates with night. It is indeed a flywheel storing the energy of many diurnal revolutions. We may, then, in a rough estimate consider that its temperature, and therefore its radiation, remains constant during the 24 hours.

If the total radiation from a sq. cm. column per second is A, there will be a stream D downwards and U upwards where $D + U = A$. We can find an expression for A by equating it to the average absorption. Considering an equatorial band 1 cm. wide, the average energy entering it per sq. cm. in the 24 hours is $\frac{S}{\pi}$. Let the average amount absorbed be $\frac{\bar{a}S}{\pi}$. The value of a at sea-level varies for clear sky from perhaps 0·3 with the zenith sun to very nearly 1 with the setting sun. Let the average radiation from the surface during the 24 hours be $\bar{R}$, of which $a_1\bar{R}$ is absorbed by the atmosphere. Then neglecting conduction through the air, the constant-temperature assumption gives us

$$A = \frac{\bar{a}S}{\pi} + a_1\bar{R}.$$

If a fraction $\frac{1}{n}$ is radiated downwards

$$D = \frac{\bar{a}S}{n\pi} + \frac{a_1\bar{R}}{n}.$$

The actual surface-temperature depends not only on radiation but also on conduction both by ground and air. But we shall neglect this conduction and shall suppose that the surface has reached an equilibrium between receipt and expenditure of radiation. This is a condition to which the surface tends at or soon after noon by day, and before dawn at night. We shall suppose that the low-temperature radiation from the surface is either transmitted or absorbed, so that, using the previous notation,

$$t_1 + a_1 = 1 \quad \text{and} \quad r_1 = 0.$$

If R_d is the equilibrium surface-radiation reached, we suppose about noon,

$$R_d = tS + \frac{\bar{a}S}{n\pi} + \frac{a_1\bar{R}}{n}.$$

If R_n is the equilibrium surface-radiation in the later part of the night, we have to omit tS, and

$$R_n = \frac{\bar{a}S}{n\pi} + \frac{a_1\bar{R}}{n}.$$

To proceed further, we must express $\bar{R}$ in terms of S. We can only do this by some assumption. Probably it is not very far from the truth to assume that $\bar{R} = \frac{1}{2}(R_d + R_n)$, and we shall take this value. It gives us

$$\bar{R} = \frac{\dfrac{t}{2} + \dfrac{\bar{a}}{n\pi}}{1 - \dfrac{a_1}{n}} S,$$

and substituting in the values of day and night radiations we get

$$\frac{R_d}{S} = t + \frac{\bar{a}}{n\pi} + \frac{a_1}{n} \cdot \frac{\dfrac{t}{2} + \dfrac{\bar{a}}{n\pi}}{1 - \dfrac{a_1}{n}},$$

$$\frac{R_n}{S} = \frac{\bar{a}}{n\pi} + \frac{a_1}{n} \frac{\dfrac{t}{2} + \dfrac{\bar{a}}{n\pi}}{1 - \dfrac{a_1}{n}}.$$

Though these formulae are only obtained by making large assumptions, and by neglecting important considerations, they nevertheless show the tendency of the day and night effect, and it is worth while to apply them to the Earth, taking the best data at our command.

At the surface let us take $t = 0{\cdot}42$ and $a_1 = 0{\cdot}5$ as before. For $\bar{a}$ we have no trustworthy observations, and I doubt whether a calculation from Langley's observations is of any more value than an estimate. Since a varies from perhaps about 0·3 to 1, let us take $\bar{a} = 0{\cdot}628$ or $2\pi/10$, a value simplifying arithmetic.

At the level of Camp Whitney 3550 metres above sea-level, with barometer about 500 mm., and therefore with about $\frac{1}{3}$ of the atmosphere below it, we may take $t = 0{\cdot}6$ and $a_1 = 0{\cdot}4$. For $\bar{a}$ we must take a value much smaller than that at sea-level. Since the most absorbing third of the atmosphere is below, I do not think it is far wrong to take $\bar{a}$ as having half the value at the lower level, and I therefore put $\bar{a} = 0{\cdot}314$. But I have also examined the consequences of putting it equal to 0·419, i.e. $\frac{4}{3}$ of its value at the lower level, and the results are given below to show how much the figures are affected by the variation in the value taken.

We have no data for n. I have therefore calculated the values of R_d and R_n in terms of S for successive values of n equal to 1, $\frac{5}{4}$, $\frac{4}{3}$, $\frac{3}{2}$, 2; corresponding to D equal to A, $\frac{4}{5}A$, $\frac{3}{4}A$, $\frac{2}{3}A$, and $\frac{1}{2}A$ respectively.

In the following tables the values of R_d/S and R_n/S are given, and also the mean $\bar{R}/S = \frac{1}{2}(R_d + R_n)\,S$. Then follow the ratios of the day and night temperatures, θ_d and θ_n, to the temperature θ of a black surface radiating S, and then the mean value $\bar{\theta}/\theta$. The last column gives the range $\theta_d - \theta_n$ on the supposition that $\bar{\theta} = 300°$ A.

The third table is only given to show that the change in the value of $\bar{a}$ does not greatly affect the results. The value of $\bar{a}$ of Table II is much more reasonable if that of Table I is near the truth. We need, therefore, only compare the results given in the first two tables.

If we take the same values of n in each table, the value of $\bar{R}$ is less at the higher level than at the lower in every case except that in which n has the extreme and probably inadmissible value of 2. The value of $\bar{\theta}$ is less at the higher level in every case. But it appears most probable that $1/n$ or D/A is greater at the lower level than at the higher. For consider a thin layer of air at sea-level. It is radiating equally up and down, but of the half going upwards a considerable fraction will be intercepted by the superincumbent and strongly absorbing layers. Now consider a thin layer close to the surface at the higher level. It, too, radiates half up and half down. But of the half going upwards a less fraction will be intercepted since the superincumbent layers are now less absorbing. Thus D/A will be greater at the lower than at the higher level*. We should, therefore, compare the results for any value of D/A in Table I with the results in Table II for a somewhat lower value.

We may exclude the extreme cases of $n = 2$ and $n = 1$, as the true value is certainly between these, and confine our examination to intermediate values.

Suppose, for example, that $D/A = 4/5$ at the lower level, while it is $3/4$ at the upper level. Then $\bar{\theta}/\theta = 0{\cdot}88$ from Table I at the lower level, while $\bar{\theta}/\theta = 0{\cdot}83$ from Table II at the upper level. Or if $D/A = 3/4$ at the lower level, while it is $2/3$ at the upper level, $\bar{\theta}/\theta = 0{\cdot}86$ below, while $\bar{\theta}/\theta = 0{\cdot}81$ above. Or in each case the mean temperature is higher at sea-level by about 5 in 87 or by about 17° in 300°.

* Another consideration leading to the same conclusion is that the atmosphere acts like a plate with its lower surface much warmer than its upper. When we only have the part above an elevated region the difference of temperature between the surfaces is much less than for the whole air, and the radiations up and down are more nearly equal.

TABLE I.

At sea-level. $t = 0{\cdot}42$, $a_1 = 0{\cdot}5$, $\bar{a} = 0{\cdot}628$.

n	D/A	R_d/S	R_n/S	$\bar{R}/S$	$\theta_d\ \theta$	$\theta_n\ \theta$	θ/θ	Range about 300° A.
1	1	1·03	0·61	0·83	1·01	0·88	0·95	41°
5/4	4/5	0·83	0·41	0·62	0·95	0·80	0·88	51
4/3	3/4	0·79	0·37	0·58	0·94	0·78	0·86	56
3/2	2/3	0·72	0·30	0·51	0·92	0·74	0·83	65
2	1/2	0·62	0·20	0·41	0·89	0·67	0·78	85

TABLE II.

At 3550 m. above sea-level. Barometer 500 mm.
$t = 0{\cdot}6$, $a_1 = 0{\cdot}4$, $\bar{a} = 0{\cdot}314$.

n	D/A	R_d/S	R_n/S	R/S	θ_d/θ	θ_n/θ	$\theta\ \theta$	Range about 300 A.
1	1	0·97	0·37	0·67	0·99	0·78	0·89	71
5/4	4/5	0·86	0·26	0·56	0·96	0·71	0·84	89
4 3	3/4	0·84	0·24	0·54	0·96	0·70	0·83	94
3/2	2/3	0·80	0·20	0·50	0·95	0·67	0·81	100
2	1/2	0·74	0·14	0·44	0·93	0·61	0·77	125

TABLE III.

At 3550 m. above sea-level and with $t = 0{\cdot}6$, $a_1 = 0{\cdot}4$,
but with $\bar{a} = 0{\cdot}419 = 2/3$ of $0{\cdot}628$.

n	D/A	R_d/S	R_n/S	R/S	$\theta_d\ \theta$	$\theta_n\ \theta$	$\theta\ \theta$	Range about 300 A.
1	1	1·02	0·42	0·72	1·01	0·81	0·91	66
5 4	4 5	0·90	0·30	0·60	0·97	0·74	0·86	80°
4/3	3/4	0·87	0·27	0·57	0·97	0·72	0·85	88
3 2	2/3	0·83	0·23	0·53	0·95	0·69	0·82	95
2	1/2	0·76	0·16	0·46	0·93	0·63	0·75	120

It is to be observed that the lower mean temperature at a higher level must hold good if the higher level is so much higher that there is practically no atmosphere above. For then $t = 1$ and $a_1 = 0$, so that $R_d = S$ and $R_n = 0$. Therefore $\theta_d/\theta = 1$ and $\theta_n/\theta = 0$ and $\bar{\theta}/\theta = 1/2$.

The lower mean temperature of elevated parts of the earth's surface is a well-established fact. Perhaps if it were only observed in the case of mountain peaks it might be ascribed to the cold air blowing against them. The fall of temperature in free air as we go upwards tends towards that given by convective equilibrium, though recent observations show that it is not so great as that given by the adiabatic law. Thus for a rise of 3500 metres the adiabatic law would give a fall of about 32° C. if the sea-level temperature were 300° A.; whereas the observations of Teisserenc de Bort at Trappes show a mean annual fall of about 16° C. for this rise (*Encyc. Brit.* vol. 30, Meteorology, p. 695). A continual blast of air thus cooled might of course reduce the temperature on the mountain peaks, even if radiation did not tend to any such reduction. But we can hardly account in this way for the equally well-established lower temperature of elevated continental plateaus. According to Abbe (*loc. cit.* p. 694) 0°·5 C. must be subtracted from sea-level temperature for every 100 metres general elevation of the land-surface or about 18° for an elevation of 3500 metres, and this fall may be ascribed to radiation in some such way as that here set forth.

If the atmosphere of Mars is comparable with our own atmosphere at high levels, and if the effect is of the same general character in the two cases, it appears probable that the surface-temperature of Mars is actually lower by many degrees than that which the surface of the Earth would have at the same distance from the Sun.

24.

THE MOMENTUM OF A BEAM OF LIGHT.

[*Atti del IV Congresso internazionale dei Matematici* (Rome), **3**, 1909, pp. 169–174.]

[The substance of this paper is contained in the Address to the French Physical Society, March 1910. *Collected Papers*, Art. 70. ED.]

25.

ON PRESSURE PERPENDICULAR TO THE SHEAR-PLANES IN FINITE PURE SHEARS, AND ON THE LENGTHENING OF LOADED WIRES WHEN TWISTED.

[*Roy. Soc. Proc.* A, **82**, 1909, pp. 546–559.]

[*Read* June 24, 1909.]

In the *Philosophical Magazine*, vol. 9, 1905, p. 397*, I gave an analysis of the stresses in a pure shear which appeared to show that if ϵ is the angle of shear and if n is the rigidity, then a pressure $n\epsilon^2$ exists perpendicular to the planes of shear. That analysis is, I believe, faulty in that the diagonals of the rhombus into which a square is sheared are not the lines of greatest elongation and contraction, and are not at right angles after the shear, when second-order quantities are taken into account, i.e., quantities of the order of ϵ^2; I think the following analysis is more correct, and though it does not give a definite result, it leaves the existence of a longitudinal pressure an open question. The question appears to be answered in the affirmative by some experiments, described in the second part of the paper, in which loaded wires when twisted were found to lengthen by a small amount proportional to the square of the twist.

I. *Stresses in a Pure Shear.*

Let a square $ABCD$ (Fig. 1) of side a be sheared into $EFCD$ by motion through $AE = d$, the volume being constant. The angle of shear is $ADE = \epsilon$, and $\tan \epsilon = d/a$ exactly; neglecting ϵ^3, we may put $\epsilon = d/a$.

To find which line is stretched most by the shear, consider the line r drawn from D to P and making an angle θ with DC before stretching.

Let it stretch to ρ, making an angle θ' with DC; we have $r = a/\sin\theta$ and $\rho = a/\sin\theta'$;

also $$\rho^2 = r^2 + 2rd\cos\theta + d^2;$$

thus $$\rho^2/r^2 = 1 + 2d/r \, . \cos\theta + d^2/r^2,$$

$$= 1 + 2d/a \, . \sin\theta\cos\theta + d^2/a^2 \, . \sin^2\theta.$$

* [*Collected Papers*, Art. 22, p. 338.]

Differentiating ρ^2/r^2 with respect to θ, it is a maximum when

$$2d/a \,.\, \cos 2\theta + d^2/a^2 \,.\, \sin 2\theta = 0,$$

or $$\tan 2\theta = -\,2a/d = -\,2 \cot \epsilon.$$

Put $$\theta = 45^\circ + \delta, \qquad \text{then} \qquad \tan 2\delta = \tfrac{1}{2} \tan \epsilon,$$

or $\delta = \frac{1}{4}\epsilon$ to the second order, so that r makes an angle $\frac{1}{4}\epsilon$ with the diagonal DB of the square through D, and on the upper side.

If the same shear is now made in the opposite direction ρ contracts to r, and the same directions of ρ before, and r after shear, give the maximum contraction. It is almost obvious that ρ makes an angle $\frac{1}{4}\epsilon$ with DB on the lower side, but it may be verified by putting

$$r^2 = \rho^2 - 2\rho d \cos \theta' + d^2,$$

and finding the maximum value of r^2/ρ^2 after putting $\rho = a/\sin \theta'$ on the right.

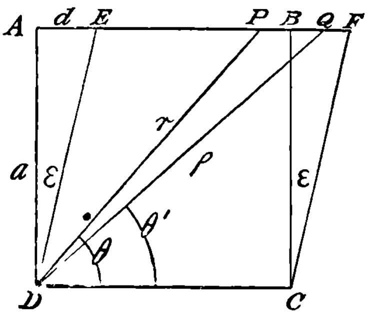


Fig. 1.

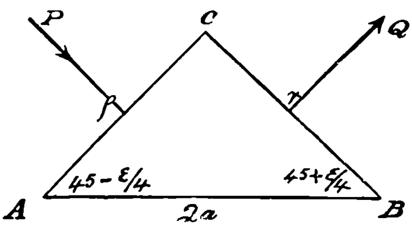


Fig. 2.

Hence the lines of maximum elongation and contraction are at $\frac{1}{4}\epsilon$ with the diagonals of the square, and are at right angles before and after the strain, to the order of ϵ^2. It is noteworthy that as the shear increases the fibres which undergo maximum elongation and contraction change.

To find r and ρ put

$$r = a/\sin \theta = a \sin (45^\circ + \tfrac{1}{4}\epsilon),$$

then $$r = \sqrt{2}\,a\,(1 - \tfrac{1}{4}\epsilon + \tfrac{3}{32}\epsilon^2);$$

and changing the sign of ϵ we get

$$\rho = \sqrt{2}\,a\,(1 + \tfrac{1}{4}\epsilon + \tfrac{3}{32}\epsilon^2).$$

It is easily seen that the elongation and contraction are respectively

$$e = (\rho - r)/r = \tfrac{1}{2}\epsilon\,(1 + \tfrac{1}{4}\epsilon); \qquad c = (r - \rho)/\rho = \tfrac{1}{2}\epsilon\,(1 - \tfrac{1}{4}\epsilon).$$

We shall now consider the stresses. We shall assume that a pressure P is put on in the direction of maximum contraction and a tension Q in the direction of maximum elongation, these being, as we have seen, at right angles; and we shall consider the equilibrium of the wedge ABC (Fig. 2) when sheared, assuming that P and Q are the only forces on AC and BC. Let $AB = 2a$; $AC = \rho$; $BC = r$; these having the values just found.

Resolve in a direction perpendicular to the base, and let R be the pressure against the base, then

$$\begin{aligned} R \,.\, 2a &= P\rho \cos(45^\circ - \tfrac{1}{4}\epsilon) - Qr \cos(45^\circ + \tfrac{1}{4}\epsilon) \\ &= Pa \cot(45^\circ - \tfrac{1}{4}\epsilon) - Qa \cot(45^\circ + \tfrac{1}{4}\epsilon) \\ &= Pa \frac{1 + \frac{1}{4}\epsilon}{1 - \frac{1}{4}\epsilon} - Qa \frac{1 - \frac{1}{4}\epsilon}{1 + \frac{1}{4}\epsilon} \\ &= Pa(1 + \tfrac{1}{2}\epsilon + \tfrac{1}{8}\epsilon^2) - Qa(1 - \tfrac{1}{2}\epsilon + \tfrac{1}{8}\epsilon^2) \\ &= (P - Q)\, a + (P + Q) \,.\, \tfrac{1}{2}a\epsilon + (P - Q) \,.\, \tfrac{1}{8}a\epsilon^2, \end{aligned}$$

where P and Q can only be taken as equal to the first order. Proceeding to the second order, we must put

$$P = n\epsilon + p\epsilon^2;$$

then

$$Q = n\epsilon - p\epsilon^2,$$

where p is a constant to the second order.

Thus

$$P - Q = 2p\epsilon^2 \quad \text{and} \quad P + Q = 2n\epsilon,$$

and

$$R = (\tfrac{1}{2}n + p)\,\epsilon^2,$$

the third term being negligible. If we resolve parallel to the base, it is easily found that the tangential stress is

$$T = \tfrac{1}{2}(P + Q) = n\epsilon.$$

If the shear is produced by a tangential stress T, then it requires the system P, Q, and R to maintain equilibrium with it.

It is possible that a stress exists perpendicular to the plane of the figure in Fig. 1. It can only be assumed that the changes of dimension in that direction neutralise each other to the first order when equal pushes and pulls are put on in the plane of the figure; when the dimensions perpendicular to the figure are constrained to remain the same to the second order—and this is our supposition—it may require a tension or pressure to effect this.

Let us suppose that a pressure $S = q\epsilon^2$ is introduced, a tension if q is negative. To make $R = 0$ we should require to have $p = -\tfrac{1}{2}n$; also P would then be less than Q. If pressure perpendicular to AC is exerted alone, and then tension perpendicular to BC is exerted alone, it appears probable that for very large equal compressions and extensions P is greater than Q. If we suppose that when they are simultaneous the tendency is in the same direction, then R should have a positive value, or the longitudinal pressure perpendicular to AB should exist.

Let us examine the consequences of the supposition that both R and S exist. Let a thin tube of length l and of radius a be fixed at one end, and let the other end be twisted through an angle θ so that the angle of shear is $\epsilon = a\theta/l$. Let an end-pressure $R = (\frac{1}{2}n + p)\,\epsilon^2$ be put on, and also a side-pressure $S = q\epsilon^2$ so as to maintain constant dimensions.

The side-pressure S may be replaced by a uniform pressure S over the whole surface, and a tension S over the ends. We have then an end-pressure $R - S$ and a pressure S all over.

Now suppose that these forces are removed. Through the removal of $R - S$ we shall have a lengthening dl_1 given by

$$(\tfrac{1}{2}n + p - q)\,\epsilon^2 = Y\,dl_1/l,$$

and a contraction δ_1 of the diameter given by

$$\delta_1/2a = \sigma\,dl_1/l,$$

where Y is Young's modulus and σ is Poisson's ratio.

Through the removal of the pressure S we shall have a lengthening dl_2 given by $q\epsilon^2 = 3K dl_2/l$, where K is the bulk-modulus, and an expansion δ_2 of the diameter given by $q\epsilon^2 = 3K\delta_2/2a$.

The end-lengthening is therefore

$$dl = dl_1 + dl_2 = \{(\tfrac{1}{2}n + p - q)/Y + q\ 3K\}\,l\epsilon^2;$$

or putting $$1/3K = 3/Y - 1/n,$$

$$dl = \{(\tfrac{1}{2}n + p)/Y + (2/Y - 1/n)\,q\}\,l\epsilon^2 = sl\epsilon^2 = sa^2\theta^2/l,$$

where s is put for $$(\tfrac{1}{2}n + p)/Y + (2/Y - 1/n)\,q.$$

The diameter decreases by

$$\begin{aligned}\delta = \delta_1 - \delta_2 &= \{(\tfrac{1}{2}n + p - q)\,\sigma/Y - q/3K\}\,2a\epsilon^2 \\ &= \{(\tfrac{1}{2}n + p)\,\sigma/Y - [(3 + \sigma)/Y - 1/n]\,q\}\,2a^3\theta^2/l^2.\end{aligned}$$

It would not be easy to test this result with a thin tube. But if we suppose that a wire extends by the amount equal to the average extension of the tubes into which it may be resolved, we get

$$dl = \frac{1}{\pi a^2}\int_0^a \frac{2\pi r s r^2\theta^2}{l}\,dr = \tfrac{1}{2}sa^2\theta^2/l.$$

I now proceed to describe some experiments which show that such an extension exists.

II. *The Lengthening of Loaded Wires when twisted.*

Experiments were made on several wires hung vertically from a fixed support, and loaded in order that kinks or remnants of the spiral due to the coiling to which they had been subjected might be taken out. This was considered to be effected when the stretch for a given addition of load was sensibly the same whether the wire was twisted or not. An account of the twisting of a steel wire before this stage was reached will be given later. Fig. 3 represents the arrangement more or less diagrammatically. The upper end of the wire to be twisted was fixed to a stout bracket *B* near the ceiling. The wire was always about 231 cm. long; its lower end was clamped in jaws in the upper end of a turned steel rod *rr*, 51 cm. long, which passed through a hole in the table *T* on which was the observing microscope *M*, and a parallel-plate micrometer *m*. One division of this micrometer was equal to 0·00974 mm. At the lower end of the rod was a horizontal iron cross-piece *cc*, 19 cm. long and 1·6 cm. square. From the lower end was suspended a carrier for the weights, or for the two stouter wires the weight itself, connected to the rod by a flat steel strip twisted in its middle, so that the upper and lower halves were in two vertical planes at right angles. Below the weights was a set of vanes immersed in a shallow bath of oil to damp vibrations. This bath rested on a circular turntable *tt*, on which were two uprights *u*, *u* at opposite ends of a diameter, with horizontal screws at their upper ends which could be brought to bear against the ends of the cross-piece as shown in the plan, Fig. 4. The screws ended in small steel balls and the sides of the cross-piece were polished. On rotating the turntable the screws came against the cross-piece and turned it round; and so the wire was twisted by a couple with vertical axis. The axis of the turntable was made vertical by means of the levelling-screws *l*, *l*. To adjust this axis in the axis of the wire prolonged, the turntable could be moved over the base-plate by means of the horizontal screws *s*, of which only one is represented in Fig. 3. All are shown in Fig. 4.

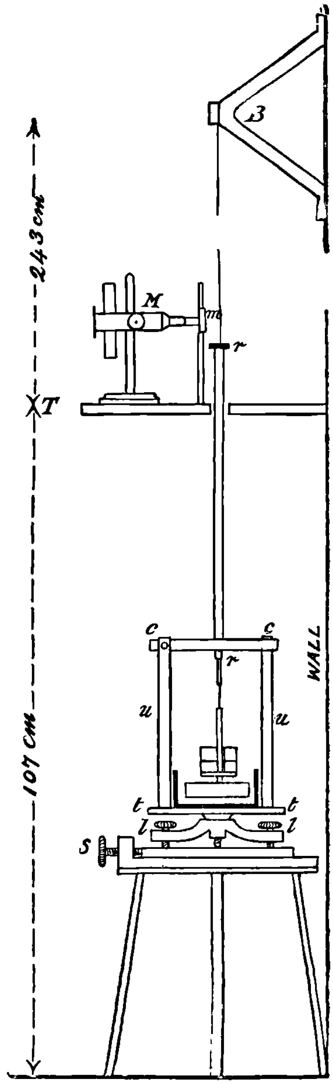


Fig. 3. Elevation of Arrangement for Twisting the Wires.

A horizontal microscope, not represented in the figure, was attached to one of the uprights and focussed on the edge of the rod *rr*. The adjustment by the screws *s* was continued until the microscope always saw the edge of the rod in the middle of the field, however the turntable might be turned.

To give a definite point of view in the microscope M, in the earlier experiments starch-grains were put on the wire about 1 cm. from the lower end. These were illuminated, and a suitable one was selected.

In the later experiments a needle, about 1 cm. long, was fixed, point upwards, on the upper end of the rod close alongside the wire, and the needle-point was viewed. This was better than the starch-grains.

In the earlier work the temperature of the room was fairly steady, and the changes in length due to temperature-variations were too slow to give trouble. But in some gusty weather occurring later there were such rapid and considerable variations in the temperature of the room that it was necessary to enclose the wire in a wooden tube. After this was done temperature gave no further trouble, whatever the weather.

In order to observe the effect of a twist the turntable was levelled and adjusted axially when the wire and cross-piece were free. The turntable was rotated till the screws on the uprights just touched the cross-piece. Then chalk-marks were made on the turntable and on the plate below, one just over the other. The microscope was adjusted exactly to sight the upper or lower edge of a starch-grain on its horizontal cross-wire, and the micrometer was read. Then the turntable was rotated so many whole turns, and the micrometer-plate was moved till the edge of the grain was again on the cross-wire and the micrometer was read again.

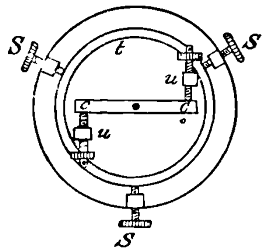


Fig. 4. Plan of the Cross-piece and Turntable.

Except in the case of a wire stretched only by the weight of the rod and cross-piece, in some experiments described later, there was always a lengthening on twisting, of the same order whether the twist was clockwise or counter-clockwise. The lengthening was nearly proportional to the square of the twist put on. It was necessary to limit the twist to a few turns to avoid permanent set, and when such a small twist had been given and the wire was untwisted it returned sensibly to its original length.

The lowering was entirely due to twisting and not to any giving of the support, for when a microscope was sighted on a point on the wire close to the upper end, no change in level could be detected, when the wire was twisted through 5 turns at its lower end. This was further verified by an experiment on a steel wire from the same piece as No. 3 below, which showed that the extension half-way down the wire was, within the limits of experimental error, half that at the lower end. A microscope and micrometer were fixed on a table half-way up the wire, and a needle-point was fixed here as well as at the lower end. At each twist and untwist both micrometers were read. I give the observations in this experiment in full, as they will show the sort of accuracy attained.

The lower end was twisted from a starting twist of $\frac{1}{4}$ turn to $4\frac{1}{4}$ turns.

Micrometer-Readings at Lower End.

$\frac{1}{4}$ turn	$4\frac{1}{4}$ turns	Lowering
22·3	18·6	3·7
22·5	19·0	3·5
23·0	19·2	3·8
22·6	19·4	3·2
22·9	19·6	3·3
22·6	19·5	3·1
23·0	19·5	3·5
23·0	18·9	4·1
22·7	19·2	3·5
22·9	19·6	3·3

Mean lowering, 3·50 divisions.
One division of micrometer = 0·00974 mm.
The lowering is 0·0341 mm.

Micrometer-Readings Half-way up the Wire.

$\frac{1}{4}$ turn	$4\frac{1}{4}$ turns	Lowering
30·4	28·3	2·1
30·5	28·4	2·1
30·2	28·1	2·1
30·5	28·5	2·0
30·5	28·0	2·5
30·4	28·7	1·7
31·0	28·4	2·6
30·6	28·5	2·1
31·8*	29·9	1·9
31·6*	29·1†	2·5

Mean lowering, 2·16 divisions.
One division of micrometer = 0·00751 mm.
The lowering is 0·0162 mm.

If the lowering at the end is accurate, that half-way up should be 0·0171 mm. The observed lowering is as nearly equal to this as could be expected.

With the first wire, determinations of extension due to an addition of 520 grammes were made both in the untwisted and twisted conditions, as it was only when these became sensibly equal that the lowering on twist became equal for different loads. The extra load could be put on or taken off by lowering or raising a lever, not represented in Fig. 3. It is unnecessary

* Another point on the needle sighted.

† [This number has been corrected from 29·9 in accordance with the original MS. A few other small slips in the results which follow have been similarly corrected. ED.]

to describe the details of this arrangement. The experiments with the other wires were made with such loads that it was not considered necessary to observe the stretch due to addition of load.

Results.

1a. Steel piano-wire, diameter 0·720 mm. (mean of 10 measurements at different points), length to observing point in this and all cases 230 cm. Permanent set, after putting on eight turns twist and then untwisting, only a very few degrees. Total load, 7081 grammes.

The twist is termed clockwise when the turntable as viewed from above is moved clockwise.

Clockwise twist, 0–4 turns;	lowering	0·0181 mm.,	mean of 10 observations.
,, ,, 0–8 ,,	,,	0·0732	,, ,, ,,

The ratio of these is 4·04 : 1.

The extension due to an addition of 520 grammes was:

No twist on the wire		0·143 mm.,	mean of 10 observations.
4 turns	,,	0·141	,, ,, ,,
8 ,,	,,	0·143	,, ,, ,,

1b. Same wire.

Total load, 9081 grammes.

Clockwise twist, 0–4 turns;	lowering	0·0180 mm.,	mean of 20 observations.
,, ,, 0–8 ,,	,,	0·0749	,, ,, ,,

The ratio of these is 4·15 : 1.

The extension due to an addition of 520 grammes was:

No twist on the wire		0·142 mm.,	mean of 10 observations.
8 turns	,,	0·144	,, ,, ,,

Taking the mean lowering for the two loads of 7081 and 9081 grammes for eight turns twist, viz., 0·074 mm., and taking it as proportional to the square of the twist, the lowering for one turn is 0·00116 mm., and

$$s = 2l\,dl/a^2\theta^2 = 1{\cdot}043.$$

The moduli of elasticity of this wire were found to be

$$n = 0{\cdot}769 \times 10^{12}, \qquad Y = 2{\cdot}013 \times 10^{12},$$

whence $n/Y = 0{\cdot}382$. The value of n, found for loads of 1081 grammes and 9081 grammes respectively, was identical.

2. The same wire was raised to a red heat, by an electric current, with the load of 9081 grammes on it. It lengthened about 3 cm., and this length was cut off. The surface oxidised, and when the oxide was rubbed off the diameter was 0·696 mm. (mean of 10 measurements).

The permanent set after twisting and untwisting was greater, and so only three turns were given.

Total load, 9081 grammes.

Clockwise twist, 0–3 turns; lowering 0·0129 mm., mean of five observations.

The extension due to an addition of 520 grammes was:

No twist on the wire		0·155 mm., mean of 10 observations.			
3 turns	,,	0·154	,,	,,	,,

The lowering for one turn according to the square-law is: 0·00143 mm., whence $s = 1{\cdot}376$ mm.

The five values of the lowering were: 1·5, 1·2, 1·2, 1·2, 1·5 divisions, mean 1·32 divisions. With such small lowering no accuracy could be expected, and it would be difficult to verify the square-law.

The moduli of elasticity for the softened wire were:

$$n = 0{\cdot}809 \times 10^{12} \quad \text{and} \quad Y = 2{\cdot}06 \times 10^{12},$$

whence

$$n/Y = 0{\cdot}393.$$

3. Steel piano-wire, diameter 0·970 mm. (mean of 10 measurements).

A needle-point fixed at the side of the wire was viewed in the microscope. After twisting and untwisting, a slight permanent set threw the point out of focus if the start was from no twist. A quarter-turn was therefore put on initially, and the twisting was from this, and the untwisting was back to it.

Total load, 19,504 grammes.

Clockwise twist, $\frac{1}{4}$–$2\frac{1}{4}$ turns; lowering 0·0088 mm., mean of 10 observations.
,, ,, $\frac{1}{4}$–$4\frac{1}{4}$,, ,, 0·0343 ,, ,, ,,
Counter-clockwise twist, $\frac{1}{4}$–$2\frac{1}{4}$ turns; lowering 0·0090 mm., mean of 10 obs.
,, ,, ,, $\frac{1}{4}$–$4\frac{1}{4}$,, ,, 0·0344 ,, ,,

Mean lowering, $\frac{1}{4}$–$2\frac{1}{4}$ turns, 0·0089 mm.
,, ,, $\frac{1}{4}$–$4\frac{1}{4}$ turns, 0·0344 ,,

By the square-law the lowerings for $4\frac{1}{4}$, $2\frac{1}{4}$, and $\frac{1}{4}$ should be as 289 : 81 : 1, and the difference should be as 288 : 80 = 18 : 5.

The observed differences are as 19·4 : 5.

The lowering for one turn deduced from the difference between $\frac{1}{4}$ and $4\frac{1}{4}$ is 0·00191 mm., whence $s = 0{\cdot}946$.

Comparing the lowerings for one turn of this wire with the hard wire No. 1, if the lowering is proportional to the square of the diameter, we ought to have for No. 1 a lowering of $0{\cdot}00191 \times (72/97)^2 = 0{\cdot}00105$ mm. The observed lowering was 0·00116 mm., which is as near the calculated value as could be expected.

4. The same wire was then raised to a red heat by an electric current with the load on. After being rubbed down its diameter was 0·947 mm. (mean of 10 measurements). Same load as in experiment 3.

Clockwise twist, $\frac{1}{4}$–$3\frac{1}{4}$ turns, lowering 0·0207 mm., mean of 10 observations.

The deduced lowering for one turn is 0·00197 mm.

The value of s is 1·025 mm.

Comparing the lowerings for one turn of this wire with the softened wire No. 2, the square-law for the diameter should give for No. 2 a lowering $0{\cdot}00197 \times (696/947)^2 = 0{\cdot}00106$ mm. The observed lowering was 0·00143 mm., a considerable divergence.

5. Copper wire, diameter 0·655 mm. (mean of 10 measurements). Load, 7081 grammes.

Clockwise twist, $\frac{1}{4}$–$2\frac{1}{4}$ turns; lowering 0·0066 mm., mean of 10 observations.
Counter-clockwise twist, $\frac{1}{4}$–$2\frac{1}{4}$ turns; lowering 0·0083 mm., mean of 10 obs.

It was not safe to give a greater twist owing to the largeness of the permanent set. With $2\frac{1}{4}$ turns the set was still small.

The larger value of the lowering for the counter-clockwise twist is almost certainly real, and not merely error of observation. Some other observations showed an even greater excess, though they were very irregular owing to temperature-variations, and are not worth recording. The extension due to an addition of 520 grammes was:

No twist on the wire		0·268 mm.,	mean of 20 observations.
3 turns	,,	0·269	,, ,, ,,

Taking the mean for clockwise and counter-clockwise twist, the lowering for one turn is 0·00149 mm., and $s = 1{\cdot}62$ mm.

6. Brass wire, diameter 0·928 mm. (mean of 10 measurements). Load 19,504 grammes.

Clockwise twist,	$\frac{1}{4}$–$2\frac{1}{4}$ turns;	lowering	0·0169 mm.,	mean of 10 observations.
,, ,,	$\frac{1}{4}$–$4\frac{1}{4}$,,	,,	0·0540	,, ,, ,,
Counter-clockwise twist,	$\frac{1}{4}$–$2\frac{1}{4}$ turns;	lowering	0·0135 mm.,	mean of 10 obs.
,, ,, ,,	$\frac{1}{4}$–$4\frac{1}{4}$,,	,,	0·0479	,, ,,

The difference between clockwise and counter-clockwise twisting is too large for errors of observation.

For the square-law the lowerings for $\frac{1}{4}$–$4\frac{1}{4}$ and for $\frac{1}{4}$–$2\frac{1}{4}$ turns should be in the ratio 18 : 5. They are in the ratios 16 : 5 for clockwise, and 17·7 : 5 for counter-clockwise twisting. The lowering for one turn clockwise, as deduced from $\frac{1}{4}$-$4\frac{1}{4}$ turns, is 0·00300 mm., and for one turn counter-clockwise is 0·00265 mm.

The mean value of $s = 1{\cdot}537$ mm.

Experiments with Smaller Loads.

When the piano-wire diameter 0·72 mm. was loaded only with the rod and cross-bar weighing 1081 grammes, there was a *rise* on twisting.

Clockwise twist,	0–4	turns, rise	0·041 mm.
,, ,,	0–8	,,	0·139 ,,
Counter-clockwise twist,	0–4	,,	0·023 ,,
,, ,, ,,	0–8	,,	0·108 ,,

The extension for an addition of 520 grammes was:

No twist on wire	0·137	mm.,	mean of	6 observations.
4 turns clockwise	0·170	,,	,,	6 ,,
8 ,, ,,	0·237	,,	,,	3 ,,
4 turns counter-clockwise	0·156	,,	,,	6 ,,
8 ,, .. ,.	0·238	,,	,,	3 ,,

If by means of the observed extensions we calculate the positions of the point viewed, when the load of 1081 grammes is taken off, we find that the total rise for clockwise twist would be: for four turns 0·110 mm., and for eight turns 0·347 mm.

The rise appears to be due to coiling up of the wire on twisting, through some remnant of the spiral condition in which it existed before suspension. This is confirmed by the very large increase in extension, due to addition of load as the twist on the wire is increased. It may be a coincidence that the rise on twisting and the increase of stretch are both nearly proportional to the square of the number of turns.

Experiments were then made with greater loads to find how the lowering and extension changed. Only clockwise twist was observed.

Load 3081 grammes, the rise changed to lowering.

Twist,	0–4 turns;	lowering	0·0131	mm.,	mean of	20 observations.
,,	0 8 ,,	,,	0·0498	,,	,,	,,

The extension due to an addition of 520 grammes was:

No twist on the wire	0·144	mm.,	mean of	10	observations.
4 turns ,,	0·143	,,	,,		,,
8 ,, ,,	0·149	,,	,,		,,

Showing a still slight excess of extension in the most twisted condition.

Load 5081 grammes.

Twist,	0–4 turns;	lowering	0·0164	mm.,	mean of	20 observations.
,,	0–8 ,,	,,	0·0660	,,	,,	,,

The extension due to an addition of 520 grammes was:

No twist on the wire		0·141 mm.,	mean of	10	observations.
4 turns	,,	0·142 ,,	,,	15	,,
8 ,,	,,	0·144 ,,	,,	15	,,

The results for loads of 7081 and 9081 grammes are already recorded under 1*a* and 1*b*. There is obviously a tendency for the lowering to increase with load until the extensions under different twists become more nearly equal with equal added load.

When the same wire was softened and loaded with 3081 grammes the lowering for three turns was 0·0093 mm. (mean of 10 observations).

The extension due to an addition of 520 grammes was:

No twist on the wire		0·149 mm.,	mean of	10	observations.
3 turns	,,	0·147	,,	,,	,,

With load 9081 grammes the same wire gave the results recorded under 2, which show a greater lowering for an equal twist but the same extension with added load.

The copper wire diameter 0·655 mm. (No. 5 above) with load 4081 grammes gave:

Clockwise twist, 0–3 turns, 0·00965 mm., mean of 10 observations.
Counter-clockwise twist, 0 3 turns, 0·0156 mm., mean of 10 observations.

Taking the mean of these, the lowering for one turn is 0·0014 mm.

The extension due to an addition of 520 grammes was:

No twist on the wire		0·268 mm.,	mean of	5	observations;
3 turns	,,	0·270	,,	,,	,,

extensions agreeing very nearly with those recorded above for a load of 7081 grammes on the same wire.

Remarks on the Results of Measurements.

The lowering was never so much as 0·1 mm. and was usually much less. The accuracy attained could hardly be expected to be great. The measurements, however, appear to show that when a wire is sufficiently loaded to be straightened, it is lengthened by twisting by an amount proportional to the square of the twist and, with a given number of turns, inversely as the length.

It might be thought possible that the effect observed was due to rise of temperature, either through adiabatic strain or through dissipation of strain-energy as heat. But the observations give no support to this explanation. When the wire was extended by twisting, it remained extended, and when untwisted it returned. Temperature-effects would be a maximum the instant after twisting, and would then gradually subside. It may be noted that the adiabatic change of temperature is proportional to $a^2\theta^2/l$, but it is a cooling, and its amount is such as to shorten the wire, in the case of steel, by something of the order of 1/100 of the observed extension. If we suppose that some definite fraction of the strain-energy put in is dissipated, again the change, now a warming, is proportional to $a^2\theta^2/l$. The whole strain-energy, in the case of steel, would only raise the temperature by an amount accounting for something of the order of 1/10 the observed extension, and, in fact, only an exceedingly minute fraction of the strain-energy is dissipated.

A comparison of the wires (1) and (3) appears to show that the lengthening for a given twist is proportional to the square of the radius.

If we put the lengthening

$$dl = sa^2\theta^2/2l,$$

s for steel is in the neighbourhood of 1. For copper and brass s is in the neighbourhood of 1·5. The lowering for the copper and brass wires tested for twists in opposite directions is not the same.

With a hard steel wire with small load the end of the wire *rises* on twisting, probably through coiling.

The value of $s = (\frac{1}{2}n + p)/Y - (2/Y - 1/n)\,q$ appears to be measurable, but its value gives us no clue to the values of p and q.

If we could assume $q = 0$, then for steel we should have p about $2n$, but I see no justification for the assumption.

If we could measure the decrease in diameter, we should obtain the value of $(\frac{1}{2}n + p - q)\,\sigma/Y - \{(3 + \sigma)/Y - 1/n\}\,q$, and knowing n, Y and σ we should be able to find p and q. But a thin wire is quite unsuitable for this measurement. The decrease is probably of the order of $2a/l \times$ lengthening. With the wires I have used this is of the order $1/1000 \times$ lengthening, and an accuracy of measurement of 10^{-6} mm. would be required at least. With a shaft of considerable diameter it might be possible to measure the quantity, though the experimental difficulties are obviously very great*.

The Effect of the Lengthening of a Wire on its Torsional Vibration.

If a wire is loaded with mass M having moment of inertia I, when M is set vibrating torsionally it falls and rises as it swings, its distance below the highest point being given by

$$x = \tfrac{1}{2}sa^2\theta^2/l.$$

The kinetic energy is $\quad T = \frac{1}{2}I\dot{\theta}^2 + \frac{1}{2}M\dot{x}^2.$

The last term is easily found to be negligible.

The potential energy is

$$V = \tfrac{1}{4}n\pi a^4\theta^2/l - Mgx = \tfrac{1}{4}n\pi a^4\theta^2/l - \tfrac{1}{2}Mgsa^2\theta^2/l.$$

* [For the experimental carrying out of these measurements see *Collected Papers*, Art. 30. Ed.

The equation of motion is

$$I\ddot{\theta} + (\tfrac{1}{2}n\pi a^4/l - Mgsa^2/l)\,\theta = 0.$$

Whence
$$T^2 = \frac{8\pi^2 Il}{n\pi a^4\,(1 - 2Mgs\ n\pi a^2)},$$

and T is greater than it would be if s were 0, by the factor

$$1 + Mgs/n\pi a^2.$$

If Y is Young's modulus and if e is the elongation of the wire due to the load Mg,

$$Mg/\pi a^2 = Ye,$$

so that the factor may be conveniently written as $1 + seY/n$.

If the vibrations are used to determine the modulus of rigidity n, then the value of n will be greater than that deduced by neglect of s, and by the factor $1 + 2seY/n$.

To give an idea of the effect on the determination of the modulus of rigidity, let us suppose that a quite straight steel wire, diameter 0·7 mm., has a load of 2000 grammes. For steel Y/n is about 2·6. For the given diameter e is about 2×10^{-4}. We have found that s is about 1. The correcting factor is then about 1·001, or the true rigidity exceeds the value calculated in the ordinary way by about 1 in 1000. If the wire is not sufficiently loaded to be straight the value of s is less. If very lightly loaded the sign of s may be changed and the true rigidity may be less than the value as ordinarily calculated. The correction is hardly needful in practice, as the modulus of rigidity is probably not measurable to three figures.

Distortional Waves.

In purely distortional waves in a medium of great extent it is evident that the pressure S perpendicular to the axes of shear, if it exists, will not produce any motion. To keep the waves purely distortional, i.e. with motion perpendicular to the direction of propagation only, a force must be applied from outside dR/dx per cubic centimetre in the direction of propagation. If this force is not applied then longitudinal motion must result, obviously of the second order, unless $\tfrac{1}{2}n + p = 0$. This is probably the condition for an incompressible medium. If $\tfrac{1}{2}n + p$ is not zero it appears possible that dispersion may exist. If the longitudinal motion is neglected the pressure in the direction of propagation is $(\tfrac{1}{2}n + p)\,\epsilon^2$ and all that we can say, at present, is that it is probably of the order of $n\epsilon^2$.

26.

THE WAVE-MOTION OF A REVOLVING SHAFT, AND A SUGGESTION AS TO THE ANGULAR MOMENTUM IN A BEAM OF CIRCULARLY POLARISED LIGHT.

[*Roy. Soc. Proc.* A, **82**, 1909, pp. 560–567.]

[*Read* June 24, 1909.]

When a shaft of circular section is revolving uniformly, and is transmitting power uniformly, a row of particles originally in a line parallel to the axis will lie in a spiral of constant pitch, and the position of the shaft at any instant may be described by the position of this spiral.

Let us suppose that the power is transmitted from left to right, and that as viewed from the left the revolution is clockwise. Then the spiral is a left-handed screw. Let it be on the surface, and there make an angle ϵ with the axis. Let the radius of the shaft be a, and let one turn of the spiral have length λ along the axis. We may term λ the wave-length of the spiral. We have $\tan\epsilon = 2\pi a/\lambda$. If the orientation of the section at the origin at time t is given by $\theta = 2\pi Nt$, where N is the number of revolutions per second, the orientation of the section at x is given by

$$\theta = 2\pi Nt - \frac{x}{a}\tan\epsilon = \frac{2\pi}{\lambda}(N\lambda t - x), \quad \text{..................(1)}$$

which means movement of orientation from left to right with velocity $N\lambda$.

The equation of motion for twist-waves on a shaft of circular section is

$$\frac{d^2\theta}{dt^2} = U_n{}^2\frac{d^2\theta}{dx^2}, \quad \text{..................................(2)}$$

where $U_n{}^2$ = modulus of rigidity/density = n/ρ.

Though (1) satisfies (2), it can hardly be termed a solution, for $d^2\theta/dt^2$, and $d^2\theta/dx^2$ in (2) are both zero. But we may adapt a solution of (2) to fit (1) if we assume certain conditions in (1).

The periodic value

$$\theta = \Theta\sin\frac{2\pi}{l}(U_n t - x)$$

satisfies (2), and is a wave-motion with velocity U_n and wave-length l. Make

l so great that for any time or for any distance under observation $U_n t/l$ and x/l are so small that the angle may be put for the sine. Then

$$\theta = \Theta \frac{2\pi}{l}(U_n t - x). \qquad \text{.........................(3)}$$

This is uniform rotation. It means that we only deal with the part of the wave near a node, and that we make the wave-length l so great that for a long distance the 'displacement-curve' obtained by plotting θ against t coincides with the tangent at the node. We must distinguish, of course, between the wave-length l of the periodic motion and the wave-length λ of the spiral.

We can only make (1) coincide with (3) by putting

$$\Theta/l = 1/\lambda \quad \text{and} \quad N\lambda = U_n.$$

Then it follows that for a given value of N, the impressed speed of uniform rotation, there is only one value of λ or one value of ϵ for which the motion may be regarded as part of a natural wave-system, transmitted by the elastic forces of the material with velocity $= \sqrt{(n/\rho)}$. There is therefore only one 'natural' rate of transmission of energy.

The value of ϵ is given by

$$\tan\epsilon = 2\pi a/\lambda = 2\pi aN/N\lambda = 2\pi aN/U_n = 2\pi aN\sqrt{(\rho/n)}.$$

Suppose, for instance, that a steel shaft with radius $a = 2$ cm., density $\rho = 7{\cdot}8$, and rigidity $n = 10^{12}$ is making $N = 10$ revs. per sec. We may put $\tan\epsilon = \epsilon$, since it is very small. The shaft is twisted through 2π in length λ or through $2\pi/\lambda$ per centimetre, and the torque across a section is

$$G = \tfrac{1}{2}n\pi a^4\, 2\pi/\lambda = n\pi^2 a^4 N\sqrt{(\rho/n)},$$

since $$\lambda = \frac{U_n}{N} = \frac{1}{N}\sqrt{\frac{n}{\rho}}.$$

The energy transmitted per second is

$$2\pi NG = 2\pi^3 a^4 N^2\sqrt{(n\rho)}.$$

Putting 1 H.P. $= 746 \times 10^7$ ergs per second, this gives about 38 H.P.

But a shaft revolving with given speed N can transmit any power, subject to the limitation that the strain is not too great for the material. When the power is not that 'naturally' transmitted, we must regard the waves as 'forced.' The velocity of transmission is no longer U_n, and forces will have to be applied from outside in addition to the internal elastic forces to give the new velocity.

Let H be the couple applied per unit length from outside. Then the equation of motion becomes

$$\frac{d^2\theta}{dt^2} = U_n{}^2\frac{d^2\theta}{dx^2} + \frac{2H}{\pi a^4\rho},$$

where $\frac{1}{2}\pi a^4$ is the moment of inertia of the cross-section. Assuming that the condition travels on with velocity U unchanged in form,

$$\frac{d\theta}{dt} = -U\frac{d\theta}{dx} \quad \text{and} \quad H = \tfrac{1}{2}\pi a^4 \rho \left(U^2 - U_n{}^2\right)\frac{d^2\theta}{dx^2},$$

or H has only to be applied where $d^2\theta/dx^2$ has value, that is where the twist is changing.

The following adaptation of Rankine's tube-method of obtaining wave-velocities* gives these results in a more direct manner. Suppose that the shaft is indefinitely extended both ways. Any twist-disturbance may be propagated unchanged in form with any velocity we choose to assign, if we apply from outside the distribution of torque which, added to the torque due to strain, will make the change in twist required by the given wave-motion travelling at the assigned speed.

Let the velocity of propagation be U from left to right, and let the displacement at any section be θ, positive if clockwise when seen from the left. The twist per unit length is

$$\frac{d\theta}{dx} = -\frac{1}{U}\frac{d\theta}{dt} = -\frac{\dot{\theta}}{U}.$$

The torque across a section from left to right in clockwise direction is

$$-\tfrac{1}{2}n\pi a^4\frac{d\theta}{dx} = \frac{n\pi a^4}{2U}\,.\,\dot{\theta}.$$

Let the shaft be moved from right to left with velocity U; then the disturbance is fixed in space, and if we imagine two fixed planes drawn perpendicular to the axis, one, A, at a point where the disturbance is θ and the other, B, outside the wave-system, where there is no disturbance, the condition between A and B remains constant, except that the matter undergoing that condition is changing. Hence the total angular momentum between A and B is constant. But no angular momentum enters at B, since the shaft is there untwisted and has merely linear motion. At A, then, there must be on the whole no transfer of angular momentum from right to left. Now, angular momentum is transferred in three ways:

1. By the carriage by rotating matter. The angular momentum per unit length is $\frac{1}{2}\rho\pi a^4\dot{\theta}$, and since length U per second passes out at A, it carries out $\frac{1}{2}\rho\pi a^4\dot{\theta}U$.

2. By the torque exerted by matter on the right of A on matter on the left of A. This takes out $-n\pi a^4\dot{\theta}/2U$.

3. By the stream of angular momentum by which we may represent the forces applied from outside to make the velocity U instead of U_n.

* *Phil. Trans.* 1870, p 277.

If H is the couple applied per unit length, we may regard it as due to the flow of angular momentum L along the shaft from left to right, such that $H = -\,dL/dx$. There is then angular momentum L flowing out per second from right to left. Since the total flow due to (1), (2), and (3) is zero,

$$\tfrac{1}{2}\rho\pi a^4\dot{\theta}U - n\pi a^4\dot{\theta}/2U - L = 0,$$

and $$L = \frac{\pi a^4\dot{\theta}}{2}\left(\rho U - \frac{n}{U}\right) = \frac{\rho\pi a^4\dot{\theta}}{2U}(U^2 - U_n{}^2) \quad -\frac{\rho\pi a^4}{2}\frac{d\theta}{dx}(U^2 - U_n{}^2),$$

whence $$H = -\frac{dL}{dx} = \frac{\rho\pi a^4}{2}\frac{d^2\theta}{dx^2}(U^2 \quad U_n{}^2).$$

If $H = 0$, either $U^2 = U_n{}^2$ when the velocity has its 'natural value,' or $d^2\theta/dx^2 = 0$, and the shaft is revolving with uniform twist in the part considered.

Now put on to the system a velocity U from left to right. The motion of the shaft parallel to its axis is reduced to zero, and the disturbance and the system H will travel on from left to right with velocity U. A 'forced' velocity does not imply *transfer* of physical conditions by the material with that velocity. We can only regard the conditions as reproduced at successive points by the aid of external forces. We may illustrate this point by considering the incidence of a wave against a surface. If the angle of incidence is i and the velocity of the wave is V, the line of contact moves over the surface with velocity $v = V/\sin i$, which may have any value from V to infinity. The velocity v is not that of transmission by the material of the surface, but merely the velocity of a condition impressed on the surface from outside.

Probably in all cases of transmission with forced velocity, and certainly in the case here considered, the velocity depends upon the wave-length, and there is dispersion.

With a shaft revolving N times per second $U = N\lambda$, and it is interesting to note that the group-velocity, $U - \lambda dU/d\lambda$, is zero. It is not at once evident what the group-velocity signifies in the case of uniform rotation. In ordinary cases it is the velocity of travel of the 'beat' pattern, formed by two trains of slightly different frequencies. The complete 'beat' pattern is contained between two successive points of agreement of phase of the two trains. In our case of superposition of two strain-spirals with constant speed of rotation, points of agreement of phase are points of intersection of the two spirals. At such points the phases are the same or one has gained on the other by 2π. Evidently as the shaft revolves these points remain in the same cross-section, and the group-velocity is zero.

With deep-water waves the group-velocity is half the wave-velocity, and the energy-flow is half that required for the onward march of the waves*.

* O. Reynolds, *Nature*, August 23, 1877; Lord Rayleigh, *Theory of Sound*, vol. 1, p. 477.

The energy-flow thus suffices for the onward march of the group, and the case suggests a simple relation between energy-flow and group-velocity.

But the simplicity is special to unforced trains of waves. Obviously, it does not hold when there are auxiliary working forces adding or subtracting energy along the waves. For the revolving shaft the simple relation would give us no energy-flow, whereas the strain existing in the shaft implies transmission of energy at a rate given as follows.

The twist per unit length is $d\theta/dx$, and therefore the torque across a section is $-\frac{1}{2}n\pi a^4 d\theta/dx$, or $\frac{1}{2}n\pi a^4\dot{\theta}/U$, since $d\theta/dx = -\dot{\theta}/U$. The rate of working or of energy-flow across the section is $\frac{1}{2}n\pi a^4\dot{\theta}^2/U$.

The relation of this to the strain and kinetic energy in the shaft is easily found. The strain-energy per unit length being $\frac{1}{2}$ (couple $\times$ twist per unit length) is $\frac{1}{4}n\pi a^4 (d\theta/dx)^2$, which is $\frac{1}{4}n\pi a^4\dot{\theta}^2/U^2$. The kinetic energy per unit length is $\frac{1}{4}\rho\pi a^4\dot{\theta}^2$, or, putting $\rho = n/U_n^2$, is $\frac{1}{4}n\pi a^4\dot{\theta}^2/U_n^2$.

In the case of natural velocity, for which no working forces along the shaft are needed, when $U = U_n = \sqrt{(n/\rho)}$, the kinetic energy is equal to the strain-energy at every point and the energy transmitted across a section per second is that contained in length U_n.

But if the velocity is forced this is no longer true*, and it is easily shown that the energy transferred is that in length $\dfrac{2U}{1 + U^2/U_n^2}$, which is less than U if $U > U_n$, and is greater than U if $U < U_n$.

It appears possible that always the energy is transmitted along the shaft at the speed U_n. If the forced velocity $U > U_n$, we may, perhaps, regard the system in a special sense as a natural system with a uniform rotation superposed on it.

Let us suppose that the whole of the strain-energy in length U_n is transferred per second while only the fraction μ of the kinetic energy is transferred, the fraction $1 - \mu$ being stationary.

The energy transferred : strain-energy in U_n : kinetic energy in U_n

$$= 1/U : U_n/2U^2 : U_n/2U_n^2.$$

Put $U = pU_n$, and our supposition gives

$$\frac{1}{pU_n} \quad \frac{1}{2p^2U_n} + \frac{\mu}{2U_n}, \text{ or } \mu = \frac{2}{p} \quad \frac{1}{p^2} = 1 - \left(1 - \frac{1}{p}\right)^2.$$

If the forced velocity $U < U_n$, we may regard the system as a natural one, with a uniform stationary strain superposed on it.

* In the Sellmeier model illustrating the dispersion of light, the particles may be regarded as outside the material transmitting the waves and as applying forces to the material which make the velocity forced. The simple relation between energy-flow and group-velocity probably does not hold for this model

We now suppose that the whole of the kinetic energy is transferred, but only a fraction ν of the strain-energy, and we obtain

$$\frac{1}{pU_n} = \frac{\nu}{2p^2U_n} + \frac{1}{2U_n} \quad \text{or} \quad \nu = 2p - p^2 = 1 - (1-p)^2.$$

It is perhaps worthy of note that a uniform longitudinal flow of fluid may be conceived as a case of wave-motion in a manner similar to that of the uniform rotation of a shaft.

A Suggestion as to the Angular Momentum in a Beam of Circularly Polarised Light.

A uniformly revolving shaft serves as a mechanical model of a beam of circularly polarised light. The expression for the orientation θ of any section of the shaft distant x from the origin, $\theta = 2\pi\lambda^{-1}(Ut - x)$, serves also as an expression for the orientation of the disturbance, whatever its nature, constituting circularly polarised light.

For simplicity, take a shaft consisting of a thin cylindrical tube. Let the radius be a, the cross-section of the material s, the rigidity n, and the density ρ. Let the tube make N revolutions per second, and let it have such twist on it that the velocity of transmission of the spiral indicating the twist is the natural velocity $U_n = \sqrt{(n/\rho)}$.

Repeating for this special case what we have found above, the strain-energy per unit length is $\frac{1}{2}n\epsilon^2 s$, or, since $\epsilon = a d\theta\, dx = -a\dot{\theta}\, U_n$, the strain-energy is $\frac{1}{2}na^2 s\dot{\theta}^2/U_n^2 = \frac{1}{2}\rho a^2 s\dot{\theta}^2$.

But the kinetic energy per unit length is also $\frac{1}{2}\rho a^2 s\dot{\theta}^2$, so that the total energy in length U_n is $\rho a^2 s\dot{\theta}^2 U_n$. The rate of working across a section is

$$n\epsilon s a\dot{\theta} = na^2 s\dot{\theta}^2/U_n = \rho a^2 s\dot{\theta}^2 U_n,$$

or the energy transferred across a section is the energy contained in length U_n.

If we put E for the energy in unit volume and G for the torque per unit area, we have

$$Gs\dot{\theta} = EsU_n,$$

whence
$$G = EU_n/\dot{\theta} = EN\lambda/2\pi N = E\lambda/2\pi.$$

The analogy between circularly polarised light and the mechanical model suggests that a similar relation between torque and energy may hold in a beam of such light incident normally on an absorbing surface. If so, a beam of wave-length λ containing energy E per unit volume will give up angular momentum $E\lambda/2\pi$ per second per unit area. But in the case of light-waves $E = P$, where P is the pressure exerted. We may therefore put the angular momentum delivered to unit area per second as

$$P\lambda/2\pi.$$

In the *Philosophical Magazine*, 1905, vol. 9, p. 397*, I attempted to show that the analogy between distortional waves and light-waves is still closer, in that distortional waves also exert a pressure equal to the energy per unit volume. But as I have shown in a paper on 'Pressure Perpendicular to the Shear-Planes in Finite Pure Shears, etc.†,' the attempt was faulty, and a more correct treatment of the subject only shows that there is probably a pressure. We cannot say more as to its magnitude than that if it exists it is of the order of the energy per unit volume.

When a beam is travelling through a material medium we may, perhaps, account for the angular momentum in it by the following considerations. On the electromagnetic theory the disturbance at any given point in a circularly polarised beam is a constant electric strain or displacement f uniformly revolving with angular velocity $\dot{\theta}$. In time dt it changes its direction by $d\theta$.

This may be effected by the addition of a tangential strain $fd\theta$; or the rotation is produced by the addition of tangential strain $f\dot{\theta}$ per second, or by a current $f\dot{\theta}$ along the circle described by the end of f. We may imagine that this is due to electrons drawn out from their position of equilibrium so as to give f, and then whirled round in a circle so as to give a circular convection-current $f\dot{\theta}$. Such a circular current of electrons should possess angular momentum.

Let us digress for a moment to consider an ordinary conduction-circuit as illustrating the possession of angular momentum on this theory. Let the circuit have radius a and cross-section s, and let there be N negative electrons per unit volume, each with charge e and mass m, and let these be moving round the circuit with velocity v. If i is the total current, $i = Nsve$. The angular momentum will be

$$Ns2\pi a \,.\, mva = 2\pi a^2 im/e = 2Aim/e = 2Mm/e.$$

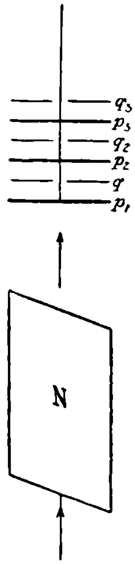
where A is the area of the circuit and M is the magnetic moment. This is of the order of $2M/10^7$.

It is easily seen that this result will hold for any circuit, whatever its form, if A is the projection of the circuit on a plane perpendicular to the axis round which the moment is taken, and if $M = Ai$. If we suppose that a current of negative electrons flows round the circuit in this way and that the reaction while their momentum is being established is on the material of the conductor, then at make of current there should be an impulse on the conductor of moment $2M/10^7$. If the circuit could be suspended so that it lay in a horizontal plane and was able to turn about a vertical axis in a space free from any magnetic field, we might be able to detect such impulse if it exists. But it is practically impossible to get a space free from magnetic intensity. If the field is H, the couple on the circuit due to it is proportional to HM. It would require exceedingly careful construction and adjustment

* [*Collected Papers*, Art. 22, p. 338.] † [*Collected Papers*, Art. 25.]

of the circuit to ensure that about the vertical axis the component of the couple due to the field was so small that its effect should not mask the effect of the impulsive couple. The electrostatic forces, too, might have to be considered as serious disturbers.

Returning to a beam of circularly polarised light, supposed to contain electrons revolving in circular orbits in fixed periodic times, the relations between energy and angular momentum are exactly the same as those in a revolving shaft or tube, and the angular momentum transmitted per second per square centimetre is $E\lambda/2\pi = P\lambda/2\pi$, where P is the pressure of the light per square centimetre on an absorbing surface.



The value of this in any practical case is very small. In light-pressure experiments, P is detected by the couple on a small disc, of area A say, at an arm b and suspended by a fibre. What we observe is the moment APb. If the same disc is suspended by a vertical fibre attached at its centre and the same beam circularly polarised in both cases is incident normally upon it, according to the value suggested the torque is $AP\lambda/2\pi$.

The ratio of the two is $\lambda/2\pi b$. Now b is usually of the order of 1 cm. Put $\lambda = 6 \times 10^{-5}$ cm., or, say, $2\pi/10^{-5}$, and the ratio becomes 10^{-5}.

It is by no means easy to measure the torque APb accurately, and it appears almost hopeless to detect one of a hundred-thousandth of the amount. The effect of the smaller torque might be multiplied to some extent, as shown in accompanying diagram.

Let a series of quarter wave plates, $p_1, p_2, p_3, \ldots$, be suspended by a fibre above a Nicol prism N, through which a beam of light is transmitted upwards, and intermediate between these let a series of quarter wave plates, $q_1, q_2, q_3, \ldots$, be fixed, each with a central hole for the free passage of the fibre. The beam emerges from N plane polarised. If N is placed so that the beam after passing through p_1 is circularly polarised, it has gained angular momentum, and therefore tends to twist p_1 round. The next plate q_1 is to be arranged so that the beam emerges from it plane polarised and in the original plane. It then passes through p_2, which is similar to p_1, and again it is circularly polarised and so exercises another torque. The process is repeated with q_2 and p_3, and so on till the beam is exhausted. By revolving N through a right angle round the beam, the effect is reversed. But, even with such multiplications, my present experience of light-forces does not give me much hope that the effect could be detected, if it has the value suggested by the mechanical model.

27.

PRELIMINARY NOTE ON THE PRESSURE OF RADIATION AGAINST THE SOURCE: THE RECOIL FROM LIGHT.

By J. H. Poynting and Guy Barlow, D.Sc.

[*British Association Report,* 1909.]

[This is merely a preliminary account of the following paper Art. 28.]

28.

THE PRESSURE OF LIGHT AGAINST THE SOURCE: THE RECOIL FROM LIGHT.

BAKERIAN LECTURE.

By J. H. Poynting and Guy Barlow, D.Sc.

[*Roy. Soc. Proc.* A, **83**, 1910, pp. 534–546.]

[*Read* March 17, 1910.]

All experiments on the pressure of radiation have hitherto been made on the force exerted by light or radiation on a receiving surface. The experiment now to be described shows the pressure of radiation against the source from which it starts, and from analogy with a gun we may term this the *recoil* from light. It does not appear practicable to show this effect by using a source in which heat is developed intrinsically. But if radiation falls on an absorbing body it heats the body and the heat so developed issues again as radiation, and it is possible to detect the effect of this issuing radiation.

Theory.

We may see the nature of the action to be looked for by considering an ideal case in which we allow a beam of light with energy P per cubic centimetre to fall normally in a perfect vacuum in turn on each of four discs, the front and back surfaces of these discs being respectively as in Fig. 1,

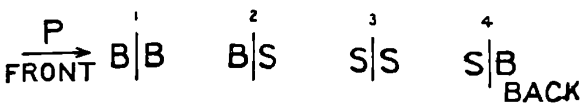


Fig. 1.

where B represents a fully absorbing or 'black' surface, and S a fully reflecting or non-radiating surface.

When the radiation falls on an absorbing face, as in the case of either of the discs (1) and (2), the temperature of the disc rises till a steady state is reached in which emission equals absorption. We may suppose that the discs are so thin that the two faces are sensibly at the same temperature. If we did not take into account the pressure due to issuing radiation, or if we only considered the initial effects before heating took place, the pressures on the first two discs would be P in each case, due to the incident beam alone, and on the last two would be $2P$, due to the sum of the incident and reflected beams. We should have, therefore, pressures respectively

$$\begin{array}{cccc} (1) & (2) & (3) & (4) \\ P & P & 2P & 2P \end{array}$$

But when a steady state is reached, the discs (1) and (2) must be giving out as much radiant energy as they receive. The first disc gives out equal amounts on the two sides, producing equal and opposite pressures. All the radiation from the second disc is given out at the front side and is equal in energy to that of the incident beam. Assuming this emitted radiation is distributed according to the cosine-law, the pressure resulting from it is easily shown to be $\frac{2}{3}P$, so that the total pressure on this disc is $\frac{5}{3}P$.

Since there is no absorption by discs (3) and (4), we still have the pressures $2P$; hence we have now

$$\begin{array}{cccc} (1) & (2) & (3) & (4) \\ P & \frac{5}{3}P & 2P & 2P \end{array}$$

In a real case these results are modified in two ways:

(i) By the possession of some small reflecting power by surface B, and of some small absorbing and radiating power by surface S.

(ii) By an inequality of temperature between front and back surfaces conditioned by the energy which is carried through from front to back to be radiated thence. The vacuum is not perfect, and there is radiometer-action due to the residual gas, which, owing to the inequality of temperature, is not the same on the two sides. This is probably the only way in which gas-action is sensible, for the effects due to ordinary convection and conduction in the residual gas are negligible. The temperature-difference, though sufficient to produce a differential radiometer-action, is so small that in estimating the radiation from the two sides of a disc we may take them as being at the same temperature.

In the experiment to be described the diffusion is so slight that we disregard it. Considering, then, only the reflection and absorption, let r be the coefficient of reflection of the surface B for the incident radiation, ρ that of S, a the coefficient of emission of B for the emitted radiation, α that of S.

It is then easy to show that the total radiation-pressures on the four discs are respectively

$$\begin{array}{cccc} (1) & (2) & (3) & (4) \\ (1+r)\,P, & \left\{1+r+\frac{2}{3}\frac{a-\alpha}{a\quad\alpha}(1-r)\right\}P, & (1+\rho)\,P, & \left\{1+\rho-\frac{2}{3}\frac{a-\alpha}{a+\alpha}(1-\rho)\right\}P. \end{array}$$

The emitted radiation is not of the same quality as the incident radiation; and strictly we are not justified in assuming that the emissive powers for the two surfaces for the one quality of radiation are in the same ratio as the absorbing powers for the other. But we shall for simplicity suppose that the ratios are the same, a supposition which enables us to proceed, and which is probably not very far from the truth. We have, therefore,

$$a = 1 - r, \qquad \alpha = 1 - \rho.$$

On this assumption Table I has been constructed, giving the four pressures for different values of r and ρ. The pressure-ratios in the last two columns are of interest in connection with the experimental results, and will be referred to later.

TABLE I. *Pressures on Discs, calculated for Different Values of the Reflection-Coefficients.*

Reflecting power of B	Reflecting power of S	Pressures taking $P=1$				Pressure-ratios	
r	ρ	BB	BS	SS	SB	$\frac{BS}{BB}$	$\frac{BS}{\frac{1}{2}(SS+SB)}$
0	1·00	1·00	1·67	2·00	2·00	1·67	1·67
0	0·95	1·00	1·60	1·95	1·92	1·60	1·66
0	0·90	1·00	1·54	1·90	1·85	1·54	1·65
0·05	1·00	1·05	1·68	2·00	2·00	1·60	1·68
0·05	0·95	1·05	1·62	1·95	1·92	1·54	1·67
0·05	0·90	1·05	1·56	1·90	1·85	1·49	1·66
0·10	1·00	1·10	1·70	2·00	2·00	1·55	1·70
0·10	0·95	1·10	1·64	1·95	1·92	1·49	1·70
0·10	0·90	1·10	1·58	1·90	1·85	1·44	1·69

The modification of the values in the above table by radiometer-action will be greater for the BB disc than for the others. In that disc energy proportional to $\frac{1}{2}P$ has to be carried through the disc, and therefore the temperature-difference is the greatest. It is only possible to guess at the relative magnitudes of the radiometer-actions for the different discs. We have therefore sought to make the vacuum so high that the action nearly disappeared.

The Experiment.

In the final form of the experiment each disc consisted of a pair of circular cover-glasses, 1·2 cm. in diameter and about 0·1 mm. thick, between which was squeezed a layer of asphaltum also about 0·1 mm. thick, the temperature being first raised sufficiently to render the asphaltum molten. It is difficult to make the discs of uniform thickness and free from bubbles of gas; but a great number were made and the four best were selected for use. Such a compound disc appears to be perfectly opaque, and its surface is the blackest black and the least diffusing that we have yet been able to obtain.

The reflecting surface was made by depositing silver on the outside of the compound disc by means of the discharge from a silver cathode in an exhausted receiver. A similar deposit on clear glass just allowed an arc light to be seen through it.

Four holes the size of the discs were cut in a stout plate of mica *ABCD*, the centres of the holes being at the corners of a 2 cm. square (Fig. 2). The discs were then fixed in these holes by a minute amount of celluloid varnish. The suspension-rod *E* and the mirror-holder *F* were attached to

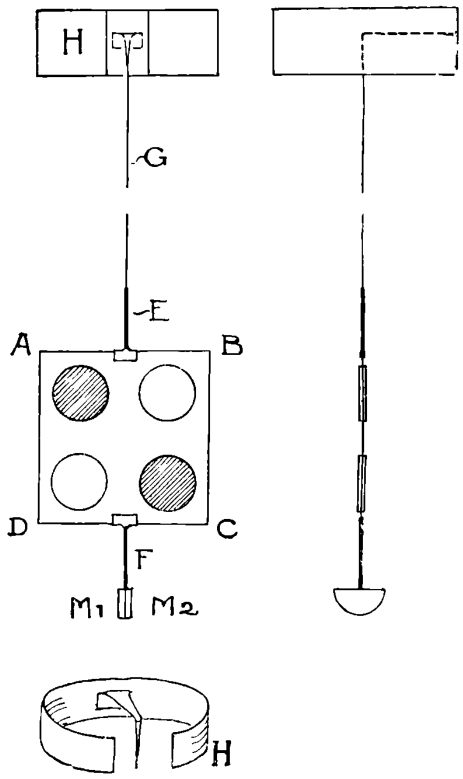


Fig. 2.

the mica plate, at the middle points of its top and bottom edges respectively, by very small copper clips without any cement. A platinised mirror was cut in half, and the two portions M_1, M_2 were mounted back to back in a suitable clip of copper foil at the extremity of the rod *F*, the plane of the mirrors being perpendicular to the mica plate. This system was suspended by a quartz fibre *G*, 9 cm. long, in the centre of a glass flask of 16 cm. diameter. The upper end of the quartz fibre was fixed to a brass collar *H* held by friction in the neck of the flask. Both ends of the fibre were silvered and coppered, so that they could be soldered to *E* and to the support. After suspension the mouth of the flask was sealed off, a lateral tube in the neck being still available for connection with the exhausting apparatus.

To carry out the exhaustion of the flask to a very high degree, the general arrangement shown in Fig. 3 was adopted, and the successive stages in the process were as follows:

(1) Preliminary exhaustion by aid of a Gaede pump. The experimental flask A was kept hot by gas-burners below it, and the charcoal bulbs C_1 and C_2 were strongly heated electrically by enclosing them in asbestos tubes containing coils of platinum wire. From time to time the whole apparatus was washed out with dry oxygen generated from the manganese dioxide in the bulb E. A small discharge tube G indicated the state of the vacuum, and examination of the spectrum gave useful information as to the gas given off from the bulbs C_1 and C_2. At the end of three days these bulbs appeared to

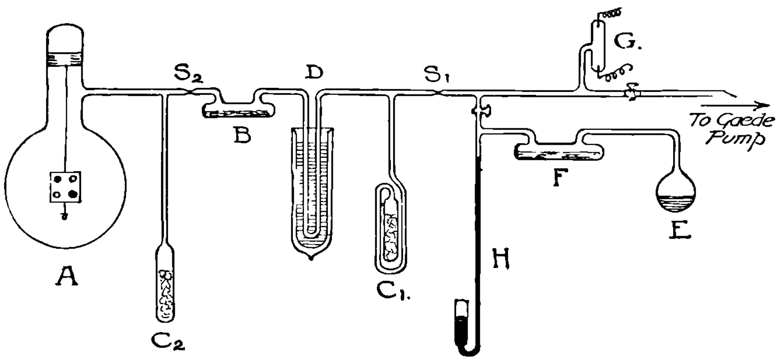


Fig. 3.

have ceased to give off any gas, and the Gaede pump would bring the vacuum down to a hard X-ray stage in about 10 minutes after admitting oxygen. The temperatures of C_1 and C_2 were then somewhat reduced, and the apparatus was sealed off at S_1. B and F in the figure represent phosphorus pentoxide drying tubes, and H is a manometer.

(2) The charcoal bulb C_1 was put in liquid air, and the temperature of the second bulb C_2 and of the flask A was then slowly reduced to the room temperature. After 30 hours the apparatus was finally sealed off at S_2.

During both these stages the U-tube D was kept always immersed in liquid air. This arrangement formed a perfect trap for mercury vapour, which otherwise diffused from the pump into the flask and attacked the silver mirrors. A roll of silver foil was placed in one limb of the U-tube, with the object of making the trap more effective, but this precaution was probably unnecessary.

(3) In the final stage of the exhaustion, the charcoal bulb C_2 was surrounded by liquid air, which was boiled off continuously at the reduced pressure of about 2 cm. of mercury for several hours before and during the whole of the measurements. Experience showed that the highest vacuum was obtained probably two hours after the application of fresh liquid air, and that it was necessary to renew the liquid air after every four or five hours.

The source of light S (Fig. 4) was an Ediswan 50-volt 'Focus lamp,' which was fed from accumulators. By means of an adjustable resistance in series with the lamp, the voltage was maintained exactly at 60 volts, the lamp then taking a current of 5·37 amperes. The light was so steady for hours at a

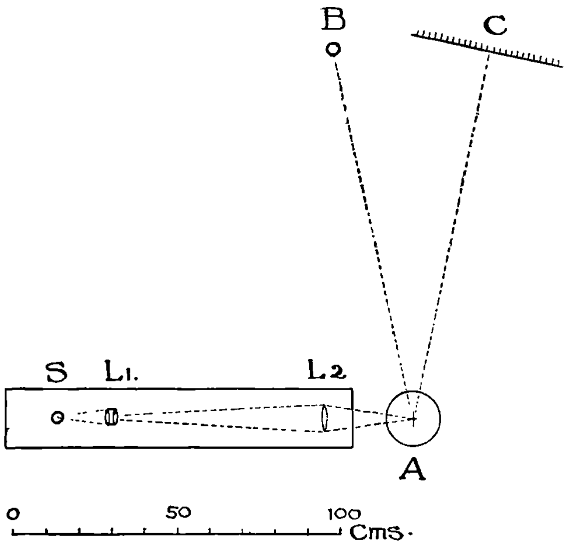


Fig. 4.

time that adjustment of the resistance was seldom necessary. A photographic lens L_1, of 15 cm. focal length, provided with an iris diaphragm, was arranged to throw an image of the lamp filament on an achromatic lens L_2 of 19 cm. focal length. This second lens then formed a uniformly illuminated image of the iris diaphragm on the disc to be worked with. By adjustment of the diaphragm this image was made rather smaller than the disc, so that when the beam was centred on the disc an unilluminated margin about $\frac{1}{2}$ mm. wide was left all round it. The lamp and lenses were fixed to a board which could be moved parallel to itself vertically and horizontally between guides, so that the beam could be easily directed on to the four discs in succession. The centring of the image on the disc was made by eye.

The flask was mounted on an iron turntable with the quartz fibre accurately in the axis of rotation. By rotation of the flask through 180° it was possible to experiment on the reverse sides of the four discs. We shall refer to the observations made in the two positions as 'direct' and 'reverse.' Thus the 'direct' *BS* disc becomes the 'reverse' *SB* disc. The flask was shielded from electrification and from extraneous radiation by enclosure in a cylindrical case of tinned iron, blackened inside and provided with windows to admit the beam of light and to allow the deflections to be observed.

For reading the deflections the image of an electric lamp *B* (Fig. 4) on a millimetre scale *C*, at a distance of 113 cm., was used. The definition of the image was sufficiently good to allow deflections to be read accurately to 0·2 mm., although the optical irregularities of the glass flask rendered a telescope useless.

In finding the centre of swing from the deflections, a curious periodic motion was observed in its position, the centre moving to and fro with simple harmonic motion. The complete period of the torsional vibrations of the suspended system was 74·6 seconds with no appreciable damping; and the period of the motion of the centre of swing was found to be about seven minutes, that is nearly 11 torsional half-periods. The periodic motion was ultimately traced to pendulum-motions of the system set up by external disturbances, for the amplitude of the displaced centre of swing was found to increase with such pendulum-motion. The pendulum-period of the system was slightly longer for motion in the plane of the mica plate than for motion in a perpendicular plane. Hence a pendulum-motion once set up changed periodically from motion in a straight line to motion in an ellipse, and the complete cycle of these changes was actually gone through in seven minutes. This meant a periodic change in the angular momentum of the system about the axis of suspension, and to neutralise this the mica plate tended to turn with equal and opposite angular momentum about the vertical axis, and so to give a twist to the fibre. Accordingly, we should expect the maximum twist to take place when the vibrations are linear, and this was observed to be the case. To eliminate this effect, 12 consecutive turning points were always taken (i.e. observations over a period of seven minutes), and the mean centre of swing calculated from these.

When the beam was allowed to fall on a disc, some initial effects were observed, of which we shall give an account below, and shall then suggest a tentative explanation.

The Results.

The observed deflections for the four discs are given in Table II.

These results are divided into two series, A and B. In Series A the beam of light was kept on the disc under experiment until the deflection seemed nearly constant, the time of exposure being generally about 20 or 30 minutes before the deflection was read. Attention was chiefly given to the *BB* and

BS discs; the deflections for the *SS* and *ṠB* discs are not so reliable, as it became afterwards evident that these discs require even longer time to recover from the initial disturbances (see below).

TABLE II.

		BB	*BS*	*SS*	*SB*
Series A Light on for about 20 or 30 mins.; zero observed between each exposure to light	*D*	14·91 15·56 15·03	20·45 23·29 20·77	32·32	27·22
	Mean *D*	15·17	21·50	32·32	27·22
	R	17·15 17·01	22·89 23·94	27·36	26·19
	Mean *R*	17·08	23·42	27·36	26·19
	Mean *D* and *R*	16·13	22·46	29·84	26·71
Series B Light on for 1 hour; zero observed only at beginning and end of observations on several discs	*D*	(13·78)* 15·20 14·71	21·07 20·44	30·71 28·51	29·15 27·95
	Mean *D*	14·96	20·76	29·61	28·55
	R	17·11	23·37	27·72	27·42
	Mean *D* and *R*	16·04	22·07	28·67	27·99
Final Values *BB* and *BS* taken as mean for Series A and B *SS* and *SB* from Series B alone		16·1	22·3	28·7	28·0

Deflections in scale-divisions for the four discs. Observations on the 'direct' and 'reverse' sides are denoted by *D* and *R* respectively.

* Rejected in taking the mean as, owing to a breakdown of the pump for exhausting the liquid air, the vacuum had doubtless deteriorated.

In the Series B, made a month later, the beam was kept on each disc for one hour before reading the deflection, with the object of obtaining a still closer approach to a steady state. One hour was also allowed before taking the zero after cutting off the light. The much greater time now required made it impracticable to observe the zero more often than twice while making observations on all four discs. Experience showed that, provided the vacuum was well maintained, the zero seldom changed more than one or two tenth-divisions during five hours. There appeared, therefore, no objection to this course.

Examination of the table shows that in most cases rather different values are given by the 'direct' and 'reverse' observations. This is particularly evident in the case of the *BB* disc, where the values, roughly 15 and 17 divisions respectively, differ by about 13 per cent. The explanation of this appears to be that the radiometer-action was still sensible and acted differently in the 'direct' and 'reverse' positions of the disc. The difference was probably due to inequality in the thickness of the two cover-glasses enclosing the asphaltum layer.

Some observations were made without using liquid air, i.e. with the charcoal bulb at room-temperature. The deflections were then always very unsteady, and the zero showed erratic behaviour. A few deflections obtained under these conditions are given in Table III. They merely serve to indicate the general effects of the gas-action. On the *BB* disc the want of symmetry referred to above is now greatly exaggerated, and points to the gas-action being a suction on the 'direct' side and a pressure on the 'reverse.' It is also to be noted that the action is a *suction* on the *BS* disc. Any tendency towards this action in the high-vacuum experiments would, therefore, tend to mask the recoil-pressure sought for.

TABLE III.

	BB	*BS*	*SS*	*SB*
Direct	5	– 19	no obs.	93
Reverse	37	– 77	45	58

Deflections for the four discs with charcoal bulb at room-temperature. (The *minus* sign indicates suction.)

On account of the uncertain values of the various corrections required, and on account of the existence of outstanding disturbances, it was felt that mere multiplication of observations would not lead to more exact results. As the final values, in scale-divisions, for the pressures on the four discs given by the experiment we therefore take

BB	*BS*	*SS*	*SB*
16·1	22·3	28·7	28·0

The values for *BB* and *BS* are the means given by both Series A and B, the values for *SS* and *SB* are from Series B alone, since for these discs the steady state was not attained properly in the observations of Series A.

Hence we have

$$\frac{BS}{BB} = 1{\cdot}39, \qquad \frac{BS}{\frac{1}{4}(SS + SB)} - 1{\cdot}58.$$

We select for comparison with these ratios those calculated for $r = 0{\cdot}05$, $\rho = 0{\cdot}95$ in the last columns of Table I, i.e. the values

$$\frac{BS}{BB} = 1{\cdot}54, \qquad \frac{BS}{\frac{1}{4}(SS + SB)} = 1{\cdot}67.$$

The latter ratio agrees better with the experiment than the former. This is what we might expect, since the latter ratio does not involve the pressure on the BB disc, and that is the pressure which is most affected by the radiometer-action. As to the actual reflection-coefficients for the surfaces of the discs, we can do but little more than make a guess. For the black surface we may take the glass surface as reflecting 4 per cent. of the incident light, and allowing another 1 per cent. for the asphaltum—probably a reasonable estimate—we have, finally, $r = 0{\cdot}05$. In the case of the silver surface the reflection was tested by means of a thermopile, and it was concluded that at least 96 per cent. of the beam used was reflected.

The Energy of the Beam.

A determination of the energy of the beam used was made and afforded a means of calculating the absolute values of the pressures to be expected.

As in the experiments of Nichols and Hull, and in other experiments on light-pressure which we have made, the energy was measured by allowing the beam to fall on a blackened disc of pure silver (2 cm. diameter and 0·28 cm. thick), and by observing the initial rate of rise of temperature by means of a constantan-silver thermo-electric junction soldered to the disc. A Rubens panzer-galvanometer was used and was adjusted to have a period of about two seconds and was then made dead-beat. The transit of each centimetre-division of the scale across the field of view of the observing telescope was recorded on the drum of an electrically driven chronograph. Just before and just after each series of transits, a set of 5-second intervals were also recorded in order to give the peripheral speed of the drum. The number of micro-volts per scale-division was then determined, and the thermo-electric power of the couple being known from a separate experiment, it was possible, by means of a graphical representation of the chronograph-record, to calculate the rate of rise of temperature, and hence the energy of the beam was calculated to be 33×10^{-6} erg per centimetre length. This would be the force in dynes on a fully absorbing surface. The moment of inertia of the suspended system was 0·770 gramme-cm.^{2}, and its period was 74·6 seconds. The arm was 1·00 cm. Hence the beam should give a deflection of 13·6 divisions of the scale used when falling on a disc fully absorbing on both sides. Assuming that the BB disc reflects 5 per cent., the deflection should be 14·3 divisions. This is in close agreement with half the value obtained with SS and SB, and the excess over 14·3 of the observed value 16·1 obtained with BB is probably to be ascribed to residual radiometer-action. The smallness of the excess shows that the radiometer-action was reduced to a very small amount.

Initial Effects.

We have already referred to the necessity for exposing the discs in general to the beam for a long time before taking the observations of the deflections. On the BB disc, however, there was no marked initial effect, and after a few periods the deflection was nearly constant. When the beam was first allowed to fall on one of the other discs an initial disturbing effect was evident, and in some cases even half an hour seemed insufficient to produce a steady state.

On the BS disc there were sometimes indications of a pressure for the first few seconds, but a strong *suction* always set in, and this suction, after reaching a maximum, rapidly subsided, giving place finally to a pressure increasing to the limiting value corresponding to the steady state.

The SS disc showed want of symmetry. On the direct side there was suction followed by pressure, as in the last case, but on the reverse side there was at first excessive pressure, reaching a maximum and then slowly falling to the value given in the steady state.

The SB disc showed an initial excess pressure very similar to that on the reverse side of the SS disc.

For both the SS and SB discs the duration of these initial effects was so drawn out that an appreciably steady state was not attained in much less than an hour. In Fig. 5 the effects on all four discs are represented by curves in which the time is taken as abscissa and the deflection as ordinate. The residual effects which follow the cutting off of the light are also indicated.

As an explanation we suggest that these effects are due to the heat of the beam driving off occluded gas from the silver films, the expulsion of the gas causing a back pressure on the film. In the case of the SS disc it was known that the silver film on one side was decidedly thicker than that on the other, so that there is no difficulty in accounting for the want of symmetry observed in that case.

This explanation is supported by two observations: first, that with a stronger beam the steady state is sooner reached; secondly, that in the case of the BS disc it was noticed that if after the establishment of the steady state one cut off the light for a minute or two, and then put it on again, an initial suction took place but was much less than originally.

Assuming that the effects observed were really due to the expulsion of occluded gas as suggested above, it is possible to form an estimate of the total mass of gas given off by calculating from the curves (Fig. 5) the total impulse given to the disc. Thus, if we assume the gas to be oxygen and suppose, further, that the molecules leave the film normally with the ordinary molecular velocity, we find that for the *BS* disc the total mass given off was about $1{\cdot}7 \times 10^{-7}$ gramme. Taking the volume of the experimental flask as 2 litres,

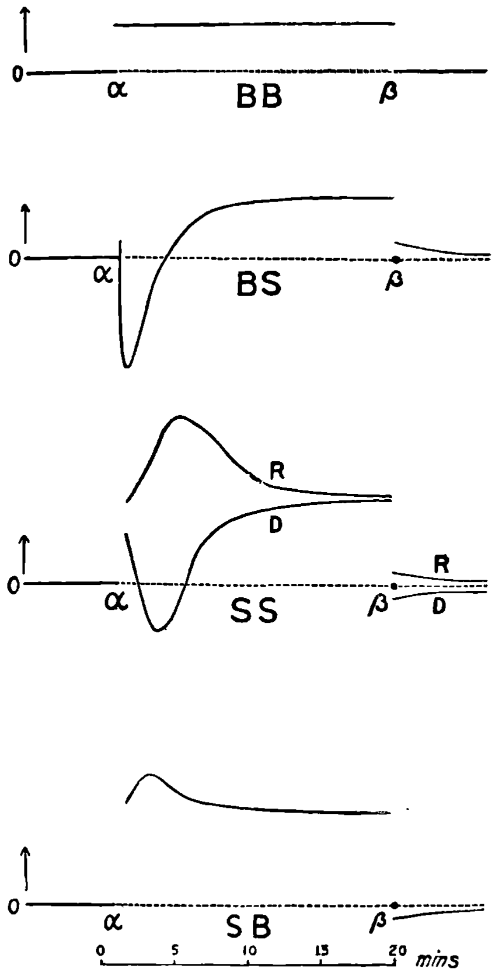


Fig. 5. *Initial Effects.* The ordinate represents the deflection, plotted against the time as abscissa. The direction of the deflection for *pressure* is shown by the arrow. For the *SS* disc curves *D* and *R* correspond to the 'direct' and 'reverse' sides respectively. The light is put on at *α* and cut off at *β*.

this quantity of gas would give a partial pressure of about $\frac{1}{20}$ dyne/cm.^{2}. So that unless the gas is rapidly absorbed by the charcoal it would appear that the vacuum might be sensibly affected. Unfortunately we had no means of forming even a rough idea of the actual pressure of the residual gas in the apparatus.

The Temperature of the Discs.

In the steady state all the energy of the beam absorbed by a disc must be radiated from the faces. By assuming the fourth-power law of radiation, we may therefore estimate roughly the rise in temperature of the disc. The results are 55° C. and 90° C. for the rise in temperature of the *BB* and *BS* discs respectively.

For the *BB* disc it was also estimated that the temperature-difference between the two faces was probably less than $\frac{1}{10}$° C. The temperature-differences for the other discs should be still less. It should be noticed that the glass is black for the issuing radiation, hence the asphaltum layer alone counts in the case of the *BB* disc.

Early Experiments with Platinum Discs.

In some early experiments we used two discs, *BB* and *BS*, of platinum foil $\frac{1}{8}$ mm. thick, the black surfaces being formed by depositing platinum black. Results were obtained somewhat like what we expected as to the ratios, but the deflections depended very greatly on the state of the vacuum, and under the best conditions were about 50 to 100 per cent. greater than the values calculated from the energy of the beam. This disagreement was doubtless chiefly due to radiometer-action. The black surface, being flocculent, is obviously badly conducting, the temperature-slope is therefore increased, and we have in consequence a big differential radiometer-action on the faces of the discs. Moreover, the polished platinum is a poor reflector, so that such discs quite fail to approach the ideal conditions. These considerations led to the use of the asphaltum discs.

29.

ON SMALL LONGITUDINAL MATERIAL WAVES ACCOMPANYING LIGHT-WAVES.

[*Roy. Soc. Proc.* A, **85**, 1911, pp. 474–476.]

All experiments on the pressure of light agree in showing that there is a flow of momentum along the beam. This flow is manifested as a force on matter wherever there is a change of medium. When the light is absorbed, the momentum is absorbed by matter. When the beam is shifted parallel to itself there is a torque on the matter effecting the shift. The momentum would therefore appear to be carried by the matter and not merely by the ether. Though there is an obvious difficulty in accepting this view when the density of the matter is so small as it is in interplanetary space, it appears to be worth while to follow out the consequences of the supposition that the force equivalent to the rate of flow of momentum across a plane perpendicular to a beam of light acts upon the matter bounded by the plane. This rate of flow per square centimetre is equal to the energy-density or energy per cubic centimetre in the beam. Of course, in experiments, only the average of the rate of flow during many seconds and the average energy per cubic centimetre in a length of beam of millions of miles is actually measured. But on the electromagnetic theory of light, which suggested the experiments and which gives the right value for the pressure, this pressure is equal to the energy-density at every point of a single wave.

Let us suppose that we have a train of plane polarised electromagnetic waves of sine-form, the magnetic intensity being given by

$$H = H_1 \sin \frac{2\pi}{\lambda}(x - vt),$$

where H_1 is the amplitude of H. The magnetic energy per cubic centimetre at any point is $\mu H^2/8\pi$, and as the electric energy is equal at each point to the magnetic energy, the total energy is $\mu H^2/4\pi$.

The energy per unit volume is $\int_0^1 \frac{\mu H^2}{4\pi}\, dx = \mu H_1^2/8\pi$.

The pressure p across a transverse surface is

$$p = \mu H^2/4\pi = \frac{\mu H_1^2}{4\pi} \sin^2 \frac{2\pi}{\lambda}(x - vt)$$
$$= \frac{\mu H_1^2}{8\pi}\left\{1 - \cos\frac{4\pi}{\lambda}(x - vt)\right\}.$$

The force on an element of length dx is

$$-\frac{dp}{dx}dx = -\frac{\mu H_1^2}{8\pi}\frac{4\pi}{\lambda}\sin\frac{4\pi}{\lambda}(x - vt)\,dx$$
$$= -\frac{\mu H_1^2}{2\lambda}\sin\frac{4\pi}{\lambda}(x - vt)\,dx.$$

If ξ is the linear longitudinal displacement of the element there will be a force due to elastic change of volume

$$q\frac{d^2\xi}{dx^2}dx,$$

where q is the elastic constant for compression or extension.

If ρ is the density of the material, the equation of motion is

$$\rho\frac{d^2\xi}{dt^2} = q\cdot\frac{d^2\xi}{dx^2} - \frac{\mu H_1^2}{2\lambda}\sin\frac{4\pi}{\lambda}(x - vt).$$

Assume $$\xi = A\sin\frac{4\pi}{\lambda}(x - vt - \epsilon).$$

Then, substituting,

$$\left(\rho A\,.\frac{16\pi^2}{\lambda^2}v^2 - qA\frac{16\pi^2}{\lambda^2}\right)\sin\frac{4\pi}{\lambda}(x - vt - \epsilon) = \frac{\mu H_1^2}{2\lambda}\sin\frac{4\pi}{\lambda}(x - vt).$$

Putting $x = 0$ and $t = 0$, we see that $\epsilon = 0$. Putting $q = \rho u^2$, where u is the velocity of free elastic waves of the q type, and assuming that the longitudinal waves are forced waves, keeping exact time with the waves of light, we have

$$A = \frac{\lambda\mu H_1^2}{32\pi^2\rho(v^2 - u^2)}.$$

As u/v is negligible for all ordinary matter,

$$\xi = \frac{\lambda\mu H_1^2}{32\pi^2\rho v^2}\sin\frac{4\pi}{\lambda}(x - vt),$$
$$\dot{\xi} = -\frac{\mu H_1^2}{8\pi\rho v}\cos\frac{4\pi}{\lambda}(x - vt).$$

The potential energy in these waves is negligible in comparison with the kinetic. We have then

$$\text{Energy per unit volume} = \tfrac{1}{2}\int_0^1 \rho\dot{\xi}^2 dx = \frac{\mu^2 H_1^4}{256\pi^2\rho v^2}.$$

As the electromagnetic energy per unit volume is $\mu H_1^2/8\pi$,

$$\frac{\text{Energy in longitudinal waves}}{\text{Electromagnetic energy}} = \frac{\mu H_1^2}{32\pi\rho v^2} = \frac{1}{8}\frac{\mu H_1^2}{8\pi}\bigg/\frac{\rho v^2}{2},$$

which is one-eighth of the electromagnetic energy divided by the energy which the matter would have if it were moving with the velocity of light in that matter.

This shows how infinitesimal is the fraction of the energy of the beam which is located in these waves of compression of the material.

The fraction is proportional to the intensity of the beam.

As an example, take a beam of the intensity of full sunlight just outside the earth's atmosphere, in which the energy-flow is about $1{\cdot}4 \times 10^6$ ergs/sec. The energy-density $\mu H_1^2/8\pi$ is therefore $1{\cdot}4 \times 10^6 \div v$. Put $v = 3 \times 10^{10}/n$, where n is the refractive index. The fraction is

$$\frac{1}{4} \cdot \frac{1{\cdot}4 \times 10^6}{27 \times 10^{30}} \frac{n^3}{\rho}, \text{ or about } 1{\cdot}3 \times 10^{-26} n^3/\rho.$$

At the surface of the sun it would be about 40,000 times as much, say, $5 \times 10^{-22} n^3/\rho$.

It is interesting to note that if a beam of light is incident on any reflecting or absorbing surface and if the pressure of light is periodic with the waves it must give rise to ordinary elastic waves in the material of frequency double that of the light-waves.

30.

ON THE CHANGES IN THE DIMENSIONS OF A STEEL WIRE WHEN TWISTED, AND ON THE PRESSURE OF DISTORTIONAL WAVES IN STEEL.

[*Roy. Soc. Proc.* A, **86**, 1912, pp. 534–561.]

[*Read* March 21, 1912.]

In the *Proceedings of the Royal Society** there is an account of some experiments which I made to show that wires when twisted lengthen by an amount proportional to the square of the angle of twist, a result expected from an analysis of the strains in a finite pure shear. In those experiments it was necessary to put considerable loads on the wires.

I have now succeeded in measuring the change in the diameter of a wire when twisted, as well as the longitudinal extension, and have found that the change, a contraction, is also proportional to the square of the angle of twist. It has been now found that the change is sensibly the same for large loads and for the smallest load which could be used, when the wire was sufficiently straightened before being twisted, so that apparently the only function of the load is to straighten the wire.

To measure change in the diameter the wire was fastened at the bottom of a long narrow tube, the 'wire-tube,' filled with water. It passed out from the top of the wire-tube through a water-tight leather washer. A capillary glass tube rose vertically from an orifice in the side of the tube, into which it was cemented, and the change of the water level in the capillary when the wire was twisted indicated the change in the volume of the wire within the wire-tube.

Description of the Apparatus.

The apparatus used for the measurement of the effects is shown in Fig. 1, where, for convenience of representation, various parts are put into the plane of the figure, though actually they were in different planes.

An iron bracket B projected from the wall of the laboratory, and a tripod rested on it, on three levelling-screws. The tripod carried a conical bearing

* Series A, 1909, vol. 82, p. 546. [*Collected Papers*, Art. 25.]

for the twisting head-piece T. When the axis of this was exactly vertical, the tripod was fixed to the bracket by clamping-screws s, s. The twisting-head was provided with a circular plate P, with marks at 90° intervals, which could be set against a fixed index i. In practice only whole turns were given, so that only one mark was used, except in one experiment described later.

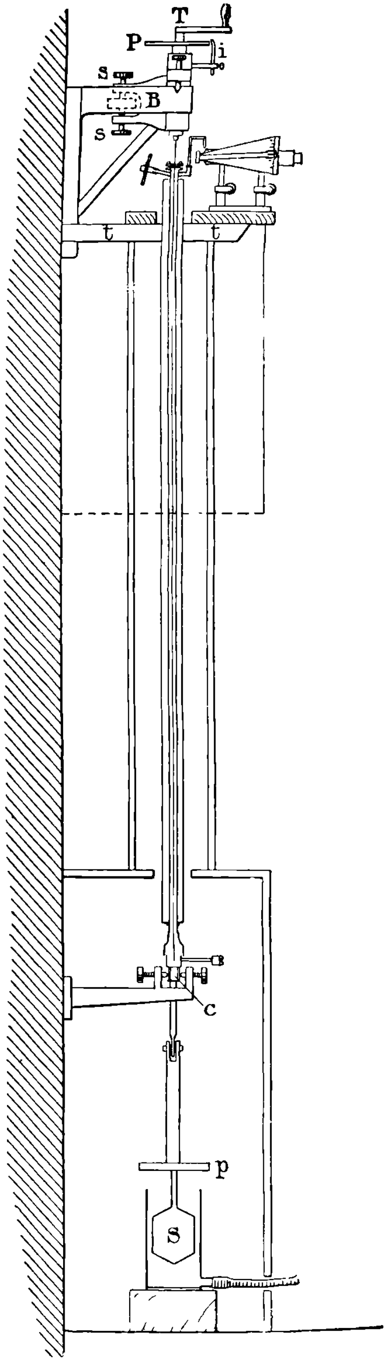


Fig. 1.

At the lower end of T, there was a chuck into which the upper end of the wire was inserted, and a tightening-screw made a firm grip. The wire in all cases was very nearly 160·5 cm. long. At its lower end it was gripped by a similar chuck attached to a steel cross-piece C, about 29 cm. long, seen endwise in the figure.

Polished steel plates were screwed on to the vertical sides of this cross-piece near its ends. Four horizontal screws, working in brackets projecting from the wall, and with small steel balls at their ends, were screwed up so as just not to touch the steel plates when there was no twist on the wire. But when a twist was put on, the cross-piece moved up against two of the screws, and was thus fixed in position. Below the cross-piece there was a rod to which was attached another rod carrying a platform p, and on this weights could be placed. Each weight was in two semicircular halves. Below the platform was a lead weight S, which I call the sinker, with a volume of 1020 c.c. This hung in a can, and near the can was a water cistern, not shown, connected to it by a rubber tube. When the cistern was pulled up water flowed into the can so as just to cover the sinker and lessen the load by 1020 grm. When the cistern was let down the water flowed back into it and the load increased to its full value. This was used to determine Young's modulus and Poisson's ratio.

A table t was fixed to the wall independently of the bracket B, to carry the observing microscope, and the observer sat on a platform built up about 1·5 metres from the floor. The brass wire-tube had an internal diameter about 2 mm. The top of the tube was fixed in a horizontal brass plate, in which was a hole about 0·25 mm. wider than the wire. On this plate rested a well-vaselined leather washer about 1·5 mm. thick, drilled so that it was fairly tight round the wire. On the washer was another brass plate, with a hole in it about 0·35 mm. wider than the wire. Four screws passed freely through holes in this upper plate, and were screwed into the lower plate. Springs between the heads of the screws and the upper surface of the upper plate gave sufficient pressure on the washer. It was found necessary to have the holes in the plates somewhat larger than the wire, in order to adjust the wire and the wire-tube both vertical. An arm, not shown in Fig. 1, projecting from the lower part of the apparatus, with a sliding weight on it in the plane of the upper side-tubes, sufficed to make this vertical adjustment. At first I tried india-rubber washers. They were quite good when first put in, but they deteriorated rather rapidly, and, when they began to perish, they let a small quantity of water out of the tube when the wire moved. The leather washer only required renewal once, when it began to let water escape, and then, on examination, it appeared to be due to action on the wire, which was perceptibly rough on the surface where it emerged from the tube. A short length was cut off the lower end of the wire, and an equal length was let down through the upper chuck, so that once more a smooth part of the wire passed through the washer. There was no further difficulty, and no evidence again of any escape of water.

At the upper end of the wire-tube there were two side-tubes. A glass capillary tube was cemented into one, and bent as shown on the right in the figure, the vertical branch being about 10 cm. long. When the tube was filled with water, the level in the capillary was adjusted at the level of the microscope about 7 cm. above the level of the washer, as this was about the rise of water in the capillary due to surface-tension, and there would therefore be no hydrostatic pressure on the water at the level of the washer. The tendency to leak would thereby be lessened, but the precaution was probably needless. Into the tube on the left a plunger passed through a leather stuffing-box. The plunger had a diameter of 0·2060 cm., and it was driven to or fro by a micrometer-screw of $\frac{1}{2}$ mm. pitch. On the head of this screw was a 10 cm. plate, with 500 divisions on its circumference. This plunger was used ordinarily to adjust the level of the water in the capillary. But it was also used to calibrate the capillary. For this calibration, the usual observing microscope was replaced by a microscope-cathetometer, and the change of water-level in the capillary was measured for one turn in or one turn out of the screw. Turning always in or always out, it was hardly possible to hit exactly on a whole turn, but a correction could be made, of course, for the fraction of a division in the micrometer head in excess or defect of a whole turn. The mean of 10 measurements when the micrometer was driven inwards 0·5 mm. gave a rise of 11·135 mm., with a range of 0·155 mm. between greatest and least. The mean of 10 measurements when the micrometer was drawn out 0·5 mm. gave a fall of 11·190 mm., with a range of 0·045 mm. The value was taken as 11·16 mm. This gives the cross-section of the capillary as 0·001493 sq. cm., and its diameter as 0·0436 cm.

At the lower end the wire-tube was soldered on to a screw-cap which could be screwed over the chuck gripping the lower end of the wire.

Below the chuck was a side-tube used to fill the wire-tube with water. For this purpose the side-tube was connected with a flask in which water was boiled. The steam passed up through the crevices in the chuck and out at the plunger-tube, from which the plunger was removed. A funnel containing water was connected on to the plunger-tube, and when the water in this was boiling freely, through the passage of the steam, the flask was allowed to cool and water was sucked back into the wire-tube. When it was full the flask was detached and a cap was screwed on to the lower tube. The plunger was replaced and the capillary, which had been closed meanwhile, was opened. By driving in the plunger the water was raised up to the level of the washer and to any desired point in the capillary.

When the apparatus was not being used the open end of the capillary was under water in a beaker, the plunger being driven in so that the capillary was entirely filled with water. The apparatus thus remained full of water whatever change of temperature might occur. When required for work the beaker was withdrawn and the plunger was screwed out till the meniscus was in the field of view of the microscope.

The wire-tube was surrounded by an outer tube about 2·5 cm. diameter, filled with water. This merely served as a means of reducing the effect of outside or inside temperature-changes. A wooden casing covered with tin foil surrounded the whole from the floor up to the table to lengthen out still further any effects due to temperature-change.

To observe the changes of level due to twisting, a microscope with a 1-inch objective and provided with a parallel-plate micrometer was used. The micrometer-scale was calibrated by means of a millimetre divided to tenths on a standard invar bar. Twelve determinations of 0·4 mm. gave 107·2 micrometer-divisions equal to 1 mm., the determinations falling within about 1 per cent. range. Then 1 micrometer-division = 0·00933 mm. Since the cross-section of the capillary is 0·001493 sq. cm., one division of the micrometer signifies a change of volume of the water in the wire-tube of $1{\cdot}393 \times 10^{-6}$ c.c.

When it was desired to read the height of the water in the tube the micrometer-plate was moved till the cross-wire in the microscope just touched the image of the lowest point of the capillary meniscus. The field was well illuminated by a small lamp behind the capillary, but the image was not always very distinct, and settings of the micrometer could not be trusted to, I think, two or three tenths of a division in some cases, though usually they were more exact. Close to the tube and between it and the micrometer-plate a small vertical plate of glass was fixed to the tube at 45° to the line of sight, and this reflected the point of a needle which was also fixed to the tube, so that its image was in the same plane as, and close to, the image of the meniscus. This enabled the observer to note the position of either the meniscus or the needle-point without moving the microscope.

When the wire lengthened the wire-tube was let down by an equal amount, and the needle-point fell. Let us call this fall NP.

At the same time the wire contracted laterally, and the meniscus fell in the tube, and the fall relative to the tube gave the change in volume. The fall observed was that relative to the tube plus that of the tube or NP.

Hence, if the fall observed in the microscope is T the fall relative to the tube is $T - NP$.

The Wires and their Preparation.

Two piano-steel wires were used in the experiments here described, No. I with a mean diameter 0·0986 cm., the diameters in two planes at right angles being measured with a micrometer every decimetre of its length. The measurements ranged from 0·0980 to 0·0989 at different points. No. II had a mean diameter of 0·1210 cm., measured in the same way, with a range at different points from 0·1207 to 0·1212.

It was found necessary to straighten these wires, for, unstraightened, they showed the effect with light loads noticed in the previous paper*, an apparent shortening on twisting, due, I think, to coiling. To straighten them they were loaded and an electric current was passed through them. No. I was loaded with 50 kgrm., and received a current of 10 amperes. No. II was loaded with 60 kgrm., and received a current of 16 amperes. In each case the wire drew out slightly and then stopped, acquiring a blue temper without rising to a red heat.

Of course the wires became circularly magnetised, but the magnetisation can hardly have contributed to the results here to be described, as these results are of the same character and order as results obtained with heavily

* [*Collected Papers*, Art. 25.]

loaded unannealed wires in a number of preliminary experiments made before the experiments took their final shape.

A few experiments were made on a hard-drawn copper wire, mean diameter 0·1219 cm. (with a range from 0·1216 to 0·1224 cm.).

The Method of Measuring the Lowering on Twisting.

In making any determination of NP or T the following plan was adopted. Suppose, for instance, that the value of T was to be found for four turns of the wire clockwise as seen from above. The position of the meniscus was read for no twist at a given minute, then my assistant put on four turns clockwise—denoted by C_4—then he gave a signal just before, and again exactly at the next half minute, and I set the cross-wire on the meniscus at the half minute. The micrometer was read, and the twist was taken off. At the next half minute the micrometer was set as before. Again C_4 was put on, and so on, usually for 32 observations. The first two or three readings were not taken into account, as initially there was usually some irregularity, due probably to settling down in the bearing. The readings were combined in threes in the usual way to give $T = \frac{1}{2}(a + c) - b$ to eliminate as far as possible any march of the zero reading. With the meniscus there was almost always a march, due chiefly to temperature-change, for, of course, the arrangement was a very sensitive thermometer. The mean result of the 32 observations was equivalent to 15 or 16 independent determinations. To determine NP the same course was followed, except that the time was not noted. For, though there was often a march in the zero, it was very much smaller, and the observations were made at sufficiently nearly equal intervals of time without noting exact times. This small march was doubtless partly due to temperature-change, but also partly due, I believe, to further settling down of the cone of the twisting-head into its bearing through slow squeezing out of the oil. In reconstruction I should try the effect of replacing the conical bearing by a ball-bearing.

Before the reading was made it was found to be absolutely necessary to move the head-piece some ten or fifteen times to and fro through a small angle—perhaps diminishing from 20°—on each side of the final position. If this was not done the wire did not sink down or rise up to its final position, probably owing to some small friction in the leather washer. After the alternating motion of the head-piece had been given ten or fifteen times no further alternation made any difference in the reading. It was only after finding the necessity of this that I obtained consistent readings.

The maximum and minimum values of NP in a set of 30 determinations usually differed by about 0·4 division, only once rising to 0·8 division.

The maximum and minimum values of T in a set of 30 differed more, as might be expected; the difference averaging 1·7 divisions, and once rising to nearly 5 divisions. This, however, was in the case of the least load, when the weight was probably insufficient to keep the wire-tube quite vertical.

Subsidiary Experiments.

The three following subsidiary experiments were made in order to justify the methods of measuring the changes of dimensions on twisting:

1. To show that the lengthening on twisting is not due to a change in Young's modulus, Y.

This is satisfactorily proved by the experiments on twisting described below. For, suppose that we have a stress P applied to the end of the wire by a load stretching length l by dl when the wire is not twisted, we have $dl = Pl/Y$. Now let the wire thus loaded be twisted through, say, four turns and let the lowering through twisting be δ. If this is due to a change in Young's modulus to Y', $dl + \delta = Pl/Y'$. Whence $\delta - Pl\,(Y'^{-1} - Y^{-1})$, and δ should be proportional to P, whereas it is found to be very nearly the same for loads varying from 5 to 50 kgrm. (approximately).

It appeared worth while, however, to test the question directly, by finding the extension of wire I for very different loads when 1·02 kgrm. was added, first with the wire untwisted, then with the wire twisted through four turns clockwise. The following results, in micrometer-divisions, were obtained, each the mean of a number of measurements:

TABLE I.

Load	No twist Lowering for 1·02 kgrm.	C_4 twist Lowering for 1·02 kgrm.
18·5	10·55	10·52
38·5	10·68	10·80
48·5	10·50	10·39 *
Means	10·58	10·57

Thus a twist of four turns produced no measurable change in Young's modulus.

2. To show that the rise and fall of the liquid meniscus were due to, and measured, the change in volume of the wire in the wire-tube.

The most satisfactory way of showing this appeared to consist in using the apparatus to measure Poisson's ratio σ. The load was altered by 1·02 kgrm. by alternately immersing the sinker in water and letting the water run out.

* [This corrected value is that given in the original manuscript. ED.]

The rise and fall of the needle-point gave the end-extension, and Young's modulus Y could then be calculated; while the rise and fall of the meniscus gave the volume-change, and the side-contraction and Poisson's ratio σ could be calculated. The rigidity-modulus n could be obtained from $n = \frac{1}{2}Y/(1 + \sigma)$.

When the load was increased there was some yield of the supporting bracket. To determine its amount, a needle-point was fixed on the bracket close to the upper chuck and sighted by the microscope. A series of loads up to 50 kgrm. showed that the lowering per kilogramme was 0·06 division. An addition of 1·02 kgrm., therefore, lowered the bracket by 0·061 division, and this had to be subtracted from the NP reading when used to find Y. In the lowering of the meniscus $T - NP$, it obviously did not come into consideration. The observed change of volume given by $T - NP$ had to be corrected by a factor about 160·5/156, since only 156 cm. of wire were within the wire-tube and 4·5 cm. were outside. The actual length outside varied from 4·2 to 4·6, and the factor was varied accordingly.

No doubt better values of Y and σ might have been obtained with a larger change of load, but, to test the apparatus, it was important to observe lowerings of the same order as those observed in the twisting. In the following Table II the values of $T - NP$ and NP, due to an addition of 1·02 kgrm., are given in micrometer-divisions corrected as above described. Each value is the mean of 30. The range between maximum and minimum in a set averaged 1·7 divisions for T and 0·57 division for NP:

TABLE II. *Elastic Moduli and Poisson's Ratio.*

Load in kgrm.	NP	$T-NP$	σ	$10^{-12}\,Y$	$10^{-12}\,n$
		Steel Wire I, Diameter 0·0986 cm.			
18·5	10·65	29·86	0·272	2·11	0·830
28·5	10·60	29·57	0·271	2·12	0·835
38·5	10·74	29·37	0·265	2·09	0·828
48·5	10·44	29·05	0·270	2·16	0·849
Mean values	—	—	0·270	2·12	0·835
		Same Wire with C_4 Twist on it			
48·5	10·27	28·04	0·265	2·18	0·861
		Steel Wire II, Diameter 0·1210 cm.			
48·5	7·05	31·22	0·287	2·12	0·825
		Hard-drawn Copper Wire, Diameter 0·1219 cm.			
18·5	11·20	61·52	0·331	1·31	0·493

The values found for a steel wire after annealing, given in the paper already referred to*, were $Y = 2{\cdot}06 \times 10^{12}$, $n = 0{\cdot}809 \times 10^{12}$ (by vibration), whence $\sigma = 0{\cdot}273$.

The values for the steel wires are sufficiently near to each other and to the values previously found to show that the tube-readings gave, at any rate, very nearly the true changes in volume.

3. To show that the changes were very nearly isothermal.

The change in temperature of a solid sheared adiabatically through ϵ is

$$d\theta = -\lambda n\theta\epsilon^2/2JC_p\rho,$$

where λn is the decrease in rigidity per degree rise, θ is the absolute temperature, C_p is the specific heat, and ρ is the density.

Let us suppose that a steel wire 156 cm. long and 0·05 cm. radius nearly wire I—is twisted through one turn. It is sufficient to investigate the effect for one turn, for both the adiabatic temperature-change and the twisting-effects are proportional to the square of the shear, and therefore in a ratio independent of the shear. Then $\epsilon = 2\pi r/156$, where r is the distance of an element from the axis. The mean change in temperature of such a wire is

$$\int_0^a \frac{2\pi r\, d\theta\, dr}{\pi a^2} = -\frac{\pi^2 a^2 \lambda n\theta}{JC_p\rho 156^2},$$

where $a = 0{\cdot}05$.

For steel we may put $n = 10^{12}$, $\lambda = 2 \times 10^{-4}$, $\rho = 7{\cdot}8$, $C_p = 0{\cdot}112$. Taking θ as 300° A. we find the heat in calories developed by the twist to be about -18×10^{-4} calories. Or on untwisting $+18 \times 10^{-4}$ calories.

If this heat were confined to the steel it would alter its temperature by about 1/600° C. and its linear dimensions by about $1{\cdot}8 \times 10^{-8}$ in 1. The twisting through one turn, as will be seen below, alters the radius by about $3{\cdot}19 \times 10^{-7}$ in 1 and the length by about $1{\cdot}72 \times 10^{-6}$. The effects of an adiabatic change of temperature would, therefore, be appreciable compared with the effects of twisting, especially on the radius. But the wire shares its heat, positive or negative, with the water in the wire-tube, and here it may produce a serious effect, if it gets no farther than the wire-tube, owing to the considerable coefficient of expansion of the water. Let us suppose that the heat or cold is shared with the water in the wire-tube so rapidly that both are at one temperature. The water, having approximately three times the volume of the steel, has about 7/9 the heat-capacity of steel plus water. So that the water would receive about 14×10^{-4} calories. If a mass of water at a temperature at which its cubical expansion is α receives H calories its change of volume is $H\alpha$, whatever the total volume. In our case the temperature was usually about 12° C., at which α is about 10^{-4}. Then the volume-change would be about 14×10^{-8} c.c. and since one micrometer-division is about 14×10^{-7} c.c.

* *Loc. cit.* p. 554. [*Collected Papers*, p. 366.]

along the capillary, one turn, through thermal effect alone, would produce a fall on twisting and a rise on untwisting of about 0·1 division. The actual change observed on twisting through one turn was about 0·56 division. If then the heat or cold only slowly spread from the wire, or again if it were rapidly shared with the water in the wire-tube but only slowly spread thence, the measurements would be seriously affected. But it is obvious that there must, in reality, be a rapid adjustment of temperature between the wire-tube and the outer water-jacket, and it was important to find out how rapidly the adjustment progressed. Fortunately the wire was insulated from the tube where it passed through the washer, so that it was easy to pass an electric current through it by connecting the terminals of a battery, one to the bracket, the other to the wire-tube. Heating-currents of the order of 1 to 2 amperes were thus passed along the wire. The current was put on for 2 seconds, the meniscus rushing up meanwhile fairly uniformly, and the point to which it rose was read on the micrometer. Then, 15 seconds after the cut off, the position of the meniscus was read again and the mean of a number of determinations showed that after 15 seconds only 0·032 of the original rise remained. The original rise varied from 7 to 18 divisions with different currents. If the twisting were made instantaneously and the reading of the fall in the tube were made 15 seconds later, about $0{\cdot}032 \times 0{\cdot}1 \div 0{\cdot}56 = 0{\cdot}006$ of the fall would be due to the cooling on twisting. But this is a very considerable over-estimate. The twisting was usually begun 25 seconds before reading and ended more than 15 seconds before. The effect of temperature change may, I think, be estimated at less than 1/300 of the whole. It was impossible to assign even an approximate value to it and as it proved to be so small it was neglected. The effect would have been reduced altogether beyond consideration if the readings had been taken at intervals of one minute, but this would have introduced errors, probably much worse, through irregularities in the march of temperature.

In the experiments on Poisson's ratio the adiabatic change of temperature on adding a load which stretches length l by dl is

$$d\theta = -\alpha Y \theta \, dl / J C_p \rho l,$$

where αY is the change in Young's modulus per degree. The value of α for steel is about 1/4000. This gives the heat for a stretch of 10 divisions as about -9×10^{-3} calories. With uniform temperature of steel and water in the wire-tube the water would have about 7×10^{-3} calories, and its effect would be about 0·5 division. After 15 seconds it would be about 0·016 division. As the change of level observed was about 30 divisions the effect is negligible. The direct effect in lengthening or shortening the wire is easily shown to be very much smaller.

Measurement of the Changes of Dimensions on Twisting.

The method of making the measurements has been described already. In the case of wire I, NP, or the lengthening of the wire, was observed for various loads for four turns and for two turns clockwise twist, denoted by C_4 and C_2, and for four turns and for two turns counter-clockwise twist, denoted by CC_4 and CC_2, each value being the mean of 30 determinations once or twice of 40—made as described above. As permanent set came in with five turns, four turns was the maximum twist employed. The mean values of T were also determined for the same four twists and $T - NP$ was corrected for the length of wire outside the wire-tube. For one load on wire I the lowerings for C_3 and CC_3 were also observed.

In all cases the lowering w could be represented very nearly by the parabola

$$L\,(n + c)^2 = w + b,$$

where n is the number of turns put on and L, c, and b are constants, not, of course, the same for NP and $T - NP$. The constant c represents the fraction of a turn, always on the counter-clockwise side of the point of no twist, about which the lowering is symmetrical. Putting $n = -c$, $b = -w$ is a small shortening, or for a counter-clockwise twist c the wire has a minimum length. The existence of c and b is due to want of homogeneity in the wire. They may be explained by supposing that the wire in the apparently neutral condition consists of a core and a sheath twisted against each other, as will be shown in the theory given later. Owing to want of exact centering the image 'wobbled' somewhat in the field during twisting, and only returned to the same vertical line after a whole number of turns, so that it was futile to attempt to measure b. But there was fairly conclusive evidence that it had a real existence.

According to the theory given, L is the all-important quantity. The internal strain only shifts the vertex of the parabola without altering its size.

To find the constants of a parabola which should fairly represent the results, it was assumed that the curve went through the point $w = 0$, $n - 0$, so that $Lc^2 = b$.

The equation then becomes

$$L\,(n^2 + 2nc) = w.$$

Let w be the lowering for C_n, and w' that for CC_n, then

$$L\,(n^2 - 2nc) = w',$$

whence

$$L = (w + w')/2n^2 \quad \text{and} \quad c = (w - w')\ 4nL = \tfrac{1}{2}n\,(w - w')\,(w + w').$$

The errors are given by

$$\delta L = \delta\,(w + w')/2n^2, \quad \text{and} \quad \delta c = \delta\,(w - w')/4nL,$$

assuming that L is without error in c.

If then we find the value of L, say L_4, from the lowerings at C_4 and CC_4, and the value of L, say L_2, from those at C_2 and CC_2, the value of the former should have four times the weight of the latter and we may take the best value of L as $\frac{1}{5}(4L_4 + L_2)$.

The value of c determined from C_4 and CC_4 should have twice the weight of that determined from C_2 and CC_2, and we may take the best value as $\frac{1}{3}(2c_4 + c_2)$.

In the following tables the results are set out. In Table III the lengthening of the wire I is given in micrometer-divisions, and below each lengthening the difference, calculated – observed, is put in italics, the calculated values being those given by the parabolas of which the constants are given in Table IV. Similar tables are given for wire II, and for the hard-drawn copper wire, but for a single load only, sufficient to secure good centering. After the experience with various loads with wire I, it appeared unnecessary to vary the load in the other cases. With wire II it was not thought advisable to go beyond three turns, and with the copper wire beyond one turn owing to permanent set, which began to be very considerable beyond those limits. The mode of calculating the best parabola was modified accordingly.

TABLE III. *Lowering NP for Steel Wire I, Diameter* 0·0986 *cm.*

Load	C_4	C_2	O	CC_2	CC_4
kgrm.					
48·5	5·095	1·452	0	0·768	4·233
	+ 0·06	*– 0·03*	*0*	*+ 0·13*	*– 0·13*
38·5	5·353	1·422	0	0·818	4·152
	– 0·05	*+ 0·06*	*0*	*+ 0·06*	*– 0·05*
28·5	5·265	1·500	0	0·858	4·259
	+ 0·01	*+ 0·03*	*0*	*+ 0·04*	*– 0·10*
18·5	5·382	1·680	0	0·905	4·091
	+ 0·14	*0·13*	*0*	*– 0·04*	*+ 0·06*
4·7	5·407	1·742	0	0·797	3·865
	+ 0·13	*– 0·15*	*0*	*– 0·03*	*+ 0·04*

For load 28·5, C_3 was 3·048, and CC_3 was 2·187, and these were taken into account in calculating the parabola, the difference in each case being + *0·05*.

TABLE IV. *Constants of Parabolas for Table III.*

Load	L	c	b
48·5	0·289	0·226	0·015
38·5	0·294	0·256	0·019
28·5	0·295	0·237	0·017
18·5	0·302	0·281	0·024
4·7	0·295	0·345	0·035

There was probably some permanent set given in the last two owing to accidental over-twisting of the wire.

The mean value of L is 0·295, and within errors of observation it is independent of the load. It is hardly likely that this is strictly true.

TABLE V. *Lowering of the Meniscus $T - NP$ for Wire I, corrected for length Outside the Tube.*

Load	C_4	C_2	O	CC_2	CC_4
48·5	11·69	3·94	0	0·07	6·65
	+ *0·33*	− *0·17*	*0*	+ *0·63*	*0·78*
38·5	11·63	3·94	0	0·84	6·36
	+ *0·22*	− *0·28*	*0*	+ *0·06*	*0·22*
28·5	11·82	4·00	0	1·13	6·38
	− *0·25*	− *0·28*	*0*	− *0·23*	*0·04*
18·5	10·99	2·84	0	1·00	6·17
	+ *0·36*	+ *0·37*	*0*	− *0·02*	− *0·02*
4·7	10·17	2·54	0	0·99	8·24
	− *0·14*	+ *0·30*	*0*	+ *0·57*	− *0·72*

For load 28·5, C_3 was 7·33 and CC_3 was 2·86. These were used in calculating the parabola, and the differences were respectively $-$ *0·01* and + *0·23*.

TABLE VI. *Constants of Parabolas for Table V.*

Load	L	c	b
48·5	0·559	0·69	0·26
38·5	0·569	0·61	0·21
28·5	0·578	0·61	0·21
18·5	0·525	0·54	0·15
4·7	0·548	0·29	0·05

As the errors of observation with the last two loads were about double those for the earlier loads, they are only given half the weight in finding L. The value of L is taken as 0·561.

TABLE VII. *Lowering NP for Steel Wire II, Diameter* 0·1210 *cm. Two Independent Sets.*

	Load	C_3	C_2	O	CC_2	CC_3
I	48·5	4·922	2·368	0	1·740	4·000
		+ *0·05*	− *0·06*	*0*	− *0·04*	+ *0·05*
II	48·5	4·983	2·243	0	1·708	4·049
		− *0·06*	+ *0·05*	*0*	− *0·01*	*0·00*

TABLE VIII. *Constants of Parabolas for Table VII.*

	L	c	b
I	0·501	0·154	0·012
II	0·499	0·147	0·011

The mean value of L is 0·500.

TABLE IX. *Lowering of the Meniscus $T - NP$ for Wire II, corrected for length Outside the Tube.*

	Load	C_3	C_2	O	CC_2	CC_3
I	48·5	15·16	7·73	0	3·90	9·22
		+ 0·20	*- 0·25*	*0*	*- 0·31*	*+ 0·33*
II	48·5	15·48	7·61	0	3·71	9·55
		+ 0·04	*- 0·06*	*0*	*- 0·07*	*+ 0·10*

TABLE X. *Constants of Parabolas for Table IX.*

	L	c	b
I	1·385	0·35	0·17
II	1·398	0·35	0·17

The mean value of L is 1·392.

Copper Wire. Diameter, 0·1219 cm.

I was only able to use C_1 and CC_1 owing to permanent set.

The values of NP were 1·043 and 0·415, and the parabola going through these points and the origin is $0{\cdot}73\ (n + 0{\cdot}22)^2 = w + 0{\cdot}03$.

The values of $T - NP$, corrected for 4·2 cm. outside the tube, were 8·007 and 1·433, and the parabola is $4{\cdot}72\ (n + 0{\cdot}35)^2 = w + 0{\cdot}58$.

The work given in the former paper appears to justify the assumption of the parabolic law for copper.

The End-Elongation, Side-Contraction, and Volume-Increase.

Steel Wire I. Diameter, 0·0986 cm.

If w is the end-lowering for one turn from the position of minimum length assumed to be L divisions,

$$\begin{aligned} w &= L \times \text{length of one micrometer-division} \\ &= 0{\cdot}295 \times 933 \times 10^{-6} = 2{\cdot}75 \times 10^{-4}\ \text{cm.} \end{aligned}$$

The length is $l = 160{\cdot}5$. Then

$$w/l = 1{\cdot}71 \times 10^{-6}.$$

If u is the decrease in the radius a for one turn,

$$2\pi aul = L \times \text{volume of one micrometer-division of capillary},$$
$$u = (0{\cdot}561 \times 1{\cdot}393 \times 10^{-6}) \div (2\pi \times 0{\cdot}0493 \times 160{\cdot}5)$$
$$= 1{\cdot}57 \times 10^{-8} \text{ cm}.$$

The radius is 0·0493 cm. Then

$$u/a = 3{\cdot}19 \times 10^{-7}.$$

The ratio side-contraction/end-elongation, namely,

$$u/a \div w/l = 0{\cdot}187.$$

If dv is the volume-increase in total volume v,

$$dv/v = (\pi a^2 w - 2\pi alu)/\pi a^2 l$$
$$= w/l - 2u/a = 1{\cdot}07 \times 10^{-6}.$$

All the quantities w/l, u/a, dv/v are proportional to the square of the twist from the point of minimum length. The ratio $u/a \div w/l$ is the same for all twists.

Steel Wire II. Diameter, 0·1210 cm.

Using the values given in the tables, we have for one turn

$$w = 4{\cdot}66 \times 10^{-4} \text{ cm.}, \qquad u/a = 5{\cdot}24 \times 10^{-7},$$
$$w/l = 2{\cdot}90 \times 10^{-6}, \qquad u/a \div w/l = 0{\cdot}181,$$
$$u = 3{\cdot}17 \times 10^{-8} \text{ cm.}, \qquad dv/v = 1{\cdot}85 \times 10^{-6}.$$

Copper Wire. Diameter, 0·1219 cm.

The corresponding quantities given by the single set of observations are not of such weight as those for wires I and II, but I add them here:

$$w = 6{\cdot}81 \times 10^{-4} \text{ cm.}, \qquad u/a = 1{\cdot}75 \times 10^{-6},$$
$$w/l = 4{\cdot}25 \times 10^{-6}, \qquad u/a \div w/l = 0{\cdot}41,$$
$$u = 10{\cdot}7 \times 10^{-8} \text{ cm.}, \qquad dv/v = 0{\cdot}75 \times 10^{-6}.$$

On comparing the results for wires I and II we see that side-contraction $\div$ end-elongation is very nearly the same for both. The theory given below makes both w/l and u/a proportional to the square of the radius for wires of the same material undergoing the same twist. But as far as these two wires are concerned they are very nearly proportional to $(\text{radius})^{2{\cdot}5}$. I do not think the discrepancy is to be ascribed to experimental error. Perhaps the theory is inadequate, but I think that it is more probable that slight differences in the material, not greatly affecting the ordinary elastic moduli, may produce very considerable changes in what we may term the secondary moduli, which, in the theory below, are denoted by p and q.

I should like to have taken observations on several more steel wires with a wider range of diameters, but I am not able to continue the work at present.

Experimental Verification of a Reciprocal Relation.

In the *Philosophical Magazine* for November, 1911, vol. 22, p. 740, Dr. R. A. Houston has expressed the reciprocal relation between the stretching and twisting of a wire (confined within limits of reversibility) in the form

$$\left(\frac{d\theta}{dF}\right)_{G\text{ const.}} = \left(\frac{dw}{dG}\right)_{F\text{ const.}}, \quad \text{.........................(1)}$$

where F is the end pull and w the increase in length, G the torque and θ the twist on the wire (I use letters for length and torque differing from Dr. Houston's).

As the apparatus only needed small modification it appeared to be worth while to see how nearly this relation was verified, and wire II was used for the purpose. Incidentally, the value of the rigidity was obtained by the method of statical torque.

When the observations needed are worked out it is found that they are identical, as of course was to be expected, with those needed to verify the relation

$$\left(\frac{dF}{d\theta}\right)_{w\text{ const.}} = \left(\frac{dG}{dw}\right)_{\theta\text{ const.}}, \quad \text{.........................(2)}$$

which is the more direct expression of the Conservation of Energy in these phenomena.

Taking equation (1) we require to know on the left the extra twist $d\theta$ which must be put upon the wire to keep G the same when a load dF is added. For this purpose the wire was initially loaded with 18·5 kgrm., and the head was turned through a right angle. The bar at the bottom was also turned through a right angle from its usual position. On the cross-bar a mirror was fixed reflecting into a telescope a millimetre-scale 156·5 cm. away. The ends of the cross-bar were rounded into arcs of a circle with centre in the axis of the wire and radius 14·70 cm. Horizontal threads passed off these arcs to two very light horizontal spiral springs which stretched very uniformly in proportion to the pull up to 40 or 50 grm. These springs were attached to the bases of two travelling microscopes, of which the horizontal scales merely were used to measure any change in stretch. Initially, the wire was without twist, and the position of the cross-bar on the scale was read. It would have been at least very difficult to determine directly the total stretch of the springs required to keep the cross-bar in position when the head was twisted, so the following plan was adopted:

A half-turn CC was put on by the head-piece, and the springs were stretched so as to bring the cross-bar to its original position. Then a further turn and a half CC was put on, and the *additional* stretch of each spring needed to keep the cross-bar in position was read. This additional stretch multiplied by 4/3 gave the total stretch of the springs, and thus the pull exerted at each end of the cross-arm to maintain two turns twist on the wire. The full stretches thus computed were 12·340 cm. on the left and 12·384 cm. on the right, corresponding to pulls, according to previous calibration, of 49·24 grm. and 46·87 grm., mean 48·06 grm. The torque was therefore

$$G = 48{\cdot}06 \times 981 \times 29{\cdot}4 = 1{\cdot}38 \times 10^6 \text{ dyne-cm.}$$

From this the rigidity is

$$n = 0{\cdot}838 \times 10^{12}.$$

The tube-method gave $0{\cdot}825 \times 10^{12}$, and the nearness of the two values appears to show that the springs could be trusted fairly well.

A load $dF = 30$ kgrm. was then added, and the torque for two turns was thereby diminished. The springs therefore contracted, and it was observed that they pulled the cross-bar round through 15·85 mm. on the scale the mean of five different observations ranging from 15·45 to 16·55, or through an angle 0·00507 radian.

Denoting this angle by $\delta\theta$, and the radius of the cross-bar arm by k, and the decrease of torque by δG,

$$\frac{\delta G}{G} = \frac{k\delta\theta}{s}, \qquad \text{(3)}$$

where s is the whole mean stretch of the springs for two turns.

But we require the twist $d\theta$, which must be put on the wire from its initial two turns, and in the opposite direction to $\delta\theta$, to restore the torque to G. This is given by

$$\frac{G}{G - \delta G} = \frac{\theta + d\theta}{\theta - \delta\theta}, \quad \text{where } \theta - 4\pi,$$

whence, on substituting for $\delta G/G$ from (3), we get

$$d\theta = \left(\frac{k\theta}{s} - 1\right)\delta\theta, \quad \text{and} \quad \frac{d\theta}{dF} - \left(\frac{k\theta}{s} - 1\right)\frac{\delta\theta}{dF}. \qquad \text{(4)}$$

Taking the right hand of equation (1), we require to know the lowering dw for a change dG in the torque under constant load. We get the lowering from the equation

$$L(n - c)^2 = w + b,$$

giving $$dw = 2L(n - c)\,dn = L(n - c)\frac{d\theta}{\pi}.$$

Also $$dG = \frac{Gd\theta}{\theta}.$$

Then $$\frac{dw}{dG} = \frac{L(n - c)\,\theta}{\pi G}. \qquad \text{(5)}$$

Equating (4) and (5), we ought to find

$$L = \frac{\pi G}{n-c}\left(\frac{k\theta}{s} - 1\right)\frac{\delta\theta}{\theta dF}. \quad \text{.......................} \ (6)$$

Substituting the known values on the right, viz., $G = 1{\cdot}38 \times 10^6$, $n = 2$, $c = 0{\cdot}15$, $k = 14{\cdot}7$, $\theta = 4\pi$, $s = 12{\cdot}37$, $\delta\theta = 0{\cdot}00507$, $dF = 30 \times 981000$, we get

$$L = 4{\cdot}48 \times 10^{-4}.$$

The observed value of L is given as w on p. 411, viz.,

$$L = 4{\cdot}66 \times 10^{-4},$$

showing as close an agreement as could be expected, considering the errors of observation.

Taking the second reciprocal relation (2), to find $\left(\frac{dF}{d\theta}\right)_{w\,\text{const.}}$ we must twist through $d\theta$ and observe dw, and then calculate what load dF must be removed to restore the original length.

We have $\quad dw = L(n-c)\frac{d\theta}{\pi}$, and $dF = \pi a^2 Y dw/l$.

Then

$$\frac{dF}{d\theta} = \frac{L(n-c)a^2Y}{l}. \quad \text{............................} (7)$$

To find $\left(\frac{dG}{dw}\right)_{\theta\,\text{const.}}$, we put on a load W and observe $\delta\theta$. If δG is the diminution in torque at this point and dG the diminution in torque with the original twist θ,

$$\frac{G - \delta G}{G - dG} = \frac{\theta - \delta\theta}{\theta}.$$

Substituting for $\delta G/G$ from (3), this gives

$$dG = \left(\frac{k\theta}{s} - 1\right)\frac{G\delta\theta}{\theta},$$

the change in torque for addition W when θ is constant.

The value of dw for this load is

$$dw = lW/\pi a^2 Y,$$

and

$$\left(\frac{dG}{dw}\right)_{\theta\,\text{const.}} = \left(\frac{k\theta}{s} - 1\right)\frac{\pi a^2 YG}{lW\theta}\delta\theta. \quad \text{.....................} (8)$$

Equating (7) and (8) and putting dF for W, we get

$$L = \frac{\pi G}{n-c}\left(\frac{k\theta}{s} - 1\right)\frac{\delta\theta}{\theta dF},$$

the same equation as before.

If we could use a wire without any internal strain when untwisted, c would be zero, and we could calculate L, the lowering for one turn, from observations on the torque and load alone.

A Theory of the Changes of Dimension on Twisting: The Stresses in a Finite Pure Shear.

In the paper already referred to* I showed that in a finite pure shear ϵ such as is represented in Fig. 2, in which a cube of section $ABCD$ is sheared into a figure of section $ABKL$ through an angle $CBK = \epsilon$, the thicknesses perpendicular to AB and to the plane of the figure remaining constant, the lines of maximum elongation and contraction are, to the order of ϵ^2, at right angles before the shear, making an angle $\epsilon/4$ with the diagonals of the square, as AE and BG. After the shear they are again at right angles to the order of ϵ^2, and make an angle $\epsilon/4$ with the diagonals on the other side as AF and BH. Since we have elongation in one direction AF, and contraction in a direction BH at right angles, the shear may be maintained by a pressure P along BH and a tension Q along AF as far as forces in the plane of the figure are concerned.

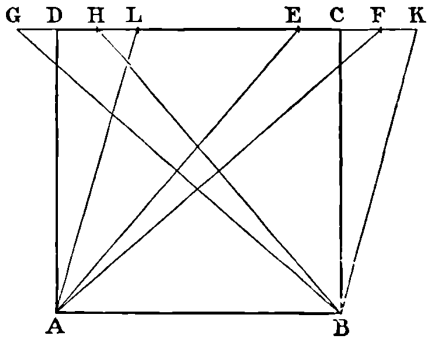


Fig. 2.

If we go to the first order of ϵ only,

$$P = Q = n\epsilon.$$

If we go to the second order we must put

$$P = n\epsilon + p\epsilon^2,$$

where p is a constant to that order.

If we reverse ϵ, P becomes equal to $-Q$, so that we have

$$-Q = -n\epsilon + p\epsilon^2 \quad \text{or} \quad Q = n\epsilon - p\epsilon^2.$$

We can only assume that there is no pressure or tension perpendicular to the plane of the figure, if we neglect ϵ^2. Going to the second order, we have to allow the possibility of a pressure of that order, which we may put as

$$S = q\epsilon^2,$$

where if q is negative the force is a tension.

Considering the equilibrium of the wedge ABC, Fig. 3, with AC in the direction of greatest elongation and BC in that of greatest contraction, I showed that the tangential stress along AB is, to the second order,

$$T = n\epsilon,$$

and that a pressure is required perpendicular to AB given by

$$R = (\tfrac{1}{2}n + p)\,\epsilon^2.$$

* *Loc. cit.* p. 546. [*Collected Papers*, p. 358.]

The analysis stopped here and was incomplete, as no account was taken of the stresses on the plane CD, Fig. 3, perpendicular to AB. It requires to be supplemented as follows:

Considering the equilibrium of the wedge CDB, let us suppose that on CD there is a tangential stress T' along CD, and a pressure R' perpendicular to it.

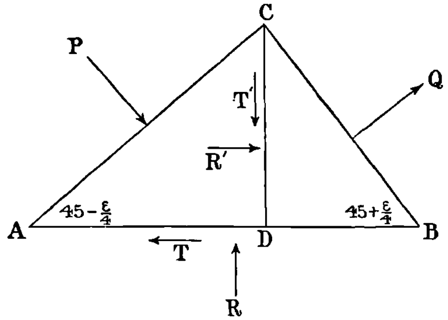


Fig. 3.

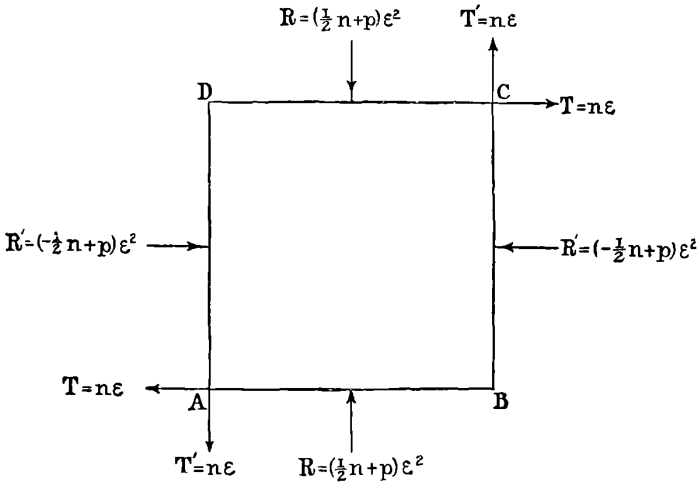


Fig. 4.

Resolving all the forces on CDB in a direction parallel to DB,

$$R' \,.\, CB \sin (45 + \tfrac{1}{4}\epsilon) - T \,.\, CB \cos (45 + \tfrac{1}{4}\epsilon) + Q \,.\, CB \sin (45 + \tfrac{1}{4}\epsilon) = 0,$$

whence
$$R' = T \cot (45 + \tfrac{1}{4}\epsilon) - Q;$$

or, since $T = n\epsilon$ and $\cot (45 + \tfrac{1}{4}\epsilon) = (1 - \tfrac{1}{2}\epsilon)$, neglecting ϵ^2, as it is multiplied by ϵ,

$$R' = n\epsilon (1 - \tfrac{1}{2}\epsilon) - n\epsilon + p\epsilon^2 = (- \tfrac{1}{2}n + p)\, \epsilon^2.$$

Resolving in a direction parallel to CD,

$$T' . CB \sin(45 + \tfrac{1}{4}\epsilon) - R . CB \cos(45 + \tfrac{1}{4}\epsilon) - Q . CB \cos(45 + \tfrac{1}{4}\epsilon) = 0,$$

whence $\quad T' = (R + Q) \cot(45 + \tfrac{1}{4}\epsilon) = \{(\tfrac{1}{2}n + p)\,\epsilon^2 + n\epsilon - p\epsilon^2\}\,(1 - \tfrac{1}{2}\epsilon)$

$= n\epsilon$ to the second order.

On a unit cube of the material in the sheared condition then, we have, as in Fig. 4,

Tangential stresses along AB and CD each	$n\epsilon$.
Tangential stresses along AD and BC each.........	$n\epsilon$.
Pressures perpendicular to AB and CD each	$(\frac{1}{2}n + p)\,\epsilon^2$.
Pressures perpendicular to AD and BC each	$(-\frac{1}{2}n + p)\,\epsilon^2$.
And pressures perpendicular to the plane of the figure each $q\epsilon^2$, or, in more convenient form...	$(q - p)\,\epsilon^2 + p\epsilon^2$.

The Strains in a Finite Shear-Stress consisting of Tangential Stress T, T' only.

If an element is subjected to the system of stresses just investigated, when we put on to it a system of tensions equal and opposite to the second order pressures we have just found, we leave only the tangential stresses $T = T' - n\epsilon$. The strains due to these tensions must be superposed on the shear ϵ, and we shall then have the strains due to the tangential stresses only.

We have then to examine the strains due to tensions

$(\frac{1}{2}n + p)\,\epsilon^2$ on AB and CD (Fig. 4).

$(-\frac{1}{2}n + p)\,\epsilon^2$ on AD and BC.

$(q - p)\,\epsilon^2 + p\epsilon^2$ perpendicular to the plane of the figure.

Through the tension $p\epsilon^2$ on every face we get an extension in all directions $p\epsilon^2/3K$, where K is the bulk-modulus.

The tensions $\frac{1}{2}n\epsilon^2$ on AB and CD and the pressures $\frac{1}{2}n\epsilon^2$ on AD and BC constitute a shear-stress giving an elongation parallel to BC of $\frac{1}{4}n\epsilon^2/n = \frac{1}{4}\epsilon^2$, and a contraction parallel to AB also $\frac{1}{4}\epsilon^2$.

The tensions $(q - p)\,\epsilon^2$ perpendicular to the plane of the figure give an elongation perpendicular to that plane $\dfrac{1}{Y}(q - p)\,\epsilon^2$, and contractions at right angles, viz., along AB and AD, $\dfrac{\sigma}{Y}(q - p)\,\epsilon^2$, where Y is Young's modulus and σ is Poisson's ratio.

Collecting the results, we have secondary strains accompanying the shear ϵ as follows:

An elongation parallel to BC $\quad = \dfrac{1}{4}\epsilon^2 + \dfrac{p\epsilon^2}{3K} - \dfrac{\sigma}{Y}(q - p)\,\epsilon^2.$

" " AB $\quad = -\dfrac{1}{4}\epsilon^2 + \dfrac{p\epsilon^2}{3K} - \dfrac{\sigma}{Y}(q - p)\,\epsilon^2.$

" perpendicular to the plane $-\dfrac{p\epsilon^2}{3K} + \dfrac{1}{Y}(q - p)\,\epsilon^2.$

As in the experiments, described above, Y and σ were determined directly, it will be convenient to replace K from the equation $\frac{1}{3K} = \frac{1-2\sigma}{Y}$, and the secondary strains become

$$\left(\frac{1}{4} + \frac{1-\sigma}{Y} p - \frac{\sigma}{Y} q\right)\epsilon^2. \quad \left(-\frac{1}{4} + \frac{1-\sigma}{Y} p - \frac{\sigma}{Y} q\right)\epsilon^2, \quad \left(-\frac{2\sigma}{Y} p + \frac{1}{Y} q\right)\epsilon^2.$$

Equations Representing the Changes in the Dimensions of a Wire Subject to a Torque.

I am indebted to Sir Joseph Larmor for his kindness in indicating how the following equations should be formed and solved. Let us assume that we put on to a wire of length l and radius a a pure shear-stress proportional to the distance r from the axis, and twisting the length l through angle θ. Then in addition to the shear $\epsilon = r\theta/l$, this stress would produce in an element unconstrained by neighbouring material what we may term 'free strains' with the values just found, which we may write as αr^2 radial, βr^2 transverse to the radius, and γr^2 longitudinal; where

$$\left.\begin{aligned} \alpha &= \left(-\frac{2\sigma}{Y} p + \frac{1}{Y} q\right)\frac{\theta^2}{l^2} \\ \beta &= \left(-\frac{1}{4} + \frac{1-\sigma}{Y} p - \frac{\sigma}{Y} q\right)\frac{\theta^2}{l^2} \\ \gamma &= \left(+\frac{1}{4} + \frac{1-\sigma}{Y} p - \frac{\sigma}{Y} q\right)\frac{\theta^2}{l^2} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

If u is the actual radial displacement, and if w is the actual longitudinal displacement, the strains in addition to the shear ϵ are, in cylindrical co-ordinates,

$$du/dr, \qquad u/r, \quad \text{and} \quad dw/dz.$$

The differences between these actual strains and the 'free strains,' viz.,

$$e = \frac{du}{dr} - \alpha r^2, \quad f = \frac{u}{r} - \beta r^2, \quad g = \frac{dw}{dz} - \gamma r^2, \quad \ldots\ldots\ldots\ldots(2)$$

imply 'secondary stresses' in the wire due to adjustment of strain in neighbouring elements. Let these be denoted by R, Θ, W.

To find R, Θ, and W, we treat e, f, g as if they were strains in an independent system. Putting $\Delta = e + f + g$, the equations are

$$R = \lambda\Delta + 2\mu e, \qquad \Theta = \lambda\Delta + 2\mu f, \qquad W = \lambda\Delta + 2\mu g, \quad \ldots\ldots\ldots(3)$$

where $\quad \lambda = K - \frac{2}{3}n = \frac{\sigma Y}{(1+\sigma)(1-2\sigma)} \quad$ and $\quad \mu = n = \frac{Y}{2(1+\sigma)}.$

The forces R, Θ, and W must form a system in equilibrium, there being no external forces to balance. Considering the equilibrium of the element $ABCD$, Fig. 5,

$$d\,(Rr\delta\theta) = \Theta\,\delta\theta\,dr, \quad \text{whence} \quad r\frac{dR}{dr} + R = \Theta. \quad \ldots(4)$$

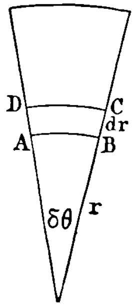


Fig. 5.

We obtain another equation by assuming that the wire is so gripped at each end that sections perpendicular to the axis remain perpendicular to the axis after twisting. Indeed, we have already assumed this in omitting equations for shear-stress in (3). Hence w is independent of r and dw/dz is constant over a section for a given wire with a given twist. Let us put

$$dw/dz = h.$$

Further, the load is constant, so that

$$\int_0^a Wrdr = 0. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(5)$$

Substituting in (4) from (3) we obtain

$$r\frac{d^2u}{dr^2} + \frac{du}{dr} - \frac{u}{r} = \frac{2\lambda\,(\alpha+\beta+\gamma) + 6\mu\alpha - 2\mu\beta}{\lambda + 2\mu}\,r^2. \quad \ldots\ldots\ldots(6)$$

By putting $u/r = v$, we easily find the solution

$$u = Ar^3 + Br + Cr^{-1}, \quad \ldots\ldots\ldots\ldots\ldots\ldots(7)$$

where
$$A = \frac{2\lambda\,(\alpha+\beta+\gamma) + 6\mu\alpha - 2\mu\beta}{8\,(\lambda + 2\mu)},$$

and B and C are arbitrary constants to be determined by the boundary-conditions.

If the wire is unstrained in all parts before twisting, the solution applies with the same constants for all parts.

In order that $u = 0$ when $r = 0$, we must have $C = 0$, so that

$$u = Ar^3 + Br. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(8)$$

When $r = a$, $R = 0$.

Substituting from (8) in the value of R in (3), and putting $R = 0$ when $r = a$, we get

$$2\,(\lambda+\mu)\,B + \lambda h = \{\lambda\,(\alpha+\beta+\gamma) + 2\mu\alpha - (4\lambda + 6\mu)\,A\}\,a^2. \quad \ldots(9)$$

From equation (5) we obtain another relation between B and h, when we substitute for u from (8) in W from (3) and integrate from $r = 0$ to $r = a$, viz.,

$$\lambda B + (\tfrac{1}{2}\lambda + \mu)\,h = \{\tfrac{1}{4}\lambda\,(\alpha+\beta+\gamma) + \tfrac{1}{2}\mu\gamma - \lambda A\}\,a^2, \quad \ldots\ldots\ldots(10)$$

and from (9) and (10) we can find B and h.

Since A is a linear function of α, β, and γ, and each of these is proportional to θ^2, h and B are proportional to θ^2. Substituting for B in (8), u is also proportional to θ^2. The theory, then, gives the parabolic law for the twisting of a wire initially unstrained both for lengthening and for side-contraction. It also gives the lengthening and side-contraction w/l and u/a for different wires of the same material as proportional to a^2.

So far the theory does not, of course, give any account of the fact that the wires examined are always unsymmetrical, that the effects always date from a point c, on the counter-clockwise side in the wires examined, c being different for w and u. This want of symmetry implies initial internal strain, probably, in reality, very complicated. Let us examine a simple case in which there is a core, radius a, twisted initially against a sheath, outer radius b, and let the opposing twists be respectively θ_c and θ_s. When we put a twist θ from outside on to the core as a whole the core is twisted through $\theta + \theta_c$, and the sheath through $\theta - \theta_s$. For the core and sheath respectively we have

$$u_c = Ar^3 + Br,$$
$$u_s = A'r^3 + B'r + C'r^{-1},$$

where A is a linear function of α, β, γ, and therefore proportional to $(\theta + \theta_c)^2$ and A' is the same function of α', β', γ', say, and therefore proportional to $(\theta - \theta_s)^2$.

To find the constants we have

$$\begin{aligned} u_c &= u_s \quad \text{when} \quad r = a, \\ R_c &= R_s \quad\quad ,, \quad\quad ,, \\ R_s &= 0 \quad\quad ,, \quad r = b, \end{aligned}$$

and
$$\int_0^a W_c r\,dr + \int_a^b W_s r\,dr = 0.$$

These give us four equations to find B, B', C', h of the form (it appears needless to give the detailed work)

$$B = P_1(\theta + \theta_c)^2 + Q_1(\theta - \theta_s)^2, \qquad C' = P_3(\theta + \theta_c)^2 + Q_3(\theta - \theta_s)^2,$$
$$B' = P_2(\theta + \theta_c)^2 + Q_2(\theta - \theta_s)^2, \qquad h = P_4(\theta + \theta_c)^2 + Q_4(\theta - \theta_s)^2;$$

and, substituting for A', B', C' in u_s, and putting $r = b$, we get

$$u_b = P_5(\theta + \theta_c)^2 + Q_5(\theta - \theta_s)^2.$$

Both h and u_b are of the form

$$D\theta^2 + E\theta + F,$$

where D does not contain θ_c or θ_s. As the parabolas depend only on D, E and F merely giving the position of the vertex, θ_c and θ_s only affect that position.

To find that position we may put $dh/d\theta = 0$ for the one, $du/d\theta = 0$ for the other, and since h and u are different functions of θ_c and θ_s the vertices will be at different points for the two quantities h and u.

Taking this simple case as a guide we shall assume that internal strain only affects the position and not the size of the parabola representing the change of linear dimensions on twisting.

Hence if we could obtain a wire without internal strain we should have

$$Ln^2 = w,$$

where L has the value found in the experiments on the actual, initially strained wire, and we may regard the values u/a and $w\ l$ for one turn as the values for a wire initially without internal strain.

The Values of p and q in the Secondary Stresses.

We are now able to find the values of p and q. For the known values of Y and σ enable us to find α, β and γ in equations (1) in terms of p and q for a known twist, which we shall take as one turn, or as 2π in length $l = 160{\cdot}5$ cm. We also know λ and μ, since

$$\lambda = \sigma Y/(1+\sigma)(1-2\sigma) \quad \text{and} \quad \mu \quad n \quad Y\ 2(1+\sigma).$$

Substituting for λ, μ, α, β, and γ we can determine A in terms of p and q. Then from equations (9) and (10) we can find B and h in terms of p and q. Equating $Aa^2 + B$ to the observed value of $u\ a$ (which is negative), and h to the observed value of w/l, we have two linear equations in p and q.

The arithmetic is straightforward, though very lengthy, and may be omitted. I have used a slide rule in the calculations.

Using the values of the elastic constants Y and σ from Table II, and the values of u/a and w/l, on p. 411, I find for wire I

$$p = 1{\cdot}67 \times 10^{12}, \qquad q - -0{\cdot}70 \times 10^{12},$$

so that the force perpendicular to the plane of the figure in Fig. 4 is a tension and not a pressure.

The Pressure in the Direction of Propagation in Distortional Waves and the Longitudinal Waves Produced by the Pressure.

If we had a train of waves purely distortional, that is, a train in which the strain could be represented by a pure shear ϵ, there would be a pressure in the direction of propagation $(\frac{1}{2}n + p)\,\epsilon^2$. But as ϵ varies from point to point in the train, the pressure due to the shear-strain varies, and there must be longitudinal disturbance, longitudinal waves, accompanying the distortional waves. The longitudinal strain implies that the material yields under the pressure, and the pressure will, in general, have a different value from that in a pure shear.

Let us represent the distortional train by

$$\epsilon = \eta \sin \frac{2\pi}{\lambda} (x - vt),$$

where $v^2 = n/\rho$ and η is the amplitude of the shear.

If ξ is the longitudinal displacement at the point where the shear is ϵ, $d\xi/dx$ is the elongation of the element about the point.

Now if we shear a cube, and remove the pressure $(\frac{1}{2}n + p)\,\epsilon^2$, the cube elongates in that direction, and if the dimensions in the two directions at right angles are maintained the same, the removal of the pressure produces elongation

$$\nu^{-1} (\tfrac{1}{2}n + p)\,\epsilon^2, \quad \text{where} \quad \nu = \lambda + 2\mu = K + \tfrac{4}{3}n.$$

This we may term the 'free elongation' in the direction of propagation on the supposition that there is no change of length at right angles to it.

The pressure due to the shear falls from its full value $(\frac{1}{2}n + p)\,\epsilon^2$ to 0 while the elongation increases from 0 to its full value $\nu^{-1} (\frac{1}{2}n + p)\,\epsilon^2$. When the elongation is $d\xi/dx$ the pressure remaining is

$$P = \frac{\nu^{-1} (\frac{1}{2}n + p)\,\epsilon^2 - d\xi/dx}{\nu^{-1} (\frac{1}{2}n + p)\,\epsilon^2} (\tfrac{1}{2}n + p)\,\epsilon^2 = (\tfrac{1}{2}n + p)\,\epsilon^2 - \nu\, d\xi/dx$$

$$= (\tfrac{1}{2}n + p)\,\eta^2 \sin^2 \frac{2\pi}{\lambda} (x - vt) - \nu\, d\xi/dx.$$

The equation of motion for the longitudinal waves is

$$\rho \frac{d^2\xi}{dt^2} = -\frac{dP}{dx} = -(\tfrac{1}{2}n + p)\,\eta^2 \frac{2\pi}{\lambda} \sin \frac{4\pi}{\lambda} (x - vt) + \nu\, d^2\xi/dx^2,$$

an equation similar in form to that for the longitudinal waves which I have attempted to show must accompany light-waves*.

If we put
$$\xi = A \sin \frac{4\pi}{\lambda} (x - vt - a),$$

and substitute in the above equation, we find on putting $x = 0, t = 0$, that $a = 0$, and

$$A = \frac{(\frac{1}{2}n + p)\,\eta^2\lambda}{8\pi\,(\rho v^2 - \nu)},$$

or if v' is the velocity of free longitudinal waves, since $\rho v'^2 = \nu$ and $v'^2 > v^2$,

$$A = -\frac{(\frac{1}{2}n + p)\,\eta^2\lambda}{8\pi\rho\,(v'^2 - v^2)}.$$

If we substitute for $d\xi/dx$ in P, we get

$$P = \tfrac{1}{2} (\tfrac{1}{2}n + p)\,\eta^2 \left\{1 - \cos \frac{4\pi}{\lambda} (x - vt)\right\} - \nu A \frac{4\pi}{\lambda} \cos \frac{4\pi}{\lambda} (x - vt).$$

* *Roy. Soc. Proc.* A, vol. 85, p. 474. [*Collected Papers*, Art. 29.]

We may regard this as made up by a steady pressure $\frac{1}{2}(\frac{1}{2}n + p)\eta^2$ and a purely periodic pressure, of which the average is zero.

If E is the energy per cubic centimetre at any point in the distortional waves, it is half kinetic energy, half strain-energy. The latter is $\frac{1}{2}n\epsilon^2$, so that the total is $n\epsilon^2$ or

$$E = n\eta^2 \sin^2 \frac{2\pi}{\lambda}(x - vt) = \frac{1}{2}n\eta^2 \left\{1 - \cos\frac{4\pi}{\lambda}(x - vt)\right\}.$$

Then the average value is

$$\bar{E} = \frac{1}{2}n\eta^2.$$

If we denote the average pressure by P,

$$\bar{P} = \frac{\frac{1}{2}n + p}{n}\bar{E}.$$

If we use the values of n and p found for wire I, we find

$$\bar{P} = 2{\cdot}50\bar{E}.$$

If we put the energy per cubic centimetre in the longitudinal waves as $\frac{\rho}{2}\left(\frac{d\xi}{dt}\right)^2 + \frac{\nu}{2}\left(\frac{d\xi}{dx}\right)^2$, we find that

$$\frac{\text{Average energy in longitudinal waves}}{\text{Average energy in distortional waves}} = \frac{1}{8\rho}\frac{v'^2 v^2}{(v'^2 - v^2)^2}\frac{(\frac{1}{2}n + p)^2}{n}\eta^2,$$

so that the ratio is proportional to η^2 and therefore in any actual waves it is very small.

The pressures at right angles to the line of propagation will not produce any disturbance in a wave-front where η is constant. Round the edges of the wave-front, however, where η is diminishing as we go outwards, they may have effects, and it appears likely that they may give rise to disturbances propagated sideways.

I have much pleasure in recording my hearty thanks to Mr. G. O. Harrison, mechanic in the laboratory workshop, for his great help in planning the apparatus used in the experiments described in this paper, for his skill in constructing it, and for his assistance in making the observations.

31.

THE CHANGES IN THE LENGTH AND VOLUME OF AN INDIA-RUBBER CORD WHEN TWISTED.

[*The India-Rubber Journal*, October 4, 1913, p. 6.]

In some investigations on the way in which pressure might be produced by transverse waves in a solid, analogous to the minute pressure produced by light-waves, the author was led to expect that a wire with a constant load on it would lengthen, when twisted, by an amount proportional to the square of the twist, and he gave at the Winnipeg meeting of the British Association an account of experiments which fully verified the expectation. According to the theory used, there should also be accompanying the lengthening a diminution in the radius, and a description has been published in the *Proceedings of the Royal Society** of experiments which show that the diminution exists and follows the same law. The changes are very minute, of the order of a millionth in the length and in the diameter when a steel wire 160 cm. long and 1 mm. diameter is twisted through one turn. The volume is also slightly increased. If instead of allowing the length to increase it had been kept constant by reducing the load, there would with steel have been a slight outstanding increase in the volume.

The author thought it might be interesting to look for similar effects in india-rubber. To investigate the lengthening, he used a rubber cord 118 cm. long and 1·2 cm. diameter, of which the upper end was attached to a vertical axis which could be rotated in a bearing. The lower end was attached to a horizontal cross-piece between four stops, which allowed the cross-piece, and therefore the end of the cord, to rise or fall, but prevented rotation. To the cross-piece there was attached the ordinary wheel-barometer device for magnifying up and down motion. There was a lengthening on twisting, somewhat irregular, not proportional to the square of the twist, but increasing rather less rapidly. Two turns of twist gave an average lengthening of 0·088 cm., or about 750 in a million, vastly greater than the lengthening of the steel wire with a similar twist. But, as with steel wire, the lengthening is rather more than proportional to the square of the diameter. A rubber

* [*Collected Papers*, Art. 30.]

cord 1 mm. in diameter, and with the length and twist of the steel, would probably have increased in length by an amount of the same order as that observed with steel.

To find whether there was a change in diameter, a cord of the same length and diameter as that used for the lengthening was enclosed in a vertical glass tube with brass ends, the lower end of the cord being attached to the lower brass end, and the upper end to a vertical axis coming into the tube through as close fitting a bearing in the upper brass end as could be made. This axis could be rotated, and so any twist could be put on the cord while it remained of constant length. The tube was filled with water, and as it was provided at one side with a capillary tube which issued through a hole near the top and rose above the upper end, any change in the volume of the rubber on twisting would have been indicated by a rise or fall of the water surface in the capillary, and this was viewed by a measuring microscope. When two turns were put on the rubber, small changes of volume were observed, now one way, now the other, probably due to errors of experiment. But the changes were very small, and the mean change so minute that it appears safe to say that the real change in volume was not so much as one in two millions. It was therefore, if it existed at all, of an order not greater than for the steel wire above described. If the cord had been only 1 mm. in diameter like the steel, and had been of the same length, and had been subjected to the same twist, the change in volume would have been vastly less than in the case of steel.

[This appears to be the only published notice of an account of this work which was given at the meeting of the British Association at Birmingham in 1913. ED.]

APPENDIX BY SIR J. LARMOR ON THE MOMENTUM OF RADIATION.

[The following extract from a lecture 'On the Dynamics of Radiation' by Sir Joseph Larmor, read before the Fifth International Congress of Mathematicians at Cambridge in August 1912, is inserted here, after consultation with the Author (whose permission was requested), in further elucidation and illustration, chiefly from the side of the electric theory, of Poynting's experiments resting on the momentum of radiation. ED.]

General theory of pressure exerted by waves.

If a perfectly reflecting structure has the property of being able to advance through an elastic medium, the seat of free undulations, without producing disturbance of structure in that medium, then it follows from the principle of energy alone that these waves must exert forces against such a reflector, constituting a pressure equal in intensity at each point to the energy of the waves per unit volume. Cf. p. 432, *infra*. The only hypothesis, required in order to justify this general result, is that the velocity of the undulations in the medium must be independent of their wave-length; viz., the medium is to be non-dispersive, as is the free aether of space.

This proposition, being derived solely from consideration of conservation of the energy, must hold good, whatever be the character of the mechanism of propagation that is concerned in the waves. But the elucidation of the nature of the pressure of the waves, of its mode of operation, is of course concerned with the constitution of the medium. The way to enlarge ideas on such matters is by study of special cases: and the simplest cases will be the most instructive.

Let us consider then transverse undulations travelling on a cord of linear density ρ_0, which is stretched to tension T_0. Waves of all lengths will travel with the same velocity, namely $c = (T_0/\rho_0)^{\frac{1}{2}}$, so that the condition of absence of dispersion is satisfied. A solitary wave of limited length, in its transmission along the cord, deflects each straight portion of it in succession into a curved arc. This process implies increase in length, and therefore increased tension, at first locally. But we adhere for the present to the simplest case, where the cord is inextensible or rather the elastic modulus of extension is indefinitely great. The very beginnings of a local disturbance of tension will then be equalised along the cord with speed practically infinite; and we may therefore take it that at each instant the tension stands adjusted to be the same (T_0) all along it. The pressure or pull of the undulations at any point is concerned only with the component of this tension in the direction of the cord; this is

$$T_0\left(1 + \frac{d\eta^2}{dx^2}\right)^{-\frac{1}{2}},$$

where η is the transverse displacement of the part of the cord at distance x

measured along it; thus, up to the second order of approximation, the pull of the cord is

$$T_0 - \tfrac{1}{2}T_0\left(\frac{d\eta}{dx}\right)^2.$$

The tension of the cord therefore gives rise statically to an undulation pressure

$$\tfrac{1}{2}T_0\left(\frac{d\eta}{dx}\right)^2, \quad \text{or } \tfrac{1}{2}T_0C^{-2}\left(\frac{d\eta}{dt}\right)^2, \quad \text{or } \tfrac{1}{2}\rho_0\left(\frac{d\eta}{dt}\right)^2.$$

The first of these three equivalent expressions can be interpreted as the potential energy per unit length arising from the gathering up of the extra length in the curved arc of the cord, against the operation of the tension T_0; the last of them represents the kinetic energy per unit length of the undulations. Thus there is a pressure in the wave, arising from this statical cause, which is at each point equal to half its total energy per unit length.

There is the other half of the total pressure still to be accounted for. That part has a very different origin. As the tension is instantaneously adjusted to the same value all along, because the cord is taken to be inextensible, there must be extra mass gathered up into the curved segment which travels along it as the undulation. The mass in this arc is

$$\int \rho_0\left(1 + \frac{d\eta^2}{dx^2}\right)^{\frac{1}{2}} dx,$$

or to the second order is approximately

$$\rho_0 l + \int \tfrac{1}{2}\rho_0\left(\frac{d\eta}{dx}\right)^2 dx.$$

In the element δx there is extra mass of amount

$$\tfrac{1}{2}\rho_0\left(\frac{d\eta}{dx}\right)^2 \delta x,$$

which is carried along with the velocity C of the undulatory propagation. This implies momentum associated with the undulation, and of amount at each point equal to $\tfrac{1}{2}\rho_0 C\left(\frac{d\eta}{dx}\right)^2$ per unit length. Another portion of the undulation pressure is here revealed, equal to the rate at which the momentum is transmitted past a given point of the cord; this part is represented by $\tfrac{1}{2}\rho_0 C^2\left(\frac{d\eta}{dx}\right)^2$ or $\tfrac{1}{2}\rho_0\left(\frac{d\eta}{dt}\right)^2$, and so is equal to the component previously determined.

In our case of undulations travelling on a stretched cord, the pressure exerted by the waves arises therefore as to one half from transmitted intrinsic stress and as to the other half from transmitted momentum.

The kinetic energy of the cord can be considered either to be energy belonging to the transverse vibration, viz., $\int \tfrac{1}{2}\rho\left(\frac{d\eta}{dt}\right)^2 ds$, or to be the energy of the convected excess of mass moving with the velocity of propagation c*, viz., $\int \frac{1}{2}\rho \left(\frac{d\eta}{dx}\right)^2 c^2 dx$; for these quantities are equal by virtue of the condition of steady propagation $\frac{d\eta}{dt} = c\frac{d\eta}{dx}$.

On the other hand the momentum that propagates the waves is transverse, of amount $\rho \frac{d\eta}{dt}$ per unit length; it is the rate of change of this momentum that appears in the equation of propagation

$$\frac{d}{dt}\left(\rho \frac{d\eta}{dt}\right) = \frac{d}{dx}\left(T\frac{d\eta}{dx}\right).$$

But the longitudinal momentum with which we have been here specially concerned is $\frac{1}{2}\rho \left(\frac{d\eta}{dx}\right)^2 c$ per unit length, which is $\frac{1}{2}\frac{d\eta}{dx}\cdot\rho\frac{d\eta}{dt}$. Its ratio to the transverse momentum is very small, being $\frac{1}{2}\frac{d\eta}{dx}$; it is a second-order phenomenon and is not essential to the propagation of the waves. It is in fact a special feature, and there are types of wave-motion in which it does not occur. The criterion for its presence is that the medium must be such that the reflector on which the pressure is exerted can advance through it, sweeping the radiation along in front of it, but not disturbing the structure; possibly intrinsic strain, typified by the tension of the cord, may be an essential feature in the structure of such a medium.

If we derive the dynamical equation of propagation along the cord from the Principle of Action $\delta \int (T - W)\, dt = 0$, where

$$T = \int \tfrac{1}{2}\rho \left(\frac{d\eta}{dt}\right)^2 ds \text{ and } W = \int \tfrac{1}{2}T_0 \left(\frac{d\eta}{dx}\right)^2 dx,$$

the existence of the pressure of the undulations escapes our analysis. A corresponding remark applies to the deduction of the equations of the electrodynamic field from the Principle of Action†. In that mode of analysis the forces constituting the pressure of radiation are not in evidence throughout the medium; they are revealed only at the place where the field of the waves affects the electrons belonging to the reflector. Problems connected with the Faraday-Maxwell stress lie deeper; they involve the structure of the medium to a degree which the propagation of disturbance by radiation does not by itself give us means to determine.

We therefore proceed to look into that problem more closely. We now postulate Maxwell's statical stress system; also Maxwell's magnetic stress system, which is, presumably, to be taken as of the nature of a kinetic reaction. But when we assert the existence of these stresses. there remain over uncompensated terms in the mechanical forcive on the electrons which

* [This specification is fictitious; indeed a factor $\frac{1}{2}$ has been dropped in its expression just following. There is however *actual* energy of longitudinal motion; as it belongs to the whole mass of the cord, which moves together, it is very small in amount, its ratio to the energy of transverse vibration being $\frac{1}{4}(d\eta/dx)^2$. J. L.]

† Cf. Larmor, *Trans. Camb. Phil. Soc.* vol. 18 (1900), p. 318; or *Aether and Matter*, Chapter vi.

may be interpreted as due to a distribution of momentum in the medium*. The pressure of a train of radiation is, on this hypothetical synthesis of stress and momentum, due entirely (p. 431) to the advancing momentum that is absorbed by the surface pressed, for here also the momentum travels with the waves. This is in contrast with the case of the cord analysed above, in which only half of the pressure is due to that momentum.

The pressure of radiation against a material body, of amount given by the law specified by Maxwell for free space, is demonstrably included in the Maxwellian scheme of electrodynamics, when that scheme is expanded so as to recognise the electrons with their fields of force as the link of communication between aether and matter. But the illustration of the stretched cord may be held to indicate that it is not yet secure to travel further along with Maxwell, and accept as realities the Faraday-Maxwell stress in the electric field, and the momentum which necessarily accompanies it; it shows that other dynamical possibilities of explanation are not yet excluded. And, viewing the subject from the other side, we recognise how important have been the experimental verifications of the law of pressure of radiation which we owe to Lebedew, too early lost to science, to Nichols and Hull, and to Poynting and Barlow. The law of radiation-pressure in free space is not a necessary one for all types of wave-motion; on the other hand if it had not been verified in fact, the theory of electrons could not have stood without modification.

The pressure of radiation, according to Maxwell's law, enters fundamentally in the Bartoli-Boltzmann deduction of the fourth-power law of connection between total radiation in an enclosure and temperature. Thus in this domain also. when we pass beyond the generalities of thermodynamics, we may expect to find that the laws of distribution of natural radiant energy depend on structure which is deeper seated than anything expressed in the Maxwellian equations of propagation. The other definitely secure relation in this field, the displacement-theorem of Wien, involves nothing additional as regards structure, except the principle that operations of compression of a field of natural radiation in free space are reversible. The most pressing present problem of mathematical physics is to ascertain whether we can evade this further investigation into aethereal structure, for purposes of determination of average distribution of radiant energy, by help of the Boltzmann-Planck expansion of thermodynamic principles, which proceeds by comparison of the probabilities of the various distributions of energy that are formally conceivable among the parts of the material system which is its receptacle.

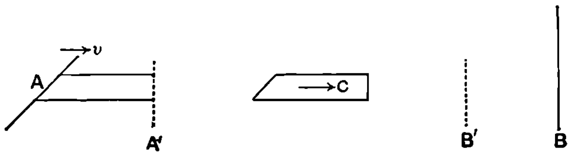
Momentum intrinsically associated with Radiation.

We will now follow up, after Poynting†, the hypothesis thus implied in modern statements of the Maxwellian formula for electric stress, namely that the pressure of radiation arises wholly from momentum carried along by the waves. Consider an isolated beam of definite length emitted obliquely from a definite area of surface A and absorbed completely by another area B. The

* For the extension to the most general case of material media cf. *Phil. Trans.* vol. 190 (1897). p 253

† Cf. *Phil. Trans.* vol. 202, A (1903). [*Collected Papers*, Art. 20.]

automatic arrangements that are necessary to ensure this operation are easily specified, and need not detain us. In fact by drawing aside an impervious screen from A we can fill a chamber AA' with radiation; and then closing A and opening A', it can emerge and travel along to B, where it can be absorbed without other disturbance, by aid of a pair of screens B and B' in like manner. Let the emitting surface A be travelling in any direction while the absorber B is at rest. What is emitted by A is wholly gained by B, for the surrounding aether is quiescent both before and after the operation. Also, the system is not subject to external influences; therefore its total momentum must be conserved, what is lost by A being transferred ultimately to B, but by the special hypothesis now under consideration, existing meantime as momentum in the beam of radiation as it travels across. If v be the component of the velocity of A in the direction of the beam, the duration of emission of the beam from A is $(1 - v/C)^{-1}$ times the duration of its absorption by the fixed absorber B. Hence the intensity of pressure of a beam of issuing radiation on the moving radiator must be affected by a factor $(1 - v/C)$ multiplying its density of energy; for pressure multiplied by time is the momentum which is transferred unchanged by the beam to the absorber for which v is null. We can verify readily that the pressure of a beam against a moving absorber involves the same factor $(1 - v/C)$. If the

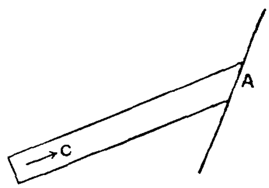


emitter were advancing with the velocity of light this factor would make the pressure vanish, because the emitter would keep permanently in touch with the beam: if the absorber were receding with the velocity of light there would be no pressure on it, because it would just keep ahead of the beam.

There seems to be no manner other than these two, by altered intrinsic stress or by convected momentum, in which a beam of limited length can exert pressure while it remains in contact with the obstacle and no longer. In the illustration of the stretched cord the intrinsic stress is transmitted and adjusted by tensional waves which travel with velocity assumed to be practically infinite. If we look closer into the mode of this adjustment of tension, it proves to be by the transmission of longitudinal momentum; though in order that the pressure may keep in step, the momentum must travel with a much greater velocity, proper to tensional waves. In fact longitudinal stress cannot be altered except by fulfilling itself through the transfer of momentum, and it is merely a question of what speeds of transference come in o operation.

In the general problem of aethereal propagation, the analogy of the cord suggests that we must be careful to avoid undue restriction of ideas, so as, for example, not to exclude the operation, in a way similar to this adjustment of tension by longitudinal propagation, of the immense but unknown speed of propagation of gravitation. We shall find presently that the phenomena of absorption lead to another complication.

So long, however, as we hold to the theory of Maxwellian electric stress with associated momentum, there can be no doubt as to the validity of Poynting's modification of the pressure formula for a moving reflector, from which he has derived such interesting consequences in cosmical astronomy. To confirm this, we have only to contemplate a beam of radiation of finite length l advancing upon an obstacle A in which it is absorbed. The rear of it moves on with velocity C; hence if the body A is in motion with velocity whose component along the beam is v, the beam will be absorbed or passed on, at any rate removed, in a time $l/(C-v)$. But by electron theory the beam possesses a distribution of at any rate *quasi*-momentum identical with the distribution of its energy, and this has disappeared or has passed on in this time. There must therefore be a thrust on the obstructing body, directed along the beam and equal to $\epsilon\,(1-v/C)$, where ϵ is the energy of the beam per unit length which is also the distribution of the *quasi*-momentum along the free beam.



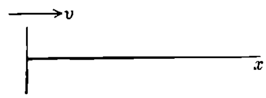
The back pressure on a radiating body travelling through free space, which is exerted by a given stream of radiation, is by this formula smaller on its front than on its rear; so that if its radiation were unaffected by its motion, the body would be subject to acceleration at the expense of its internal thermal energy. This of course could not be the actual case.

The modifying feature is that the intensity of radiation, which corresponds to a given temperature, is greater in front than in rear. The temperature determines the amplitude and velocity of the ionic motions in the radiator, which are the same whether it be at rest or in uniform motion: thus it determines the amplitude of the oscillation in the waves of aethereal radiation that are excited by them and travel out from them. Of this oscillation the intensity of the magnetic field represents the velocity. If the radiator is advancing with velocity v in a direction inclined at an angle θ to an emitted ray, the wave-length in free aether is shortened in the ratio $1-\frac{v}{C}\cos\theta$; thus the period of the radiation is shortened in the same ratio; thus the velocity of vibration, which represents the magnetic field, is altered in the inverse ratio, and the energy per unit volume in the square of that ratio, viz., that energy is now $\epsilon\left(1-\frac{v}{C}\cos\theta\right)^{-2}$; and the back pressure it exerts involves a further factor $1-\frac{v}{C}\cos\theta$ owing to the convection; so that that pressure is $\epsilon\left(1-\frac{v}{C}\cos\theta\right)^{1}$, where ϵ is the energy per unit volume of the natural radiation emitted from the body when at rest. The pressural reaction on the source is in fact E'/C, where E' is the actual energy emitted in the ray per unit time.

Limitation of the analogy of a stretched cord.

In the case of the inextensible stretched cord, the extra length due to the curved arc in the undulation is proportional to the energy of the motion. The loss of energy by absorption would imply slackening of the tension; and the propositions as to pressure of the waves, including Poynting's modification for a moving source, would not hold good unless there were some device at the fixed ends of the cord for restoring the tension. The hypothesis of convected momentum would imply something of the same kind in electron structure.

It is therefore worth while to verify directly that the modified formula for pressure against a moving total reflector holds good in the case of the cord, when there is no absorption so that the reflection is total. This analysis will also contain the proof of the generalisation of the formula for radiant pressure that was enunciated on p. 426 *supra**.



Let the wave-train advancing to the reflector and the reflected wave-train be represented respectively by

$$\dot{\eta}_1 = A_1 \cos m_1 (x + ct),$$
$$\dot{\eta}_2 = A_2 \cos m_2 (x - ct).$$

At the reflector, where $x = vt$, we must have

$$\int \dot{\eta}_1 dt = \int \dot{\eta}_2 dt;$$

this involves two conditions,

$$\frac{A_1}{m_1} = \frac{A_2}{m_2} \quad \text{and} \quad m_1 (c + v) - m_2 (c - v).$$

Now the energies *per unit length* in these two simple wave-trains are

$$\tfrac{1}{2}\rho A_1^2 \quad \text{and} \quad \tfrac{1}{2}\rho A_2^2;$$

thus the gain of energy *per unit time* due to the reflection is

$$\delta E = (c - v)\,\tfrac{1}{2}\rho A_2^2 - (c + v)\,\tfrac{1}{2}\rho A_1^2$$
$$= \tfrac{1}{2}\rho A_1^2 \left\{(c - v)\left(\frac{c + v}{c - v}\right)^2 - (c + v)\right\}$$
$$= \tfrac{1}{2}\rho A_1^2 \,.\, 2\,\frac{c + v}{c - v}\, v.$$

* See Larmor, *Brit. Assoc. Report*, 1900. [The statement that follows here is too brief, unless reference is made back to the original, especially as a *minus* sign has fallen out on the right of the third formula below. The reflector consists of a disc with a small hole in it through which the cord passes; this disc can move along the cord sweeping the waves in front of it while the cord and its tension remain continuous through the hole—the condition of reflection being thus $\eta_1 + \eta_2 = 0$ when $x = vt$. In like manner a material perfect reflector sweeps the radiation in front of it, but its molecular constitution is to be such that it allows the aether and its structure to penetrate across it unchanged. For a fuller statement, see *Encyclopaedia Britannica*, ed. 9 or 10, article 'Radiation.' J. L.]

This change of energy must arise as the work of a pressure P exerted by the moving reflector, namely it is Pv; hence

$$P = \tfrac{1}{2}\rho A_1^2 \,.\, 2\frac{c+v}{c-v}.$$

The total energy per unit length, incident and reflected, existing in front of the reflector is

$$E_1 + E_2 = \tfrac{1}{2}\rho A_1^2 + \tfrac{1}{2}\rho A_2^2$$

$$= \tfrac{1}{2}\rho A_1^2 \,.\, 2\frac{c^2 + v^2}{(c-v)^2}.$$

Hence finally
$$P = (E_1 + E_2)\frac{c^2 - v^2}{c^2 + v^2},$$

becoming equal to the total density of energy $E_1 + E_2$, in accordance with Maxwell's law, when v is small.

If we assume Poynting's modified formula for the pressure of a wave-train against a travelling obstacle, the value ought to be

$$P = E_1\left(1 + \frac{v}{c}\right) + E_2\left(1 - \frac{v}{c}\right);$$

and the truth of this is readily verified.

It may be remarked that, if the relation connecting strain with stress contained quadratic terms, pressural forces such as we are examining would arise in a simple wave-train*. But such a medium would be dispersive, so that a simple train of waves would not travel without change, in contrast to what we know of transmission by the aether of space.

The question is then suggested how far a cognate momentum can be regarded as arising from change of aethereal inertia produced by travelling electric strain. It will be represented by inertia attached to moving tubes of electric force. The conclusion is reached that such a scheme can be consistently constructed for any steady electric system convected with uniform speed; also that it holds for any field of pure radiation, that is any field in which the electric and magnetic forces are everywhere at right angles: but that in other cases it is not possible. On the other hand any *changing* electrodynamic field whatever is constituted by the superposition of pure radiations from all the electrons belonging to its source.

A discussion follows of the frictional resistance to the motion through space of a radiating body, whose mere existence, as is pointed out, had been predicted by Balfour Stewart as early as 1871. Estimates are made for bodies of various forms, including one for the sphere which verifies Poynting's formula in *Phil. Trans.* 1893 (p. 330 *supra*). The important applications to cosmical astronomy which Poynting has there developed do not seem to have yet received the attention they deserve.

Then it is recalled that if we assume the real existence of the Maxwell stress in the aether, suitably modified for modern ideas, as the source of all

* Cf. Poynting, *Roy. Soc. Proc.* vol. 86, A (1912), pp. 534–561, where the pressure exerted by torsional waves in an elastic medium, such as steel, is exhaustively investigated on both the experimental and the mathematical side. [*Collected Papers*, Art. 30]

mechanical interactions between electric systems, and we retain the ascertained mechanical electrodynamic forces as part of its effect, then another phenomenon is required to make up the complete result, and this can be represented as a distribution of momentum in the aether of density equal to the vector-product of aethereal displacement and magnetic induction. In the case of trains of waves, the latter agrees with Poynting's momentum of radiation. As regards the resultant momentum and forces for any self-contained system, the Maxwell stress is eliminated, and no hypothesis as to its reality is involved.

But for such a complete system, free from external disturbance, we require to compare this outstanding force, visualised as rate of change of some kind of latent momentum, when the system is convected with uniform velocity v, with what it would be for the same system at rest in the aether; for although the system remains the same the convection modifies the electrodynamic field around each electron which it contains, and thus may modify the effective electromagnetic mass of that electron as well as the distribution of latent momentum. When this comparison is made by aid of the classical correlation first employed by H. A. Lorentz, it turns out* that the forcive acting on the convected system exceeds that acting on the same system when stationary in the aether, by the effect of convection of latent momentum specified exactly as before, together with a force equal to $v \frac{d}{dt}\left(\frac{E}{c^2}\right)$, where E is the energy in the system. On the principle that force is expressed as $\frac{d}{dt}(mv)$ we can infer that an increase δE of the electrodynamic energy of a system increases the effective mass of the system by $\delta E\ c^2$. This additional result, as well as the momentum result, is necessitated beyond cavil by the ascertained laws of electrodynamics, which however are themselves established only when $(v/c)^2$ is negligible† : extension of its validity beyond that limit requires new postulates of 'relativity.' In astronomical applications such as Poynting's, the effect of any change of mass due to cooling is totally insignificant compared with the results which he derives from the latent momentum. J. L.

* [Larmor: 'On the Dynamics of Radiation,' *Fifth International Congress of Mathematicians*, Aug. 1912, vol. 1.]

† [Cf. Larmor, *Aether and Matter*, 1900.]

PART IV.

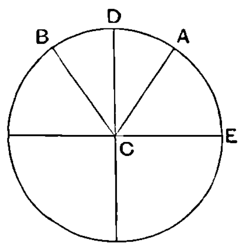
LIGHT.

32.

ON A SIMPLE FORM OF SACCHARIMETER.

[*Phil. Mag.* **10**, 1880, pp. 18–21.]

The general principle of the modification of the saccharimeter which I shall describe in this paper is well known, and has already been applied in the construction of several standard instruments, such as Jellett's and Laurent's. This principle consists in altering the pencil of rays proceeding from the polariser in such a way that, instead of the whole pencil having the same plane of polarisation, the planes of the two halves are slightly inclined to each other. The analyser is therefore not able to darken the whole field of view at once. In one position of the analyser the one half of the field is quite dark; in another position, slightly different, the other half is dark; while when the analyser is halfway between these two positions, the two halves of the field are equally illuminated. This will be seen from the accompanying figure.



Let CA be the trace of the plane of polarisation of the right half of the pencil, and CB that of the other half. Let CD bisect ACB. Then, if CE represent the plane of polarisation of the light which alone the analyser will allow to pass, when the analyser is turned so that CE is perpendicular to CA the right-hand side of the field is dark. When CE is perpendicular to CB the right-hand is partially illuminated (as CA has a component along CE), while the left-hand is dark. Halfway between these positions, when CE is perpendicular to CD, both sides appear equally illuminated. The analyser being turned round till this equality of illumination is obtained, its position is noted on the attached circle. When an active substance is now inserted in the path of the rays, the planes *CA*, *CB* are both rotated through the same angle, and the analyser has to be rotated through this angle to give the equal illumination once more. The circle again being read, the difference of readings gives the rotation due to the interposed substance.

In Jellett's saccharimeter the inclination of the planes of polarisation of the two halves of the field is obtained by interposing a prism of Iceland spar. This is formed by cutting a rhomb nearly parallel to its optic axis, reversing one of the pieces, and then cementing the two together again with the plane of separation bisecting the pencil of rays.

In Laurent's instrument, for which homogeneous light is used, half the pencil is passed through a plate of quartz cut with its axis in the surface and parallel to its edge, the thickness being such that the extraordinary ray is retarded half a wave-length behind the ordinary. On emergence the directions of vibration in the two parts of the pencil, one of which has traversed the quartz, are equally inclined to the edge of the crystal. The inclination of the two to each other can be very easily altered by simply turning the polariser.

The following arrangement is in place of the Iceland spar in Jellett's instrument, and of the quartz plate in Laurent's. It seems to be somewhat simpler, and gives fairly good results.

A circular plate of quartz cut perpendicular to the axis is divided along a diameter, and one half slightly reduced in thickness. The two halves are then reunited and interposed in the path of the pencil and at right angles to its direction. Since one half of the pencil passes through a slightly greater thickness of quartz, its plane of polarisation is slightly more rotated than that of the other half; and the pencil therefore emerges with the planes of polarisation of its two halves slightly inclined to each other. It is of course always necessary to use homogeneous light to avoid dispersion.

Mr. Glazebrook has very kindly given me the following numbers, which are taken at random from a large number of sets of readings he has obtained for the electromagnetic rotation of certain solutions of NaCl in water; the difference of thickness of the two plates being ·1 mm., and the inclination of the planes of polarisation being therefore about 2° for the sodium light used. The circle to which the analyser was attached reads to 3′; but the vernier-divisions can easily be further subdivided by eye.

In order to vary the inclination of the two planes of polarisation to each other, one of the halves of the quartz plate might be arranged like a Babinet's compensator, so that the difference of the two might be varied at will. The chief objection to the method seems to be that the quartz plate has to be adjusted very exactly perpendicular to the axis of the pencil.

			Circle-readings
I.	Current direct ...	...	23° 45′
			23 46
			23 45
			23 45
	Current reversed	...	21 36
			21 34
			21 39
II.	Current direct ...	...	23 15
			23 16
			23 18
	Current reversed	...	22 19
			22 19
			22 20
III.	Current direct ...	...	23 30
			23 30
			23 28
	Current reversed	...	21 45
			21 47
			21 48

A still simpler arrangement, which has as yet only been tried in a somewhat rough form, consists in a cell containing some active liquid, say sugar solution. This cell is interposed in the path of the pencil; and in it is inserted a piece of plate-glass several millimetres thick, arranged so that one half the pencil passes through it. This half therefore passes through a less thickness of the active substance than the other half, and is less rotated. The two then emerge as before, having their planes of polarisation slightly inclined to each other. This inclination, and consequently the sensitiveness of the instrument can be varied either by varying the strength of the active solution, or the thickness of the plate of glass inserted in the cell.

This arrangement, as far as it has been tested, gives as good results as the previous one, while it is much more easily constructed and adjusted.

33.

ON THE LAW OF THE PROPAGATION OF LIGHT*.

By J. H. Poynting and E. F. J. Love, B.A.

[*Birmingham Phil. Soc. Proc.* **5** (1887), pp. 354–363.]

[*Read* March 31, 1887.]

The general law for the propagation of light, applying both to transparent and absorbing media, is that the intensity of illumination at a distance d from a given source is proportional to e^{-cd}/d^2. For transparent media $c = 0$.

We shall describe in this paper a new experimental method of showing that this is the law of propagation. While the method gives, we believe, the first exact experimental verification of the law for absorbing media, a combination of the result with those of ordinary photometric observations shows that for transparent media $c = 0$, and so for these our method proves the inverse-square law.

Since various proofs of the inverse-square law already exist it may perhaps be necessary to justify an addition to the list by pointing out the weak points in its predecessors.

In what may be called the *a priori* proof a cone is drawn with the source as vertex, and cross-sections of the cone are taken at different distances. Since the areas of these sections vary as the squares of their distances from the vertex, the amount of light falling on unit area of each is stated to vary inversely as the square of the distance. This assumes (1) that there is something, constant in amount, travelling out with a constant velocity; and (2) that the illumination is proportional to the amount of this incident per second on unit area. In fact, it assumes the conservation of light-energy, and it identifies intensity of illumination with the amount of light-energy received.

It appears to be sometimes supposed that the law is proved by the consistency of the results obtained in photometry. Thus let I_1, I_2 be the illuminations on a screen at unit distance from two sources; assuming the law, when the illuminations are equal at distances d_1 and d_2 respectively, we have $\frac{I_1}{d_1^2} = \frac{I_2}{d_2^2}$.

* The substance of this paper was communicated to Section A at the 1886 meeting of the British Association. The experiments with sodium light have been carried out since then.

This equation, holding for one pair of distances, will hold for any pair in which the ratio $d_1 : d_2$ is constant. Carstaedt (*Pogg. Ann.* 150, p. 551), using the Bunsen photometer, has shown experimentally that the ratio is constant within the limits of experimental error. But with *any* power of the distance the ratio should be constant. For the equation $\frac{I_1}{d_1^n} \quad \frac{I_2}{d_2^n}$ will be satisfied by any pair of values of $d_1 : d_2$ if $\frac{d_1}{d_2} = \left(\frac{I_1}{I_2}\right)^{\frac{1}{n}}$.

The simplest direct proof is that which shows that 1 candle at a distance 1 produces an illumination equal to that of 4 candles at distance 2, of 9 candles at distance 3, and so on. But this method is wanting in exactness, and must be considered rather as a lecture-room illustration than as an accurate proof.

Crookes (*Phil. Trans.*, 1876, p. 325) has given another proof depending on the radiometer-effect. He shows by subsidiary experiments that this effect is proportional to intensity of illumination, and then he determines by the effect the intensity of the light received from a standard candle at varying distances. The proof is interesting, but does not appear to be very exact.

Another proof is based on the obseivation that a unifoim illuminating surface looked at through a narrow blackened tube appears equally bright at all distances so long as the illuminating surface entirely fills up the aperture. The area illuminated on the retina is constant, while the area illuminating it varies as the square of the distance. Hence the illuminating power per unit area of the surface varies inversely as the square of the distance. This is probably more accurate than the previous methods, but it requires the observer to be assured that there is no gradual change in the illumination as the distance of the source changes. The eye, however, is not very sensitive to gradual changes of illumination, seeking always to counteract them by altering the aperture of the pupil. But the eye is very sensitive to difference of illumination of two surfaces presented to it at the same time.

The proof which we now give has therefore been devised to depend on equality of illumination of two surfaces seen together. It may be regarded as a development of the last proof, but instead of employing a single illuminating surface two illuminating surfaces at different distances are viewed through a narrow blackened tube, each surface occupying half the field of view. The illuminating powers of the two surfaces are adjusted till for a given distance of the tube they appear equally bright. They then appear equally bright for any other distance of the tube.

We shall first, assuming the truth of this statement, deduce from it the law of propagation, and then give an account of the experimental verification.

I. *Theoretical.*

Let two uniform illuminating surfaces be arranged so that when viewed through a narrow tube each occupies half the field, and let them be placed at distances from the eye-end of the tube d_1 and d_2 respectively. Then the surfaces sending light to a point at the eye-end of the tube may be put equal to kd_1^2, kd_2^2, respectively.

Let I_1, I_2 be the illuminations produced per unit area of the illuminating surfaces on a screen held at unit distance from an element of the illuminating surfaces.

Let $\dfrac{f(x)}{x^2}$ be the law of propagation, so that if I be the intensity of illumination of a screen at unit distance from a source $\dfrac{If(x)}{x^2}$ is that at distance x.

If the two surfaces appear equally bright we have

$$I_1 k d_1^2 \frac{f(d_1)}{d_1^2} = I_2 k d_2^2 \frac{f(d_2)}{d_2^2} \quad \text{......................(1)}$$

or

$$I_1 f(d_1) = I_2 f(d_2). \quad \text{.........................(2)}$$

But this equation is still true by experiment if both d_1 and d_2 be increased by any the same quantity ϵ,

$$\therefore\ I_1 f(d_1+\epsilon) = I_2 f(d_2+\epsilon). \quad \text{...................(3)}$$

Put $d_1 + \epsilon = x$ and $d_2 - d_1 = y$,

$$\therefore\ I_1 f(x) = I_2 f(x+y). \quad \text{.........................(4)}$$

For given values of I_1, I_2 and y this is true for all values of x. Hence differentiating (4) we have

$$I_1 f'(x) = I_2 f'(x+y). \quad \text{.........................(5)}$$

Dividing (5) by (4)

$$\frac{f'(x)}{f(x)} = \frac{f'(x+y)}{f(x+y)}. \quad \text{...........................(6)}$$

Put

$$\frac{f'(x)}{f(x)} = \chi(x), \quad \text{...............................(7)}$$

$$\therefore\ \chi(x) = \chi(x+y). \quad \text{................................(8)}$$

Now this is true for all values of y, and can therefore only be satisfied by

$$\chi(x) = \text{constant} = -c, \quad \text{.........................(9)}$$

$$\therefore\ \frac{f'(x)}{f(x)} = -c,$$

or

$$f(x) = Ae^{-cx}.$$

But at distance 1, $\qquad f(1) = 1,$

$$\therefore\ 1 = Ae^{-c},$$

or

$$A = e^{c}.$$

Hence the intensity of illumination at distance x from the source is

$$\frac{Ie^{-c(x-1)}}{x^2}.$$

Now ordinary photometric measures in transparent media such as air show that if two sources give equal illuminations at distances d_1 and d_2, the equality of illumination is maintained for all distances so long as $d_1 : d_2$ is constant.

The equality of illumination requires that

$$\frac{I_1 e^{-c(d_1-1)}}{d_1^2} = \frac{I_2 e^{-c\ d_2-1}}{d_2^2}.$$

Putting $d_2 = kd_1$, we obtain

$$\frac{I_1 e^{-c(d_1-1)}}{d_1^2} = \frac{I_2 e^{-c(kd_1-1)}}{k^2 d_1^2}.$$

$$\therefore\ e^{-cd_1(1-k)} = \frac{I_2}{k^2 I_1},$$

which can only be true for all values of d_1 when $c = 0$.

Hence for transparent media we have the ordinary inverse-square law.

When c has a value differing from zero e^{-c} is the 'coefficient of absorption.'

Of course c varies in general for different colours, so that we get equality of tint only when we are using monochromatic light.

In this case equation (2) becomes

$$I_1 e^{-c(d_1-1)} = I_2 e^{-c(d_2-1)},$$

or

$$\frac{I_1}{I_2} = e^{-c(d_2-d_1)},$$

$$\therefore\ e^{-c} = \left(\frac{I_1}{I_2}\right)^{\frac{1}{d_2-d_1}}.$$

If, then, we arrange the apparatus in such a way that I_1 and I_2 can be determined e^{-c} can be found from the above equation. We have not attempted to adapt the apparatus for this purpose, but content ourselves with pointing it out as a possible method of finding the coefficient of absorption.

II. *Experimental.*

The object of the experiments was to prove the statement already made—that 'if two luminous surfaces at different distances are viewed through a narrow blackened tube, each surface occupying half the field of view, and the illuminating powers of the two surfaces are adjusted so that for a given distance of the tube they appear equally bright, they will then appear equally bright for any other distance of the tube.'

The statement requires to be demonstrated for transparent and absorbing media. It was accordingly resolved to make observations in air with both white and monochromatic light; but as liquids exerting a perceptible general absorption of white light are not to our knowledge obtainable, the experiments with an absorbing medium were carried out with monochromatic light only.

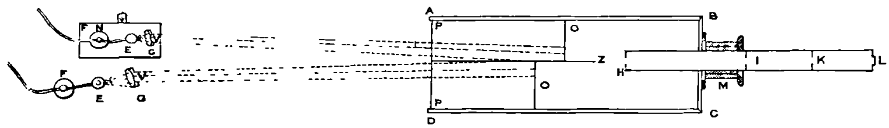


Fig. 1.

The apparatus employed (*vide* Fig. 1) consisted of a trough, *ABCD*, $12'' \times 4'' \times 4''$ internally; the bottom, sides, and one end are of paraffined wood blackened, the other end being closed by a piece of plane parallel glass, *PP*. A hole was pierced in the wooden end through which passed a brass tube, *HL*, $\frac{3}{4}''$ in diameter, arranged to slide in a stuffing-box, *M*, fixed to the end of the trough. The end, *L*, of the tube was closed by a glass plate. The bright surfaces were obtained as follows: A large plate of opal glass was carefully examined, and a part which appeared homogeneous was cut out and divided into two. Each of the pieces was then placed in the trough (at *O*, *O*) so as to fill up half the field of view of the brass tube, but at different distances from it, care being taken that the adjacent edges should be those along which the plate was cut. The plates were illuminated by lights, *E*, *E*, at some distance from the trough. In front of the lights were placed convex lenses, *G*, *G*, somewhat nearer than their own focal length, so as to produce the effect of a brighter beam coming from a greater distance. On the side of each lens next the trough was placed a blackened screen, *V*, *V*, with a rectangular slit cut in it, so as to produce a well-defined beam. One lamp and lens was fixed, the other placed on a wooden tray moving parallel to itself in guides, so that its distance from the trough could be varied at pleasure, changes in its position being read off on a millimetre-scale, *NN*, attached to the tray, with the aid of a fiducial mark affixed to the experiment table. To prevent light from either lamp reaching the opal on the other side, a blackened strip of zinc, *Z*, was fixed in the middle line.

To get rid of internal reflection in the apparatus, the tube and trough were well blackened. The tube was provided in addition with four stops, H, I, K, L, placed respectively at the two ends, at the middle, and at one-fourth of the length from the eye-end. The diameters of the stops were $H\ \frac{3}{8}''$, $I\ \frac{9}{32}''$, $K\ \frac{15}{64}''$, $L\ \frac{3}{16}''$. To prevent stray light from reaching the eyes, a card-board screen about 12″ in diameter was hung on the tube. The experiments were carried on in the dark chamber of the Physics Laboratory at Mason College.

As sources of *white* light two galvanometer-lamps, which burn with a very steady light, were employed. With *monochromatic* light the difficulties were greater; for the ordinary form of sodium flame was quite unsuitable, owing to the different quantities of light emitted from different parts of the surface, and the altogether uncontrollable fluctuations in brightness. The following arrangement, represented in Fig. 2, was, however, found successful:

A stream of coal-gas, whose pressure was rendered pretty uniform by passing through a 'Stott' governor, was sent into a large glass flask, F, by a tube, QQ, which just passed through the cork. Some granulated zinc was

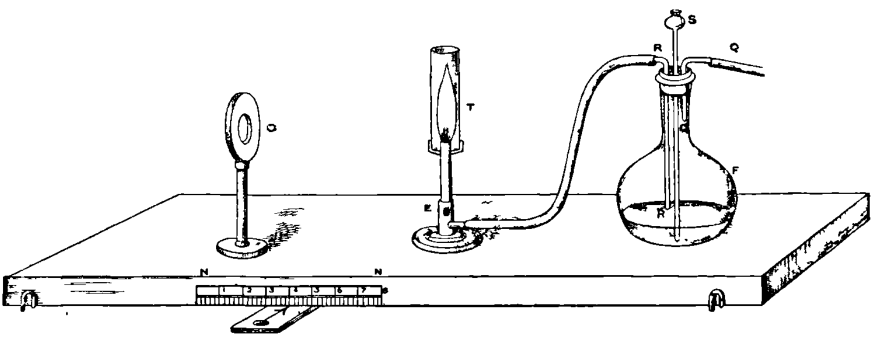


Fig. 2.

placed at the bottom of the flask, together with a small quantity of concentrated salt-solution, with a large excess of solid salt; hydrochloric acid was added by the thistle funnel, S. The hydrogen slowly evolved from the zinc and acid kept up a constant spirting of salt-solution into the gas; and as the lower end of the exit tube, RR, was near the surface of the liquid, the gas came up charged with a quantity of finely divided salt-solution*. This mixture was led to a large Bunsen burner, E, and a light of uniform intensity in all parts obtained, which was freed from flickering by surrounding the flame with an Argand lamp chimney. One of these arrangements was used for each opal. As absorbing medium, a dilute solution of cobalt carbonate in ammonium carbonate, which energetically absorbs yellow light, was employed.

* This arrangement is similar to that employed in Bunsen's well-known apparatus for exhibiting the sodium absorption.

The experiments were conducted as follows: The point at which the intensities of the lights were equal having been ascertained, the tube was withdrawn, or pushed in, so as to alter its distance from the opals, and in every case the two halves of the field remained equally bright. The distance through which it was necessary to move the tray, *NN*, carrying one of the lamps and its lens, in order to produce a sensible difference in the intensity of the two halves of the field, was then noted. The distance of a source of light which would have produced a pencil of the same dimensions and angle as that actually employed was then found by measuring the width of the bright image of the rectangular slit formed on a screen interposed for the purpose at a known distance. In all the sets of experiments, the relative intensities of the lights, their distances, and the distance apart of the opals, were varied.

The accuracy obtained was rather remarkable. It appeared that a difference of about 1 part in 500 in the intensities of the two halves of the field was perceptible for white light; and of about 1 in 270 in the case of sodium light. This could, however, only be obtained by giving the eyes a period of rest in darkness before commencing the experiments; accordingly the shifting of the tray and registering of its position had to be done by an assistant.

We append the details of one complete experiment as an example:

Light employed: Sodium Flame. Medium: Air.

	Distance from image to slit..............	22·5 cm.
	Width of image	4·4 cm.
	,, ,, slit	1·8 cm.
∴	Effective distance of source from slit	15·6 cm.

The tube was at first drawn out.

Distance of slit from opal	40·30 cm.	Left side darker.
,, ,, ,,	40·23 cm.	Equal.
,, ,, ,,	40·18 cm.	Right side darker.

The tube was then pushed in, and with the last distance, 40·18, the right side was still the darker. When the distance from the slit to the opal was 40·23 the equality was restored.

The eye was therefore sensitive to a change in the distance of the moveable source of ·05 to ·07 cm.—say ·07 cm.

Since the effective distance of source from slit is 15·6 cm., and of slit from opal 40·23 cm., the total distance from source to opal is 55·83 cm.

Smallest perceptible change of distance $= \frac{1}{798}$;

whence ,, ,, ,, brightness $= \frac{1}{399}$.

The results of the experiments are collected in the following table:

Kind of light	Effective distance	Smallest perceptible change of distance	Change of intensity
White in air ...	112·80 cm.	0·10 cm.	1 in 564
	103·25 „	0·10 „	1 „ 516
Sodium in air ...	55·83 „	0·07 „	1 „ 399
	55·60 „	0·10 „	1 „ 278
Sodium in absorbing medium	54·30 „	0·10 „	1 „ 272
	61·96 „	0·05 „	1 „ 620

Since in no case was there any difference between the positions for equality when the tube was drawn out and pushed in, we may say that, within the limits of error above mentioned, the law of propagation holds good for both transparent and absorbing media.

NOTE IN CORRECTION TO ABOVE PAPER.

[*Birmingham Phil. Soc. Proc.* **6** (1888), p. 168.]

In a paper read by the authors on March 31st, 1887, an account was given of a method of experimentally verifying the law of propagation of light both for transparent and absorbing media. In the paper it was stated that the method used seemed to lead to much more than the usual accuracy in the estimation of small differences of intensity of illumination. At the suggestion of Lord Rayleigh we have re-examined the method, and have come to the conclusion that the experiments were affected by some source of error which we cannot now detect—leading us to under-estimate the differences noted, as we now find that we only attain the ordinary accuracy, detecting a difference not far from 1 %. This does not, of course, affect the method as a proof of the law, but only gives a wider range for the possible errors of experiment. We much regret, however, that the published figures should have given to the experiments an appearance of accuracy to which they have no real claim.

34.

HAZE.

[*Nature*, **39**, 1889, pp. 323–324.]

I have for some time given in my lectures an explanation of the common summer haze which appears to me to be very probable. I do not know whether it is new, but it has not been referred to in the discussion raised by Prof. Tyndall's letter on Alpine haze*. Some time since I mentioned it to Prof. Lodge, and at his suggestion I send it to you, though its extension to other kinds of haze is somewhat speculative.

It is that haze is often due to local convection-currents in the air, which render it optically heterogeneous. The light received from any object is, therefore, more or less irregularly refracted, and, through the motion of the currents, its path is continually varying. The outline of the object, instead of appearing fixed, has a tremulous motion, and so becomes ill-defined. At the same time, reflection occurs where there is refraction at the surfaces of separation of heterogeneous portions. Much of the light which, in a homogeneous medium, would come straight from the object, is thus lost for direct vision, and the contrast between neighbouring objects is lessened. The reflected light is diffused as a general glare. The combination of the quivering of outline, and the loss of direct light, with the superposition of the reflected light as a diffused glare, gives the appearance we call haze.

This explanation appears to me to accord well with the obvious facts of summer haze—the haze which is seen in the middle of a hot, cloudless, summer day. The lower layers of air, being heated by contact with the earth, rise in temperature till equilibrium is no longer possible, and convection begins, streams of the heated air rising, and streams of colder air falling to take its place. The variation of temperature and density gives optical heterogeneity. The existence of these streams is sometimes shown by the quivering of distant objects, looked at through the air close to the ground, but a telescope will often show the quivering of outline at higher levels, and when quite invisible to the naked eye. Accompanying this refraction, reflection must occur. We have a direct proof of its occurrence in the fact that the glare is greatest under the sun, where reflection occurs at angles approaching grazing incidence,

* [*Nature*, vol. 39, 1888, p. 7.]

for which it is a maximum; while it is least opposite the sun, where reflection occurs at angles approaching normal incidence, for which it is a minimum. The opening lines of *The Excursion* perfectly describe the resulting appear ance:

> "'Twas summer, and the sun had mounted high;
> Southward the landscape indistinctly glared
> Through a pale steam; but all the northern downs,
> In clearest air ascending, showed far off
> A surface dappled o'er with shadows flung
> From brooding clouds."

During the night the lower strata become colder than the upper ones, and the atmosphere passes into a state of stable equilibrium. We should therefore expect that, if the foregoing explanation is true, there would be complete absence of haze, and it is well known that the air is peculiarly clear in early morning, when we get above the fog-level.

According to this account of heat-haze, it stands in sharp contrast to fog, of which it is so often supposed to be a relative in reduced circumstances. While the one requires convection, the other usually occurs when the air is in stable equilibrium, the lowest strata being the coldest. In the fog, for example, which so frequently heralds or accompanies the break-up of a frost, the lower strata are still cold, while above the wind has changed, and the air comes up warm and vapour-laden. The vapour diffuses downwards into the lower, cold strata, and is there condensed, and it is possible that the more rapid diffusion of water-vapour has something to do with the continuance of the fog, for it would diffuse downwards more rapidly than the rest of the air with which it has come.

There are other cases of haze which may, perhaps, be explicable by optical heterogeneity. In the dry east winds of spring we frequently have a haze when the air is far from saturation-point, and the clouds, if they exist, are at a high level. It appears possible that this haze is due to small convection-currents of the cold air from above, the temperature falling too rapidly from below upwards for equilibrium.

Sometimes the heterogeneity may be due to water-vapour. After rain, when the ground is still wet, the drying of exposed surfaces often shows that the air is not saturated, yet there is a haze or mist which is supposed to be thin fog, i.e. water-dust. Evaporation must be going on, and the air must certainly be unequally charged with vapour. With this inequality there must also be convection. I have never been able to detect, with certainty, quivering of outline either in this case or in the previous one of the east wind haze, though I have sometimes suspected its existence. Possibly someone who has used a high-power telescope for terrestrial objects might be able to give information on this point. But it is to be noted that the differences of density in both these cases are much smaller than in the case of summer haze, and the currents should therefore be on a smaller scale.

It seems worthy of inquiry whether the haze observed under cumulus clouds, referred to by the Rev. W. Clement Ley*, may not also be due to water-vapour heterogeneity. The cumulus cloud indicates the existence of a large body of vapour-laden air extending no doubt below, as well as above, the condensation-level. As this mass sweeps along, the lower part of it is retarded by the earth and by the lower strata, and more or less disturbance and mixture with the surrounding air will occur. There will therefore be heterogeneity. I do not know whether this would be at all sufficient to account for the haze observed, but the suggestion seems worth considering.

* [*Nature*, vol. 39, 1888, p. 183.]

35.

A GRAPHICAL METHOD OF EXPLAINING THE DIFFRACTION BANDS AT THE EDGE OF A SHADOW.

[*Birmingham Phil. Soc. Proc.* **7** (1890), pp. 210–219.]

[*Read* Nov. 5, 1890.]

The case here treated is that of the shadow of a screen with a straight edge, interposed in the course of waves proceeding from a source so distant that they may be regarded as plane. A diagrammatic method of representing the effect due to the successive 'half-period' elements is adopted, and from the diagram is obtained a general explanation both of the bands without the geometrical shadow and of the rapid decrease of illumination within it.

Let the plane of the paper (Fig. 1) represent a wave-front. To find the illumination at any point P in the normal to the wave-front through O, we adopt the usual plan of breaking up the wave-front by a series of concentric circles round O as centre, and such that their distances from P increase successively by $\frac{\lambda}{2}$. That is, $\frac{\lambda}{2} = PA - PO \quad PB - PA - PC \quad PB$ etc. The innermost circle and the circular bands thus formed are termed half-period elements. In Fig. 1, ten of these elements are represented, drawn their true size, for a wave-length of sodium light ($59/10^5$ mm.), when P is 200 metres in advance of the wave-front. If OP is 1 metre, OA will contain 200 elements, and the first will have a radius about $OA/14$.

Taking each element to be the source of an independent disturbance transmitted to P, since the mean distance of any element differs by $\frac{\lambda}{2}$ from the next preceding or succeeding, the series of disturbances arriving at P at any instant are alternately in opposite phases and tend to neutralise each other. Assuming that the amplitude of the disturbance sent by any small area is proportional to area/distance, that sent by a circular band of radius ρ, breadth $d\rho$, distant r from P, may be represented by $2\pi\rho\, d\rho/r$ (omitting the constant factor).

But $\rho^2 = r^2 - d^2$ where $OP = d$. Hence (or by a very easy geometrical proof) $\rho\, d\rho = r\, dr$, and the disturbance is $2\pi\, dr$. Now $dr = \frac{1}{2}\lambda$ for each element, so that each sends the same disturbance $\pi\lambda$.

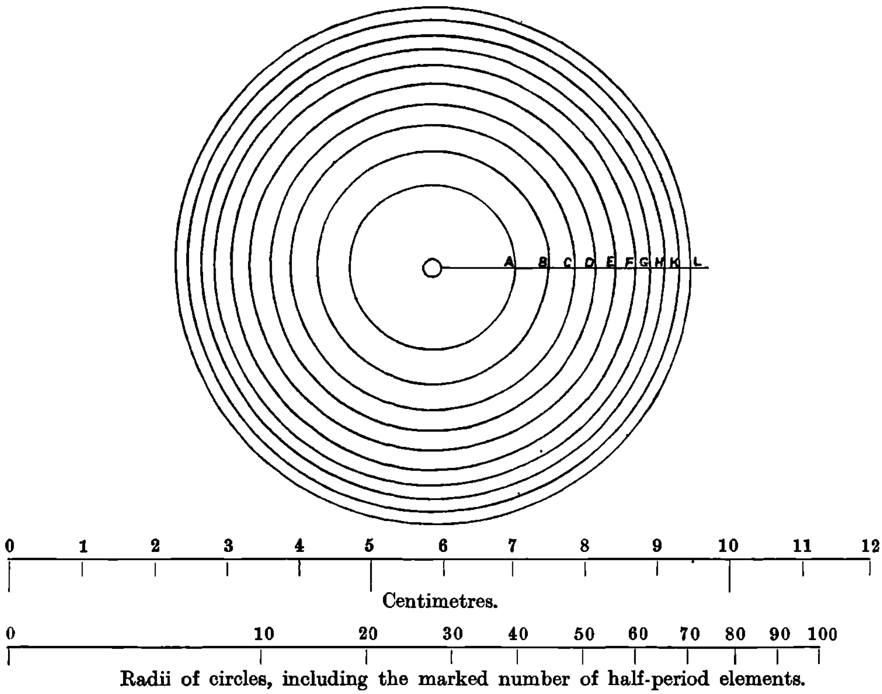


Fig. 1. Wave-front divided into circular bands, increasing successively by a half wave-length of sodium light (·00059 mm.) in distance from a point 200 metres from the plane of the figure. Underneath are the radii for every tenth band up to the hundredth. Actual size.

But this takes no account of the inclination of the line of propagation to the normal to the wave-front, whereas we must suppose that the disturbance propagated decreases as this line gets further from the normal.

To represent the effect due to successive elements, take along a line oo' (Fig. 2) equal distances, $oa = ab = bc \ldots = \dfrac{\lambda}{2}$; draw a line, hh', parallel to oo' and distant 2π from it, and draw perpendiculars through $o, a, b, \ldots$ to meet this line. A series of rectangles, each of area $\pi\lambda$, is thus formed, and they may be taken as representing the effects of the elementary bands, with breadths OA, AB, BC, in Fig. 1, on the supposition of no diminution from inclination. Now, draw a curve, $hklm\ldots$, such that its height from any point in oo' represents the effect due to a half-period element about the corresponding point in Fig. 1. This curve will slowly descend and ultimately approach indefinitely near the base line oo'. It cuts off the tops of the rectangles, and the areas beneath it, gradually diminishing from $\pi\lambda$ to 0, represent the disturbances sent by the successive elements. As these are alternately in opposite phases, tending to neutralise each other, they are marked alternately + and −. Thus, the second nearly neutralises the first, the fourth nearly neutralises the second, and so on, each pair, however, leaving a small balance over of the same sign as the first element. To find the sum of all these small remnants, imagine the figure cut out in paper: white in front, black at the back, and with folds made alternately in opposite directions, like those of a fan, along the lines *ak*, *bl*, *cm*, etc. Now, folding the whole figure down on to the first

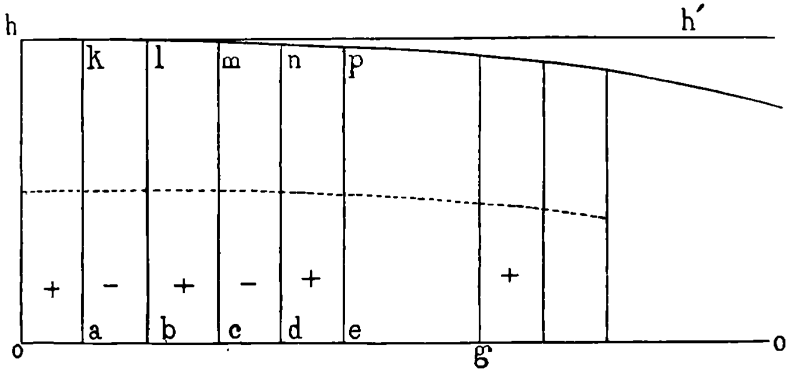


Fig. 2. Areas representing the disturbance at *P*, due to successive half-period elements. The curve *hklm*... shows the diminution due to inclination. The dotted curve has half the height.

element, evidently the white area left will be the sum of the remnants. In Fig. 3 the white area is represented, and it is seen to consist of a very great number of triangles, each of height *oa*, and with total base *oh*; so that the total area is half that of the rectangle *ah*, and the total disturbance is half that which would be produced by the first element alone.

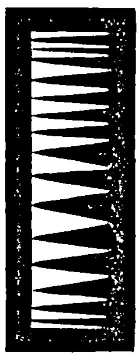


Fig. 3. The area of the positive elements left when the negative elements have been folded over.

We may at once see the effect at *P* of the interposition of a small round opaque disc, with centre at *O*. We must draw half-period circles, *starting now from the edge of the disc*, and Fig. 2 will still represent the series of separate disturbances if we begin at a point *g*, some way from *o*. The total effect will still be as in Fig. 3, half the first area at *g*, which is nearly equal to the area *ok*, since the curve *hkl*... only descends very slowly. The effect at *P* is nearly the same as if the opaque disc were removed. We might also easily show that there are successive bright and dark regions along the axis of a small circular hole in an opaque screen.

Let us now examine the effect of a screen with a straight edge stopping out any part of the wave. We shall suppose the screen to extend indefinitely on the left and consider the disturbance at *P*, as the edge occupies various positions with regard to *O*.

First, let the screen pass through O (Fig. 1) so that P is at the edge of the geometrical shadow. In this position the screen merely cuts off half of each element, and the areas in Fig. 2 must be reduced to half their height as represented by the dotted line. The result of folding over is to leave half of the reduced first element, so that the total disturbance is half as great as that due to the uninterrupted wave, and the intensity (varying as the square of the disturbance) is one-quarter.

Next let the screen be drawn to the right so that P advances without the geometrical shadow. Every half-period element in Fig. 1 has now more than half its area left, though the areas of the elements distant from O decrease towards half their full value as a limit. Further, the rate of increase in the amount cut off from each successive element by the screen is more rapid as the edge is nearer O. This may easily be seen by laying an edge on Fig. 1 in different positions. Thus, if it passes through A, very soon a large fraction of half of each element is cut off; while, if it passes through E, the approach to the half element is much more gradual.

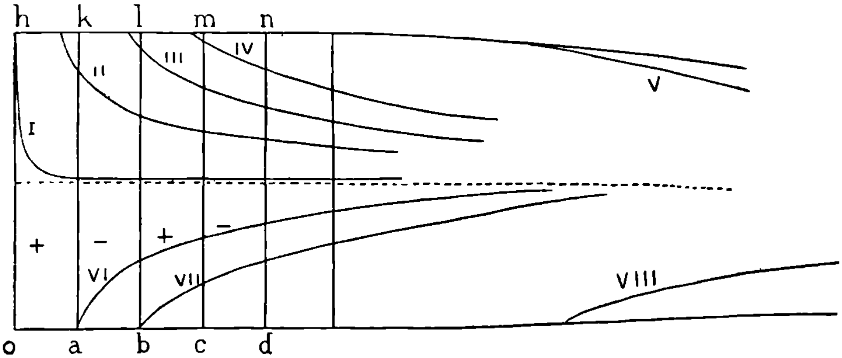


Fig. 4. Showing the amounts cut off the successive half-period elements by the screen. In I–V the screen is to the right of O (Fig. 1). In VI–VIII it is to the left of O.

The curves I–V (Fig. 4) represent the reduction in effect—the amount cut off each element—when the screen is placed in Fig. 1, just to the right of O, just to the left of A, of B, of C, and finally some way from O. Figs. 5–9 represent the result of folding over in each case, as in Fig. 3.

It is evident that Fig. 5 gives little more than the half-effect at the edge of the geometrical shadow. Fig. 6 gives a good deal more than Fig. 3, i.e., more than the illumination when the screen is taken away. Fig. 7 gives considerably less, Fig. 8 somewhat more. That is to say, there are a series of maxima and minima, and exact calculation shows that these are opposite points a little to the left of A, B, C respectively. Fig. 9 shows the effect when the screen is some distance from O; and now there is only a slight irregularity in the fifth triangle, so that the area is only slightly different from that in

Fig. 3. The maxima and minima therefore rapidly die away, and soon the illumination is sensibly equal to that existing when the screen is removed.

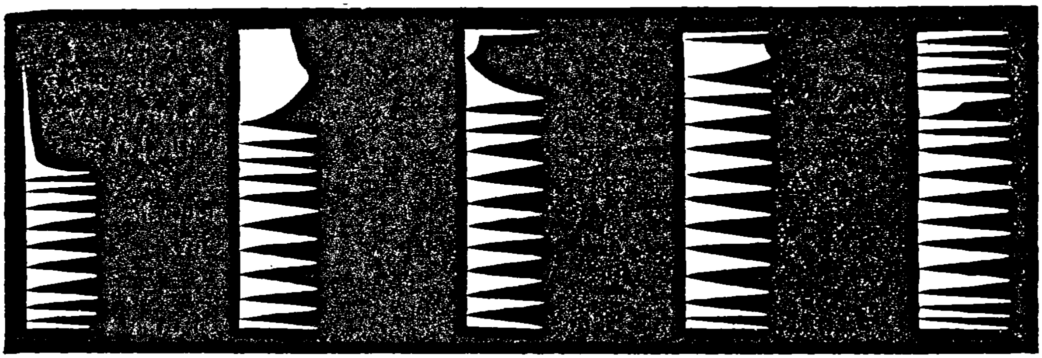


Fig. 5. Just without the geometrical shadow.

Fig. 6. First maximum.

Fig. 7. First minimum.

Fig. 8. Second maximum.

Fig. 9. Distant maximum.

If the screen is now put with its edge to the left of O, P is within the geometrical shadow. There will now be a circular area round O, and touching the edge, which will be entirely cut off, and starting from the circumference of this, successive half-period elements will have areas which increase, more rapidly the nearer the screen is to O, up to half their full area as limit. This is illustrated by the curves VI, VII, VIII, Fig. 4. To find the effect for any position of the screen we may proceed thus. Any curve representing the

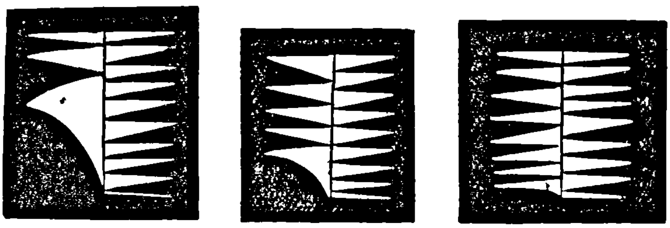


Fig. 10. A short distance within the geometrical shadow.

Fig. 11. Further within.

Fig. 12. Some distance within.

effect of successive elements rises nearer and nearer to the dotted half-height curve, but this latter is itself descending, so that ultimately the former curve must also descend. Take two successive elements about the point of maximum height, and for ease of interpretation it is better to have an odd number of elements to the left of the two selected. Fold down on to these two from each side, and the white areas remaining show how much of each is unneutralised

—on the left by the elements up to that point, on the right by the elements beyond it.

Figs. 10, 11, 12 show the nature of the results for curves VI, VII, VIII.

Now the amounts left over in the two elements are in opposite phases, and therefore tend to neutralise each other. But in Fig. 10 the neutralisation is not complete owing to the defect in the left hand through the curvature of the sides of the triangles. In Fig. 11 there is still a defect on the left for the same reason, but much smaller in quantity, and the two areas are more nearly equal. In Fig. 12 they are sensibly the same, and the neutralisation is practically complete. In other words, no light reaches the point.

Evidently, then, while without the geometrical shadow there are a series of bright and dark bands rapidly decreasing in their difference from each other, within the geometrical shadow the illumination always decreases, and so rapidly that at a very short distance within there is nearly complete darkness.

Note on the Equation to the Curves in Fig. 4.

The equation is easily shown to be

$$y - \frac{1}{\pi} \sin^{-1} \sqrt{\frac{x}{x+h}},$$

where y is the fraction of the element cut off or left, as the case may be, h is the distance of the starting point from o or h, and x the abscissa, measured from the starting point. If $h \ \frac{\lambda}{2}$, i.e., if the curve starts from a or k, we get the following values:

$$x = \frac{\lambda}{2} \quad \lambda \quad \frac{3\lambda}{2} \quad 2\lambda \quad \frac{5\lambda}{2} \quad \infty\,.$$

$$y = \cdot 25 \quad \cdot 30 \quad \cdot 33 \quad \cdot 35 \quad \cdot 37 \quad \cdot 5.$$

If h is increased, x must be increased in the same ratio, to give the same value of y. Hence the gentleness of slope is proportional to h.

36.

ON A PARALLEL-PLATE DOUBLE-IMAGE MICROMETER.

[*Roy. Astr. Soc. Monthly Notices*, **52**, 1892, pp. 556–560.]

If a ray of light passes through a plate of glass with parallel faces it emerges parallel to its original direction, but with any other than normal incidence it is shifted through a distance proportional nearly to the tangent of the angle of incidence, up to quite considerable values of that angle.

I have already published an account of a use of this property in obtaining a fine adjustment for a cathetometer-telescope (*Phil. Trans.* vol. 182 (1891), A, p. 588)*. A parallel plate moveable round a horizontal axis is placed just in front of the object-glass. The telescope is brought nearly to the level of the point to be sighted, and the final adjustment is completed by tilting the plate until the point appears on the cross-wire.

We have for some time used a similar device with a microscope in the Mason College Physical Laboratory for the measurement of small objects, placing the parallel plate between the stage and objective. It is very easy and very rapid in use. The plate might be placed, for the measurement of smaller objects, between the objective and eyepiece, but we have never required the sensitiveness which would thus be obtained†.

I have lately applied the principle in the construction of a double-image micrometer for a telescope, and as the instrument, as far as I can test it, appears to be successful, it may be worthy of description.

A circular parallel plate of glass, rather more than 1 inch in diameter and about $\frac{1}{8}$ inch thick, being cut down the middle, one half is fixed in one semicircle of a ring, while the other is placed in the other semicircle, attached, however, to an axis passing through a bearing in the ring, so that the plate can be rotated, the axis being in the plane of the fixed plate and perpendicular to its edge at the middle point. The ring is fixed in the tube of a 4-inch telescope, of about 67 inches focal length, 16 inches in front of the focal plane, the division between the two plates being vertical. The axis of the moveable plate passes out horizontally through the side of the tube, and a pointer is

* [*Collected Papers*, Art. 3, p. 66.]

† [An instrument was subsequently constructed in this way for laboratory use. Ed.]

attached at right angles to it moving over a fixed straight scale, the pointer being nearly at the middle of the scale when the two plates are in one plane.

On looking at a star with the plates thus in one plane one image only is seen. But on moving the pointer the one plate is tilted and all the rays through it are shifted, so that two images appear, their separation being nearly proportional to the movement of the pointer along the scale. In the fixed position of the micrometer, with the division between the plates vertical, it is possible only to measure vertical distances. For general use, of course, it should be possible to revolve it about the axis of the telescope. It was fixed merely for simplicity of construction in preparing it to test the method.

The separation of the images for a given angle of tilt might be calculated by the method given at the end of this note, knowing the exact thickness of the plate and its refractive index. But it appeared preferable to obtain the value per division of the scale by experiment. For this purpose the eye-piece of the telescope was removed and a fine scale of 500 divisions to the inch was fixed in the focal plane of the objective. Another telescope focussed for an infinite distance was then placed in front of the objective and the image of the fine scale was viewed by it. The moveable plate was then tilted so that the lines of the fine scale in one image moved one place on, two places on, and so on, the positions of coincidence being easily determined. The tilts corresponded, of course, to displacements of the image, in the use of the micrometer, by successive 1-500ths of an inch. Within more than ten divisions on each side of the middle of the pointer-scale the movements were proportional to the shifting of the fine scale, and the mean of a number of readings made twenty-one divisions of the pointer-scale correspond exactly to eight divisions of the fine scale. The effective focal length of the objective might be taken as 67·16 inches, this being the distance from the near side of the flint glass to the focal plane. For exact work it would be necessary to calculate the position of the principal point for the system of lenses, but this would be quite needless refinement here. Hence

$$1 \text{ division of pointer-scale} = \frac{8}{21} \times \frac{1}{500} \times \frac{1}{67{\cdot}16} \text{ radius, or } 2{\cdot}34 \text{ seconds.}$$

As ten divisions of the pointer-scale occupy a length of 12·68 mm., and the scale is 67·5 mm. from the axis, ten divisions means an angle of tilt less than 11°, and up to this the error in taking the displacement as proportional to the movement of the pointer along the scale is not more than 1 in 600. I have found that with my telescope it is quite sufficient to move the pointer by hand, though with a steady stand and good definition a screw-motion would be useful.

I have tested the micrometer by measuring the distance between the components of *Castor* when one is vertically over the other, and by measuring a vertical diameter of *Saturn*, using a power of 100. I have no experience of micrometer-work, and do not know the accuracy to be expected, but, considering the very inferior definition of my telescope, and the unsteadiness of the stand, the results obtained appear to be hopeful for the success of the method.

The following are the results, in each case, of course, giving double the actual angle.

Distance between two components of Castor.

April 22, 1892. *Readings of Pointer-Scale.*

1st position with one component in one image on the other component in the other image	2nd position with one component in one image on the other component in the other image
34·2	28·6
33·1	28·1
33·1	28·1
33·1	27·7
32·9	28·5

Rejecting the first readings as made without experience, the mean difference is 4·95 divisions.

Hence angular separation of the components is

$$\tfrac{1}{2} \times 4{\cdot}95 \times 2{\cdot}34 = 5''{\cdot}79.$$

May 9, 1892. *Readings of Pointer-Scale.*

1st Position	2nd Position	1st Position	2nd Position
27·4	22·0	27·3	22·1
27·0	22·0	27·3	22·6
26·9	22·0	27·7	22·0
27·0	22·0	27·0	22·5
27·4	22·5	27·0	22 4

The mean difference is 4·99 divisions.

Hence angular separation of the components is

$$\tfrac{1}{2} \times 4{\cdot}99 \times 2{\cdot}34 = 5''{\cdot}84.$$

In Denning's *Telescopic Work* (p. 304) the separation is given as 5″·68.

Vertical Diameter of Saturn.

April 22, 1892. *Readings of Pointer-Scale.*

1st position of moveable image touching fixed image	2nd position of moveable image touching fixed image
23·0	37·7
23·0	37·6
23·0	37·8

The mean difference is 14·7 divisions.

Hence vertical diameter is

$$\tfrac{1}{2} \times 14{\cdot}7 \times 2{\cdot}34 = 17''{\cdot}2.$$

I did not observe the angle the measured diameter made with the polar diameter, but from the westerly position of the planet I estimate it at over 30°. The polar diameter on this evening was

$$17''{\cdot}2,$$

so that the above is rather too small.

May 9, 1892. *Readings of Pointer-Scale.*

1st Position	2nd Position
17·5	31·7
17·6	31·6
17·7	31·7
17·6	31·9
17·7	31·6

The mean difference is 14·08 divisions.

Hence vertical diameter is

$$\tfrac{1}{2} \times 14{\cdot}08 \times 2{\cdot}34 = 16''{\cdot}5.$$

The diameter measured appeared to make about 20° with the polar diameter, which on this evening was

$$16''{\cdot}8.$$

Note on the calculated Value of the shifting of a Ray incident on a Parallel Plate.

Let the angle of incidence be ϕ, that of refraction ψ. Let t be the thickness of the plate, and μ its index of refraction. It is easily shown that the shift is

$$
\begin{aligned}
s &= \frac{t \sin(\phi - \psi)}{\cos\psi} \\
&= t \tan\phi \, \frac{\cos\phi \sin(\phi - \psi)}{\sin\phi \cos\psi} \\
&= t \tan\phi \, \frac{(1 - \sin^2\phi)^{\frac{1}{2}}}{\sin\phi} \left\{ \sin\phi - \frac{(1 - \sin^2\phi)^{\frac{1}{2}} \sin\phi}{\mu \left(1 - \frac{\sin^2\phi}{\mu}\right)^{\frac{1}{2}}} \right\} \\
&= t \tan\phi \left\{ (1 - \sin^2\phi)^{\frac{1}{2}} - \frac{1 - \sin^2\phi}{\mu} \left(1 - \frac{\sin^2\phi}{\mu^2}\right)^{-\frac{1}{2}} \right\} \\
&= t \tan\phi \left\{ 1 - \frac{\sin^2\phi}{2} - \frac{\sin^4\phi}{8} - \frac{\sin^6\phi}{16} - \text{etc.} \right. \\
&\qquad \left. - \frac{1 - \sin^2\phi}{\mu} \left(1 + \tfrac{1}{2} \frac{\sin^2\phi}{\mu^2} + \tfrac{3}{8} \frac{\sin^4\phi}{\mu^4} + \tfrac{5}{16} \frac{\sin^6\phi}{\mu^6} \quad \text{etc.}\right) \right\} \\
&= t \tan\phi \left\{ \frac{\mu - 1}{\mu} - \sin^2\phi \left(\frac{1}{2} - \frac{1}{\mu} + \frac{1}{2\mu^3}\right) \right. \\
&\qquad - \sin^4\phi \left(\frac{1}{8} - \frac{1}{2\mu^3} + \frac{3}{8\mu^5}\right) \\
&\qquad \left. - \sin^6\phi \left(\frac{1}{16} - \frac{3}{8\mu^5} + \frac{5}{16\mu^7}\right) - \text{etc.} \right\}.
\end{aligned}
$$

Taking $\mu = \frac{3}{2}$ and ϕ successively 5°, 10°, and 20°, then approximately

$$s = \frac{t}{3} \tan 5° \,(1 + {\cdot}00042)^{*},$$

$$s = \frac{t}{3} \tan 10° \,(1 + {\cdot}00160),$$

$$s = \frac{t}{3} \tan 20° \,(1 + {\cdot}00526).$$

* [These expressions are given with the wrong sign in the original paper. A slight correction has also been made in the values of the numerical coefficients. ED.]

37.

HISTORICAL NOTE ON THE PARALLEL-PLATE DOUBLE-IMAGE MICROMETER.

[*Roy. Astr. Soc. Monthly Notices*, **53**, 1893, p. 330.]

A description of a parallel-plate double-image micrometer which I designed last year, under the belief that it was new, appeared in the *Monthly Notices*, vol. 52, no. 8, p. 556*.

I have just found from the article 'Micrometer' in the *Encyclopædia Britannica* that the device is more than fifty years old, having been invented by Clausen in 1841 for apparently the first time. I have not access to his description, which is given in *Ast. Nach.* vol. 18, 1841, cols. 95–96.

In the following year the micrometer was re-invented by Porro (*C.R.* vol. 41, 1855, p. 1058). Porro used both the double-image form for the telescope and single-image form for the microscope, with the parallel plate between stage and objective. He saw the advantage which the instrument possesses, in that it can be placed on an independent support. He was evidently convinced of the great value of the parallel plate for micrometry, and appears to have made considerable use of it. Secchi (*C.R.* vol. 41, 1855, p. 906) tried the micrometer, which he ascribed to Porro. He found it likely to be serviceable, but had not time to develope it.

Helmholtz appears to have been the third inventor of the instrument. He used it in his ophthalmometer, which is simply a short-range telescope with two parallel plates in front of the object-glass rotating in opposite directions about an axis, through equal angles. Each plate covers one half of the object-glass, and so rotation produces a double image. (*Physiol. Optik*, ed. 1867, p. 8.)

My own design appears, therefore, to stand at least fourth on the list, and I am exceedingly sorry to have troubled the Society with an account of a device as a novelty when it turns out to be so old. But though old, it is not, I am convinced, well known.

* [*Collected Papers*, Art. 36.]

Now that I have no claim to its invention, I may perhaps fairly express the opinion that the instrument is of great value. I have used it a good deal in the single-plate form in front of the objectives of cathetometer telescopes and microscopes, and find it accurate and easy to use. It has the further merit that it is very cheap. If once introduced into laboratories I am sure that it would be found applicable—with good results—for many purposes. Should this note and its predecessor aid in such introduction, there will be some excuse for the re-invention of the instrument.

38.

A METHOD OF MAKING A HALF-SHADOW FIELD IN A POLARIMETER BY TWO INCLINED GLASS PLATES.

[*British Association Report,* 1899, pp. 662–663.]

When a beam of light polarised in a plane neither parallel nor perpendicular to the plane of incidence falls on a plate of glass with parallel sides, Fresnel's formula shows that it emerges still plane polarised, but with the plane of polarisation rotated away from the plane of incidence. Regarding the incident beam as resolved into two, polarised respectively in and perpendicular to the plane of incidence, the former suffers most loss by reflection at the two faces, so that in emergence it bears a less proportion to the latter, and the resultant plane is therefore turned round from the plane of incidence.

To make use of this rotation to obtain a half-shadow field—i.e., a field divided into two halves in which the planes of polarisation are slightly inclined to each other—two glass plates are bevelled each at one edge and fitted with the bevels together to form a V. This V is fixed in a frame and put in the usual position of the half-wave plate, with the sharp edge down the middle of the field and turned towards the polariser. The frame can be rotated about a horizontal axis—the 'tilt-axis'—through the middle of the edge and perpendicular to the axis of the instrument.

Let us suppose the plane of polarisation of the light incident on the V plates to be vertical. If the edge of the V is also vertical, the light passes through the two plates unchanged in plane, and the two halves will suffer extinction at the same time when the analyser is crossed. But if the V is turned through any angle about the tilt-axis the planes of polarisation of the two halves on emergence from the V are rotated each slightly from the vertical in opposite directions by equal amounts, and now when the analyser is crossed the two halves have equal brightness, and extinction occurs for the two in different positions of the analyser. The V therefore serves to give a half-shadow field. The sensitiveness of the instrument can be increased or diminished by lessening or increasing the tilt of the V.

In general, where light comes through a parallel plate, that which comes straight through is mixed with that which has suffered two or more internal reflections, and if the incident beam is polarised the components of the emergent beam have suffered different rotations. But looking towards the V from the concave side (i.e. the analyser side), it can easily be shown that there is a strip on each side of the junction of the plates in which the light has no admixture of internally reflected beams, and is therefore in each strip all in one plane of polarisation. The thicker the plates the wider are these strips, and they must be so thick that the strips wholly fill the aperture of a diaphragm placed just in front of the V.

Like all other half-shadow instruments, this instrument gives the best results with monochromatic light, but the same V of course serves equally well for any single wave-length.

If the angle between the plates is twice the complement of the polarising angle, say 67°, no light is reflected when the edge of the V is vertical, and therefore this angle gives the best illumination. If the angle is about 80° the strips mentioned above have the greatest width. Probably any angle between these values will serve almost equally well, and I do not find in practice much difference in efficiency with different angles.

The device acts well for projection with a lantern if very thick plates are used.

PART V.

MISCELLANEOUS.

39.

CHANGE OF STATE: SOLID-LIQUID.

[*Phil. Mag.* **12**, 1881, pp. 32–48.]

Two distinct types of change of state from solid to liquid have usually been recognised. The most familiar of these is the ice-water type, in which, as the temperature rises, the solid remains quite solid up to the melting-point; when this is reached it begins to melt *at the surface*, and the temperature remains constant till the whole is liquid, when the temperature again rises. Corresponding to this change of state there is a definite latent heat. In the second class of bodies, of which sealing-wax and phosphorus are examples, there is a gradual softening as the temperature rises; and this softening takes place *throughout the mass*. There is no definite arrest of the rise of temperature, and no definite latent heat.

It has sometimes been supposed that the ice-water type is merely a limiting case of the sealing-wax type, where the softening takes place, but through a very small range of temperature. Prof. Forbes held this view, and by it attempted to explain regelation; but subsequent experiments have not supported the theory, and I believe it is now generally abandoned.

Since, in the ice-water form of change of state, fusion only takes place at the surface, it seems much more probable that it is an exchange phenomenon analogous to the change which, according to the kinetic theory, takes place when water is evaporating. Just as in the case of water-steam, a steady state is reached when the number of molecules escaping from the surface of the water into the gas is equal to the number passing from the gas into the water, so in the case of water-ice a steady state (that is to say, the melting-point of ice) is probably reached when the number of molecules passing from the ice into the water is equal to the number passing from the water to the ice. For the analogue of the sealing-wax type of melting we must probably take the change of state which takes place in a liquid-gas above its critical point, where it changes gradually from a state rather liquid than gaseous to a state certainly gaseous.

In this paper I shall attempt to support this view of solid-liquid change of state. The following is a summary of the argument and the conclusions arrived at.

It is assumed that the maximum vapour-tension of a substance at any temperature is an indication of the number of molecules crossing its surface in a condition to escape. Now Regnault's experiments show that at 0 ice and water have the same vapour-tension; that is, the number of molecules crossing the surface of the ice ready to escape is equal to the number crossing the surface of the water in the same condition. Hence, when the two are in contact at 0°, the interchange of molecules is equal. For temperatures below 0°, Kirchhoff has shown that the vapour-tension of water is greater than that of ice, and above 0° it is less than that of ice if ice can exist. (Another proof of this theorem is here given.) It is, then, easy to give a general explanation of the phenomena of melting and freezing by supposing that, if the temperature is not at the melting-point, the substance in the state with the greater vapour-tension will lose at the expense of the state with the less vapour-tension.

To explain the alteration of the melting-point by pressure, we must suppose that pressure alters the vapour-tension, and therefore the rate of escape of molecules, and that this alteration is different for the two states. Sir William Thomson has shown that a liquid in a capillary tube is in equilibrium with its vapour at a greater or less tension than at the plane surface according as the surface is convex or concave, upwards, and has given a formula for the difference. Accompanying this curvature of surface is a difference of pressure in the liquid; and I suppose the variation of vapour-tension to be due to the difference of pressure. A proof is given of Sir W. Thomson's formula, which seems to bring out more clearly the connection of the phenomenon with the pressure, and which seems to apply to solids as well as liquids. According to this formula, the steady state (the melting-point) may be reached at any temperature if the pressure can be so adjusted that the vapour-tensions in the two states at that temperature and pressure are equal. The resulting lowering of the melting-point by pressure agrees n amount with that given by the well-known formula of Prof. J. Thomson.

It follows from this mode of regarding the subject, that, if in any way the ice can be subjected to pressure while the water in contact with it is not so subjected, then the lowering of the melting-point per atmosphere is about $11\frac{1}{2}$ times as great as when both are compressed. I give the results of some experiments which I have made to test this, and which certainly seem to indicate that the fall of melting-point if the ice alone be compressed is much greater than the amount usually supposed.

The isothermals for ice-water are then discussed. It has been supposed that, if we could employ a sufficiently low temperature and high pressure, then ice would pass continuously into water; that is, the isothermals would have no horizontal part corresponding to a mixture of ice and water, and we should have a critical point. Assuming, however, that a mixture of ice and water completely freed from foreign gases can be subjected to great negative pressure or tension, it seems probable that there is another critical point at a temperature above 0° and at a high negative pressure; that is, the water-ice line is a closed curve. We know that below 0° the water isothermals can be prolonged below the horizontal portion, since water is unfrozen in certain cases,—and that the ice isothermals can be prolonged above the horizontal portion; for ice, at 0° say, can be suddenly compressed without melting in the interior. This suggests that the true form of the isothermals is a continuous curve, of the nature which Prof. J. Thomson has suggested in the case of liquids and their vapours.

If we suppose that the curves are continuous in the same manner for ice-water above 0°, then Prof. Carnelley's 'Hot Ice' would seem to be represented by the prolongations upwards of the ice isothermals beyond the horizontal line to where they meet the line of no pressure. The critical point, which certain assumptions roughly fix at about 14° C., would then be an upper limit, or rather above the limit, to the temperature of hot ice in a vacuum.

In conclusion, it is pointed out that the sealing-wax type of melting is probably similar to the change of ice into water below the lower, or above the upper, critical points, if these exist.

Melting and Freezing of the Ice-water Type at ordinary Temperatures and Pressures.

It seems to have been conclusively proved by experiment that, in bodies of the ice-water type, change of state, either from solid to liquid, or the reverse, takes place only at the surface, or at a surface separating dissimilar portions. This would also seem to follow from the fact that the change of state always requires a certain finite amount of energy to be abstracted from, or supplied to, the mass without alteration of temperature. In the middle of a homogeneous body, where the temperature varies gradually, we must have the energy per unit of volume a continuous quantity as we pass from point to point. Hence, when at any point there is sufficient energy per unit of volume to change the state, either the surrounding temperature must be far above the ordinary temperature for change of state, or the surrounding substance must occupy an intermediate condition between the two states.

On the former supposition we should certainly not have the ordinary change of state, though something of the sort may occur in the case of Dr. Carnelley's 'hot ice'; and in the latter we should have the sealing-wax type, and no signs of this have been observed.

Since, then, change of state is a surface phenomenon, we are led at once to connect it with the escape of molecules which we know to be always taking place from the surface, as indicated by the definite vapour-tension which the body possesses, whether solid or liquid. Now Regnault's experiments have shown that at 0° ice and water have the same vapour-tension, and at the same time a mixture of ice and water at that temperature maintains the same proportion between the two constituents as long as no heat is allowed to pass into or out of it; that is, as many molecules escape from the water into the ice as pass in the opposite direction from the ice into the water. We seem, then, to be justified in assuming that *the number of molecules coming up to a given surface with a sufficient velocity to escape is indicated by the maximum vapour-pressure at that temperature.*

Now suppose that we have a mixture of ice and water below 0 . Kirchhoff has shown (*Pogg. Ann.* vol. 103, p. 206) that below 0 the vapour-tension of water exceeds that of ice by ·044 mm. of mercury per degree; and his reasoning will equally prove that it falls below it by the same amount above 0 , if ice can exist at such a temperature. Prof. J. Thomson has subsequently (*Brit. Assoc. Report,* 1872, p. 24; *Proc. Roy. Soc.* 1873; *Nature,* vol. 9, p. 392) arrived at a similar conclusion independently. A proof differing in arrangement from Kirchhoff's, and following out rather the line indicated by Thomson, will be given below.

In a mixture, then, of ice and water below 0°, since the water has the greater vapour-tension, more molecules will cross the surface from the water to the ice than in the opposite direction. The ice will therefore gain, while the water loses. At the same time the molecules will possess less energy when arranged as ice. Hence the temperature of the whole will rise, and this rise will go on till 0° is reached, when there is once more equilibrium or till the whole is converted to ice, if that condition be previously reached. This seems sufficiently to explain the action of a small piece of ice dropped into water below 0°; and the fact that the change of state is a surface phenomenon seems to show that the presence of some ice is necessary to commence change of state.

If a mixture of ice and water at 0° be supplied with heat, as soon as the temperature rises ever so little above 0° the equilibrium of exchange is destroyed; for the vapour-tension of ice becomes greater than that of water, and therefore the number of molecules entering the water from the ice is greater than the number going in the opposite direction. But since the water arrangement requires more energy, heat is absorbed, and the mixture has a tendency to fall back to 0°.

Before going on to discuss the effect of pressure on the melting-point, I give a proof, with a somewhat more general result, of Kirchhoff's formula,

$$\frac{d\varpi'}{dt} - \frac{d\varpi}{dt} = \cdot 044 \text{ mm. of mercury,}$$

where ϖ' is the maximum vapour-tension of ice, and ϖ that of water.

Start with a volume v of water at temperature $-t°$. Let it evaporate, always at the temperature $-t°$, in a cylinder which it does not wet, at its maximum vapour-tension ϖ, which we suppose to be maintained by a piston. Let the ultimate volume of the water-vapour be V. Then the external work done in the expansion is $\varpi(V - v)$.

Now let the vapour further expand, always at the same temperature and in equilibrium with the pressure, till we have reached a volume V' at the maximum vapour-tension ϖ' of ice. Assuming Boyle's law to hold, the work done in this expansion is $\varpi' V' \log \frac{\varpi}{\varpi'}$; and this would be 0 if $\varpi = \varpi'$.

Now introduce a particle of ice at $-t°$ into the cylinder, and condensation into ice will go on till all the vapour has disappeared. If the ultimate volume of the ice is v', the work done on the substance is $\varpi'(V' - v')$.

Increase the pressure from ϖ' to $\varpi' + p$ till the melting-point is lowered to $-t°$. If κ' is the coefficient of cubic compressibility of ice, $\frac{p^2}{2}\kappa' v'$ is the work done in the compression. Introducing a drop of water, allow the whole to melt into water under the pressure $\varpi' + p$, the work done during the melting being

$$(\varpi' + p)\{v'(1 - p\kappa') - v(1 - p\kappa)\},$$

where κ is the coefficient of cubic compressibility for water.

Now let the water expand to its original volume v by gradually reducing the pressure to ϖ. The external work done is $\frac{p^2}{2}\kappa v$.

We now have the substance in its original state; and the cycle through which it has been taken was reversible at every step; therefore

$$\int \frac{dQ}{T} = 0.$$

But T is constant; therefore

$$\int dQ = 0.$$

Then the total external work is zero, or

$$\varpi (V - v) + \varpi' V' \log \frac{\varpi}{\varpi'} - \varpi' (V' - v')$$
$$- (\varpi' + p) \{v' (1 - p\kappa') - v (1 - p\kappa)\} - \frac{p^2}{2} \kappa' v' + \frac{p^2}{2} \kappa v - 0.$$

By means of the equation

$$\varpi V = \varpi' V',$$

and neglecting products of ϖ and κ, this reduces to

$$\varpi V \log \frac{\varpi}{\varpi'} = p \left\{ v' \left(1 - \frac{p\kappa'}{2}\right) - v \left(1 - \frac{p\kappa}{2}\right) \right\} \quad (\varpi \quad \varpi') v. \text{ ......(1)}$$

Neglecting the term $(\varpi - \varpi') v$, and putting for ϖV, $\varpi_0 V_0 a T$, where ϖ_0, V_0 are the pressure and volume at 0° C., and T the absolute temperature, we have

$$\log \frac{\varpi}{\varpi'} = \frac{p \left\{ v' \left(1 - \frac{p\kappa'}{2}\right) - v \left(1 - \frac{p\kappa}{2}\right) \right\}}{\varpi_0 V_0 a T}. \text{ ....(2)}$$

For temperatures near 0° C. we may neglect products of p and κ, and we obtain as an approximation

$$\frac{\varpi}{\varpi'} = 1 + \frac{p (v' - v)}{\varpi' V'},$$

or

$$\varpi - \varpi' = p \frac{v' - v}{V'}. \text{ ....(3)}$$

At 0°, $\quad v' - v = \cdot 087, \quad V' = 209037,$

and the pressure required to lower the melting-point t is $\frac{760t}{\cdot 00733}$ mm. of mercury by the well-known formula. Substituting in equation (3), we get

$$\varpi - \varpi' = \cdot 044 t,$$

or

$$\frac{d\varpi'}{dt} - \frac{d\varpi}{dt} = \cdot 044 \text{ mm. of mercury, ...(4)}$$

which is Kirchhoff's result.

If the temperature be much below 0° C., we cannot make these approximations without further examination, as the terms containing κ and κ' in (2) may rise into importance.

It may be noticed that (2) could be used as an equation to determine p, the pressure required to produce a fall of the melting-point to T, if there were any accurate experimental method of measuring ϖ and ϖ'.

Effect of Pressure on the Melting-point.

If we are right in regarding the change from the solid to the liquid state as an exchange-phenomenon in which the rate of exchange is indicated by the vapour-tension, we ought to be able to show that the pressure which lowers the melting-point to a certain temperature will so alter the rate at which the two states of the substance give off molecules from their surfaces, that at that temperature there will be an equilibrium of exchange. That is, we ought to be able to show that pressure alters the vapour-tensions of the two states, but alters them by different amounts, so that the equality of vapour-tensions now occurs at the new melting-point.

Now in the ordinary case, where the vapour-tension is measured we have the substance only under the pressure of its own vapour; but in the rise or fall of a liquid in a capillary tube we may have a substance in contact with its own vapour when the substance is at a very different pressure from the vapour in contact with it.

Sir William Thomson has shown (*Proc. Roy. Soc. Edinb.* 1870, vol. 7, p. 63; Maxwell's *Heat,* 1877, p. 287) that if a liquid rises in a capillary tube so that its surface is concave upwards, and (we may add) the pressure of the liquid is less than at the plane surface, then the equilibrium vapour-tension is less than at the plane surface. If the liquid falls in the tube, so that the surface is convex and the pressure greater than at the plane surface, then the equilibrium vapour-tension is greater. It has been supposed that this difference of vapour-tensions is due to the curvature of the surface; and FitzGerald has suggested that we may thus perhaps obtain a connection between 'two apparently unrelated quantities,' the evaporation and the surface-tension (*Phil. Mag.* (5), vol. 8, p. 384). But while a very slight impurity in a liquid can greatly alter the surface-tension, it has not been shown that it alters the evaporation to the same degree. I think that we must look for the explanation elsewhere than in the curvature of the surface; and I shall endeavour to show that we may account for the effect by the difference of pressures of the liquid at the curved and plane surfaces. The curvature of the surface is then, as it were, an accidental accompaniment of the difference of pressure, and not the cause of the variation in the vapour-tension. We might therefore expect to find the variation taking place also at flat surfaces if the pressure be altered, and with solid as well as with liquid bodies. We cannot directly investigate the vapour-tension of flat surfaces under pressure; but I shall assume that we may here take, instead, the rate at which exchange takes place when the solid and liquid are in contact with each other.

Sir W. Thomson's formula is

$$p = \varpi - \frac{2T\sigma}{r(\rho - \sigma)}, \quad \text{.............................}(5)$$

where

p is the vapour-tension in contact with the concave surface,
ϖ is the vapour-tension in contact with the plane surface,
T is the surface-tension of the liquid,
ρ and σ the densities of the liquid and its vapour respectively,
r the radius of curvature of the curved surface.

If P be the difference between the hydrostatic pressures just beneath the curved surface and just beneath the plane surface, equation (5) may easily be put in the form

$$p = \varpi - P\frac{\sigma}{\rho}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(6)$$

or a pressure P in the liquid increases the vapour-tension by an amount $P\frac{\sigma}{\rho}$.

The following proof of this formula, $p - \varpi - P\frac{\sigma}{\rho}$, is, I believe, applicable to both solids and liquids, and obtains a more general form for the result.

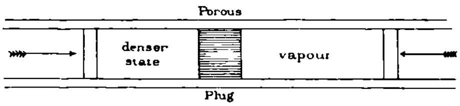


Fig. 1.

Let a volume v of a body (solid or liquid) be in a perfectly conducting cylinder (Fig. 1) so arranged that the temperature is always constant. A porous plug, which the substance if liquid does not wet, is in the cylinder; and the holes in the plug are to be so fine that any required pressure can be applied to the liquid without forcing it beyond the further surface of the plug, the curved surface of the liquid there withstanding the pressure. A piston to which pressure can be applied is in contact with the substance; and beyond the plug is another moveable piston to which any pressure can be applied, the arrows in the figure indicating the direction in which the external pressures are applied to the pistons.

Let the volume of the substance in the denser state at the pressure of its normal vapour-tension ϖ for the given temperature be v. Let V be the volume of the whole as vapour at the pressure ϖ. Let the equilibrium vapour-tension when the denser state is subjected to a greater pressure $\varpi + P$ be p, and let the volume of the whole as vapour at the pressure p be V'. Let the coefficient of cubic compressibility be κ. Now take the body through the following cycle.

Increase the pressure to $\varpi + P$ on the left-hand piston, and then let the substance evaporate through the plug to the right-hand side, pushing out the piston there at pressure p till the whole is evaporated to a volume V'. If

p be greater than ϖ, let the vapour expand from V', always in equilibrium with the pressure, finally arriving at a volume V and pressure ϖ. Now cover the porous plug, and, if necessary, commence condensation by introducing a small amount of the substance. Push in the right-hand piston at the pressure ϖ till the whole is condensed to volume v.

We have now conducted the substance through a cycle each step of which is reversible*. Then

$$\int \frac{dQ}{T} = 0.$$

But temperature T is constant; then

$$\int dQ = 0,$$

and the external work is, on the whole, zero. This gives us

$$\left(\varpi + \frac{P}{2}\right) P\kappa v + (\varpi + P)\, v\,(1 - P\kappa v) - pV' - \varpi V \log\frac{p}{\varpi} + \varpi\,(V - v) = 0. \quad \text{...(7)}$$

But since, at low temperatures such as we are here considering, Boyle's law is almost exact, we have

$$\varpi V = pV'.$$

Then, neglecting terms containing $\varpi\kappa$,

$$\varpi V \log\frac{p}{\varpi} = Pv\left(1 - \frac{P\kappa}{2}\right),$$

or

$$\frac{p}{\varpi} = e^{\frac{Pv}{\varpi V}\left(1 - \frac{P\kappa}{2}\right)}. \quad \text{.......................(8)}$$

For ordinary values of P this gives

$$p - \varpi = \frac{Pv}{V} = \frac{P\sigma}{\rho}, \quad \text{...........................(9)}$$

which agrees with Sir W. Thomson's result in equation (6).

It may be worth while to point out the following result of the reasoning on which the above proof is based.

In a quantity of liquid at a uniform temperature, the number of molecules interchanged across a surface will increase as we descend, owing to the increase

* It seems difficult to imagine a plug which would satisfy the condition of reversibility for the solid under great pressure in contact with its vapour. Perhaps the following would answer the requirements, if an ordinary porous plug is insufficient. Suppose the solid in a finely-divided state, and contained in a liquid which wets it but is of a very slightly greater specific gravity, and whose vapour-tension is negligible. During evaporation turn the cylinder with the vapour-chamber upwards. The particles of solid will rise up through the pores, and a small fraction of their surface will protrude, but they will otherwise be subjected to a pressure $\varpi + p$. For condensation reverse the cylinder. As the solid condenses on the surface it will rise up as fast as it is formed, and so increase the volume of the chamber and force back the piston.

of pressure. If near the surface the number be proportional to the vapour-tension at the surface, then at any depth the number will be proportional to the pressure in an atmosphere of vapour at that level which, at the level of the surface, has the pressure of the vapour in equilibrium; that is, the liquid will behave as a non-vaporising solid through whose interspaces the vapour can move freely.

Assuming, then, that equation (9) holds both for solids and liquids, let us apply it to the case of ice and water in contact with each other at a temperature $-t^\circ$ and at a pressure P, such that $-t^\circ$ is the melting-point.

Let ϖ be the normal vapour-tension of water at $-t^\circ$,
ϖ' ,, ,, ,, ice at $-t^\circ$,
p be the altered vapour-tension of water,
p' ,, ,, ,, ice,
ρ the density of water, v its specific volume,
ρ' ,, ,, ice, v' ,, ,,
σ ,, ,, their vapour, V its specific volume.

Taking σ and V as the same for ice and water as an approximation, then equation (9) gives us

$$\left.\begin{aligned} p &= \varpi + P\frac{\sigma}{\rho} - \varpi + \frac{Pv}{V}. \\ p' &= \varpi' + P\frac{\sigma}{\rho'}, \quad \varpi' + \frac{Pv'}{V}. \end{aligned}\right\} \qquad \ldots\ldots\ldots\ldots(10)$$

Subtracting, we have $\quad p - p' - \varpi - \varpi' - P\dfrac{v' - v}{V}. \qquad \ldots\ldots\ldots\ldots(11)$

But by equation (3) we have

$$\varpi - \varpi' = P\frac{v' - v}{V};$$

then $$p - p' = 0. \qquad \ldots\ldots\ldots\ldots(12)$$

Or, under the pressure P at the melting-point, the vapour-tension of ice equals that of water, and there is an equal interchange of molecules taking place. According to this, then, we may thus regard the alteration of melting-point by pressure. The pressure increases the number of molecules given off from the surfaces in contact with each other in both states; but the increase is greater in the case of the less dense state. Now, in the case of ice-water, ice is the less dense state, and below 0° it has the less vapour-tension. Hence a sufficiently great increase of pressure, while increasing both vapour-tensions, can make that of ice overtake that of water, or can lower the melting-point. For paraffin, the liquid is the less dense state. Then, increase of pressure can only render the two vapour-tensions equal above the normal melting-point when the liquid vapour-tension is less than that of the solid.

Suppose now only one of the two states (the ice) to be subjected to increase of pressure. For instance, let the ice be compressed on a porous plate through which the water can circulate freely. Then the pressure increases the rate at which molecules escape from the ice into the water, but does not affect the rate of escape of the water-molecules into the ice, and a much less pressure will suffice to produce equilibrium of exchange for a given temperature below 0° than when both ice and water are subjected to the pressure.

To calculate the fall in melting-point produced by a pressure P' on the ice alone, we have, instead of (10),

$$\left.\begin{aligned} p &= \varpi, \\ p' &= \varpi' + \frac{P'v'}{V}. \end{aligned}\right\} \qquad \text{.........................(13)}$$

If we have $p = p'$, we have the melting-point; and in this case, by subtracting, we obtain

$$\varpi - \varpi' = \frac{P'v'}{V}. \qquad \text{..............................(14)}$$

Now the pressure required to lower the melting-point to the same degree when both ice and water are compressed is given by

$$\varpi \quad \varpi' = \frac{P\,(v' - v)}{V}; \qquad \text{...........................(15)}$$

or

$$\frac{P'}{P} = \frac{v' - v}{v}$$

$$= \cdot 087. \qquad \text{..................................(16)}$$

Or the fall in melting-point caused by a given pressure on the ice alone is about $11\frac{1}{2}$ times as great as when both ice and water are compressed. That is, 1 atmosphere lowers the melting-point about ·0843° C., and 11·7 atmospheres lower it 1° C. This result may be obtained in the same way as Prof. J. Thomson's formula, on the supposition that the process is reversible; but as I was led to the result by the above considerations, I have given only this proof.

This seems to have an important bearing on ordinary cases of regelation, when two pieces of ice are brought into contact at one or two points. About that point the ice will be subjected to great pressure; but the melted water is not necessarily subject to the pressure, and accordingly the melting-point may be lowered by $11\frac{1}{2}$ times as much as has been formerly supposed. I have made some experiments to test this result; and the best arrangement I have yet devised has been the following: A block of ice, fitting in a hollow iron cylinder with open ends, was laid on a bed of sand on the top of which was placed one junction of a copper-iron thermocouple; the other junction was placed in melting ice. When the two junctions reached the same temperature, as indicated by a galvanometer in the thermocouple circuit, pressure was applied to the ice by a hydraulic press. The water from the melting of the ice was able to escape freely through the sand, and was therefore only at atmospheric pressure. The results so far have been very variable, sometimes indicating no greater lowering of the melting-point than that usually assumed —·0073° per atmosphere. But in several cases the lowering has been decidedly greater. The following experiment gives the greatest value I have yet obtained for the lowering of the melting-point. The galvanometer-deflection per 1° difference in the temperature was determined by separate experiments to be 9·4 divisions.

Time, April 30	Pressure, in atmospheres	Galvanometer deflections, in divisions	Temperature of the cooler junction	Calculated temperature, at ·0073 per atmosphere
h m				
12 51	18	4·5	·48 C.	- ·13 C.
12 53	18	5·3	- ·56	·13
12 57	18	5·0	- ·53	·13
1 30	9	2·5	- ·27	- ·065
1 32	9	3·3	- ·34	·065

It will be seen, by a comparison of the last two columns, that the lowering here was four or five times that given by the usual formula. I have not thought it necessary to give details of the other results, as I have not yet had time to investigate the causes of failure. I hope to pursue the subject shortly*.

Perhaps the following imaginary experiment may serve as a simple illustration of the last two sections. Suppose two cylinders, one containing ice, the other water at the same temperature, to be connected above by a tube through which the vapour can pass, and let them be in contact with their own vapour only.

At 0°, or rather at + ·0073 , their vapour-tensions being equal, as soon as the pressure reaches 4·6 mm. then the ice and water will remain unaltered in amount as long as no heat is allowed to pass into or out of the cylinders. If the temperature be kept slightly below 0°, then, since the vapour-tension of water is now greater than that of ice, the water will gradually distil over into the ice-vessel and there condense as ice, the average temperature rising. If the temperature be kept constant, however, the whole of the water will in time go over into the ice-vessel. If the temperature be slightly above 0 (supposing it possible still to keep the ice solid), then the ice has the greater vapour-tension and will gradually distil over into the water-vessel, and the average temperature will fall. In time, if the temperature be kept above 0 , the whole of the ice will go over into the water-vessel.

* [See following letter. Ed.]

If, now, the ice and water be subjected to pressure by porous pistons which the water does not wet (the pressure in each cylinder being the same), then, if the temperature be 0°, an increase of pressure will cause more evaporation from the ice than from the water; that is, the ice will distil over into the water-cylinder and form water there. To obtain equilibrium again, the temperature must be lowered to such a point that the pressure makes the two vapour-tensions once more equal, when the ice and water will remain unaltered in amount—that is, the melting-point will be reached. If now the ice alone be subjected to pressure, its vapour-tension will be increased while that of the water remains the same. And now the pressure required to produce equilibrium of vapour-tensions at a given temperature below 0° will only be about $\frac{2}{23}$ of that required when both are subjected to the same pressure.

The suppositions which I have made amount to this—that if the space filled with vapour be abolished and the ice and water be brought directly into contact with each other, then the rate of escape of molecules will be the same as before in each case, or bear the same proportion to it.

Isothermals of Ice-water: Critical Points.

If we draw the isothermals for ice and water on a pressure-volume diagram, they are of the general form shown in Fig. 2, though the figure is entirely out of proportion.

If we may assume that the compressibility of water is considerably greater than that of ice, the horizontal part of the isothermals representing a mixture of ice and water will increase as the temperature falls below 0°, at least just at first. Then, if we call the line passing through the points where the isothermals turn to or from the horizontal part the ice-water line, this line will at first diverge as the temperature falls. Now, while ice contracts on cooling, its coefficient of expansion between − 19° and 0° being given as ·000122 by Brunner, Despretz has shown that water expands on cooling below 0° even more than it expands for an equal rise above 8°. Hence the isothermals for ice and water approach each other at ordinary pressures as the temperature falls.

Using Brunner's coefficient for ice, and for water Hällström's formula (Jamin, *Cours de Physique*, vol. 2),

$$\frac{v_0}{v_t} = 1 + \cdot 000052939t - \cdot 0000065322t^2 + \cdot 00000001445t^3,$$

and supposing that water could be cooled without freezing, it will be found that between − 120° and − 130° ice and water would have the same specific volume. This might lead us to suspect that the divergence of the two branches of the water-ice line would not continue if we could examine the isothermals at very low temperatures and high pressures, and that, as the temperature fell, the two states would at some point begin to approach (that is, the horizontal part of the isothermals would decrease), and that ultimately ice would pass gradually into water without any abrupt change of volume—that

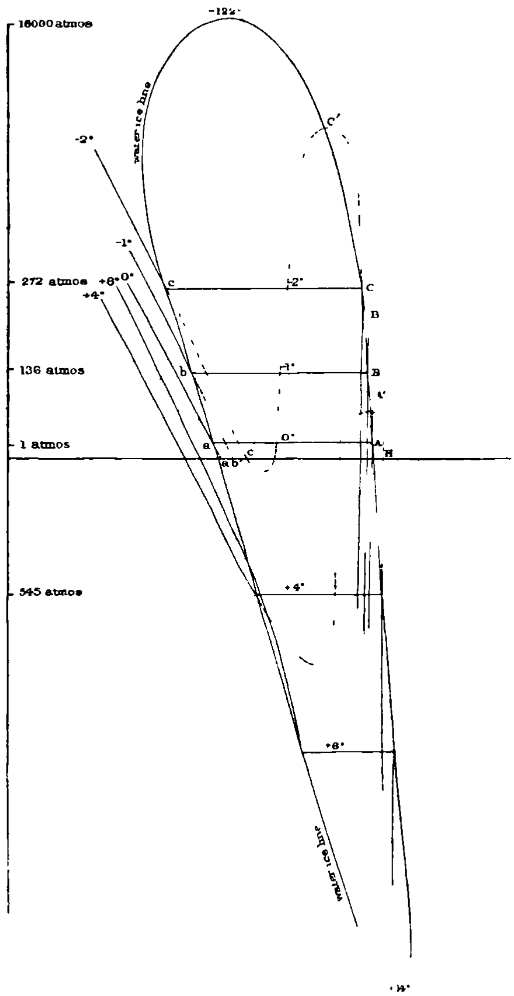


Fig. 2.

s, there would be a critical point. Below this critical point ice and water would probably be identical.

A similar conclusion is arrived at from the latent-heat equation. On the supposition that at the critical point the latent heat vanishes, the temperature given by that equation is $-122°{\cdot}5$ C., with a pressure of over 16,000 atmospheres (Baynes, *Thermodynamics*, p. 169).

It is usually assumed that we must stop the isothermal at the base-line of no pressure. But we know that water can be subjected to a negative pressure; as, for instance, when it rises in a capillary tube in a vacuum, or when it adheres to a barometer-tube at a height greater than that of the barometric column. It seems probable that, if perfectly freed from foreign gases, it might even be subjected to a very high negative pressure without the particles being torn asunder. So, too, a mixture of ice and water might probably be subjected to tension. It seems at least worth while to draw the isothermals for ice and water on such a supposition.

Prof. J. Thomson's result for the alteration of the melting-point by pressure would hold for at least a short distance above 0° when we replace pressure by tension. Assuming it to hold for 4°, we should have to put on a tension of $4 \div \cdot 00733$ atmosphere $= 545$ atmospheres. But if the expansion of water under a tension equals its compression under an equal pressure, the expansion is about $\frac{1}{21000}$ per atmosphere*; so that the volume of the water at 4°, under a tension of 545 atmospheres, will be 1·026. The ice, whose volume at 4° under no pressure would be 1·088, probably will not expand nearly so much under tension. The change of volume on melting will therefore probably be not very far from

$$1 \cdot 088 - 1 \cdot 026 = \cdot 062,$$

against a change at 0° of ·087. Then the two branches of the ice-line will converge very considerably for temperatures above 0° and with negative pressures. At this rate of convergence the meeting-point is at about 14° C. At higher temperature the ice would pass gradually into water—that is, we should here have another critical point,—the two critical points being at opposite ends of the closed curve which represents the water-ice line.

On considering the isothermals below 0°, it will be noticed that the water-isothermals, at least as far as that for $-20°$, can be prolonged downwards past the horizontal line to meet the line of no pressure; for Despretz succeeded in cooling water to $-20°$ in thermometer-tubes without freezing. These prolongations are represented by aa', bb', cc' (Fig. 2). Similarly the ice-isothermals can be at least slightly prolonged upwards past the horizontal line. For, suppose we take a block of ice at 0° and suddenly subject it to great pressure. Since it expands on heating, then sudden compression produces, if anything, a slight rise in the temperature. At the same time the melting-point is lowered, and the ice begins to melt at the surface, and in time the whole will be lowered to the new melting-point. But just at first, and until it falls to that temperature, we have the ice on the prolongation of the isothermals upwards as at AA' or BB' in Fig. 2. In a certain sense, then, we have 'hot ice.'

* Might not the truth of this supposition be tested by the propagation of sound through the water above a barometric column at a negative pressure?

Since, then, the water-isothermals may be prolonged downwards and the ice-isothermals upwards, we may probably here adopt Prof. J. Thomson's suggestion as to the true shape of the isothermals in the case of liquid-and-gas mixtures (*Brit. Assoc. Report*, 1871, p. 30; Maxwell's *Heat*, p. 125). This is indicated by the dotted line for $-2°$ in the figure. If the isothermals also have this shape above 0° (as indicated by the dotted line for the 4° isothermal), then at first the ice-isothermals will be prolonged upwards to meet the line of no pressure, as, for instance, that of 4° at H. This seems to be the place where we must put Dr. Carnelley's 'hot ice,' on the diagram, if its temperature be really proved to be above 0°.

But if the critical point for the higher temperature exist, it is evident that, before this temperature is reached, the prolongations of the ice-isothermals will cease to reach up to the line of no pressure, and the limit to the temperature of hot ice in a vacuum is that of the last isothermal which touches the line of no pressure. To obtain ice at still higher temperatures, it would apparently have to be subjected to great tension. If the above calculation for the critical point is at all near the truth, then the highest temperature possible for ice in a vacuum is something below 14 C.

The view here advocated as to the nature of the melting of ice, would show that its fixity is as much a 'constant accident' as the fixed boiling-point of water. If we have a piece of ice at any temperature and allow no water to form on its surface, then I see no reason why it should melt if heat be supplied to it by conduction from bodies which, when melted, it does not wet. I think, then, we ought to expect its temperature to rise, as Dr. Carnelley has apparently found to be the case.

Dr. Lodge has pointed out (*Nature*, Jan. 20, 1881) that, as far as we know, 'there is no definite subliming-point for a solid, any more than there is a definite evaporating-point for a liquid.' Hence, with such a mode of supplying the heat as above described, the temperature might perhaps be expected to rise to that of the last isothermal which reaches the line of no pressure. When it has reached this point the whole will be in an unstable state, and we might expect a further supply of heat to cause a sudden change into water. If, however, at any point in this process of raising the temperature the vapour-tension is allowed to rise nearly to its maximum, it will exceed that of water, which has a lower maximum; then a layer of water will be formed on the ice, and we shall have melting with a tendency of the temperature towards 0°.

The Sealing-wax Type of Melting.

We have seen that there is some reason to suppose that ice would pass gradually into water at a sufficiently low temperature and with sufficiently high pressure; that is, there would be no abrupt change of volume at a constant temperature, and no definite latent heat. But these are just the characteristics of the melting of substances of the sealing-wax type; and I think it exceedingly probable that we have such substances at temperatures below their critical points, or at least that they are analogous to water-ice below its critical point. If sealing-wax have a critical point, then if we start with some in the solid state at ordinary temperature, and while raising the temperature we increase the pressure so as always to keep it solid till above the critical point, if we reduce the pressure again to a certain point and at the same time a small amount of liquid sealing-wax be introduced, we ought to have a liquefaction of the whole with a finite expansion of volume; that is to say, we should have changed the ordinary sealing-wax type of melting into the ice-water type. It might, perhaps, be possible to test the truth of this supposition experimentally.

[*Phil. Mag.* **12**, 1881, p. 232.]

MASON COLLEGE, BIRMINGHAM.
August 4, 1881.

GENTLEMEN,

In a paper on the Change from the Solid to the Liquid State, which appeared in the July number of the *Philosophical Magazine*, I described some experiments on the temperature of melting ice when the ice is under much greater pressure than the water in contact with it. In those experiments I thought I had obtained some confirmation of the preceding theory, that the melting-point would be lowered much more than when both ice and water are submitted to the same pressure.

I have lately repeated the experiments more fully; but I do not find my previous results confirmed. I can only suppose that they were wrong, and that the error arose from insufficient precautions as to the temperature of the thermo-junction which was placed in melting ice. I now find that the water freezes in the pores of the sand around the junction placed under the compressed ice. As, therefore, the conditions necessary to success are not realised, there is no reason to expect any lowering beyond that which takes place when both ice and water are submitted to the same pressure.

I regret that the hasty publication of my previous experiments should render it necessary for me to ask you to publish this explanation.

I remain, Gentlemen,

Yours faithfully,

J. H. POYNTING.

40.

NOTE ON A METHOD OF DETERMINING SPECIFIC HEAT BY MIXTURE.

[*Birmingham Phil. Soc. Proc.* **4** (1883), pp. 47–54.]

[*Read* November 8, 1883.]

In the ordinary method of determining the specific heat of solids by mixture, the solid experimented upon is heated and then dropped into cold water. The heat given up by the solid raises the temperature of the water by a measurable amount, and from this the specific heat can be calculated. The chief difficulty of the experiment lies in the heating of the solid to a definite measurable temperature, and in the preservation of that temperature till the moment when it enters the liquid. In the following method this difficulty is avoided by having the liquid hot and the solid cold. Other sources of error, however, are introduced by the change, but I think that their effect is so much lessened by the arrangement of the calorimeter and the mode of experiment, that the apparatus may be of use, at least for teaching purposes. I do not suppose that it can compare in accuracy with the Bunsen calorimeter, or with a thoroughly well arranged Regnault's apparatus, but these are both far more costly and difficult to use.

The calorimeter consists of a narrow copper vessel $1\frac{3}{4}$ in. diameter and 4 in. deep (Fig. 1). In this fits a shallow bucket, with the bottom well perforated (Fig. 2). A wire arch (w, w) rising above the top of the calorimeter is attached to the bucket, and by this it can be pulled up and down. A string fastened to the top of the wire arch passes over a pulley fixed above the calorimeter, and then to the observer, who may read the thermometer (Fig. 1, t) from a distance with a telescope. By pulling the string the bucket is raised, and it is pulled down again on relaxing the string by india rubber bands (b, b) stretching from the ends of a light wooden cross-piece (c, c) attached to the wire arch, down to hooks on the base-board.

Four or five copper bands (Fig. 3) are attached to the inside of the calorimeter to form a cavity for the thermometer, and to protect it while allowing free circulation of the water. The bucket is shaped with a deep groove down one side (Fig. 2) to enable it to pass these bands.

The course of an experiment is as follows: Suppose we wish to find the specific heat of lead. A known weight of shot is placed in a small beaker on the table, so as to be nearly at the temperature of the room, and the temperature is exactly determined by a thermometer placed in the shot. Some hot water is placed in the calorimeter, but the temperature is observed at intervals of half-a-minute, the bucket being worked up and down all the time to stir the water and keep it at the same temperature in all parts. Four

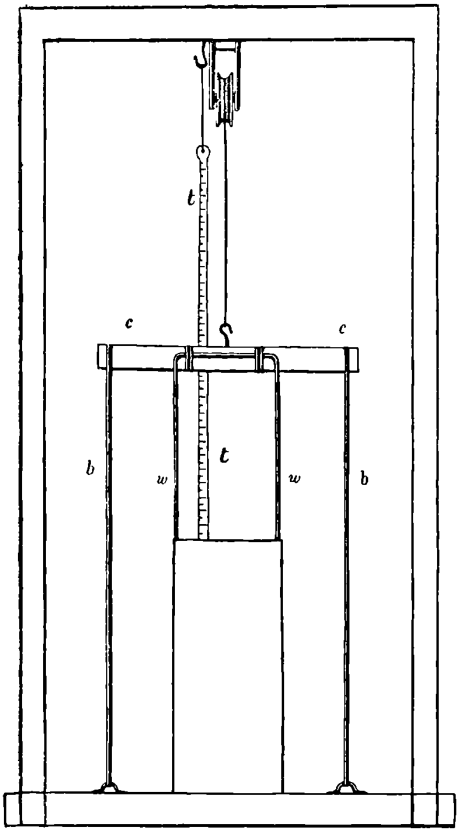


Fig. 1.

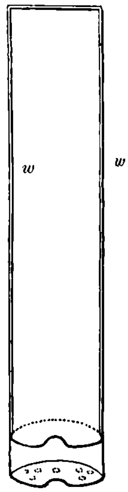


Fig. 2.

Fig. 3.

temperatures are thus observed, and at the next half-minute the shot are poured into the bucket—preferably by an assistant. The stirring must go on without intermission, as the shot are colder than the water at first; if they remain at the bottom, the water coming in contact with them tends to remain there also. It is to keep up a thorough circulation that the stirrer is in the form of a bucket, in which the substance may be placed. The shot will probably not arrive at the temperature of the water in much less than a minute, so that after the mixture a minute is allowed to elapse before another reading of the thermometer is taken, and then three more readings are taken at succeeding minute-intervals. The calorimeter with its contents is then weighed, and the weight of the vessel and shot subtracted to give the weight of the water.

The two series, each of four readings, must now be made, to give the temperature of the water the instant before mixture and the temperature the instant after mixture, on the supposition that the heat is instantly taken up by the solid. The difference between these two is the fall in temperature due to the heat absorbed by the solid, and will enable us to determine its specific heat.

How this may be done will be seen most simply, I think, by the following graphic method:

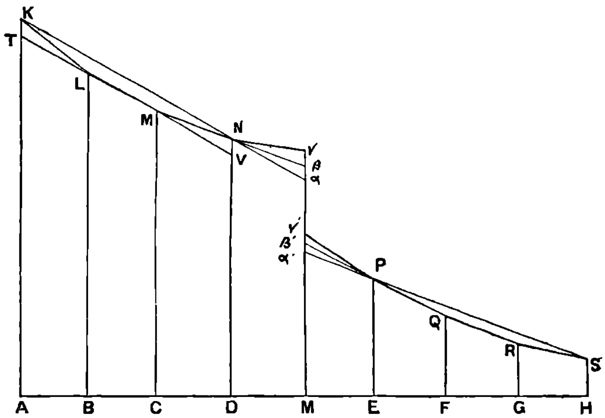


Fig. 4.

Suppose that we are able to observe the temperatures perfectly accurately, then we shall find that the rate of cooling in successive intervals decreases as the liquid approaches the temperature of surrounding bodies, but the decrease is—quite nearly enough for our purpose—the same for each succeeding interval up to the moment of mixture.

In Fig. 4 let the times be represented by equal intervals along AMH, so that $AB = BC = CD = DM = \frac{1}{2}$ minute, where M is the moment of mixture. After mixture we may reduce the scale so that $ME = EF = FG = GH = 1$ minute. Let AK, BL, CM, DN represent the observed temperatures before, and EP, FQ, GR, HS those after mixture. Produce LM both ways to meet AK and DN in T and V respectively. Then KT is the decrease in the rate of cooling between the first and second intervals, NV that between the second and third intervals, and we are supposing this decrease the same, so that $KT = NV$. If, then, K and N be joined, KN is parallel to LM. It follows that if KN produced meets the ordinate through M in α, and MN produced meets it in β, the triangle NMV = the triangle $\beta N\alpha$, and therefore $\alpha\beta = NV$. But β is the point arrived at if the cooling is the same in the fourth as in the third interval; in reality it is less by the decrease per interval, or the actual temperature at the moment of mixture is $M\gamma$ where $\beta\gamma = \alpha\beta$. It is also seen that $\alpha\gamma$ = the sum of the distances by which L and M are below the line KN.

Now, in practice we do not observe the temperatures accurately, but we shall probably arrive as nearly as possible at the point γ by plotting on squared paper the four points K, L, M, N as actually observed, joining KN and producing it to cut the ordinate through M in α, and then drawing $\alpha\gamma$ = the sum of the depths of L and M below the line KN. A precisely similar construction may be given for arriving at the temperature after mixture by drawing SP to meet $M\gamma$ in α', and then drawing $\alpha'\gamma'$ = the sum of the depths of Q and R below PS; $\gamma\gamma'$ is then the difference required.

The arithmetical formulae equivalent to these constructions may easily be arrived at. Denoting by the letters the corresponding observed temperatures we find

$$M\gamma = \frac{2K + 7N - 3(L + M)}{3},$$

$$M\gamma' = \frac{2S + 7P - 3(Q + R)}{3}.$$

If other numbers of readings or other intervals either from choice or accident are taken, it is possible to construct formulae to give the result.

After the difference $\gamma\gamma'$ is determined the usual formula for finding the specific heat is employed. I append an experiment with full details to serve as an example.

As far as I have yet tested the apparatus it works fairly well. As here described it will only serve as a class experiment, but it will be easy to improve the calorimeter by suspending it by silk cords within another polished metal vessel and using a thermometer with a more open scale.

Details of Experiment.

Weight of calorimeter, stirrer, and thermometer	177·29 grms.
Weight of shot	126·38 grms.
Water-equivalent of calorimeter about ...	6·00 grms.
Temperature of shot before mixture	17·2° C.

Some boiling water was put into the calorimeter to fill it to about two inches, and when it had been left for a few minutes temperatures were taken as follows:

Times			Temperatures
4 h.	15 m.	0 sec.	$70{\cdot}40 = K$
4 h.	15 m.	30 sec.	$69{\cdot}45 = L$
4 h.	16 m.	0 sec.	$68{\cdot}45 = M$
4 h.	16 m.	30 sec.	$67{\cdot}45 = N$
4 h.	17 m.	0 sec.	shot poured in
4 h.	18 m.	0 sec.	$62{\cdot}50 = P$
4 h.	19 m.	0 sec.	$61{\cdot}15 = Q$
4 h.	20 m.	0 sec.	$59{\cdot}80 = R$
4 h.	21 m.	0 sec.	$58{\cdot}55 = S$

Temperature before mixture at 4 h. 17 m.

$$= \frac{2K + 7N}{3} - (L + M) = 66{\cdot}42^\circ.$$

Temperature after mixture at 4 h. 17 m.

$$= \frac{2S + 7P}{3} - (Q + R) = 63{\cdot}92^\circ.$$

Fall of temperature of water due to heat absorbed by the shot	$= 66{\cdot}42 - 63{\cdot}92 =$	$2{\cdot}50^\circ$.
Rise of temperature of shot	$= 63{\cdot}92 - 17{\cdot}2$	$= 46{\cdot}72^\circ$.
Weight of calorimeter and water and shot ...	$= 377{\cdot}70$ grms.	
Subtracting weight of calorimeter and shot ...	$= 303{\cdot}67$ grms.	
Weight of water	$= 74{\cdot}03$ grms.	
Adding water-equivalent of calorimeter, total ...	$= 80{\cdot}03$ grms.	

If σ = specific heat of shot, $\sigma \times$ weight of lead $\times$ rise in temp. = (weight of water + cal. equiv.) $\times$ fall in temp.; or

$$\sigma = \frac{80{\cdot}03 \times 2{\cdot}50}{126{\cdot}38 \times 46{\cdot}72} = 0{\cdot}0339.$$

Another experiment conducted in the same manner gave

$$\sigma = 0{\cdot}0314.$$

Mean value, $\sigma = 0{\cdot}0327.$

The specific heat of lead (Everett's *Units and Physical Constants*, p. 82) is 0·0314.

41.

OSMOTIC PRESSURE.

[*Phil. Mag.* **42**, 1896, pp. 289–300.]

Since the osmotic pressure of a solution is of the same order as the 'gas pressure' of the dissolved substance at the same density, we are naturally tempted to think of it as an extra pressure produced by the motion of the dissolved molecules. But if we start from this supposition we soon find ourselves surrounded by the difficulties of the dissociation-hypothesis. These are so great that it appears worth while to examine our ideas of liquid structure in the hope that they will suggest to us some hypothesis which will free us from the necessity of assuming dissociation.

I shall try to show in this paper that osmotic pressure may be accounted for as an indirect result arising, not from dissociation but from its very opposite, the greater complexity of the molecules in the solution, due to some kind of combination between salt and solvent.

The facts of liquid viscosity, diffusion, and surface conversion to vapour may apparently be represented by imagining a liquid to be, in the main, a solid structure, inasmuch as the molecules cohere and resist strain of any kind. But the molecules have so much energy, potential or kinetic or both, that they are not very far from instability. In a mass of connected molecules irregularly distributed and irregularly vibrating, concentrations of energy must occur, and at the points of concentration individual molecules may receive so much energy that they are able to do the work needed to free them from their immediate surroundings. Such molecules will travel off, and as they lose their energy will form new connections with new surroundings. Thus the solid structure is continually breaking down and renewing itself. If we impose a shear-strain on the structure, the strain will of course disappear with the structure in which it is produced. But the breaking down will always lag slightly behind the imposition of the shear, and the still surviving shear-strain will be accompanied by a resistance the same in kind as the resistance to shear in a solid, though in a liquid it is only recognised as viscosity. This is the view first set forth by Poisson and developed by Maxwell, and it is to be noted that it gives an explanation of liquid viscosity entirely different from the diffusion explanation which so satisfactorily accounts for gaseous viscosity.

We may obtain an expression for the coefficient of viscosity by the following method, which is perhaps rather simpler than that of Maxwell. We must assume that a certain fraction, say λ, of the molecules of the liquid get free per second, and that this fraction remains practically the same when the liquid is sheared. Hence if s is the strain still existing at any instant, it is breaking down at the rate λs per second. If the liquid is moving steadily in parallel planes perpendicular to an axis along which x is measured, and if the velocity is v at a distance x from the reference plane, $\frac{dv}{dx}$ is the rate at which shear is being imposed on the liquid. But since the steady state is reached the rate of imposition equals the rate of decay, or

$$\frac{dv}{dx} = \lambda s. \quad \text{(1)}$$

If n is the coefficient of rigidity of the structure, the stress due to s is ns, and by our supposition this is the viscous stress, or

$$\eta \frac{dv}{dx} = ns, \quad \text{(2)}$$

where η is the coefficient of viscosity. Dividing (2) by (1) we obtain

$$\eta = \frac{n}{\lambda}. \quad \text{(3)}$$

We may compare the liquid breakdown here imagined with that which must occur in an electrolytic conductor. If D is the 'displacement' or 'induction' in an electrolyte, and if μ is the factor of decay per second, μD is the quantity disappearing per second and dissipating its energy as heat. This may be equated in the steady state to the new 'displacement' or 'induction' introduced per second per square centimetre, or to the current-density C. Hence

$$C = \mu D = \frac{\mu K E}{4\pi}, \quad \text{(4)}$$

where E is the slope of potential, and K is the specific inductive capacity. But Ohm's law gives us

$$C = \frac{E}{\rho}, \quad \text{(5)}$$

where ρ is the specific resistance; whence

$$\rho = \frac{4\pi}{\mu K}. \quad \text{(6)}$$

Returning to equation (3), we see that if n is constant, η varies inversely as λ. For instance, when the temperature rises the molecules have more energy, the breaking down of structure is more frequent, and λ is greater. Probably n is not very much altered, though it doubtless tends to decrease. Hence η should decrease, and this is in accordance with observation. On the other hand, when a salt is dissolved in a liquid, if, as we are going to suppose, it makes the molecules on the average less energetic by partially combining the more energetic solvent-molecules with the less energetic salt molecules, they are on the average rather further from instability, λ is less and η is greater. This again agrees with observation.

At the same time the specific electric resistance ρ is diminished. This would require that in (6) either μ or K, or both, should be increased, probably both; and this brings out a point which must be noted, that the factor of decay λ in (3) is not likely to be the same as μ in (6); for while one relates rather to the molecules and their relative positions, the other most probably relates to the atoms and their positions in the molecules.

Maxwell (*Proc. Roy. Soc.* vol. 148, 1873) gave an account of some experiments which he made to test this view of liquid viscosity by shearing a liquid and looking out for double refraction. He could only observe it in the case of Canada balsam, in which it had already been found by Mach, and here the 'rate of relaxation' was so great that he could not observe any double refraction after the shearing motion ceased. Kundt (*Ann. Phys. Chem.* vol. 13, 1881) made a series of experiments and found double refraction in many sheared liquids, notably in olive-oil, but never in a pure liquid with a definite chemical constitution. The more complex the molecules apparently the less is λ, and the greater is the shear-strain still remaining at any instant of the motion. But in liquids such as water or glycerine, the decay is so rapid that no optically appreciable amount remains.

Still it is very possible that olive-oil is only an extreme case, and that water and other apparently inactive liquids would show the effect if we could sufficiently increase the shear, and I think Kundt's results may be claimed as supporting the hypothesis. Possibly, too, the observation of Quincke, that double refraction is observed in a liquid close to a very hot wire, gives further support. The unequal heating may perhaps be regarded as producing shear-strains in the solid structure which are renewed by the supply of heat as fast as they break down.

In the case of breaking down of structure near the surface of a liquid the moving molecules may succeed in escaping altogether, and may fly off as gas-molecules if they are directed upwards and have enough energy. Of course there may be many molecules able to move about and yet not able to evaporate; for though they may be able to travel when in the body of the liquid, they may not have energy enough to get clear away from their neighbours when these are all on one side and all pull in one direction as they do at the surface. In the case of practically non-evaporating liquids, such as mercury at ordinary temperatures, we must suppose that only a very minute fraction are thus able to do the work needed to overcome the large cohesion of their neighbours.

It will be convenient to use the term 'mobility' to describe the number of 'free' or 'mobilised' molecules crossing a square centimetre per second in a liquid, where by 'free' or 'mobilised' we mean those which are changing their surroundings and forming new connections. Evidently we may extend the term to a gas, remembering that then all the molecules are mobilised, and that the mobility is proportional to the pressure.

When a square centimetre is taken on the surface of a liquid, the mobility upwards is the rate of evaporation, and the mobility of the vapour downwards is the rate of condensation. When the two mobilities are equal the pressure of the vapour is the vapour-tension.

The mobility in the body of the liquid is probably far greater than that at the surface for the reason already given; viz., that in the one case the neighbouring molecules entirely surround one which tends to get free, while at the surface they are all on one side and so tend to pull back and retain a molecule which may be inclined to move away. If, however, the internal mobility at a given temperature is altered, say by the pressure, or by the presence of some substance in solution, the surface-mobility will be altered too. We shall assume that it is altered in the same ratio as the internal mobility, an assumption which appears to be justified by the account which it will enable us to give of the effect of pressure and of solution.

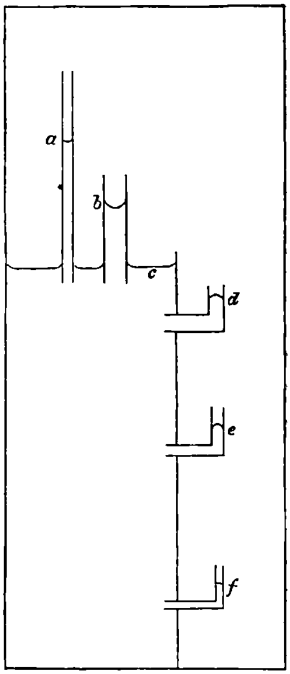
Let us now apply this idea to the familiar case of rise in a capillary tube standing in a liquid having only its own vapour above it. Or let us take the more general case of a liquid in a vessel with tubes which are wet rising above the flat surface, and with tubes which are not wet coming out of the side and turning upwards, and of such diameters that the liquid does not rise to the top of the tube, as in the Figure. Thomson's theorem shows that there is ultimately a balance between evaporation and condensation at each surface, or that the vapour-tension is less at the surfaces a and b than the normal amount existing at c, while at d, e, and f it is greater. In other words, the surface-mobility gradually increases as we go downwards. This is usually connected with the curvature of the liquid surface, but, as I have tried to show in a former paper (*Proc. Phys. Soc.* vol. 4, p. 271; *Phil. Mag.* July, 1881)*, it should rather be connected with the increased pressure of the liquid just under the surface as we descend; the curvature of the surface is a non-essential accompaniment.

Taking the pressure of the vapour at the flat surface c in the Figure as ϖ, and the densities of liquid and vapour as ρ and σ respectively, then at a level h

* [*Collected Papers*, Art. 39.]

below or above c the hydrostatic pressure is greater or less than at c by $g\rho h$, $= P$ say, while the vapour-pressure is greater or less than at c by $g\sigma h = \frac{P\sigma}{\rho}$; or the increase in vapour-pressure at a surface as we descend is proportional to the increase in hydrostatic pressure just under that surface. This is accounted for if we suppose that the increased hydrostatic pressure results in increased mobility, and therefore increased evaporation from the surface. The vapour-pressure increases from ϖ to

$$\varpi + \frac{P\sigma}{\rho} = \varpi\left(1 + \frac{P\sigma}{\varpi\rho}\right);$$



or the coefficient of increase of its mobility is $\frac{\sigma}{\varpi\rho}$ per unit of hydrostatic pressure, and this is the coefficient we must assume for the increase of internal liquid mobility to account for the facts on this theory.

We have no direct evidence that increase of pressure does thus increase liquid mobility. The justification is to be sought in such explanations of known facts as that just given*.

It is perhaps worth noting that we obtain the true state of affairs externally if we picture the liquid in the Figure as a kind of open framework, or as a spongy structure through which the molecules of vapour pass freely so that they are at the same pressure within and without the liquid at the same level. But this conception must be used only to give us the net result, and not as representing the actual condition.

If, in addition to the vapour, any soluble gas is present in the vessel, it too will exist both inside and out in quantities increasing as we descend, and it must be in equilibrium at all levels. So that if near the flat surface the density of the gas in solution is n times the density at the same level outside, the same ratio will hold at all depths. Again the net external result is the same as if we picture to ourselves a spongy structure through which the gas passes freely.

* Liquid viscosity should decrease if mobility increases, and should therefore, in our view, decrease with increase of pressure were mobility alone concerned. But rigidity also comes in, and we must ascribe to this complication the result that, in water, pressure lessens the viscosity while in turpentine it increases it (Cohen, *Wied. Ann.* no. 4, 1892). But it would appear fair to seek support for the supposition of increased mobility in the 'flowing' of solids under great stresses, as in the stamping and wiredrawing of metal, when the molecules undoubtedly change their positions with very greatly increased rapidity when under great strain.

As a further illustration of the change of mobility with pressure, we may take the alteration of the melting-point which I have discussed in the paper mentioned above. Thus, in the case of water, water and ice are in equilibrium under 1 atmo. at 0°, and therefore have equal vapour-tensions and equal surface-mobilities. If, however, we put on pressure, the coefficients of increase of mobility are, as we have just seen, $\frac{\sigma}{\varpi\rho}$ and $\frac{\sigma}{\varpi\rho'}$, where ρ and ρ' are the densities of water and ice, and σ and ϖ the density and pressure of the vapour respectively. Since ρ is greater than ρ' the water mobility is increased less than the ice mobility, and so at the surface of contact the ice sends more molecules to the water than it receives in return, that is to say, it melts. Below 0° the vapour-pressures and mobilities at atmospheric pressure are different, the mobility of water being greater than that of ice. But if we put on sufficient pressure we may once more equalise the mobilities and so lower the melting-point to the new temperature. Thus if ϖ and ϖ' are the vapour-pressures of water and ice at $-d\theta$, and P is the pressure making the mobilities equal, or the pressure reducing the melting-point to $-d\theta$,

$$\varpi\left(1+\frac{P\sigma}{\varpi\rho}\right)=\varpi'\left(1+\frac{P\sigma}{\varpi'\rho'}\right),$$

or

$$\varpi-\varpi'=P\sigma\left(\frac{1}{\rho'}\quad\frac{1}{\rho}\right), \quad\text{..........................(7)}$$

a formula equivalent to that of Kirchhoff deduced by purely thermodynamic considerations. For using the ordinary formula for lowering of melting-point,

$$P\left(\frac{1}{\rho'}-\frac{1}{\rho}\right)=L\frac{d\theta}{\theta}, \quad\text{..........................(7 a)}$$

we obtain Kirchhoff's result,

$$\varpi-\varpi'=\frac{L\sigma\,d\theta}{\theta}.$$

Now let us consider the case of a dilute solution of a non-evaporating salt. We know by direct observation that the vapour-tension is reduced by the presence of the salt, and we must suppose, on the hypothesis here advocated, that this reduction is due to a decrease in the mobility of the liquid. Let us follow out this idea by imagining that we have in the same chamber maintained at a constant temperature two deep vessels, one containing the pure solvent the other a dilute solution. In this chamber we shall suppose that above the liquids there is only the vapour of the solvent. To begin with, we may suppose that each vessel is half full and at the same level. Then the pure solvent will distil over into the solution, and will continue to do so until the difference in level in the two vessels is such that each surface is in equilibrium with the vapour at its level. The hydrostatic pressure in the solution at the level of the surface of the pure solvent will then be the osmotic pressure. If we imagine a number of non-wettable tubes inserted, as in the Figure, in the sides of the two vessels at various depths and turned upwards, the diameters being so adjusted that the liquid does not flow out of any of them, then in any pair at the same level we realise Fitz-Gerald's semi-permeable membrane; and at each level the two liquids must have equal vapour-tensions, which implies that their mobilities are equal at each level. This also comes out from our equations. Let ϖ, ϖ' be the flat surface vapour-tensions of solvent and solution, ρ the density of the liquid—practically the same for each—and H the final difference in level between the two surfaces, so that the osmotic pressure $P = g\rho H$. If M, M' be the mobilities at the surface-levels,

$$\frac{M}{M'} = \frac{\varpi}{\varpi'}.$$

Now as we descend in the solution the mobility increases, and the rate of increase is $\frac{\sigma}{\varpi'\rho}$ per unit pressure. For depth H this increase is $g\rho\frac{H\sigma}{\varpi'\rho} = g\frac{H\sigma}{\varpi'}$, or the mobility

$$M'' = M'\left(1 + g\frac{H\sigma}{\varpi'}\right) = \frac{\varpi'}{\varpi}M\left(1 + g\frac{H\sigma}{\varpi'}\right)$$

$$= \frac{M}{\varpi}(\varpi' + gH\sigma).$$

But

$$\varpi = \varpi' + gH\sigma,$$

whence $M'' = M$, or at the level of the surface of the solvent the mobilities are equal. This equality will be maintained if we descend equal distances in the two liquids below that level. So that if we now connect the two vessels at any level by a horizontal tube with a semi-permeable membrane in it, the mobilities of the solvent on the two sides of the membrane are equal, and therefore the solvent diffuses through at equal rates in the two directions.

We may then explain in the following general terms the rise which occurs when we place a semi-permeable vessel containing a solution into a solvent. The molecules of the solvent are entering the membrane on both sides, but the mobility or number set free per second from the pure solvent is greater than the number set free from the solution. The membrane goes on absorbing the solvent from each side till it becomes saturated, i.e. holds so much that it returns as many molecules as it receives. It is receiving more from the pure solvent side, and therefore when saturated for that side it is super-saturated for the other. Consequently more molecules are sent into the solution than are received from it, and the solution grows until the growing pressure so much increases the mobility that it is equal on both sides of the membrane.

If the solution and solvent are in two vessels separated by an indefinitely produced vertical and semi-permeable membrane, it is evident that ultimately the two will be in equilibrium at every level, whether in liquid or vapour.

We may apply the same idea to the change of melting-point in a solution. In the solution the solid mobility is unchanged, but that of the solution is lowered by the fraction $\frac{P\sigma}{\varpi\rho}$, where P is the osmotic pressure; and to find the new melting-point, we must find the temperature $d\theta$ below the normal melting-point at which this is equal to the difference between the liquid and solid mobilities.

Taking pressures to represent mobilities,

$$\frac{\varpi - \varpi'}{\varpi} = \frac{P\sigma}{\varpi\rho}.$$

But

$$\varpi - \varpi' = \frac{L\sigma\, d\theta}{\theta},$$

whence we obtain the ordinary result

$$d\theta = \frac{P\theta}{L\rho}. \qquad \text{.....................................(8)}$$

Comparing the above result with the lowering due to pressure (7 and 7 a), we see that the lowering due to a given osmotic pressure in the solution is greater than that due to an equal pressure on the pure solvent in the ratio

$$\frac{1}{\rho} : \left(\frac{1}{\rho'} \quad \frac{1}{\rho}\right),$$

or

$$v : (v' - v).$$

In the case of ice and water the ratio is $1 : (1{\cdot}092 - 1)$
$= 1 : {\cdot}092$
$= 11 : 1.$

It now remains to see if we can give any reasonable account of the decrease in mobility in a liquid when a salt is present in solution. If the molecules of salt were simply mixed with those of the solvent, or if they combined to form stable non-evaporating compounds with the solvent, which compounds were simply mixed, then the mixture should have the same vapour-tension as the pure solvent. For we might regard the salt or compound molecules at the surface as equally reducing the effective evaporating and the effective condensing area, somewhat as a perforated plate or gauze laid on the surface would do. But the salt probably combines with the solvent to form unstable molecules which continually interchange constituents, so that when near the surface they may serve equally with those of the pure solvent to entangle the molecules of vapour coming downwards, these descending vapour-molecules taking the place of molecules attached to the salt. Probably, however, they are less energetic than the pure solvent-molecules and do not contribute so much to evaporation. We shall make the supposition that they do not contribute at all.

Let then each of the salt molecules combine on the average with a of the molecules of the solvent, and in such a way that it prevents those a molecules from evaporating while the compound molecules formed will entangle returning molecules, each of the a being replaceable by a vapour-molecule. Then we may regard the solution as solvent, having a number of molecules simply mixed up and inactive as regards evaporation but active in effecting condensation.

If N is the number of gramme-molecules of solvent per litre, and n the number of gramme-molecules of salt added, the number of molecules of solvent left is $N - an$. Were the n compound molecules quite inactive both as regards evaporation and condensation the mobilities outwards and inwards would be altered in the same ratio and the vapour-tension would be unaltered. But we are supposing that they are inactive for evaporation only and that their a molecules of combined solvent are still active for condensation. So that in the solution there are only $N - an$ active for evaporation, while there are still N active for condensation. Hence the vapour-tension is reduced in the ratio $\frac{N - an}{N}$. Or if ϖ and ϖ' are the vapour-tensions of the solvent and solution,

$$\frac{\varpi'}{\varpi} = \frac{N - an}{N}$$

and

$$\frac{\varpi - \varpi'}{\varpi} = \frac{an}{N}.$$

If each salt molecule takes one molecule of solvent, so that $a = 1$, we have

$$\frac{\varpi - \varpi'}{\varpi} = \frac{n}{N},$$

which is the usual result deduced for dilute solutions from the van 't Hoff value of the osmotic pressure. We may, of course, work backwards from this result, and the work may be put in the following form:

If P is the pressure in the solution necessary to restore its mobility to that of the solvent, i.e. to increase it in the ratio $\varpi : \varpi'$,

$$\varpi'\left(1 + \frac{P\sigma}{\varpi'\rho}\right) = \varpi,$$

or

$$\frac{\varpi - \varpi'}{\varpi} = \frac{P\sigma}{\varpi\rho} = \frac{n}{N}$$

and

$$P = \frac{n\varpi\rho}{N\sigma}.$$

If M is the molecular weight of the solvent

$$\begin{aligned}\frac{\varpi}{\sigma} &= \frac{\varpi_0}{\sigma_0}(1 + at) \\ &= \frac{2}{M}\left(\frac{\varpi_0}{\sigma_0}\right)_H (1 + at),\end{aligned}$$

where $\left(\frac{\varpi_0}{\sigma_0}\right)_H$ is the value for hydrogen at 0°, and this is $\frac{A}{\cdot 0000896}$ where $A = 1$ atmo.

Also $NM = 1000\rho$.

Substituting these values we obtain

$$\begin{aligned}P &= \frac{n}{N} \cdot \frac{2}{M} \cdot \frac{A\rho(1 + at)}{0{\cdot}0000896} \\ &= \frac{2nA}{0{\cdot}0896}(1 + at) \\ &= 22{\cdot}3nA(1 + at).\end{aligned}$$

If a has any other value than 1 we must put

$$P = 22{\cdot}3anA(1 + at),$$

whence we see that if each salt molecule combines with two or with three molecules of solvent the osmotic pressure is double or treble the normal value.

The supposition here made is no doubt crude in its simplicity, but my attempts to introduce other considerations, such as change in density in the solution, have led to such complicated results that much more extravagant suppositions had to be made to reconcile these results with experiments. I therefore leave the hypothesis in this crude form, in which it will at least serve to show that it is not necessary to ascribe osmotic pressure to dissociation but rather to association or some kind of combination of salt and solvent.

42.

MUSICAL SANDS.

[*Nature*, 77, 1908, p. 248.]

Mr. Carus-Wilson's failure (January 9, p. 222) to obtain sounds from 'millet-seed' sand of highly spherical grains puts a difficulty in the way of the suggestion made in *Sound* by Poynting and Thomson, though I do not think that it finally disposes of it.

I have not been able to follow the friction explanation as given by Mr. Carus-Wilson (*Nature*, August 6, 1891), and I write in the hope that he may give more detail as to the moving system which produces the musical note. It appears probable that the musical sounds excited in a body by friction are due to the natural vibrations of that body. Obviously the grains of sand are far too small to give the notes heard. I suppose that the fundamental period is of the order of the time taken by an elastic wave to travel half round the grain. With elastic moduli of the order 10^{11} and density $2\frac{1}{2}$, the fundamental frequency would be not less than 10^6. What system does the friction set in vibration?

J. H. POYNTING.

THE UNIVERSITY, BIRMINGHAM,
January 11.

PART VI.

STATISTICS.

43.

THE DRUNKENNESS STATISTICS OF THE LARGE TOWNS IN ENGLAND AND WALES.

[*Manchester Lit. and Phil. Soc. Proc.* **16**, 1877, pp. 211–218.]

[*Read* April 3, 1877.]

The February number of the *Fortnightly Review* contains an article by Mr. Chamberlain on 'Municipal Public Houses,' in which he incidentally combats the proposition that 'the multiplication of public-houses has a tendency to diminish intemperance.' To prove the error of this he has given a diagram in which the 71 towns of England and Wales with a population above 20,000 are arranged in order, from Cambridge, with the greatest number of public-houses in proportion to the population (one to every 100 persons), to Plymouth with the least (one to every 340 persons).

Mr. Chamberlain has exhibited the proportion by means of a curve in black ink which I have copied in Fig. 1. The numbers are taken from the Blue Book Police (Counties and Boroughs) Reports for the year ending September, 1875. In this figure the towns of Table I are supposed to be arranged in order from left to right, and the height of the curve opposite each town represents the number of persons to each public-house. Owing to want of space, only every fifth town is actually printed. Mr. Chamberlain then puts a mark opposite each town on the same diagram showing the number of persons proceeded against for drunkenness in proportion to the population in that town during the same year, the numbers varying from one in 500 in Cambridge and Maidstone, to one in 20 in Tynemouth, South Shields, and Liverpool. The marks thus placed are joined by a red ink curve. Mr. Chamberlain remarks that 'if the paradox of the supporters of free trade in drink were sustainable by these statistics the red line would incline throughout from right to left in the opposite direction to the black line; but it will be seen that it crosses backwards and forwards with no approach to any order or law.'

The following table contains the towns arranged in their order:

TABLE I. *Table giving the proportion of public-houses to population in all towns of* 20,000 *inhabitants and upwards in England and Wales for the year* 1875. [*From Mr. Chamberlain's article.*]

No.*		1 in	No.*		1 in	No.*		1 in
1	Cambridge	100	25	Derby	140	49	Wigan	190
2	Canterbury	100	26	Ipswich	140	50	Rochdale	190
3	Oxford	110	27	Manchester	140	51	Hastings	190
4	Southampton	110	28	Salford	150	52	Great Grimsby	190
5	Shrewsbury	110	29	Chester	150	53	Stalybridge	190
6	Northampton	120	30	Blackburn	150	54	Stockport	190
7	Norwich	120	31	Gravesend	150	55	Oldham	190
8	Portsmouth	120	32	Leamington	150	56	Bolton	200
9	Reading	120	33	Newcastle-on-Tyne	150	57	Liverpool	200
10	Dover	130	34	Sheffield	160	58	Birkenhead	200
11	Swansea	130	35	York	160	59	Devonport	200
12	Great Yarmouth	130	36	Nottingham	160	60	Gateshead	200
13	Worcester	130	37	Lincoln	160	61	Carlisle	210
14	Hanley	130	38	Bath	170	62	Cardiff	220
15	Coventry	130	39	Leicester	170	63	Warrington	230
16	Scarborough	130	40	Sunderland	170	64	Halifax	230
17	Bristol	140	41	Ashton-under-Lyne	170	65	Exeter	230
18	Tynemouth	140	42	Wakefield	170	66	Huddersfield	240
19	Walsall	140	43	Macclesfield	170	67	Leeds	240
20	Brighton	140	44	Preston	180	68	Bradford	260
21	Maidstone	140	45	Hull	180	69	Southport	270
22	Colchester	140	46	South Shields	180	70	Middlesborough	300
23	Newport, Mon.	140	47	Birmingham	180	71	Plymouth	340
24	Wolverhampton	140	48	Dewsbury	180			

But it seemed to me on inspecting the diagram that on the average the number of apprehensions for drunkenness increased considerably towards the right hand of the diagram where the public-houses were fewest in proportion to the population. To see if this was really the case I grouped the numbers together in the following way. I took the mean of the numbers of persons to each apprehension for drunkenness for the first twenty towns, and put a mark on the diagram opposite the middle town of the twenty at a height representing this mean; then the mean for the twenty towns from the second to the twenty-first, and put a mark opposite the middle town of the twenty to represent this mean; then the mean for the twenty towns from the third to the twenty-second, and so on. Running a curve as nearly as possible through the fifty points thus obtained I found the curve to take the course given in Fig. 2. The numbers for the year ending September, 1876, treated in the same way, gave a similar curve, both curves slanting very decidedly in the opposite direction to the public-house curve, and would seem to indicate that the fewer public-houses there are the greater on the average the

* The numbers of the towns are those by which they are denoted in Figs. 1, 2, 3.

number of apprehensions for drunkenness. This method of examining the numbers seems justified in this respect, that grouping a large number of towns

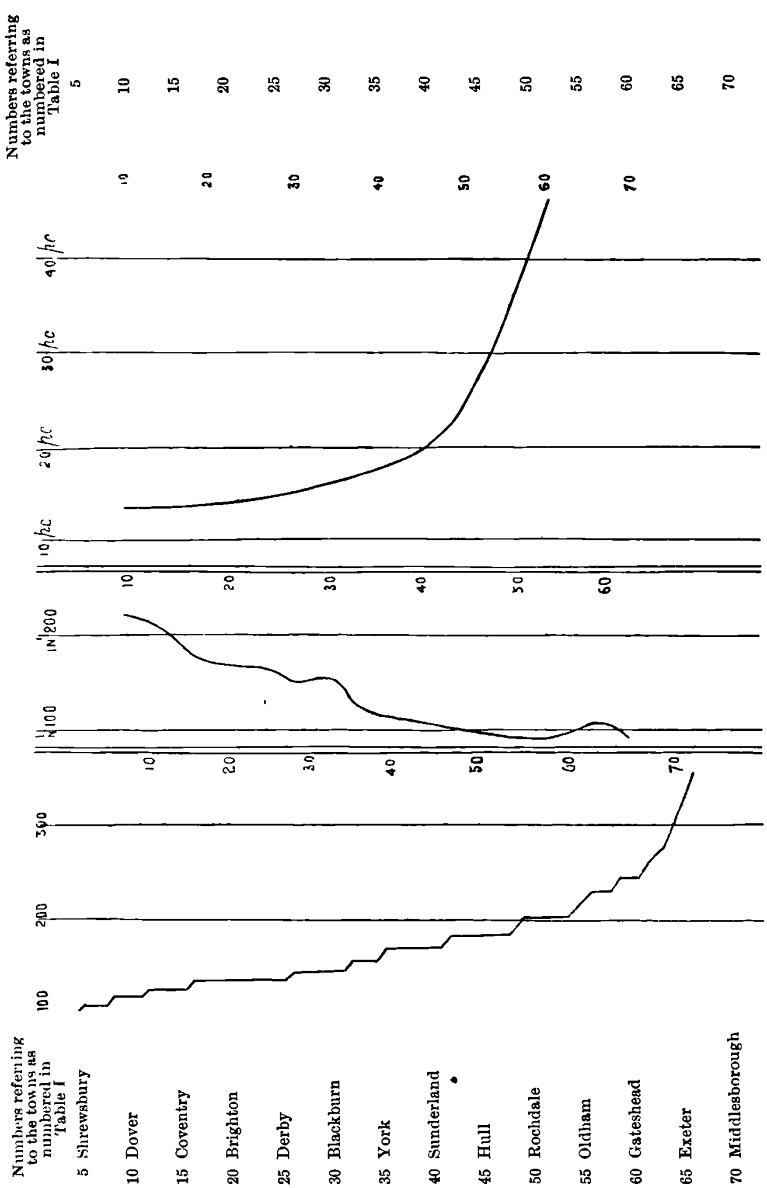


Fig. 1. Fig. 2. Fig. 3.

Fig. 1. Table showing the proportion of public-houses to population in all towns of 20,000 and upwards in England and Wales (*Fortnightly Review*, Feb. 1877). Year ending September, 1875.

Fig. 2. Table showing the proportion of persons proceeded against in year ending September 29, 1875.

Fig. 3. Table showing percentage increase of population in the towns from 1861 to 1871. The curve obtained by grouping the towns in twenties.

together will to some extent neutralise the effect of more or less stringent police regulations and other circumstances peculiar to individual towns.

It would of course be absurd to infer from these curves that *because* there are more public-houses in certain towns, therefore there is less drunkenness. It would be far more likely that the fewness of public-houses, and the prevalence of drunkenness, arise from some common cause. It seemed possible, for instance, that when there was great prosperity the population would probably increase quickly, and through the difficulty in obtaining licenses, the public-houses would not keep pace with the increase of the population.

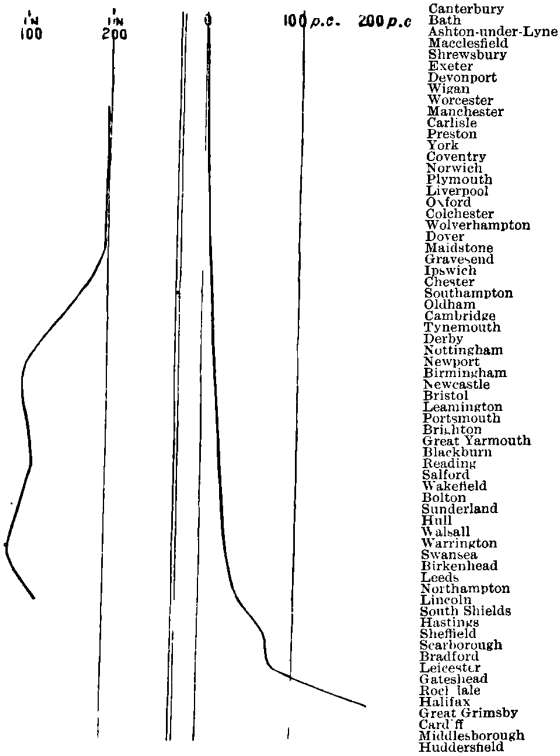


Fig. 5. Fig. 4.

Fig. 4. Table showing the percentage increase in population in the towns during the ten years 1861–71.

Fig. 5. Table showing the proportion of persons proceeded against for drunkenness. The numbers for the towns grouped in twenties, year ending Sept. 29, 1876.

At the same time, when there was great prosperity, there would be higher wages and more money to spend, and therefore more possibility of drunkenness.

I therefore grouped the towns as before, in twenties, but this time taking the numbers showing the percentage increase in population during the years 1861–71, and then drawing a curve nearly through the points thus obtained, it took the course shown in Fig. 3. This, by altering the scale, might almost be made to coincide with the public-house curve in Fig. 1.

If the towns be now arranged in order of their percentage increase of population, during these ten years, from Canterbury, Bath, and Ashton-under-Lyne, with either no increase or a decrease, to Huddersfield, with an increase of 217 per cent., the increase in population can be represented by the curve given in Fig. 4. If the figures for the apprehensions for drunkenness in these towns be grouped by twenties as before, and a curve be run through the

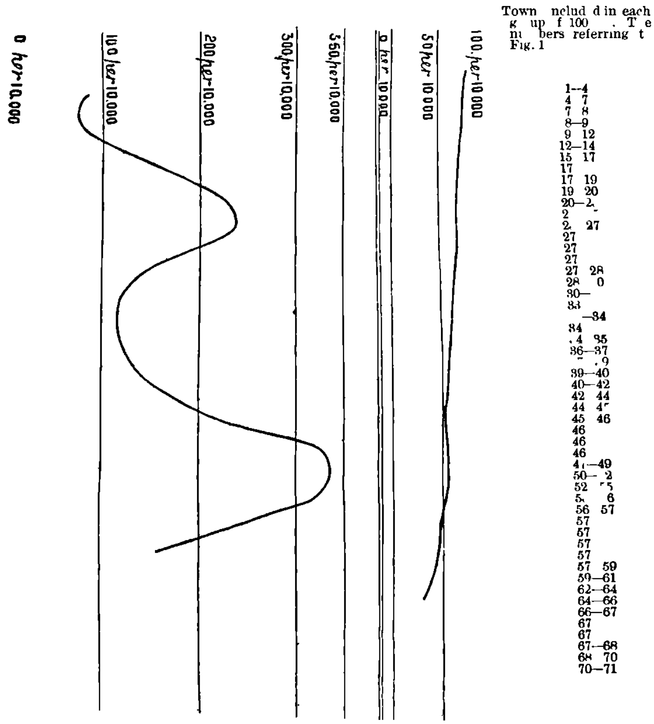


Fig. 7. Fig. 6.

Fig. 6. Table showing the proportion of public-houses per 10,000 of population. Population grouped in hundred thousands.

Fig. 7. Table showing the proportion of persons proceeded against per 10,000 of the population. The population grouped in hundred thousands, and these numbers again grouped in tens.

points representing the means, Fig. 5 is obtained, in which the curve, though not so regular as those of Figs. 3 and 4, still takes in general the opposite slant to the curve of Fig. 4.

This method of examining the numbers is unsatisfactory in this point, that a town such as Liverpool, with 490,000 inhabitants, has only the same effect on the course of the curve as Southport with 20,000 inhabitants. Fig. 6 represents the proportion of public-houses per ten thousand of the population on this principle. The population of each town is taken to the nearest ten thousand, to save trouble in calculating. The towns are then grouped together in hundred thousands, so that each square now represents, instead of a town, a population of 100,000. The numbers at the top refer to the towns with the same numbers in Fig. 1 included in each 100,000. If the numbers of apprehensions for drunkenness for 10,000 of the population in each of these populations of 100,000 be grouped by tens as before, the curve in Fig. 7 is obtained. This does not now follow the opposite course to the public-house curve so regularly as did those of Figs. 3 and 4, though it is still, on the whole, higher on the right hand where the public-house curve is lower.

The two great rises are caused chiefly: the first by Manchester and Salford; the second by Liverpool; as will be seen by referring to the numbers in Fig. 7 opposite the rises.

If the towns be arranged in order of apprehensions for drunkenness for the year 1875, from Cambridge with one in 500 to Liverpool with one in 20, it at once becomes evident that, on the whole, in the north of England, there is a greater proportion of apprehensions than in the south.

The following are the towns arranged in that order:

TABLE II.

1	Maidstone, Cambridge
3	Norwich, Devonport
5	Colchester
6	Hastings
7	Ipswich
8	Oxford
9	Portsmouth
10	Dover
11	Brighton
12	Coventry, Bath
14	Leamington
15	Canterbury
16	Great Yarmouth
17	Reading
18	Exeter
19	Plymouth, Sheffield
21	Leicester
22	Northampton
23	Bristol
24	Lincoln
25	Bradford, Birmingham, Huddersfield
28	Gravesend, Worcester, Southampton, Macclesfield, Preston
33	Leeds, Shrewsbury
35	Hull, York, Walsall
38	Hanley, Blackburn, Bolton
41	Carlisle, Cardiff, Wolverhampton, Scarborough, Swansea
46	Newport, Chester, Stalybridge
49	Halifax, Southport, Middlesborough
52	Oldham, Wigan, Nottingham, Derby
56	Wakefield
57	Sunderland, Ashton-under-Lyne, Rochdale, Great Grimsby, Stockport
62	Manchester, Salford, Newcastle, Dewsbury, Birkenhead, Gateshead, Warrington
69	Tynemouth, South Shields, Liverpool

If a map of England and Wales be drawn with the parallels of latitude and longitude, and the average numbers of apprehensions per ten thousand in each square formed by these parallels (or where the districts are populous in each half or quarter square) are marked in their respective squares, it will be seen that the drunkenness apprehensions are nearly the same along the south and south-east coasts, and that they then increase in a direction turning round more and more to the north-west, as we proceed northward. I have not had time to examine the numbers for the smaller towns and for the counties to see whether they bear this out.

The numbers from which the curves are constructed are not the exact ones obtained from the police report; usually I have taken the nearest tens to facilitate calculation.

44.

THE GEOGRAPHICAL DISTRIBUTION OF DRUNKENNESS IN ENGLAND AND WALES.

By JOHN DENDY, Jun. and J. H. POYNTING.

[*Fourth Report from the Select Committee of the House of Lords on Intemperance*, 1878, Appendix R, pp. 580–591.]

[This is a further development of the subject treated in Art. 43; it is of great interest but consists largely of tables. The summary alone, given by the authors, is printed here. ED.]

Summary.

We have made no attempt in the foregoing to support or refute any of the theories now in vogue as to the best methods of dealing with drunkenness. We are, however, convinced that many theories are founded on, and much good work consequently wasted through, an inadequate knowledge of the facts of this great social evil. We believe that here, as elsewhere, the first step towards effecting a reform ought to be a thorough examination of the facts relating to the evil to be dealt with. We do not pretend that we have made anything approaching to an exhaustive examination of the subject. All that we have attempted is to take one of the first, and therefore most imperfect, steps in what appears to us to be the right direction.

The results which appear to us to be so far established are briefly as follows:

(1) There is a marked geographical distribution of drunkenness. England may be divided into two districts as shown in the map*, the northern being decidedly more drunken than the southern, and the transition from one to the other being somewhat sudden. The great coal fields seem to coincide with the most drunken districts.

(2) There appears to be no constant direct connection between the number of public-houses and the drunkenness of the various districts. Had

* [The line of demarcation is drawn from a little south of Grimsby to the junction of Severn and Wye. ED.]

the northern and southern districts been treated together, the apparent paradox would have been supported, that the greater the number of public-houses the less the drunkenness: but, by treating the two divisions separately, and having regard to facts connected with the increase of population, we are enabled to form a more accurate conclusion.

(3) There appears to be a direct relation between the rate of increase of population and the rate of drunkenness, so that on the whole, where the population is increasing most rapidly, there is the greatest drunkenness, this being least marked in the great northern towns.

(4) On the whole in the towns where the drunkenness is greatest the population is most dense.

We have abstained from dealing with individual instances, although we are fully aware that some of the towns and counties appear to furnish striking confirmation, and also refutation, of the above mentioned results, and of the theories which have been elsewhere put forward on this subject.

We would suggest that if the police reports, the judicial statistics, and the Registrar-General's returns could be so arranged as to deal with the same areas and the same periods of time, most valuable assistance would be given to future investigations of this nature.

45.

A COMPARISON OF THE FLUCTUATIONS IN THE PRICE OF WHEAT AND IN THE COTTON AND SILK IMPORTS INTO GREAT BRITAIN.

[*Statistical Society Journal*, 1884.]

[*Read* before the Statistical Society, 15 January, 1884.]

Many endeavours have lately been made to prove a connection between the variations in the activity of the sun as indicated by sunspots, and the variations in terrestrial phenomena. In some cases this connection is proved beyond doubt, but in others it is hardly as yet satisfactorily made out. To this latter class belongs the wheat harvest, the subject of probably the first investigation in this direction. As long ago as the beginning of this century Sir William Herschel remarked ('Observations tending to investigate the Nature of the Sun,' *Phil. Trans.* 1801) that if sunspots have an influence on the weather, then we may expect to find a connection between their appearance and the amount of the harvest as indicated by the price of wheat. He thought that statistics bore out the supposition, that in years of maximum sunspots the price of wheat is on the whole lower than in years of minimum sunspots. But further observation and investigation have shown that the connection between sunspots and the price of wheat is not so simple as Herschel supposed, even if it exists at all.

If true periodic variations are made out in the sun, we may fairly expect to find the same periods represented in whatever depends in any way on the sun; but we have no reason to suppose that we may not have other variations, periodic or non-periodic, depending on meteorological conditions, and far larger in amount. It is only necessary to compare the curves showing the variations in sunspots and the harvest during a considerable period, to see at once that one is not a simple function of the other, but that if sunspots have any effect this is disguised by other and larger effects, some of which may be due to social or economical, others to meteorological causes. Indeed *a priori* we perhaps ought not to expect the sunspot effect to be very marked. For as far as concerns meteorology, the amount of a crop depends on the light, heat, and moisture during its growth. We do not yet know how each of these is affected by changes in sunspots, and it is quite possible that a change in one favourable to a crop, may be accompanied by an unfavourable change in another.

The attempt to prove the sunspot origin of the variations of harvests and crops, has probably led us somewhat away from the proper line of inquiry. This, it seems to me, should begin with such a manipulation of the statistics as to show the true fluctuations whatever they may be, with the effects of wars, increase of commerce, etc., as far as possible eliminated. Having obtained these fluctuations for various commodities, we can compare them with each other. If the meteorological conditions are due to extra-terrestrial causes, or to terrestrial causes if they affect the whole earth, then we ought to find the fluctuations to be not merely local, but to occur nearly simultaneously in widely distant countries and in different commodities.

In this paper I have attempted to follow such a course. A method of dealing with the statistics is first described which seems to give the fluctuations freed to some extent at least from 'accidental irregularities.' The results are then given of the application of the method to the price of wheat in England, France, and Bengal, taking the price as an indication of the harvest, and to the cotton and silk imports into Great Britain from various countries. Possibly the most satisfactory method of comparison would consist in a determination of any periodic variations which may exist and which are common to all. But in most cases our statistics extend over too short a time for this, and the best method available is to represent the results by curves, and to compare these together as a whole. Such a comparison, which is described in detail, does, I think, show a very close similarity in the various curves pointing probably to a common meteorological cause for the fluctuations. The results of the investigation will at once suggest the next steps in the inquiry, and these are indicated at the end of the paper.

I. *The Method of Treating the Statistics.*

As an example of the method pursued to obtain the true fluctuations, we will take the prices of wheat in England. The list from 1582 to the present time is to be found in many works (among others in Walford's 'Famines of the World,' *Journal of the Statistical Society*, 1879). From that date up to times when gazette prices are available, a record was kept by the Eton baker of the highest price of wheat at Windsor market on the market days preceding Lady Day and Michaelmas. By Professor Rogers' researches the list will probably be at least doubled in length.

As they stand the prices are not suitable for our purpose. This will at once be seen on drawing a curve whose ordinates represent the price for each year. This curve is not only so irregular that it is difficult to detect the nature of the fluctuations, but at certain periods, especially through wars, the average price rises so considerably, that what may be a very low price at a particular time may be a high price compared with the average of other times. For instance, during the four years 1804–07, the average price of wheat was 76·6 shillings per quarter. This would be a very high price for ordinary times, but was low compared with others at the beginning of the century. In order therefore to determine the fluctuations, we require to know not only the actual price, but whether that price is above or below the average *for that time.* It becomes necessary then to average the prices in some way so as to obtain a standard for each year, and we can then determine whether the price for any particular year is high or low according as it is above or below that standard. I have found it sufficient to average for ten years at a time, that is, I have taken as the standard for each year the average of the ten years of which that year is the fifth. If a curve be drawn whose ordinates represent these standard prices, it will be seen at once that all the larger irregularities are nearly smoothed down. They might be still further smoothed down by averaging the averages for say ten years at a time, but as the first process only leaves irregularities small compared with the fluctuations obtained in the way to be described directly, this is unnecessary for the present.

It may be noted that if the whole scale of prices rises, say through a long war, then the standard rises too; or if there is a gradual change through a change in the value of money, then the standard also changes. The standard will be nearly freed from any periodic changes, if such exist anywhere near ten years in period, and even changes of considerably longer or shorter period are very much lessened in their effect.

It would now be possible to represent the rises and falls in price, by comparing the price for each year with the standard for that year. But there are so many irregularities of short duration, say two or three years, that it is more convenient to take, instead of the price for each year, the average for a short period, and for this purpose I adopt four years. The price for any year then to be compared with the standard, is the average for the four years of which that year is the second.

Were there only very small variations in the standard, it would be sufficient to take the difference between the ten-yearly and the four-yearly averages. But the standard varies very considerably even in the price of wheat, and in the case of imports of cotton and silk the change is very great indeed, through the increase in commerce. The higher the standard, the greater are the differences between it and the four-yearly average. To obtain results which may be compared with each other at different times, this effect of change of standard must be eliminated. This may be done by finding what percentage the four-yearly is of the ten-yearly average. The labour of this is not very great if a slide-rule is employed, and the result is satisfactory, as will be seen on inspecting the curves given by this method and comparing them with those given by the differences.

II. *Fluctuations in Wheat Price.*

The results of the above process applied to English wheat prices since 1760, are given in Table I, Appendix, and Fig. 1 is the curve representing the fluctuations. Unfortunately there is no series of prices to be compared with that of English wheat, either for trustworthiness or length. There are, however, two other series which I have treated in the same way, and which are sufficiently long to prove a similarity in fluctuation: one, of the price of wheat at Delhi, arranged by the late Professor Jevons, and given by him (*Nature*, vol. 19, p. 588) in a paper in which he pointed out a connection between a high wheat price in Bengal and commercial crises in England, and the other of the price of French wheat from 1800 to 1850, taken from McCulloch's *Commercial Dictionary.*

These are given in Tables II and III, Appendix, and the curves in Figs. 2 and 3.

On comparing the curves (whose maxima I have lettered to aid comparison), it will be seen at once that b, c, g, h, i, and their succeeding minima correspond almost exactly in both the English and Bengal curves, that Delhi f follows slightly later than English f, and that the only break in similarity is the single Delhi maximum, where there are two English maxima d and e. The French curve corresponds still more closely, both as to maxima and minima, with the English, the only noticeable divergence being in the small maximum i, which is three years earlier in the French curve. It would probably be easy to obtain a long series of French prices, but I think it is hardly necessary, as the close similarity of the fluctuations to those in English wheat is beyond a doubt, and the period of comparison is taken as it were at random.

There is very little direct evidence to show that the fluctuations in the price of wheat are due to fluctuations in the harvest. It is of course a matter of common observation that if the harvest is bad the price will rise, and if the harvest is abundant the price will fall; but this is hardly satisfactory proof that all the fluctuations are due to this cause. We have reports of the quality of the harvest for many years, but they are not formed on any very definite principle, and are therefore not very trustworthy. Farmers proverbially take a depressed view of affairs, and this is illustrated by these reports. According to a table of good and bad seasons and harvests, for the forty-two years 1839–80, in *Whitaker's Almanac* for 1882, p. 424, the harvest has been an average one twice, it has been below the average or deficient twenty-seven times, and above the average only thirteen times. But even as it stands the table gives support to the belief that the price depends mainly on the harvest. Underneath the English curve I have marked the average harvests *a*, those above the average +, and those below –. To high prices should correspond –, and to low prices +, and on the whole this is the case, the curve I think following slightly after the harvest. But we have still better evidence since 1849 in a table of Mr. Caird's (*Journal of the Statistical Society*, 1879, p. 482), of the yield per acre (see Appendix, Table IV). I have averaged his numbers for four years, and placed the curve representing the result, Fig. 1 *a*, under the price curve, but inverted, so that maxima represent deficient harvests. It will be seen that *m*, *n*, *o*, *p* are all represented by the maxima on this curve, though, as is to be expected, the price is later than the harvest curve; for the effect of good or bad harvests lasts for some time; indeed in a paper by Messrs. Lawes and Gilbert (*Journal of the Statistical Society*, 1880, p. 326) it is stated that some of the 1863 harvest probably remained over even till 1866. The table in *Whitaker's Almanac* quoted above affords a slight further confirmation in the amount of imports to supply home deficiencies there given. I have averaged these numbers for four years (see Appendix, Table V), and represented the results by Fig. 1 *b*; *k*, *l*, *n*, *o*, *p* are all represented on this curve, the two last by a slightly greater increase in the rapidly rising rate of import. It appears then that the English fluctuations in price of wheat even lately have followed fluctuations in the harvest, and as it has at the same time been a common complaint among farmers that this has not been so much the case as formerly, I think it may be taken for granted that in general the price curve does fairly represent the variations in the harvest. We cannot prove the same in the other two cases, but it seems exceedingly improbable that the coincidence of the fluctuations is chiefly due to commercial relations between the three countries. While for instance it is very likely that the price of wheat in England affects trade with India, and so has some effect on the price of Indian wheat, it seems impossible that this would be the chief effect, and of far greater importance than the fluctuations in the Indian harvest. We are driven then to conclude that the three series of fluctuations are all effects of one common cause, and this probably a nearly simultaneous fluctuation of the seasons, giving rise to good or bad harvests at about the same time in widely distant parts of the globe.

III. *Cotton Imports.*

We will now examine the fluctuations in the imports of cotton. For this purpose I have taken the statistics in the article on Cotton in the *Encyclopaedia Britannica*, ninth edition, in which are given the amount of the imports into Great Britain from 1801 to 1876 from the East Indies, the United States, Egypt, the West Indies, and Brazil, the amount of the cotton crops in the United States, and the quantity of raw material consumed in Great Britain, this last nearly summing up the imports, or at any rate fluctuating in the same way as their sum. I have treated these statistics in the same way as those of the price of corn. The actual numbers are given in the Appendix (Tables VI to XII), and the curves representing the fluctuations in Figs. 4 to 10. In the cases of the cotton consumed in Great Britain, and the American imports and crop, I have not pursued the averaging method after 1861, owing to the break caused by the American war. The curves in Figs. 4, 6, and 7, from 1857 onwards, represent the actual quantities.

It might perhaps be assumed, without discussion, that the imports into Great Britain fairly indicate the actual crop in each country, since we take such a large portion of the whole supply; but in the case of the United States we have a proof of this, for there the total crop is recorded since 1821, as well as the British imports. Comparing the curves, Figs. 6 and 7 representing these two, it will be seen that the one follows the other very closely.

To facilitate comparison, I have lettered the maxima of the cotton consumed in Great Britain, and given the same letters to such maxima as appear to correspond in the various import curves. A careful study of these will show at once that there is a strong resemblance between the various fluctuations. The American war has affected the results between 1860 and 1870, the sudden falling off in the American supply causing a sudden increase in all other parts of the world, mainly due to increased area of cultivation. This gives rise to an apparent minimum about 1860, and a maximum about 1864–66, these probably coming somewhat sooner than they would have done in ordinary circumstances. Comparing the other parts of the curves, we notice that the same maxima are repeated in most cases; those in American cotton—the chief supply throughout a great part of the time—being contemporaneous with those in cotton consumed; but in all other cases there appears to be a slight tendency on the part of the maxima to precede those in cotton consumed. The noticeable exceptions are that in East Indies, Fig. 5, i, j are represented by a single maximum—thus resembling the Bengal wheat price at the same time; and that in West Indies, Fig. 9, there is one long-continued excess over average to represent k and l. The most noticeable resemblances are the occurrence of the peaked maximum l in all but Fig. 9, of the double maximum m, and of the maximum in 1872–73.

IV. *Comparison of Cotton and Wheat.*

I believe that there is a saying in Lancashire that when corn is up cotton is down. I do not know whether this is founded on anything more than common observation, but that it is true is evident from the close similarity between the curves of wheat price and cotton consumed in England, the cotton imports rising and falling with the price of corn. This similarity is so strong that every maximum and minimum in the one occurs in the other without exception. I have therefore attached the same letters to both curves. It will be noticed that generally the cotton maxima precede the wheat maxima by a year or two.

At first sight it might be supposed that the cotton consumption depended in some way on the price of wheat. But on comparing the cotton consumption with the cotton imports, we see that it is really only a summing up of these. This is seen best on comparing Fig. 4, the cotton consumption, with Fig. 6, the imports of American cotton, our chief supply. Now in the case of America, and probably in all the others, the imports depend on the crop, and not directly on the price of wheat. It would still be possible that the crop depended on the price of wheat in England. But if so we should expect some lagging behind of the cotton curves after the corn curve, the changes in the price of corn one year in England increasing or diminishing the demand for cotton, and so affecting the area under cultivation in the succeeding years. But comparing the American crop curve, Fig. 7, with the English harvest curve, Fig. 1 *a*, we see that deficient English harvests coincide almost exactly with good American cotton crops, without any lagging of one behind the other. In other cases the fluctuations in the cotton crops if anything slightly precede those of the harvest in England. It is also worthy of especial notice that the two English wheat maxima *i*, *j* are represented by a single maximum in both Bengal wheat and East Indian cotton, showing apparently that these two are more closely connected with each other than with English wheat. These considerations seem to lead us to this general conclusion:

The American imports of cotton into Great Britain depend upon the American crop, and this is probably the case in other countries. The imports from all countries show on the whole very similar and nearly simultaneous fluctuations. Whence we may conclude that the cotton crop all over the world varies in nearly the same way at the same time. The consumption in Great Britain shows the same fluctuations, and these again appear, but slightly later, in the price of English wheat. But this last, at a slight interval, follows the English harvest, and as far as we have evidence, the English harvest nearly coincides in its fluctuations with the cotton crop, good cotton crops corresponding to bad harvests, though in some cases following a little later. The wheat price again shows similar variations in different countries, so that probably the wheat harvest varies simultaneously in those countries. Had the connection between cotton crops and harvests been merely an economical one, we should have expected cotton to follow corn fluctuations instead of either coinciding or slightly preceding. The two sets of fluctuations seem then to be due to a common meteorological cause affecting the whole globe simultaneously, and in such a way that good corn seasons are bad cotton seasons. It would be interesting to know whether this is borne out by observations in districts where both cotton and corn are grown.

V. *Silk Imports.*

I have also treated the imports of silk in the same way. I have obtained these from Messrs. Durant's Circular (published by Darling and Son, 35, Eastcheap), which gives the imports from Italy, Brutia, Persia, Bengal, and China, from 1830 to 1881, and from Japan since 1859. The numbers are given in Tables XIII to XVIII, Appendix, and the curves representing the fluctuations in Figs. 12 to 17. Fig. 17 represents the actual Japan imports, the period being too short for averaging with advantage; Fig. 11 is the English wheat price curve. Comparing the silk curves together, it will be seen that there is a strong general resemblance except in the Chinese curve, Fig. 16. They resemble again the wheat curve, but there is a considerable interval between the maxima. I have, however, attempted to identify those which correspond, by comparing the Bengal silk with East Indian cotton, which are very much alike, and nearly simultaneous in their fluctuations, and then comparing the other silk curves with that of Bengal. On the whole the silk fluctuations are slightly before those of cotton and corn. We may especially notice the rise in 1871–72, which is simultaneous with the rise in cotton. These favourable crops occurring all over the world, though accompanied by somewhat deficient harvests, probably assisted in producing the English prosperity of the next succeeding years.

I have not been able to find any statistics of the total produce of silk in any countries, so that I cannot make the argument so complete as in the case of cotton. But there seems to be at least a strong probability that favourable or unfavourable silk seasons occur nearly simultaneously over a very wide area, and that they slightly precede favourable or unfavourable cotton seasons.

VI. *Sunspots.*

I have already mentioned that attempts have been made to show that sunspots affect the harvest. Fig. 19 represents the sunspot curve as given by Wolf's numbers (*Encyclopaedia Britannica*, 'Terrestrial Magnetism,' § 98). The only curve which at all resembles this is the Chinese silk curve, which is very similar. It would be interesting to compare a longer series of Chinese imports, but I have only been able to obtain the total of silk imports before 1830. Since then the proportion of Chinese silk to the total import has been steadily growing, so that now the fluctuations in the total imports nearly coincide with those in Chinese silk; but formerly our chief supply came from other countries, and we cannot use the earlier statistics as any indication of the China produce. In addition to this there were frequent changes in the protective and other duties on silk, probably interfering greatly with the natural trade. I have not thought it worth while to give the statistics for these earlier years.

VII. *Bank-Rate.*

Though hardly bearing on the subject of the paper, I have added in Fig. 18 a curve representing the variations in the bank-rate since 1840, averaged for four years at a time. This apparently resembles the wheat price curve, except that between 1860 and 1870 there is only one maximum instead of the two, *n* and *o*. It may perhaps help to bear out the conclusion which the late Professor Jevons sought to establish, that commercial crises depend chiefly on the amount of raw produce.

VIII. *The Direction of Future Inquiry.*

If the evidence which I have brought forward is held to justify the general conclusion that there are nearly simultaneous variations in the seasons over the whole earth, affecting different articles of raw produce, it will be seen that it becomes of great importance, not only in political economy but also in meteorology, to investigate these variations more closely. We have as yet only a very short series of meteorological observations, but we have a series of wheat prices extending over several hundred years, and these, as Herschel remarked, are our only available indication of the meteorological conditions in those times. The next step is evidently therefore an investigation into any periodicities which may exist in the price of corn, and I hope at some future time to be able to give the results of such an inquiry, upon which I am now engaged.

In conclusion, I take the opportunity to express my thanks to Professor Balfour Stewart for his valuable advice during the progress of the work.

IX. *Note on the Process of Averaging.*

Since the foregoing part of the paper was written, Professor Stokes has kindly read it. He has suggested that the numerical process employed should be further investigated to see how far the results obtained depend thereupon. I give below the mathematical examination of the method which he was kind enough to make. The following is a general description of this examination and its results, omitting the mathematical details.

According to Fourier's theorem, any quantity fluctuating irregularly with the time can be represented as made up of a definite series of terms (in general infinite in number), each of which fluctuates perfectly regularly with the time with a fixed period of its own. These terms are called simple harmonics. The height of the curve representing the original fluctuating quantity at any point is simply the algebraic sum of the heights of the curves representing the component harmonics. Perhaps an illustration from sound may make this a little clearer. Suppose that while a full band is playing, we could draw a curve to represent the pressure of the air at a given moment along a line drawn in the direction in which the sound is travelling. Such a curve would appear to fluctuate irregularly. But we know that it is really made up by the superposition of the perfectly regular waves of pressure corresponding to the separate notes sent out by all the various instruments, and a well-trained ear stationed in the line which can pick out the notes practically analyses this irregular pressure-curve, that is, breaks it up into its simple harmonics. What is true of this pressure-curve is true of all curves however irregularly they fluctuate, and the ear performs for the pressure-curve what the mathematician seeks to perform for any other curve by Fourier's theorem.

Now when a curve is submitted to a smoothing down process such as that employed above in averaging, the harmonics which may be regarded as building it up will be altered more or less in amount or height of fluctuation or amplitude. Thus, considering the effect on a single harmonic if we average for ten years at a time, it will be seen that a harmonic of ten years' period present in the original curve will be entirely destroyed, as in ten years it goes through all its fluctuations up and down. Harmonics longer than ten years will be more or less altered according as they exceed ten years by a smaller or greater amount, but the amplitude of all will be decreased. If we average for four years at a time, a harmonic of four years' period present in the original curve will be entirely destroyed, while those of longer periods will be lessened, though by smaller amounts the more the period exceeds four years. It might be possible then that the process employed—averaging for ten years, and then for four years, and comparing the latter average with the former—might nearly suppress most of the harmonics and bring out into prominence those having periods very near a particular value. If the various curves for wheat, cotton, etc., all had harmonic components near this value—and in irregular curves, the harmonics are very widely distributed, so that this might easily happen—the averaging process would make them appear to resemble each other even though there was no real physical connection.

It is necessary then to examine the effect of averaging to see if this will really happen—if harmonics near some particular value are brought out into especial prominence. We shall suppose in what follows that the final results are arrived at by *subtracting* the ten-yearly average from the four-yearly average, as this gives the same fluctuations, though different in amount, as the process of division which I have actually employed. The advantage of the subtraction process for our present purpose is that it allows us to deal with each harmonic term separately, and so simplifies the mathematical treatment.

Now let us consider in detail the effect of averaging on any harmonic. This may be best seen by drawing several complete fluctuations of the harmonic, and investigating the effect of replacing the ordinate at a definite point by the mean of a number of ordinates distributed at equal distances on the two sides of the point. Let us first take the ten-yearly average. If the point is at a node or place where the curve cuts the base-line, we see that corresponding to every ordinate on the right there is an equal ordinate opposite in sign at an equal distance on the left, so that the mean is zero, or the average curve cuts the base-line at the same point as the original harmonic curve. If the point is where the harmonic has its greatest positive amplitude, then if the harmonic has exactly a ten-year period, there will be equal positive and negative ordinates, and the mean is zero. Since this will be true in this case wherever the point is taken, the harmonic is entirely destroyed or flattened down. If the harmonic has a longer period than ten years, then the mean of the ten ordinates on the two sides of the point with greatest amplitude will be positive, since there are more positive than negative ordinates. As the period increases, the mean will increase, until, with a very long period, it approaches equality with the original ordinate. If the harmonic is somewhat less than ten years in period, then there will be more negative than positive ordinates in the ten years about the point, and the mean will be negative and reduced. This will be true for all harmonics between ten and five years. A five years' harmonic will be included just twice in ten years, and will therefore be quite destroyed. Below five years the mean is again positive for a time, but very much reduced. Similarly at the point of greatest negative amplitude. Above ten years the mean is negative; between ten and five years positive; below five years negative again, and so on.

At intermediate points in the two halves of the original harmonic, the effect of averaging can be seen to be similar in quality to the effect at the point of greatest amplitude in the half in which the point lies, and the general result is that averaging does not alter the period of a harmonic, but replaces it by one of the same period reduced in amplitude. If the period is greater than ten years, the phase is the same, that is the average curve is positive when the original curve is positive, and negative when that is negative. If the period is ten or five years exactly, the amplitude is reduced to zero, and between these values it is much less than the original amplitude, and the phase is altered to exactly the opposite.

Similarly the effect of averaging four years at a time on harmonics having periods between two and four years, is to reduce their amplitude considerably and change the phase to the opposite. A harmonic of four years' period is entirely flattened down. Above four years the phase is the same as the original, and the amplitude approaches equality with the original amplitude as the length of period rises above four years.

Now we can represent the entire process graphically. First draw a given harmonic, which we will call Curve I. Average its ordinates ten years at a time, and draw Curve II to represent the results of this process. It will be a curve of the same period as I, but always less in amplitude, and for some harmonics opposite in phase. Now average I four years at a time, and draw Curve III to represent the result, which is also of the same period as I. Superpose III on II, and subtract the ordinates of the latter from those of the former. By drawing a curve with ordinates equal to the differences we obtain a new Curve IV, also a harmonic of the same period as the original, but always less in amplitude. If the original harmonic has a ten-year period, Curve II is a straight line, and Curve IV is simply a repetition of Curve III, which has about 0·77 of the amplitude of the original curve, as will be seen from the table below, that is 0·77 of the true harmonic is saved after the averaging. For a harmonic of period rather less than ten years Curve II is opposite in phase to Curve III, and Curve IV has therefore a slightly greater amplitude, the maximum value being at about eight years when 0·84 of the true harmonic survives. Below this the amount saved falls to 0·6 at six years and 0·25 at five years, and on the other side it falls to 0·45 at sixteen years.

Thus the effect of the averaging process is practically to destroy all harmonics below five years, to save over half the amplitude at six years, a greater amount up to eight years, when about five-sixths is saved, and beyond that a continually decreasing amount, though at fifteen years still nearly one-half is saved. This wide range of periods thus to a large extent surviving the process seems to show that the fluctuations in the wheat and other curves do not arise merely from that process, but are fluctuations occurring in reality. But this is perhaps best shown by taking two entirely different numbers. I have adopted 15 and 6, at Professor Stokes's suggestion, taking the means of the former to correspond to the latter in time.

I give in Figs. 20 and 21 the results of these processes for English wheat price; Fig. 20 representing the difference between the four-yearly average and the ten-yearly average; Fig. 21, the difference between the six-yearly average and the means of the fifteen-yearly average. A comparison of the two will show that nearly all the chief fluctuations appear in each.

That the fluctuations are real seems to be further indicated by the fact that the members of each of the three groups, corn, cotton, and silk, resemble the other members of their own group much more closely in general than they resemble the members of any other group. Were the fluctuations merely a result of the arithmetical process, it would seem that there is no reason why the resemblance should not be equally strong with members of other groups.

The full mathematical investigation of the process, of which I have given a general description above, is as follows:

Quoting from a letter of Professor Stokes: 'Suppose in the first instance that we are dealing with a function of the time expressed by a simple harmonic term, I mean, a sine or cosine. Let x be a number proportional to the time, and increasing by 2π for one period. Take the coefficient of the sine or cosine for unity. Suppose we divide the whole time, extending if you like from minus infinity to plus infinity, into equal intervals, and consider the function in the first instance only for the end of each interval. Suppose that we take the mean of n consecutive values of the function, α being the constant difference of x in passing from one to the next, the terms being say:

$$\cos(x+\xi), \quad \cos(x+\xi+\alpha), \quad \cos(x+\xi+2\alpha), \text{ etc.};$$

the mean of n consecutive terms will be

$$\frac{\sin\frac{n\alpha}{2}}{n\sin\frac{\alpha}{2}} \times \cos\left(x+\xi+\frac{n-1}{2}\alpha\right),$$

which corresponds in phase with the phase of the middle term if n be odd, or with a value of $n+\xi$ half-way between those of the two middle terms if n be even, and which has for coefficient the original coefficient multiplied by C, where

$$C = \frac{\sin\frac{n\pi}{N}}{n\sin\frac{\pi}{N}}$$

if N denote the number of times that the interval of x is contained in the period.'

In the process employed in the paper, n is taken in succession equal to 4 and 10. For simplicity we will suppose the result in the latter case deducted from that in the former, instead of being divided into it. This leads to a function agreeing in phase and period with the original, but having the coefficient multiplied by C', where

$$C' = \frac{\sin\frac{4\pi}{N}}{4\sin\frac{\pi}{N}} - \frac{\sin\frac{10\pi}{N}}{10\sin\frac{\pi}{N}}.$$

The following table gives the values of C' for the values 2, 3, 4...16 of N, corresponding to a periodic inequality of 2, 3, 4...16 years:

N	C'	N	C'
2	0	10	·77
3	- ·15	11	·71
4	- ·14	12	·64
5	·25	13	·60
6	·60	14	·53
7	·79	15	·48
8	·84	16	·45
9	·82		

Thus while for eight, nine, and ten-year periods the process saves about 80 per cent. of the coefficient, it falls to 60 per cent. on the one side for six years, and to 45 per cent. on the other for sixteen years.

APPENDIX.

Table I. *The Price of Wheat per Quarter in England since* 1756, *with the Ten-Yearly and Four-Yearly Averages and the Percentage which the latter is of the former. See Fig.* 1, *p* 536.

Year	Price in shillings per quarter	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1756	40·1	—	—	—
1757	53·3	—	—	—
1758	44·4	—	—	—
1759	35·2	—	—	—
1760	32·4	39·25	32·3	82·3
1761	26·8	39·55	32·5	82·2
1762	34·7	39·95	34·8	92·1
1763	36·1	40·89	40·1	98·1
1764	41·5	41·43	42·2	101·8
1765	48·0	42·55	47·5	111·6
1766	43·1	44·73	50·5	112·9
1767	57·3	46·49	48·7	104·8
1768	53·8	48·14	48·8	101·4
1769	40·6	49·42	46·6	94·3
1770	43·6	49·60	46·3	93·3
1771	48·6	49·22	49·3	100·2
1772	52·3	48·18	51·9	107·7
1773	52·6	47·13	52·2	110·7
1774	54·3	46·54	49·0	105·3
1775	49·8	45·85	47·6	103·8
1776	39·3	45·59	44·8	98·3
1777	46·9	45·29	41·0	90·5
1778	43·3	45·46	40·4	88·9
1779	34·7	45·06	40·2	89·2
1780	36·7	44·39	41·7	93·9
1781	46·0	44·46	46·6	104·8
1782	49·3	44·01	50·0	113·6
1783	54·3	44·31	49·2	111·0
1784	50·3	46·11	46·9	101·7
1785	43·1	47·91	43·9	91·6
1786	40·0	48·17	42·9	89·1
1787	42·4	47·54	45·3	95·3
1788	46·3	47·04	49·0	104·2
1789	52·7	47·24	50·1	106·0
1790	54·7	50·45	49·7	98·5
1791	48·6	54·31	48·9	90·0
1792	43·0	55·44	48·3	87·1
1793	49·3	55·99	54·9	98·1
1794	52·3	57·62	63·8	110·7
1795	75·2	63·45	64·9	102·3
1796	78·6	70·54	64·8	91·9
1797	53·7	73·22	63·3	86·4

Year	Price in shillings per quarter	Ten yearly average	Four-yearly average	Per cent. 4 years of 10 years
1798	51·8	74·17	71·9	97·0
1799	69·0	75·17	88·3	117·4
1800	113·0	76·62	92·8	121·1
1801	119·5	76·67	90·3	117·8
1802	69·8	78·83	77·6	98·4
1803	58·8	81·78	70·1	85·7
1804	62·3	84·61	72·5	85·7
1805	89·7	83·95	76·6	91·3
1806	79·1	81·53	81·3	99·7
1807	75·3	87·20	83·2	95·4
1808	81·3	92·29	90·1	97·6
1809	97·3	93·49	95·1	101·7
1810	106·4	91·08	106·4	116·8
1811	95·3	91·02	109·5	120·3
1812	126·5	93·18	101·4	108·8
1813	109·7	93·68	94·0	100·3
1814	74·3	91·39	82·1	89·8
1815	65·6	87·54	78·8	90·0
1816	78·5	83·62	81·8	97·8
1817	96·9	75·43	84·0	111·3
1818	86·3	69·79	81·4	116·6
1819	74·5	68·75	71·2	103·5
1820	67·8	69·04	60·7	87·9
1821	56·1	67·06	55·4	82·6
1822	44·6	63·22	54·5	86·2
1823	53·3	60·63	57·6	95·0
1824	63·9	59·82	61·8	103·3
1825	68·5	59·46	62·4	105·0
1826	58·7	60·48	61·5	101·7
1827	58·5	61·89	61·0	98·6
1828	60·4	61·85	62·4	100·9
1829	66·3	60·08	64·3	107·0
1830	64·3	57·16	63·9	111·7
1831	66·3	56·14	60·5	107·8
1832	58·7	55·87	56·0	100·2
1833	52·9	56·29	49·3	87·6
1834	46·2	56·73	46·7	82·3
1835	39·3	56·93	47·4	83·3
1836	48·5	56·73	52·1	91·9
1837	55·8	56·59	59·9	105·8
1838	64·6	56·31	64·3	114·2
1839	70·7	56·82	66·5	117·0

Year	Price in shillings per quarter	Ten yearly average	Four-yearly average	Per cent 4 years of 10 years
1840	66·3	57·97	64·6	111·4
1841	64·3	58·59	59·5	101·5
1842	57·3	59·98	55·7	92·9
1843	50·1	58·57	52·4	89·4
1844	51·3	55·93	51·7	92·4
1845	50·8	53·33	56·6	106·1
1846	54·7	50·75	56·4	111·1
1847	69·7	49·09	54·8	111·6
1848	50·5	49·41	51·2	103·6
1849	44·3	51·54	43·4	84·2
1850	40·3	53·91	40·9	75·9
1851	38·5	55·36	43·2	78·0
1852	40·7	54·02	51·2	94·8
1853	53·3	53·39	60·3	112·9
1854	72·4	53·33	67·4	126·4
1855	74·7	54·63	68·1	124·6
1856	69·2	56·31	61·1	108·5
1857	56·3	57·78	53·3	92·2
1858	44·2	56·92	49·4	86·8
1859	43·7	53·70	49·1	91·4
1860	53·3	50·41	51·9	103·0
1861	55·3	48·48	52·2	107·7
1862	55·4	49·29	48·9	99·2
1863	44·7	51·25	45·5	88·8
1864	40·2	51·70	44·1	85·3
1865	41·8	51·05	49·1	96·2
1866	49·9	51·19	54·9	107·2
1867	64·4	51·35	56·5	110·0
1868	63·7	52·75	55·8	105·8
1869	48·2	54·30	53·9	99·3
1870	46·8	54·64	52·2	95·5
1871	56·7	54·27	54·8	101·0
1872	57·0	53·51	57·0	106·5
1873	58·7	51·77	54·2	104·7
1874	55·7	51·33	49·5	96·4
1875	45·2	51·08	51·0	99·8
1876	46·2	—	—	—
1877	56·8	—	—	—
1878	46·4	—	—	—
1879	43·8	—	—	—
1880	44·3	—	—	—

TABLE II. *The Price of Wheat at Delhi in Rupees per One Thousand Seers, from* 1763 *to* 1835 (*Jevons, Nature, vol.* 19, *p.* 588), *treated as in Table I. See Fig.* 2.

Year	Price in rupees	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years	Year	Price in rupees	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1763	50	—		—	1800	22	27·0	22·0	81·5
1764	35		—	—	1801	23	28·7	34·0	118·5
1765	27		—	—	1802	25	30·0	40·0	133·3
1766	24		—	—	1803	65	32·8	43·0	131·1
1767	23	30·3	23·0	75·9	1804	48	35·1	44·0	125·3
1768	21	35·3	24·0	68·0	1805	33	35·4	35·0	98·5
1769	24	37·1	26·5	71·4	1806	31	35·9	32·0	89·1
					1807	28	37·8	34·0	89·9
1770	28	38·4	31·0	80·7	1808	36	35·6	32·0	89·9
1771	33	38·5	50·0	129·9	1809	40	33·8	32·0	94·7
1772	38	37·9	55·0	145·1					
1773	100	38·3	58·0	151·4	1810	25	32·8	34·0	103·6
1774	53	39·2	54·5	139·0	1811	28	32·5	35·0	107·7
1775	40	40·9	34·0	83·1	1812	44	33·8	36·0	106·5
1776	25	43·1	27·0	62·6	1813	43	34·1	35·0	102·6
1777	17	48·4	25·0	51·7	1814	30	34·3	31·0	90·4
1778	25	55·1	30·0	54·4	1815	23	36·4	30·5	83·8
1779	33	53·8	39·5	73·4	1816	28	37·4	33·0	88·2
					1817	41	36·5	37·5	102·7
1780	45	52·3	56·0	107·1	1818	39	35·5	42·0	118·3
1781	55	52·1	89·5	171·7	1819	42	36·4	41·0	112·7
1782	91	52·6	88·0	167·3					
1783	167	52·4	81·0	154·5	1820	46	38·0	40·0	105·3
1784	40	51·5	64·0	124·3	1821	38	40·0	38·0	95·0
1785	25	49·6	27·5	55·4	1822	35	38·9	36·0	92·5
1786	23	47·4	23·0	48·5	1823	33	37·2	36·5	98·1
1787	22	46·4	23·0	49·6	1824	39	35·1	40·0	114·0
1788	23	35·1	24·0	68·4	1825	39	32·6	39·0	119·6
1789	24	34·3	26·5	77·3	1826	48	31·4	35·0	111·5
					1827	30	30·1	30·0	99·7
1790	26	33·2	41·0	123·5	1828	22	30·1	23·5	78·1
1791	33	32·3	48·5	150·1	1829	21	30·2	22·5	74·5
1792	81	31·6	50·0	158·2					
1793	54	30·1	45·0	149·5	1830	21	28·8	22·5	78·1
1794	32	29·4	28·5	96·9	1831	26	—	—	—
1795	14	29·0	19·0	65·5	1832	22	—	—	—
1796	14	28·0	13·0	46·4	1833	33	—	—	—
1797	15	22·4	13·5	60·3	1834	40	—	—	—
1798	8	23·5	15·5	66·0	1835	25	—	—	—
1799	17	25·1	17·5	69·7					

TABLE III. *The Price of Wheat in France in Francs per Setier de Paris from* 1800 *to* 1850 (*McCulloch's Commercial Dictionary*), *treated as in Table I. See Fig.* 3.

Year	Price in francs	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1800	21·5	—	—	—
1801	24·4	—	—	—
1802	24·2	—	—	—
1803	18·8	—	—	—
1804	20·2	20·00	19·85	99·2
1805	20·2	19·81	19·80	100·0
1806	20·2	19·98	18·90	94·6
1807	18·6	20·99	17·70	84·3
1808	16·7	21·37	17·50	81·9
1809	15·2	21·10	19·55	92·6
1810	19·6	21·03	23·80	113·2
1811	26·1	21·84	25·65	117·4
1812	34·3	23·60	25·10	106·3
1813	22·6	24·40	23·50	96·2
1814	17·5	24·72	22·00	89·0
1815	19·5	24·67	25·40	103·0
1816	28·3	23·84	27·20	114·1
1817	36·2	22·00	26·90	122·3
1818	24·7	21·49	24·60	114·5
1819	18·4	21·39	20·00	93·5
1820	19·1	21·01	17·80	84·7
1821	17·8	19·66	17·60	89·5
1822	15·9	17·87	16·90	94·6
1823	17·5	17·60	16·40	93·2
1824	16·5	18·02	16·10	89·4
1825	15·7	18·23	16·30	89·4
1826	14·8	18·66	17·70	94·9
1827	18·3	19·30	19·40	100·5
1828	22·0	19·18	21·00	109·5
1829	22·6	19·00	22·00	115·8
1830	21·2	18·91	22·05	116·6
1831	22·1	19·07	20·50	107·5
1832	22·3	18·99	18·85	99·3
1833	16·3	18·72	17·00	90·8
1834	14·7	18·71	15·55	83·1
1835	14·8	18·79	15·85	84·4
1836	16·4	18·41	17·00	92·3
1837	17·5	18·15	18·90	104·1
1838	19·3	18·54	20·30	109·5
1839	22·5	18·97	20·50	108·0
1840	22·0	19·38	20·60	106·2
1841	18·3	22·13	20·05	90·6
1842	19·7	23·32	19·30	82·8
1843	20·2	23·03	19·45	84·5
1844	19·0	22·31	25·50	114·3
1845	18·9	21·54	27·80	129·0
1846	43·9	—	—	—
1847	29·4	—	—	—
1848	16·4	—	—	—
1849	15·3	—	—	—
1850	14·3	—	—	—

TABLE IV. *Estimated Yield per Acre of Wheat in England,* 100 = 28 *Bushels per Acre* (*Caird, Journal of the Statistical Society,* 1879, *p.* 482), *with the Four-Yearly Average. See Fig.* 1 *a.*

Year	Yield per acre	Four-yearly average	Year	Yield per acre	Four-yearly average	Year	Yield per acre	Four-yearly average
1849	123	—	1859	92	94·5	1869	102	107·5
1850	102	103·5	1860	78	92·5	1870	112	99·0
1851	110	90·5	1861	92	105·0	1871	90	93·5
1852	79	97·0	1862	108	117·0	1872	92	92·0
1853	71	94·0	1863	141	121·5	1873	80	89·0
1854	127	97·5	1864	127	114·5	1874	106	85·0
1855	96	111·0	1865	110	100·0	1875	78	83·5
1856	96	108·0	1866	90	100·0	1876	76	84·0
1857	124	107·0	1867	74	98·0	1877	74	—
1858	116	102·5	1868	126	103·5	1878	108	—

TABLE V. *Wheat Imports into England in* 100,000 *Cwts., from* 1839 *to* 1880 (*Whitaker's Almanac,* 1882, *p.* 424), *with the Four-Yearly Average. See Fig.* 1 *b.*

Year	Im-ports	Four-yearly average	Year	Im-ports	Four-yearly average	Year	Im-ports	Four-yearly average	Year	Im-ports	Four-yearly average
1839	108	—	1850	163	164·0	1860	255	282·0	1870	298	370·0
1840	87	102·0	1851	164	167·0	1861	289	299·0	1871	388	383·0
1841	99	84·5	1852	132	162·5	1862	410	293·5	1872	419	411·0
1842	115	72·0	1853	209	150·0	1863	243	273·5	1873	428	443·0
1843	37	48·5	1854	145	160·0	1864	232	229·0	1874	408	447·5
1844	36	41·0	1855	114	144·5	1865	209	254·0	1875	518	474·0
1845	6	61·5	1856	173	154·0	1866	231	277·0	1876	436	494·5
1846	86	72·5	1857	146	169·0	1867	344	319·0	1877	533	513·0
1847	118	119·5	1858	184	189·5	1868	324	335·5	1878	491	542·0
1848	80	139·0	1859	173	225·0	1869	376	346·5	1879	591	—
1849	194	150·0							1880	553	—

TABLE VI. *Cotton Consumed in Great Britain in Million Pounds, treated as in Table I to* 1861. (*Encyclopaedia Britannica, vol.* 6.) *See Fig.* 4.

Year	Cotton consumed	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years	Year	Cotton consumed	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1801	48	—	—	—	1840	459	451·2	428·5	95·0
1802	52	—	—	—	1841	438	477·9	462·5	96·8
1803	54	—	—	—	1842	435	485·4	483·8	99·7
1804	62	—	—	—	1843	518	501·4	526·0	104·9
1805	59	60·4	63·5	105·1	1844	544	526·2	570·8	108·5
1806	59	65·4	61·8	94·5	1845	607	539·1	551·5	102·3
1807	74	67·8	62·8	92·6	1846	614	561·2	559·8	99·5
1808	55	69·5	67·8	97·6	1847	441	591·7	565·5	95·6
1809	63	69·9	73·7	105·4	1848	577	616·0	559·0	90·7
					1849	630	639·2	613·5	96·0
1810	79	72·2	79·0	109·4					
1811	98	75·2	81·0	107·7	1850	588	662·4	654·3	98·8
1812	76	78·5	77·8	99·1	1851	659	690·1	687·0	99·5
1813	71	83·9	73·8	88·0	1852	740	728·5	734·0	100·7
1814	66	88·6	77·0	86·9	1853	761	761·4	779·0	102·3
1815	82	92·7	86·0	92·8	1854	776	796·1	816·8	102·6
1816	89	95·8	96·8	101·0	1855	839	845·7	833·5	98·6
1817	107	102·8	103·8	101·1	1856	891	880·6	865·5	98·3
1818	110	111·1	111·5	100·4	1857	826	—	—	—
1819	110	121·0	117·0	96·7	1858	906	—	—	—
					1859	977	—	—	—
1820	120	129·5	126·3	97·5					
1821	129	135·6	137·3	101·2	1860	1,084	—	—	—
1822	146	144·6	148·5	102·7	1861	1,007	—	—	—
1823	154	155·5	158·0	101·6	1862	452	—	—	—
1824	165	166·4	159·0	95·6	1863	508	—	—	—
1825	167	179·2	169·8	94·7	1864	554	—	—	—
1826	150	192·6	183·0	95·0	1865	723	—	—	—
1827	197	205·7	196·0	95·3	1866	881	—	—	—
1828	218	219·0	220·5	100·7	1867	967	—	—	—
1829	219	232·8	237·0	101·8	1868	992	—	—	—
					1869	939	—	—	—
1830	248	247·9	251·8	101·6					
1831	263	267·6	268·8	100·4	1870	1,078	—	—	—
1832	277	284·5	282·5	99·3	1871	1,207	—	—	—
1833	287	304·4	296·3	97·3	1872	1,181	—	—	—
1834	303	320·7	313·8	97·8	1873	1,245	—	—	—
1835	318	341·8	333·5	97·6	1874	1,277	—	—	—
1836	347	359·3	362·0	100·7	1875	1,229	—	—	—
1837	366	375·1	378·1	100·8	1876	1,280	—	—	—
1838	417	398·2	411·0	103·2					
1839	382	422·3	424·0	100·4					

TABLE VII. *Cotton Imports into Great Britain from East India, in Thousands of Bales, treated as in Table I.* (*Encyclopaedia Britannica, vol.* 6.) *See Fig.* 5.

Year	Cotton imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1801	14	—	—	—
1802	8	—	—	—
1803	10	—	—	
1804	4	—		
1805	2	18·5	6·25	33·8
1806	8	18·6	8·50	45·7
1807	11	18·1	17·00	93·9
1808	13	17·3	34·75	200·8
1809	36	18·2	35·75	196·4
1810	79	20·2	33·75	167·0
1811	15	22·5	24·25	107·8
1812	3	33·4	8·25	24·7
1813	2	56·9	10·00	17·6
1814	13	71·7	17·00	23·7
1815	22	69·6	46·25	66·5
1816	31	71·1	105·25	148·0
1817	120	72·7	145·75	200·5
1818	288	76·3	152·50	199·9
1819	184	80·1	130·00	163·3
1820	58	84·0	72·75	86·6
1821	30	87·4	36·25	41·5
1822	19	82·8	34·50	41·7
1823	38	66·5	42·25	63·5
1824	51	56·1	53·75	95·8
1825	61	53·8	62·75	166·6
1826	65	58·5	71·25	121·8
1827	74	67·5	76·00	112·6
1828	85	73·2	68·50	93·6
1829	80	77·0	69·25	89·9
1830	35	82·7	75·25	91·0
1831	77	98·1	79·00	80·5
1832	109	105·2	92·50	87·9
1833	95	107·4	102·75	95·7
1834	89	112·7	130·25	115·6
1835	118	130·8	142·75	109·1
1836	219	150·5	147·25	97·8
1837	145	165·2	151·00	91·4
1838	107	173·9	150·25	86·4
1839	133	188·8	182·50	96·7

Year	Cotton imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1840	216	192·5	219·75	114·1
1841	274	175·5	232·00	132·2
1842	256	183·3	237·50	129·5
1843	182	195·4	207·50	106·3
1844	238	200·3	156·00	77·9
1845	155	209·5	166·25	79·3
1846	49	215·0	163·75	76·2
1847	223	211·5	170·50	80·6
1848	228	241·8	235·25	97·3
1849	182	248·8	261·75	105·2
1850	308	272·9	260·00	95·3
1851	329	314·3	335·75	106·8
1852	221	360·0	335·75	93·3
1853	485	373·3	352·50	94·4
1854	308	406·2	413·00	101·7
1855	396	431·7	461·75	107·0
1856	463	497·5	475·00	95·5
1857	680	582·6	503·75	86·5
1858	361	673·2	528·75	78·5
1859	511	822·2	605·50	73·6
1860	563	923·4	783·25	84·8
1861	987	1,063·8	1,003·25	94·3
1862	1,072	1,146·9	1,312·00	114·4
1863	1,391	1,256·0	1,417·25	112·8
1864	1,798	1,354·5	1,616·00	119·3
1865	1,408	1,404·5	1,646·00	117·2
1866	1,867	1,429·4	1,559·50	109·1
1867	1,511	1,451·0	1,581·50	109·0
1868	1,452	1,483·2	1,630·50	109·9
1869	1,496	1,558·8	1,311·75	84·1
1870	1,063	1,594·1	1,270·75	79·7
1871	1,236	1,482·9	1,164·00	78·5
1872	1,288	—	—	—
1873	1,069	—	—	—
1874	1,042	—	—	—
1875	1,055	—	—	—
1876	775	—	—	—

TABLE VIII. *Cotton Imports into Great Britain from United States, in Thousands of Bales, treated as in Table I to* 1861. (*Encyclopaedia Britannica, vol.* 6.) *See Fig.* 6.

Year	Cotton imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years	Year	Cotton imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1801	84	—	—	—	1840	1,238	1,074·7	992·00	92·3
1802	107	—	—	—	1841	902	1,091·4	1,137·50	104·2
1803	107	—	—	—	1842	1,013	1,094·3	1,139·75	104·1
1804	104	—	—	—	1843	1,397	1,129·3	1,289·25	114·2
1805	124	126·7	131·00	103·4	1844	1,247	1,195·6	1,266·50	105·9
1806	125	131·1	114·15	87·3	1845	1,500	1,190·2	1,138·25	95·6
1807	171	129·9	123·50	95·1	1846	932	1,239·4	1,170·25	94·4
1808	38	123·0	154·00	125·2	1847	874	1,317·0	1,164·75	88·4
1809	160	117·5	143·25	121·9	1848	1,375	1,330·5	1,227·75	92·3
					1849	1,478	1,372·4	1,357·75	98·9
1810	247	125·4	157·50	125·6					
1811	128	129·5	127·00	98·1	1850	1,184	1,384·7	1,461·25	105·5
1812	95	132·4	77·50	51·0	1851	1,394	1,467·3	1,474·75	100·5
1813	38	149·4	96·25	64·4	1852	1,789	1,528·1	1,595·25	104·4
1814	49	153·9	114·00	74·1	1853	1,532	1,576·9	1,652·50	104·8
1815	203	159·5	154·50	96·9	1854	1,666	1,637·7	1,644·75	100·4
1816	166	176·7	194·25	109·9	1855	1,623	1,777·4	1,632·25	91·8
1817	200	200·2	194·75	97·3	1856	1,758	1,822·1	1,681·50	92·3
1818	208	241·6	229·00	94·8	1857	1,482	—	—	—
1819	205	264·9	254·00	95·9	1858	1,863	—	—	—
					1859	2,086	—	—	—
1820	303	286·9	284·50	99·2					
1821	300	309·9	346·25	111·7	1860	2,581	—	—	—
1822	330	354·6	341·00	96·2	1861	1,841	—	—	—
1823	452	378·2	371·25	98·2	1862	72	—	—	—
1824	282	404·0	388·25	96·1	1863	132	—	—	—
1825	423	435·5	437·00	100·3	1864	198	—	—	
1826	396	466·4	477·50	102·4	1865	462	—	—	—
1827	647	496·3	487·50	98·2	1866	1,163	—	—	—
1828	444	516·6	543·00	105·1	1867	1,226	—	—	—
1829	463	561·8	533·50	95·0	1868	1,269	—		
					1869	1,040	—	—	
1830	618	595·8	579·75	97·3					
1831	609	632·7	627·75	99·2	1870	1,664	—	—	—
1832	629	652·5	656·75	100·6	1871	2,249	—	—	
1833	655	710·6	695·25	97·8	1872	1,404	—	—	—
1834	734	745·8	729·25	97·8	1873	1,898	—	—	—
1835	763	807·8	776·75	96·2	1874	1,958	—	—	
1836	765	837·1	849·50	101·5	1875	1,859	—	—	—
1837	845	875·5	862·50	98·5	1876	2,075	—	—	—
1838	1,025	949·7	980·75	103·3					
1839	815	1001·0	995·00	99·4					

TABLE IX. *Cotton Crop, United States, in Ten Thousands of Bales, treated as in Table I to* 1861. (*Encyclopaedia Britannica, vol.* 6.) *See Fig.* 7.

Year	Crop	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years	Year	Crop	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1821	37	—	—	—	1850	217	262·7	262·25	99·8
1822	54	—	—	—	1851	242	277·5	275·75	99·4
1823	59	—	—	—	1852	309	289·4	297·50	102·8
1824	51	—	—	—	1853	335	297·7	310·25	104·2
1825	57	68·1	69·00	101·3	1854	304	309·5	324·25	104·8
1826	72	74·8	74·25	99·3	1855	293	336·0	317·00	94·3
1827	96	79·3	81·50	102·8	1856	365	350·0	322·00	92·0
1828	72	84·1	88·00	104·6	1857	306	—	—	—
1829	86	91·1	90·00	98·8	1858	324	—	—	—
					1859	399	—	—	—
1830	98	97·9	96·75	98·8					
1831	104	104·3	102·00	97·8	1860	482	—	—	—
1832	99	109·0	107·75	98·8	1861	382	—	—	—
1833	107	119·9	113·00	94·2	1862				
1834	121	124·9	122·25	97·9	1863				
1835	125	136·9	131·25	95·9	1864	no	returns		
1836	136	142·9	146·25	102·3	1865				
1837	143	149·9	149·00	99·4	1866	231	—	—	—
1838	181	163·1	169·50	103·9	1867	226	—	—	—
1839	136	172·1	174·75	101·5	1868	250	—	—	—
					1869	244	—	—	—
1840	218	184·5	171·75	93·1					
1841	164	192·6	197·50	102·5	1870	316	—	—	—
1842	169	196·9	195·75	99·4	1871	435	—	—	—
1843	239	203·0	217·00	106·9	1872	297	—	—	—
1844	211	217·5	229·00	105·5	1873	393	—	—	—
1845	249	217·4	215·75	99·2	1874	417	—	—	—
1846	217	225·2	223·50	99·2	1875	383	—	—	—
1847	186	239·4	231·50	100·9	1876	467	—	—	—
1848	242	249·0	231·50	93·0	1877	—	—	—	—
1849	281	258·3	245·50	95·0	1878	—	—	—	—

TABLE X. *Cotton Imports into Great Britain from Egypt, in Thousands of Bales, treated as in Table I. (Encyclopaedia Britannica, vol. 6.) See Fig. 8.*

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1823	6	—	—	
1824	38	—	—	—
1825	111	—	—	
1826	48	—	—	—
1827	22	37·7	32·00	84·9
1828	33	37·5	23·75	63·3
1829	25	34·4	27·75	80·7
1830	15	27·7	29·75	107·4
1831	38	26·4	24·50	92·8
1832	41	28·3	22·50	79·5
1833	4	28·0	24·00	85·7
1834	7	28·8	22·50	78·1
1835	44	31·1	31·75	102·1
1836	35	31·4	37·50	119·4
1837	41	29·3	34·75	118·6
1838	30	33·8	35·50	105·0
1839	33	39·8	35·50	89·2
1840	38	43·6	34·00	78·0
1841	41	46·1	37·00	80·3
1842	20	44·1	44·25	100·3
1843	49	44·0	54·50	123·9
1844	67	47·9	64·50	134·7
1845	82	52·0	57·50	110·6
1846	60	54·6	48·00	87·9
1847	21	71·6	45·50	63·5
1848	29	77·2	50·25	65·1
1849	72	78·6	61·75	78·6
1850	79	81·9	102·00	124·5
1851	67	87·3	110·25	126·3
1852	190	92·8	108·25	116·6
1853	105	100·5	122·75	122·1
1854	81	103·4	103·75	100·3
1855	115	106·4	96·00	90·2
1856	114	109·5	102·75	95·8
1857	76	105·2	99·25	94·3
1858	106	119·5	98·00	82·0
1859	101	143·3	103·50	72·2
1860	109	173·2	113·75	65·7
1861	98	181·8	150·50	82·8
1862	147	194·0	203·00	104·7
1863	248	203·5	282·00	138·5
1864	319	216·0	295·25	136·7
1865	414	227·1	282·75	124·5
1866	200	244·5	253·25	103·6
1867	198	260·3	206·25	79·2
1868	201	268·3	211·25	78·7
1869	226	266·4	229·75	86·2
1870	220	253·1	255·75	101·0
1871	272	266·3	281·25	105·6
1872	305	—	—	—
1873	328	—	—	—
1874	300	—	—	—
1875	281	—	—	—
1876	332		—	

TABLE XI. *Cotton Imports into Great Britain from West Indies, in Thousands of Bales, treated as in Table I.* (*Encyclopaedia Britannica, vol.* 6.) *See Fig.* 9.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1801	92	—	—	—
1802	91	—	—	—
1803	46	—	—	—
1804	86	—	—	—
1805	75	81·1	80·00	98·7
1806	78	78·4	75·25	96·0
1807	81	75·7	82·25	108·6
1808	67	78·4	85·75	109·3
1809	103	77·3	81·75	105·7
1810	92	75·1	81·00	107·8
1811	65	72·2	73·50	101·8
1812	64	68·6	69·25	100·9
1813	73	67·0	66·25	98·9
1814	75	59·8	62·50	104·5
1815	53	53·7	55·50	103·3
1816	49	51·3	49·50	96·5
1817	45	49·0	44·00	89·8
1818	51	44·5	39·50	88·8
1819	31	39·6	38·50	97·2
1820	31	37·5	36·00	96·0
1821	41	34·4	35·25	102·4
1822	41	33·0	34·00	103·0
1823	28	29·9	31·75	106·2
1824	26	28·7	26·00	90·6
1825	32	26·8	26·75	99·8
1826	18	23·8	25·25	106·1
1827	31	20·5	22·00	107·3
1828	20	19·0	21·50	113·1
1829	19	18·1	15·50	85·6
1830	12	17·2	12·50	72·7
1831	11	18·7	11·00	58·8
1832	8	18·4	12·25	66·6
1833	13	19·3	15·25	79·0
1834	17	21·0	21·50	102·4
1835	23	22·0	25·25	114·8
1836	33	24·2	28·25	116·7
1837	28	25·1	31·50	125·5
1838	29	25·6	28·75	112·3
1839	36	25·6	30·00	117·2
1840	22	24·2	27·00	111·5
1841	33	21·8	22·50	103·2
1842	17	19·5	21·25	109·0
1843	18	17·4	15·25	87·7
1844	17	14·7	13·25	90·1
1845	9	13·1	10·00	76·3
1846	9	10·3	7·75	75·2
1847	5	9·9	7·75	78·3
1848	8	9·0	7·00	77·8
1849	9	8·3	7·00	84·3
1850	6	8·3	8·25	99·4
1851	5	8·5	8·25	97·1
1852	13	9·1	9·25	101·6
1853	9	8·9	10·25	115·2
1854	10	8·7	9·75	112·0
1855	9	9·1	10·25	112·6
1856	11	9·6	9·25	96·4
1857	11	10·3	8·75	85·0
1858	6	11·7	8·50	72·6
1859	7	16·7	8·25	49·4
1860	10	28·9	11·75	40·7
1861	10	39·0	15·75	40·4
1862	20	50·8	28·25	55·6
1863	23	60·3	58·50	97·0
1864	60	70·2	81·50	116·1
1865	131	80·4	108·00	134·3
1866	112	92·6	118·25	127·7
1867	129	107·2	112·00	104·5
1868	101	118·7	112·00	94·4
1869	106	124·5	113·00	103·8
1870	112	120·3	129·25	107·4
1871	133	116·1	137·25	118·2
1872	166	—	—	—
1873	138	—	—	—
1874	118	—	—	—
1875	89	—	—	—
1876	70	—	—	—

TABLE XII. *Cotton Imports into Great Britain from Brazil, in Thousands of Bales, treated as in Table I.* (*Encyclopaedia Britannica, vol.* 6.) *See Fig.* 10.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years	Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1801	70	—	—	—	1840	85	109·0	91·0	83·5
1802	75	—	—	—	1841	94	102·5	91·0	88·8
1803	76	—	—	—	1842	87	101·8	98·0	96·3
1804	48	—	—	—	1843	98	98·0	102·0	104·1
1805	51	72·4	42·0	58·0	1844	113	104·5	101·0	96·7
1806	51	77·2	43·0	57·4	1845	110	113·2	104·0	91·9
1807	19	79·6	65·0	81·6	1846	84	114·7	104·0	90·7
1808	50	85·7	88·0	102·7	1847	110	120·4	114·5	95·1
1809	141	96·0	90·5	94·3	1848	100	123·9	136·5	110·2
					1849	164	123·3	136·0	110·8
1810	143	100·0	125·0	125·0					
1811	118	107·2	124·0	115·6	1850	172	125·8	147·0	116·8
1812	99	116·7	126·0	108·0	1851	109	129·6	139·5	107·6
1813	137	127·9	119·5	93·4	1852	144	135·5	123·0	90·8
1814	151	126·4	124·5	98·5	1853	133	136·1	130·0	95·5
1815	91	130·1	120·0	92·2	1854	107	132·2	124·0	93·8
1816	123	130·4	122·5	93·9	1855	135	125·3	134·0	107·0
1817	114	134·8	131·0	97·2	1856	122	124·4	134·0	107·7
1818	162	135·6	145·5	107·3	1857	169	123·4	130·5	105·7
1819	126	134·8	147·0	109·1	1858	106	123·9	126·0	101·7
					1859	125	134·4	108·5	80·7
1820	180	145·1	142·5	98·2					
1821	121	138·3	147·0	106·3	1860	103	154·9	115·5	74·6
1822	143	138·9	138·0	99·3	1861	100	183·4	119·0	64·9
1823	145	139·4	156·0	111·9	1862	134	210·2	146·0	69·5
1824	143	142·8	144·0	100·8	1863	138	263·3	206·0	78·2
1825	194	143·9	128·0	89·0	1864	212	302·2	274·0	90·7
1826	55	148·6	134·0	90·2	1865	340	332·2	349·0	105·0
1827	120	145·8	125·5	86·1	1866	407	373·7	455·0	121·7
1828	167	147·6	159·5	108·1	1867	437	432·0	499·0	115·5
1829	160	143·7	171·5	119·4	1868	637	465·3	498·0	107·0
					1869	514	493·8	517·0	104·7
1830	191	138·6	158·5	114·4					
1831	168	148·0	159·0	107·4	1870	403	502·2	537·0	106·9
1832	115	147·7	137·5	93·1	1871	515	494·6	526·5	106·5
1833	163	144·8	131·0	90·5	1872	717	—	—	
1834	104	138·7	140·0	100·9	1873	471			
1835	143	128·1	128·0	99·9	1874	497			
1836	149	120·7	137·0	113·5	1875	424			
1837	117	117·9	126·0	106·9	1876	331			
1838	138	111·4	110·0	98·7					
1839	99	112·3	104·0	92·6					

TABLE XIII. *Silk Imports into Great Britain from Italy, in Hundreds of Bales, treated as in Table I.* (*Durant's Circular.*) $\frac{25}{28}$ *Actual Numbers from* 1830 *to* 1839, *to equalise Weights. See Fig.* 12.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1830	57	—	—	—
1831	59	—	—	—
1832	41	—	—	—
1833	56	—	—	—
1834	32	47·5	47·75	100·5
1835	51	48·9	41·00	83·9
1836	52	48·2	44·75	92·8
1837	29	51·7	44·75	86·6
1838	47	52·8	49·50	93·7
1839	51	55·8	55·25	99·0
1840	71	56·4	62·50	110·8
1841	52	55·4	66·50	120·0
1842	76	56·2	64·25	114·3
1843	67	58·9	65·50	111·2
1844	62	59·6	57·00	95·6
1845	57	56·2	49·50	88·1
1846	42	54·2	52·50	96·9
1847	37	49·3	52·75	107·0
1848	74	47·2	51·50	109·1
1849	.58	45·3	50·25	110·9
1850	37	42·5	38·50	90·6
1851	32	41·1	35·50	86·4
1852	27	40·3	37·00	91·8
1853	46	35·4	36·25	102·4
1854	43	32·2	36·50	113·3
1855	29	30·3	32·25	106·4
1856	28	29·0	27·75	95·7
1857	29	28·4	27·00	95·1
1858	25	25·9	24·50	94·6
1859	26	23·2	22·00	94·8
1860	18	21·8	21·00	96·3
1861	19	20·6	19·75	95·9
1862	21	19·2	19·25	100·3
1863	21	18·0	18·25	101·4
1864	16	16·7	17·00	101·8
1865	15	17·4	15·50	89·1
1866	16	17·7	14·75	83·3
1867	15	17·2	14·25	82·8
1868	13	16·7	16·50	98·8
1869	13	16·9	18·25	108·0
1870	25	17·0	19·00	111·7
1871	22	16·6	19·75	119·0
1872	16	16·3	18·00	110·4
1873	16	16·2	16·50	101·8
1874	18	16·2	15·50	95·7
1875	16	14·7	14·50	98·6
1876	12	13·7	13·00	94·9
1877	12			
1878	12			
1879	13	—	—	—
1880	10	—	—	—
1881	12	—	—	—

TABLE XIV. *Silk Imports into Great Britain from Brutia, in Bales, treated as in Table I.* (*Durant's Circular.*) $\frac{17}{20}$ *Actual Numbers from* 1830 *to* 1842 *to equalise Weights. See Fig.* 13.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1830	2,020	—	—	—
1831	1,520	—	—	—
1832	1,140	—	—	—
1833	1,480	—	—	—
1834	1,180	1,706·0	1,915·00	112·2
1835	2,300	1,774·0	1,740·00	98·1
1836	2,700	1,848·0	1,970·00	106·6
1837	780	2,040·0	1,855·00	90·9
1838	2,100	2,168·0	1,855·00	85·6
1839	1,840	2,353·0	2,225·00	94·6
1840	2,700	2,330·0	2,465·00	105·8
1841	2,260	2,211·0	2,695·00	121·9
1842	3,060	2,258·0	2,777·50	123·0
1843	2,760	2,192·0	2,730·00	124·5
1844	3,030	2,124·0	2,342·50	110·3
1845	2,070	2,078·0	1,965·00	94·6
1846	1,510	1,964·0	1,567·50	79·8
1847	1,250	1,808·0	1,340·00	74·1
1848	1,440	1,595·0	1,522·50	95·5
1849	1,160	1,307·0	1,490·00	114·0
1850	2,240	1,125·0	1,505·00	133·7
1851	1,120	988·0	1,372·50	138·9
1852	1,500	893·0	850·00	95·2
1853	630	775·0	632·50	81·6
1854	150	687·0	292·50	42·6
1855	250	478·0	210·00	43·9
1856	140	375·8	237·50	63·2
1857	300	233·8	245·00	104·8
1858	260	180·2	247·50	137·3
1859	280	176·6	197·00	111·5
1860	150	162·0	152·00	93·8
1861	98	157·0	105·50	67·2
1862	80	132·9	96·50	72·6
1863	94	116·4	98·00	84·2
1864	114	97·0	105·50	103·6
1865	104	120·1	91·75	76·4
1866	90	164·0	87·00	53·0
1867	59	189·9	82·50	43·4
1868	95	200·3	155·25	77·5
1869	86	205·2	274·75	133·9
1870	381	209·4	335·75	160·3
1871	537	209·5	363·75	173·6
1872	339	212·4	309·25	145·6
1873	198	211·7	211·50	99·9
1874	163	213·3	149·50	70·1
1875	146	184·6	122·00	66·1
1876	91	138·2	103·25	74·7
1877	88	—	—	—
1878	88	—	—	
1879	102	—	—	·
1880	94	—	—	—
1881	73	—	—	—

TABLE XV. *Silk Imports into Great Britain from Persia, in Ballots, treated as in Table I.* (*Durant's Circular.*) *See Fig.* 14.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1830	455	—	—	—
1831	1,569	—	—	—
1832	729	—	—	—
1833	729	—	—	—
1834	2,120	1,472·5	1,748·0	118·7
1835	2,128	1,691·2	2,148·5	127·0
1836	2,016	1,832·4	1,643·0	89·6
1837	2,330	1,890·1	1,749·0	92·5
1838	99	1,987·2	1,905·0	95·9
1839	2,550	2,105·5	2,293·0	108·9
1840	2,642	1,912·5	2,370·0	123·9
1841	2,981	1,931·9	2,157·0	111·6
1842	1,306	1,781·4	2,322·5	130·4
1843	1,700	1,910·4	1,627·0	85·2
1844	3,303	1,763·9	1,853·0	105·0
1845	198	1,925·6	1,634·0	84·9
1846	2,210	1,929·5	1,155·5	39·4
1847	825	2,102·1	1,377·0	65·5
1848	1,389	2,514·5	1,889·5	75·1
1849	1,085	2,352·0	2,438·0	103·6
1850	4,259	2,485·2	2,849·0	114·6
1851	3,020	2,450·0	4,034·0	164·6
1852	3,032	2,614·9	3,388·5	129·6
1853	5,824	2,580·5	3,016·0	116·9
1854	1,678	2,632·2	2,722·5	103·4
1855	1,530	2,446·1	1,785·0	73·0
1856	1,858	2,268·3	1,727·0	76·1
1857	2,474	2,407·0	1,745·0	72·5
1858	1,045	2,071·9	1,880·0	90·7
1859	1,602	2,239·1	1,572·0	70·2
1860	2,398	2,338·5	2,415·0	103·3
1861	1,242	2,216·6	2,633·0	118·8
1862	4,419	2,027·8	2,871·0	141·6
1863	2,473	1,926·3	3,191·5	165·7
1864	3,350	1,773·1	2,246·5	126·7
1865	2,524	1,581·7	1,775·0	112·2
1866	639	1,485·7	945·0	63·6
1867	586	1,043·8	331·0	31·7
1868	30	797·7	292·5	36·7
1869	70	467·5	216·5	46·3
1870	484	223·1	209·0	93·7
1871	282	181·7	194·5	107·0
1872	—	125·6	85·5	68·1
1873	12	122·8	35·0	28·5
1874	48	117·8	91·0	77·2
1875	80	70·4	94·5	134·2
1876	225	51·4	83·0	161·5
1877	25	—	—	—
1878	2	—	—	—
1879	20		—	—
1880	10	—	—	—
1881	89	—	—	—

TABLE XVI. *Silk Imports into Great Britain from Bengal, in Hundreds of Bales, treated as in Table I. (Durant's Circular.) ⅔ Actual Numbers for* 1858–60, *to equalise Weights. See Fig.* 15.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1830	87	—	—	—
1831	75	—	—	—
1832	66	—	—	—
1833	52	—	—	—
1834	63	74·5	66·25	88·9
1835	69	73·2	72·75	99·4
1836	81	73·3	76·75	104·7
1837	78	75·6	84·25	111·4
1838	79	78·5	81·50	103·8
1839	95	83·7	81·00	96·8
1840	74	88·8	83·50	94·0
1841	76	89·5	80·00	89·4
1842	89	89·0	90·25	101·4
1843	81	86·0	101·25	117·7
1844	115	86·6	101·00	116·6
1845	120	89·5	99·00	110·6
1846	88	90·7	82·50	91·0
1847	73	93·5	77·75	83·2
1848	49	94·4	81·50	86·3
1849	101	92·2	85·25	92·5
1850	103	88·4	102·25	115·7
1851	88	93·4	99·50	106·5
1852	117	98·6	97·00	98·3
1853	90	100·5	95·50	95·0
1854	93	97·4	100·75	103·4
1855	82	93·5	109·50	117·3
1856	138	90·8	103·25	113·7
1857	125	85·2	100·25	117·6
1858	68	83·5	81·25	97·3
1859	70	81·8	65·75	80·4
1860	62	82·6	64·00	77·5
1861	63	77·3	64·75	83·8
1862	61	72·4	68·25	94·3
1863	73	71·8	75·00	104·5
1864	76	70·4	81·00	115·1
1865	90	70·2	81·75	116·4
1866	85	71·2	78·25	109·9
1867	76	69·7	69·75	100·1
1868	62	65·9	63·50	96·4
1869	56	60·7	62·75	103·4
1870	60	53·1	58·75	110·6
1871	73	46·1	53·50	116·0
1872	46	39·3	44·50	113·2
1873	35	34·2	29·75	87·0
1874	24	29·4	22·00	74·8
1875	14	23·8	15·25	64·1
1876	15	17·1	12·00	70·2
1877	8	—	—	—
1878	11	—	—	—
1879	8	—	—	—
1880	4	—	—	—
1881	6	—	—	—

TABLE XVII. *Silk Imports into Great Britain from China, in Hundreds of Bales, treated as in Table I. (Durant's Circular.) See Fig.* 16.

Year	Imports	Ten-yearly average	Four-yearly average	Per cent. 4 years of 10 years
1830	48	—	—	—
1831	51	—	—	—
1832	83	—	—	—
1833	59	—	—	—
1834	95	83·1	90·75	109·2
1835	79	81·4	122·00	149·9
1836	130	80·0	114·50	143·1
1837	184	73·7	104·00	141·1
1838	65	70·5	79·25	112·6
1839	37	64·8	42·50	65·6
1840	31	68·3	31·25	45·8
1841	37	75·6	28·75	38·0
1842	20	79·6	30·50	38·3
1843	27	96·8	49·75	51·4
1844	38	114·5	95·50	83·4
1845	114	122·4	144·25	118·2
1846	203	141·8	194·50	137·1
1847	224	168·5	219·50	130·3
1848	237	201·4	196·25	97·4
1849	214	254·8	198·00	77·7
1850	110	295·6	210·50	71·2
1851	231	333·9	246·00	73·7
1852	287	406·3	361·50	89·0
1853	356	430·9	434·25	100·8
1854	572	487·1	509·00	104·5
1855	522	543·1	657·00	121·0
1856	586	581·4	634·75	109·2
1857	948	620·0	698·25	112·6
1858	483	628·9	719·25	114·3
1859	776	597·0	635·75	106·5
1860	670	583·7	683·25	117·0
1861	614	551·3	600·50	114·1
1862	673	489·8	496·25	101·3
1863	445	487·1	440·00	90·3
1864	253	440·5	337·25	76·6
1865	389	417·4	309·25	74·1
1866	262	406·9	360·00	83·5
1867	333	388·4	340·25	87·6
1868	456	384·7	384·50	100·0
1869	310	401·1	428·50	106·8
1870	439	393·7	436·50	110·8
1871	509	408·9	461·00	112·7
1872	488	403·3	455·50	112·9
1873	408	386·5	407·00	105·3
1874	417	383·9	388·50	101·2
1875	315	367·6	355·75	96·8
1876	414	336·3	323·50	96·2
1877	277	—	—	—
1878	288	—	—	—
1879	284	—	—	—
1880	276	—	—	—
1881	196	—	—	—

TABLE XVIII. *Silk Imports into Great Britain from Japan, in Bales. (Durant's Circular.)* *See Fig.* 17.

Year	Imports	Year	Imports
1859	555	1870	7,534
		1871	10,807
		1872	8,567
1860	7,771	1873	7,192
1861	8,969	1874	6,773
1862	10,796	1875	5,626
1863	24,694	1876	9.522
1864	11,711	1877	9,297
1865	11,688	1878	6,618
1866	7,433	1879	4,789
1867	7,000		
1868	8,677	1880	4,651
1869	8,750	1881	4,653

TABLE XIX. *Rate of Discount at the Bank of England, with the Four-Yearly Average.* *See Fig.* 18.

Year	Rate	Four-yearly average	Year	Rate	Four-yearly average
1840	5	—	1862	$2\frac{1}{2}$	4·94
1841	5	4·56	1863	$4\frac{1}{2}$	4·81
1842	$4\frac{1}{4}$	4·19	1864	$7\frac{1}{2}$	5·94
1843	4	3·63	1865	$4\frac{3}{4}$	5·44
1844	$3\frac{1}{2}$	3·38	1866	7	4·13
1845	$2\frac{3}{4}$	3·69	1867	$2\frac{1}{2}$	3·75
1846	$3\frac{1}{4}$	3·75	1868	$2\frac{1}{4}$	2·78
1847	$5\frac{1}{4}$	3·81	1869	$3\frac{1}{4}$	2·88
1848	$3\frac{3}{4}$	3·63			
1849	3	3·06			
			1870	$3\frac{1}{8}$	3·34
1850	$2\frac{1}{2}$	2·65	1871	$2\frac{7}{8}$	3·72
1851	3	2·77	1872	$4\frac{1}{8}$	3·88
1852	$2_1{}^1{}_2$	3·40	1873	$4\frac{3}{4}$	3·97
1853	$3\frac{1}{2}$	3·80	1874	$3\frac{3}{4}$	3·59
1854	5	4·75	1875	$3\frac{1}{4}$	3·13
1855	$4\frac{3}{4}$	5·56	1876	$2\frac{5}{8}$	3·13
1856	$5\frac{3}{4}$	5·13	1877	$2\frac{7}{8}$	2·91
1857	$6\frac{3}{4}$	4·63	1878	$3\frac{3}{4}$	2·94
1858	$3\frac{1}{4}$	4·25	1879	$2\frac{3}{8}$	—
1859	$2\frac{3}{4}$	3·88			
1860	$4\frac{1}{4}$	3·69	1880	2^{3}_{1}	—
1861	$5\frac{1}{4}$	4·13	1881	—	

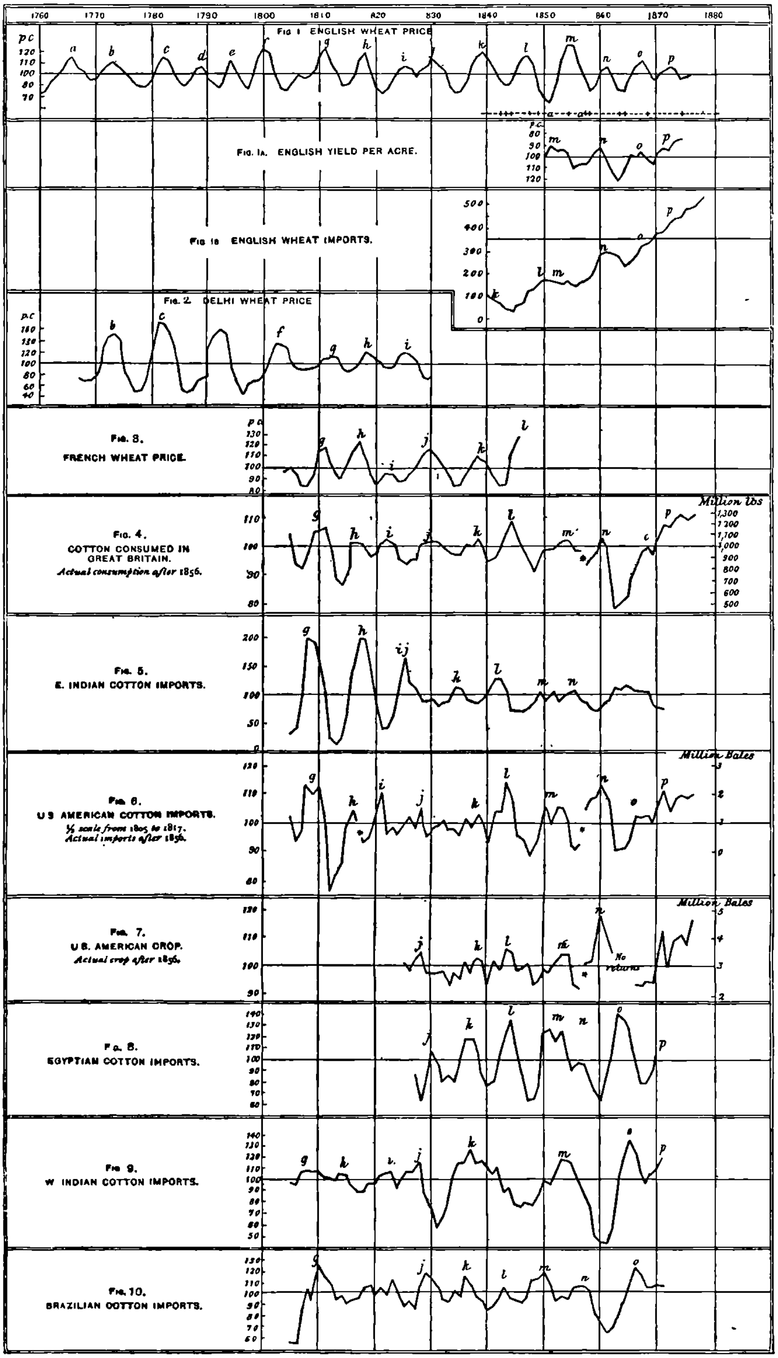


FIG. 1 ENGLISH WHEAT PRICE
FIG. 1A. ENGLISH YIELD PER ACRE.
FIG. 1B ENGLISH WHEAT IMPORTS.
FIG. 2. DELHI WHEAT PRICE
FIG. 3. FRENCH WHEAT PRICE.
FIG. 4. COTTON CONSUMED IN GREAT BRITAIN. Actual consumption after 1856.
FIG. 5. E. INDIAN COTTON IMPORTS.
FIG. 6. U S AMERICAN COTTON IMPORTS. ½ scale from 1805 to 1817. Actual imports after 1856.
FIG. 7. U.S. AMERICAN CROP. Actual crop after 1856.
FIG. 8. EGYPTIAN COTTON IMPORTS.
FIG. 9. W INDIAN COTTON IMPORTS.
FIG. 10. BRAZILIAN COTTON IMPORTS.
Million lbs
Million Bales
No returns

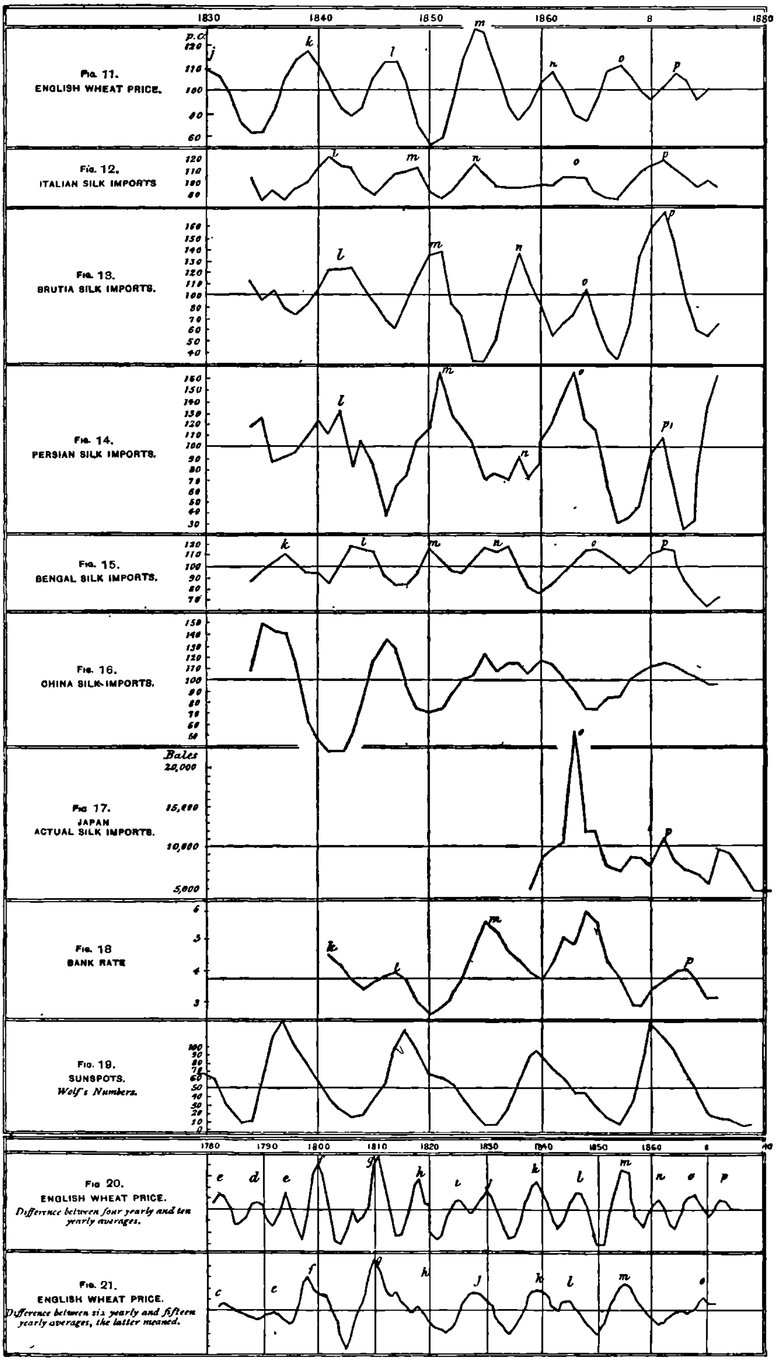


FIG. 11. ENGLISH WHEAT PRICE.
FIG. 12. ITALIAN SILK IMPORTS
FIG. 13. BRUTIA SILK IMPORTS.
FIG. 14. PERSIAN SILK IMPORTS.
FIG. 15. BENGAL SILK IMPORTS.
FIG. 16. CHINA SILK IMPORTS.
FIG. 17. JAPAN ACTUAL SILK IMPORTS.
FIG. 18 BANK RATE
FIG. 19. SUNSPOTS. Wolf's Numbers.
FIG. 20. ENGLISH WHEAT PRICE. Difference between four yearly and ten yearly averages.
FIG. 21. ENGLISH WHEAT PRICE. Difference between six yearly and fifteen yearly averages, the latter meaned.
1830
1840
1850
1860
1880
1780
1790
1800
1810
1820
Bales

PART VII.

ADDRESSES AND GENERAL ARTICLES.

46.

CHANGE OF STATE: FUSION AND SOLIDIFICATION.

[*Birmingham Phil. Soc. Proc.* **2** (1881), pp. 354–372.]

[*Read* May 12, 1881.]

Although the title of my paper is Fusion and Solidification, I must ask you to allow me to devote a considerable part of it to a description of the more familiar change from the liquid to the gaseous state which is called evaporation or boiling, according to the mode in which it takes place. I shall do this because, as I may as well confess, we know really little or nothing of the real nature of the change from the solid to the liquid state—we can only guess—and we may perhaps be helped in our guesses by the analogies which will be seen to exist between it and the change from liquid to gas.

As we shall have to make great use of the kinetic theory of matter, I will here just recall its main features. According to this theory we are to suppose that the particles of all bodies are in motion, but that in a solid the motion is chiefly vibratory, the particles only moving to and fro about their mean position, their velocity not being sufficiently great to enable them to move far away from that position. In a liquid the velocity is greater, so great that the particles no longer vibrate but move sufficiently fast to rush away from each other. But they are still continually getting entangled with each other, as they are comparatively closely packed. If the velocity be still greater the particles are able to move much further away from each other, and still retain a great velocity. They take up much more space, and get entangled with each other very much less frequently. This is the gaseous state.

Let us now study the phenomenon of evaporation in the light of this theory by the aid of the apparatus shown in Fig. 1. A is a bulb free from air, and containing water. It is connected by a tube in which is a tap, with the U-tube BC of which the end C is sealed. Suppose that there is some mercury in the bend of this U-tube, and that to begin with the communication between A and B is cut off, and the spaces above the mercury in B and C

entirely exhausted of air or vapour. Then the mercury will be at the same level in both limbs of the tube. If now the tap be turned on, the water vapour will pass out of A into the space B, and we shall find that for each temperature it will exert a definite pressure on the mercury, depressing the level in B below that in C, the difference in height between the two of course measuring the pressure. Thus at 0° C., C will ultimately be 4·6 millimetres above B; at 50°, 92 mm.; at 100°, 760 mm.; at 200°, 11,689 mm. As the temperature rises it will always take some little time for the pressure to reach its maximum or *the vapour-tension* for that temperature, for the water in A will take time to evaporate, but when it has reached that maximum pressure it will remain at it steadily.

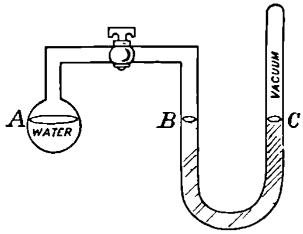


Fig. 1.

The explanation of this on the kinetic theory is, I suppose, as follows: Though in water the average velocity of the particles is not sufficient to make them all fly apart, some particles will have a velocity far above the average, and, indeed, have the average velocity of gas-particles. If such particles happen to be near the surface and moving in a direction away from it, they will be able to escape altogether; that is, they will evaporate, and soon the space above the water will be full of gaseous particles. (Incidentally, we may notice here that since the particles escaping have a great velocity, the general average velocity of those left behind will be lowered that is, the liquid remaining will be cooler.) Now these gaseous particles will be rushing about in the space above in all directions, and many of them will dart back into the water and become entangled there. The fuller the space becomes the more rush back, and at last we shall have an equilibrium between the number of molecules escaping and those rushing back. When this point is reached, we have the maximum pressure or the vapour-tension, 4·6 mm. at 0°, 760 mm. at 100° and so on. So that though we have apparently reached a steady state at the maximum pressure, the evaporation is still going on as actively as ever, but is balanced by an equal condensation.

If now the temperature rise the average velocity of the particles of the liquid will rise, and therefore the number with a velocity sufficient to escape will rise, that is the rate of evaporation increases with the temperature. We must, therefore, have the space above the water fuller of gaseous particles, and the pressure consequently greater, in order that the evaporation may be balanced by the condensation.

I have so far supposed the space above the liquid entirely free from air. But if the space A and B, Fig. 1, contained air, and C were open to the atmosphere, we should have very similar phenomena, and the increase of pressure caused by the evaporation would be almost exactly the same. And in a general way it is easy to see the reason of this. The presence of the air will interfere with the rate of escape of the water particles, for many will be beaten back on coming into collision with the air particles bombarding the surface. But at the same time the air will equally interfere with the rushing back of the gas-particles into the liquid, and we may expect the balance between evaporation and condensation to be reached when the same number of gas-particles are in the space above the liquid. The chief effect then of the presence of air will be that the evaporation and condensation will take place more slowly, and that the state of equilibrium takes a longer time to establish itself.

I have further supposed that the liquid was free from air or air bubbles. Now in practice it is exceedingly difficult, if not impossible, to get air-free water. Air will cling in minute bubbles to the sides of the containing vessel, and these play an exceedingly important part in the phenomenon of boiling. As the temperature rises the water evaporates into these bubbles, and some of them get so large that their buoyancy enables them to detach themselves and they rise to the surface. This causes the well-known singing of the kettle. But still many minute air bubbles are left too small to detach themselves so easily.

Now, suppose that we are heating water containing such bubbles in a vessel exposed to the air. When the temperature rises to about 100° C., the vapour-tension or maximum pressure of the water vapour is about 760 mm., or about the pressure of the atmosphere on the surface. Then inside an air bubble the pressure will be greater than 760 mm., for it will be that of the vapour + the small amount of air in the bubble. The bubble will, therefore, go on expanding till some of it rushes off to the top. The minute bubble left behind still contains a little air, and into this evaporation again takes place, the bubble again expands till some of it breaks off and rises to the surface. This constitutes boiling—which as you know is characterised by the rising of bubbles from the sides of the vessel, and which only takes place at the temperature at which the vapour-tension is about equal to the atmospheric pressure.

Now, after a time the air bubbles become so minute that they are able to withstand the pressure of 760 mm. + the air they contain by means of their surface-tension or tendency to contract, which increases as they grow smaller. Hence in order that they may swell sufficiently to rise to the surface the temperature must rise till the vapour-tension + pressure of the air contained is able to overcome the atmospheric pressure + the tendency of the bubble to contract. This explains the fact that long boiled water from which the larger air bubbles are removed can sometimes be heated considerably above 100° without boiling. When it does boil it does so almost explosively, for the bubble grows so rapidly when once able to overcome the atmospheric pressure.

I have thus described at some length phenomena which are probably familiar to you, because I wish to point out that these processes of boiling and evaporation by which a liquid changes to a gas are apparently *surface-phenomena,* that is, that the liquid only changes into its vapour at the top surface, where exposed to the external air, or at the surface of minute bubbles in the liquid. And further they are *exchange-phenomena,* that is, we must take into account the exchange of particles going on between the liquid and the gas, the evaporation and condensation, and we must suppose that these two balance each other when we have an apparently steady state.

There is, however, another mode of change from liquid to gas in which we do not recognise these characteristics. If, instead of heating the water in an open vessel it be heated in a closed tube free from air, and only in contact with its own vapour, so that it cannot boil, it is found that at a certain temperature, and at the pressure of the vapour at that temperature, the separating surface between liquid and vapour disappears. Above this temperature we shall never be able to have the substance in the two states at the same temperature and pressure; it is always homogeneous, either all liquid or all gas, or something intermediate in which the distinction between liquid and gas is lost. The temperature at which this occurs is called the critical point. For water it is 411·5° C. Here then change of state is no longer a surface-phenomenon, but to effect it we require an alteration of pressure, and when it takes place, if it can be said to take place at all, it takes place equally throughout the mass.

There is a very valuable method of exhibiting the relation between the volume and pressure of a substance by means of an 'indicator' diagram. Fig. 2 represents such a diagram for water-steam, and it may be worth while to describe it at length, as some important questions will thereby be suggested. The figure is not drawn at all in proportion, but only roughly represents the general nature of the diagram. Two lines, OP, OV, are taken at right angles, and the distance of any point from OP is taken to represent the volume of a substance, and its distance from OV, its pressure. Thus, suppose we take 1 gramme of water and convert it into steam at 0 , and at, say, 1 mm. pressure, this cold steam will occupy about 912,000 cubic centimetres. This will be represented by a point a, a distance proportional to 1 mm. above OV, and a distance proportional to 912,000 c.c. from OP. Suppose we have the steam in a cylinder with a tight-fitting piston, so that we can alter the pressure. As we increase the pressure from 1 mm. the volume diminishes in the same proportion, till at 4·6 mm. the volume is only about 200,000 c.c.—this is represented by the point b; then, on increasing the pressure ever so little further, the steam begins to condense into water, and will go on condensing without any further increase in pressure till it is all water, and occupies only about 1 c.c.; this is represented by the point c.

Further pressure only compresses the water exceedingly slightly, so that a line, *cd*, nearly parallel to *OP* will represent the state of the water if further compressed. So that the behaviour of steam and water at 0° is represented by the curve *abcd*. If now we take the same weight of steam at 50° and at 1 mm. pressure, the volume is about $\frac{1}{5}$ greater than at 0° for the same pressure, represented by *a′*. But as the steam is compressed it will not begin to condense till the pressure is about 90 mm., and the volume about 12,000 c.c.; represented by the point *b′*. It can now be all condensed without further increase of pressure till the point *c′* is reached, slightly greater than 1 c.c., and the further behaviour of the water is represented by a line *c′d′*, nearly parallel to *OP*.

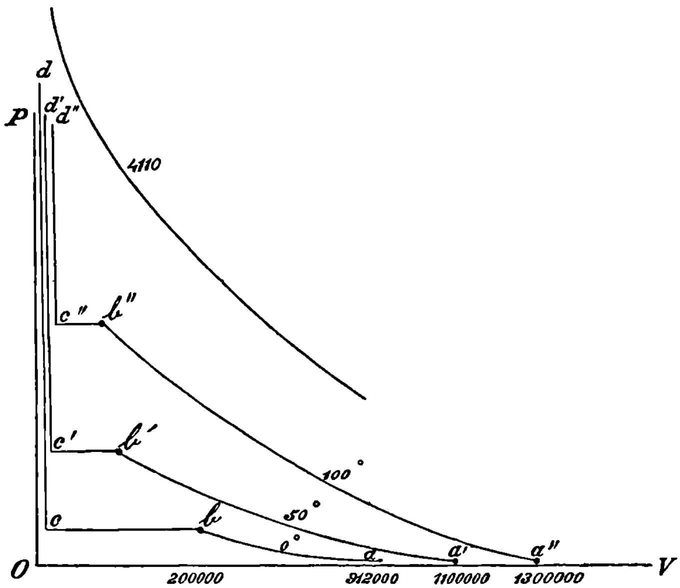


Fig. 2.

At 100° the volume of the steam at 1 mm. is about 1,300,000 c.c. (*a″*). It decreases as the pressure is increased to 760 mm. when the volume is about 1,100 c.c. (*b″*) and then condensation begins at that pressure, and goes on till all is condensed to water occupying only a little more space than the water at 0° or 50°.

So that on each line representing the behaviour of water-steam at a given temperature, or isothermal, we have a horizontal part representing the mixture of steam and water, at the pressure at which water will boil at that temperature. But it is seen that the length of this horizontal portion of the isothermal diminishes as the temperature rises, being about 180 times as long at 0° as at 100°. We should find that, on drawing the isothermals for the various temperatures up to the critical point, at that point the horizontal part entirely disappears, and that at higher temperatures the curves run on continuously, only being a little flatter at one point.

I shall here mention an important suggestion of Prof. James Thomson's as to the nature of these curves, as it has a bearing on what I shall have to say later in connection with the change from solid to liquid.

It will be noticed that though ordinarily the portions *dc* of the isothermals stop short in their downward course at *c*, and become horizontal, yet water is known at a pressure lower than that represented by *c*, under which it would ordinarily boil; as, for instance, when water evaporates under an air pump which never lets the pressure rise to its maximum, and in other cases; such a state can only be represented by a prolongation of the line *dc* downwards beyond *c* (as *ce*, Fig. 3). Again, it has lately, I believe, been found possible to increase the pressure of steam beyond *b*, where it would ordinarily condense, if there be no dust present. This state would be represented by the prolongation of the line *ab*, upwards at *bf*. Prof. J. Thomson has suggested that the true form of the curve is as drawn in Fig. 3, *abfghecd*. We can never

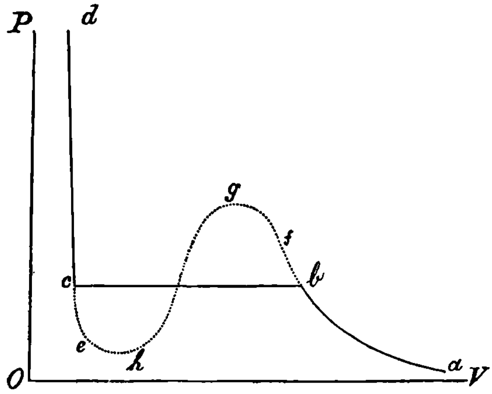


Fig. 3.

hope to experiment on the liquid in the state represented by the points between *g* and *h*, for in this state the shape of the curve would mean that the volume increased when pressure was applied—an unstable state.

To summarise the main facts which I have so far described, we see that when a liquid is heated it evaporates at the surface, whether that be the free surface or the surface of contained bubbles, and that the rate of evaporation increases with the temperature. In a closed vessel the evaporation apparently stops when the vapour has reached a certain pressure, but that, probably, the evaporation still goes on, though balanced by an equal condensation. Boiling is only a particular form of evaporation into air bubbles, when the vapour-tension is equal to the atmospheric pressure. Further, the liquid never spontaneously turns into gas in the middle of its mass. At a certain pressure the liquid can exist, partly as gas, partly as liquid. At a greater pressure it is usually all liquid, and at a less pressure usually all gas. But that in certain cases it can be all liquid at a less pressure, or all gas at a greater pressure than that in which the mixture of liquid and gas remains in equilibrium. At and above a certain temperature, however, the critical temperature, it passes quite continuously from the gaseous to the liquid state, never existing in the two states at the same time.

I shall now go on to discuss the change from the solid to the liquid and gaseous states, using the knowledge of these facts as a guide in forming any theories.

There are two quite distinct modes in which melting takes place, and as familiar examples of these two modes we may take ice and sealing-wax. We will take first the case of the melting of ice. As a piece of ice is gradually raised in temperature it is always solid till the melting-point is reached, when water begins to form *on its surface*, and it goes on melting—at the surface only—till it has all formed water, the ice remaining quite solid to the last, and the temperature remaining constant during the melting, the heat given to the substance going merely to effect the change of state, not to raise the temperature. This heat is perfectly definite in amount, and is called the latent heat—latent because it does not make its presence apparent by a rise in the temperature. There is no intermediate stage between the ice and the water, it turns directly from the quite solid to the quite liquid, at the same temperature, absorbing in the process a definite amount of heat.

Now consider solidification. Suppose we take some water and cool it down to 0°. If we continue to abstract heat from it when it reaches 0°—keeping it agitated meanwhile—it will begin to freeze, and until the whole of it is frozen the temperature will remain 0°, when it can once more be cooled.

Suppose, however, that the water be covered with a thin layer of oil, or be kept very still, it is very often possible to cool it far below 0° without solidification occurring. But the moment a particle of ice is introduced freezing sets in and the temperature begins to rise, and will go on rising as solidification continues till 0° is reached. It seems possible that in all cases the presence of some ice is necessary to begin freezing, and the agitation which in ordinary cases induces it, merely, I suppose, shakes up some of the hoar frost floating in the air into the liquid*.

The fact that melting of ice takes place only at the surface leads us at once to suspect that it is of the same nature as the change from the liquid to the gaseous state by evaporation, and that we must take into account the particles escaping from the ice just as we took into account the particles

* As will be seen later in the description of the isothermals of water-ice, if Prof. J. Thomson's suggestions as to their nature be adopted, an unstable state must ultimately be reached on reducing the pressure sufficiently. It is possible that by violent agitation this state may be reached in part of the liquid, and ice be therefore formed.

escaping from the water. We know that there is a continual escape of particles from the surface of ice; for ice has a definite vapour-tension just as water has. In fact at 0° the two have the same vapour-tensions, viz., 4·6 mm. Suppose then we made an arrangement like that in Fig. 4, *A* and *B* being two bulbs containing ice and water respectively, connected by a U-tube *C, D,* and by another pipe *E* at the top of the U containing a tap, and let the bend of the U-tube be filled with mercury.

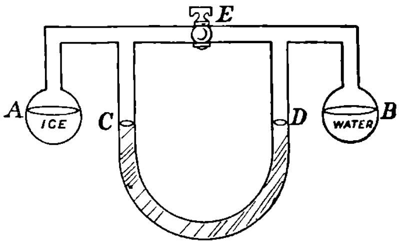


Fig. 4.

If the whole were kept at 0° the mercury would be at the same level in the two limbs of the U, for the pressure at both sides would be the same, viz., 4·6 mm. If now we opened the tap at *E* so that the two bulbs were in connection, the ice and water would remain the same in amount, for each would be in equilibrium with its vapour; the opening of the tap having made no difference in the vapour-pressure, which was already the same on each side of it.

Now, suppose we turned off the tap at *E,* and cooled the two flasks down to say − 1°, then the pressure of the water vapour would exceed that of the ice, and the mercury would rise in *C* by ·044 of a mm.; that is, the rate of escape for water would be greater than for ice, and consequently a greater pressure would be produced in *B* than in *A* before the escape or evaporation was balanced by the return or condensation. No direct experiments have been made to prove this, for the difference is only about $\frac{1}{600}$ of an inch, a quantity which it would be very difficult to measure. But it can be proved quite conclusively in some such way as this: Suppose we had a copper cylinder (Fig. 5) containing a piston *A* working perfectly smoothly, and so arranged that by means of a counterpoise *C* its pressure on the contents of the cylinder could be adjusted to anything we liked. First let some ice be in the cylinder, and let the whole arrangement be placed in a bath kept at − 1°.

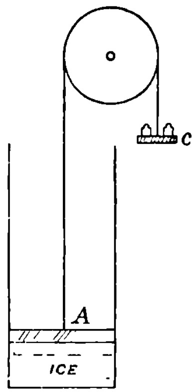


Fig. 5.

Now suppose for a moment that it is possible that ice and water at − 1 have the same vapour-tensions—that of ice as we know by Regnault's experiments being 4·26 mm.—adjust the weight of the piston so as to exert an infinitesimal amount less than this pressure upon the ice. Then the ice will begin to evaporate and push out the piston till the whole is evaporated, and we have the cylinder full of vapour at a pressure only exceedingly little below 4·26 mm. Now introduce a drop of water into the cylinder and make the pressure of the piston an infinitesimal amount greater than 4·26 mm. and the vapour will now begin to condense on to the drop of water and the piston will descend again, and this will go on till the whole is converted into water.

Now it is known that if the pressure be increased the melting-point of ice is lowered. Let us now increase the pressure till the melting-point is lowered to $-1°$. For this we should have to put on about 132 atmospheres or 1980 lbs., or $\frac{6}{7}$ of a ton to the inch on the piston. When we have thus increased the pressure, on introducing the minutest piece of ice the water will begin to freeze, and, as you know it expands in freezing about $\frac{1}{11}$ of its volume, we should then have this immense weight of $\frac{6}{7}$ ton to the inch raised up a little during the freezing, viz., through a height equal to $\frac{1}{11}$ of the depth of the water, and we should end with the substance in its original state of ice. Now in this process we notice that the work done *by* the ice in evaporating and pushing out the piston was counteracted by that done *on* the vapour in condensing it to water, so that we have a balance left over—the lifting of the immense pressure of $\frac{6}{7}$ ton to the inch. We might go on repeating this process, and should so get any amount of work out of the arrangement. This work must of course come from somewhere, i.e., heat must disappear somewhere, and the only place where it could disappear would be in the bath which must be cooled. But a most important law of Thermodynamics, the Second Law, lays it down that this is impossible, that we cannot get work out of an arrangement merely by cooling a substance in this case the bath—below the temperature of any of the surrounding bodies. It would be like attempting to work a waterwheel on the surface of a lake where the water was already in its lowest possible position. Just as we must have water falling from a higher to a lower position to make it do work, so we must have a colder body for heat to flow into so as to catch some of it to turn into work.

In the arrangement then which I have described we must somewhere have put in the work which we extract from it, and the only way to account for it is to suppose that the vapour-tension of ice at $-1°$ is less than that of water, so that we must push in the piston for some distance before the vapour will begin to condense as water.

In the same way we could show that if we could have ice above 0° its vapour-tension would be greater than that of water. In both cases it is very easy to calculate the amount of the excess.

Going back to the arrangement in Fig. 4 we now see that if the two bulbs be kept at $-1°$ the vapour-tension for the water is greater than that for the ice. If, then, the tap be turned so that there is free communication between the two vessels, the water vapour will go over into the ice vessel and there increase the pressure, so that the condensation will exceed the evaporation, and the ice will grow, the water diminishing. Similarly if the temperature be above 0°—if it is possible to have ice in such a state the ice vapour has the greater pressure, it will go over into the water vessel B, increasing the condensation there while the ice in A diminishes.

Now we have only to make the supposition, which I think is well warranted, that the rates of escape of the molecules from the ice and water bear to each other the same proportion when the two are actually in contact, and the separating space in the bulbs and the pipe E done away with, and we can explain melting and freezing very easily. When ice and water are together at 0°, and receiving no heat, the rate of escape from ice to water equals the rate of escape from water to ice, for their vapour-tensions are equal. Hence there is no change in their relative proportions. Below 0° the rate of escape from water into ice is greater than in the opposite direction, for water has the greater vapour-tension. Hence the ice will grow at the expense of the water. But the arrangement of the molecules as ice requires less energy, so that some energy is given up which goes to heat the mixture, and the temperature will rise to 0°. If now the temperature rise ever so little above 0 the balance is destroyed, and in the other direction, so that the ice sends more particles into the water than the water returns to the ice—that is, the ice melts. But this melting absorbs energy, so that the temperature always tends to fall towards 0°.

If this explanation is worth anything it ought to explain more than this; it ought to explain the fact which I mentioned just now, that the melting-point of ice is lowered by pressure. That is, we ought to be able to show that pressure affects the rate of escape of molecules but affects the rate differently for the two states of ice and water, so that the equilibrium is restored at a lower temperature than 0°. Now, Sir W. Thomson showed some years ago that the curvature of the surface affects the rate of escape of particles from a liquid. Suppose we have a capillary tube in water in a vessel free from any gas but water vapour, as in Fig. 6. Then the water rises in the tube above the surrounding level surface a, and its surface is concave upwards at b. But when the two surfaces are in equilibrium with the vapour, the pressure of the water vapour is greater at the lower level a, by the height of the vapour (ab) between the two, and at the same time the pressure in the water just underneath the surface is greater at the lower level by the height of water (ab) between the two. Or where the pressure of the water is greater, the pressure of the vapour in contact with it is greater—the increase of vapour-pressure having the same ratio to the increase of the

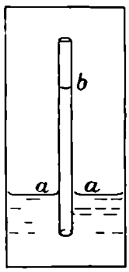


Fig. 6.

water pressure as the density of the vapour has to the density of the water*. Or the increase of vapour-tension is equal to

$$\frac{\text{Extra pressure} \times \text{density of vapour}}{\text{density of water}}.$$

If pressure has the same effect on the vapour-tension of ice then the vapour-tension should be increased by the fraction of the pressure to which the ice is subjected, represented by

$$\frac{\text{Extra pressure} \times \text{density of vapour}}{\text{density of ice}}.$$

Since the density of ice is less than that of water, the increase of the vapour-tension by pressure will be greater, or the effect of pressure in altering the rate of escape of molecules will be greater.

Let us now apply this to the case of a mixture of ice and water submitted to pressure in a closed vessel. Here we can no longer talk of vapour-tension, for no vapour will exist in the vessel owing to the pressure. But we have, according to the view which I have put forward, an escape of molecules from ice to water and from water to ice proportional to their vapour-tensions. Now below 0°—say at − 1°—the vapour-tension for ice is less than that for water, or the rate of escape of particles from water to ice is greater than from ice to water. The effect of pressure is to increase the rate of escape in both cases, but more in the case of ice than in the case of water; the two rates of escape are, therefore, brought nearer to each other, and by sufficiently increasing the pressure they can be made equal to each other. The interchange of particles is then equal, and we have equilibrium or the melting-point. The pressure required to effect this lowering of the melting-point by 1° can easily be calculated on this supposition, and the result is the same as that which Professor Thomson gave when he first predicted the effect of pressure in lowering the melting-point.

We can represent some of the phenomena of melting and freezing by indicator-diagrams, drawn on exactly the same principle as those made use of in the case of liquid and gas. Fig. 7 represents such a diagram for ice-water, though it is not drawn in proportion. Thus, taking 1 gramme of ice at 0°, and under no pressure, it will be represented by the point *a*, where *Oa* is proportional to the volume of 1 gramme of ice, i.e., 1·09 c.c. When the pressure is 1 atmosphere—represented by the height of *b* above the line *OV*, the ice begins to melt, and will continue to melt at the same pressure and temperature till it is all water, the volume being now represented by the distance of *c* from *OP*, $\frac{1}{11}$th less than the volume at *b*. (The figure immensely exaggerates this decrease of volume to prevent the crowding of the lines.)

* A proof of this can be given somewhat similar to that which is given above of the difference of vapour-tensions of ice and water. This proof applies equally, I believe, to the case of a solid under pressure, as, for instance, a block of ice compressed under a porous piston.

Intermediate states of the mixture of ice and water are represented by points along the horizontal line *bc*. Further compression of the water is represented by the almost vertical line *cd*.

If we take ice at $-1°$, its volume is less than at $0°$ as at a'. We must raise the pressure to 130 atmospheres, before melting begins at b'. When melted the ice will occupy a slightly less volume through the compression than before, so that the horizontal line $b'c'$ is slightly greater than *bc*. So

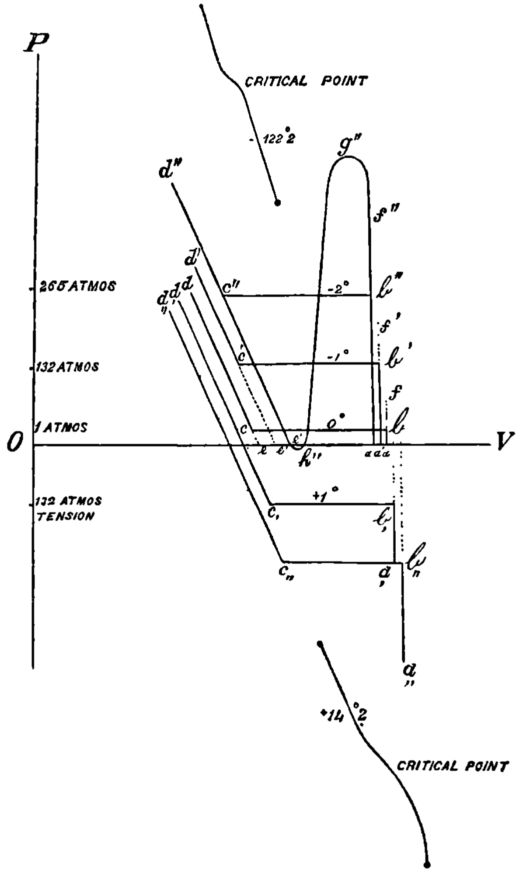


Fig. 7.

for the $-2°$ isothermal, $a''b''c''d''$, $b''c''$ is slightly greater than $b'c'$, and so on.

But we know that while ice below $0°$ contracts on cooling, water below $0°$ expands at a rapidly increasing rate, and there is good reason to suppose that ultimately the horizontal part of the isothermal would begin to diminish, and finally would disappear, if we could apply sufficient cold and pressure. When this disappearance first took place we should have a point analogous to the critical point of gases, where ice would change into water quite continuously.

I have supposed that the isothermals start at the line of no pressure, at a, a', a'', and that the pressure is always positive, for it is ordinarily assumed that we cannot have liquids at less than no pressure. This is, however, by no means the case. There are several cases in which liquids are at less than no pressure, that is to say, the external surfaces instead of pushing the molecules nearer together pull them further apart. For instance, in an experiment familiar to barometer makers, when a barometer is first made the mercury will often adhere to the tube and stand far above 30 inches, so that the statement that nature abhors a vacuum is, perhaps, not so wide of the mark. Here the mercury above 30 inches is in a state of tension, not compression, and such a state must be represented on an indicator-diagram by points below the line OV of no pressure.

So, too, if ice and water be in a vessel quite free from air, I think there is no doubt that they could be subjected to negative pressures or tension, to at least a slight extent.

I shall then assume that we might have such negative pressures, but shall not limit myself by supposing them small.

For instance, if we put on a pull of almost $\frac{6}{7}$ of a ton, instead of a pressure of the same amount, the melting-point of ice would be $+ 1°$ C. Under such a pull the water would be considerably expanded, that is, the horizontal portion $b_{,}c_{,}$ of the isothermal would be shorter than bc, and if the tension were increased the horizontal portion for the successive isothermals would apparently go on diminishing, till at last, at the rate of decrease near 0°, it would finally disappear at about 14°. Here then we should have another critical point, and ice ought to pass into water quite continuously.

It will be remembered that in the case of water and steam there are curious anomalies which are represented in Fig. 3 by the prolongation of the isothermal above and below the horizontal portions. We have exactly corresponding anomalies in the case of ice and water. Thus, water can be cooled far below 0° at the atmospheric pressure without freezing, and in such a state it must be represented by the prolongation downwards of the lines $d'c'$, $d''c''$, etc., as at ce, $c'e'$, in Fig. 7. Again, ice can be suddenly compressed at 0° by pressures far above 1 atmosphere without melting internally, and the internal ice is in a state represented by the prolongation upwards of the line ab, as at bf, $b'f'$, $b''f''$. These facts suggest that the true form of the isothermal is that suggested by Prof. Thomson for water and steam. This form is exhibited in Fig. 7, for the $- 2°$ isothermal by the curve $a''b''f''g''h''e''c''d''$. If this is the true form below 0° it should be so also above 0°, and this at once leads us to a point which has been much discussed lately. Prof. Carnelley has thought that by keeping ice in an air pump, where the vapour-pressure was never allowed to reach the maximum, and where, therefore, no condensation into water occurred, he was able to heat it far above 0 . There are great difficulties in the measurement of the temperature, so that it is somewhat doubtful whether the ice was really above 0°. But, at least, the theory I have here given says nothing against it. Indeed, there is clearly a place for it on our indicator-diagram. For if the lines $a_{,}b_{,}$, $a_{,,}b_{,,}$ can be prolonged upwards to the upper side of OV, as we must suppose, if Prof. Thomson's suggestions be true, then these prolongations would represent 'hot ice.' My own opinion is, that if the right means can be discovered, there is no impossibility in obtaining ice at a higher temperature than 0°, for the melting of ice seems to be due merely to the condensation of the escaping molecules on its surface, as water. If this condensation could be prevented, and at the same time the ice could be heated by conduction, I see no reason why its temperature should not be raised. But if the critical point at 14 have a real existence this gives us a limit to the temperature of hot ice. Above that we could not by any possibility obtain hot ice without an enormous pull on it—a pull of about 12 tons to the square inch. And even before that temperature was reached the prolongation of the isothermals upwards would fail to reach the line of pressure, and hot ice would be impossible in a vacuum.

In conclusion, I may add a few words about the other form of melting —that which takes place for instance in the case of sealing-wax. Here, as the temperature rises, we have a gradual softening *throughout the mass*, not merely at the surface. There is no definite arrest of temperature till the whole is melted, and no definite latent heat. This at once suggests that we really have a form of melting analogous to what might take place in ice at its critical points, if they exist. Just as we have in the melting of ice at ordinary temperature and pressure a change of state analogous, as I believe, to the change from liquid to gas, which takes place in boiling, so we have in the melting of sealing-wax a change analogous to that which takes place in water at temperatures above 411°, that is above its critical point.

47.

OVERTAKING THE RAYS OF LIGHT.

[*Mason College Magazine*, **1**, 1883, pp. 107–111.]

Some thirty or forty years ago a little pamphlet was published anonymously, entitled 'The Stars and the Earth; or Thoughts upon Space, Time, and Eternity,' in which the author sought to prove certain metaphysical speculations on the nature of time and space by consideration of the consequences of the finite time taken by light to travel through space. He failed, as all must fail who attempt to prove metaphysical propositions on purely physical grounds; but the physical parts of his pamphlet have an independent interest, as introducing to us the idea of magnifying or reducing the scale of time in an easily conceivable way. I propose to give some account of his 'microscope for time,' and to extend his method to show how we can conceive of the analogue of an inverted image—that is, of events moving backwards.

Since light takes time to travel through space, having a velocity of about 186,000 miles per second, it follows that the pictures of objects on our retina are pictures of the objects as they were at a greater or less interval before. With terrestrial distances this interval is so short as to be absolutely unimportant to us, except in such experiments as those of Fizeau and Foucault, where the interval taken by light to travel over a short distance was actually detected, and used to measure its speed.

But when we come to planetary and stellar distances the interval is very appreciable. Thus we see the moon as it was a second and a quarter before. Jupiter's satellites are eclipsed nearly an hour before we notice their disappearance, and the light from Neptune is four hours late. Beyond Neptune is the enormous space between our system and the fixed stars, tenanted, as far as we know, only by comets, which may either belong to our sun, or be wandering on till some new system presents sufficient attraction to warrant a visit. The light from the nearest fixed star, α Centauri, takes three and a half years to cross this space. Vega and Sirius we see as they were twenty years ago, the Pole star as it was half a century ago, and so on, until the distance of some of the faintest telescopic stars may be so great that we are only now receiving the light which left them thousands of years ago.

Now, while we are receiving light from the stars our system is also sending out light in its turn, which of course takes equally long times to travel over these distances. Let us imagine an observer with a telescope of magnifying power, which he can adjust at will to any extent, and endowed with the power of stationing himself where he will in space; further suppose him always to make his telescope magnify so much that when he looks towards the Earth he can see what goes on on the surface as easily as he would see what goes on in Edmund Street if he stationed himself near the Mermaid*. If he were now at Neptune he would see the Earth as it was four hours ago. At greater distances among the comets he would see the Earth as it was at longer times past, until at a Centauri he would only yet be at the end of 1879. At Vega or Sirius he might still be witnessing episodes of the American war. At the Pole star he would be back in the times of the first Reform Bill, and at Capella he might follow up the events which led to the Battle of Waterloo, which will only happen next year but one. He has only to go to still remoter stars to see events far back in history, while on the borders of our stellar system he might still see the Earth as Professor Lapworth is reconstructing it for us, from a very different record.

There is, then, travelling out into space in ever-widening spheres a continuous panorama of history, and we have only to imagine ourselves endowed with the power of sufficiently rapid movement and with sufficiently improved eyesight to conceive of the possibility of taking up this panorama where we like, and living over the past again as passive spectators from any point we choose. A well-known astronomer once declared it to be his ideal of happiness after death to wander from star to star and learn what was beyond the power of his instrument to reveal to him upon Earth. His pleasure might, I imagine, be somewhat alloyed if he came across the record of his own life, working its way into some obscure corner of space as rays of light.

What we have been considering is only a special case of the general proposition that every part of the universe bears upon it the impress of all that has happened in every other part in all past ages. Were we only sufficiently good mathematicians, and had we only a perfect knowledge of one portion of space, at any moment we might perhaps reconstruct the past as accurately as our imaginary observer among the stars. But such a reconstruction would be after all but a description of the past, such as the historian and the geologist are striving to complete, whereas the record out in space is at once ready for interpretation by our eyes, and would appear to us the present as much as what is going on around us now.

So far we have supposed the observer to station himself at each star. But now let us suppose him to be travelling to or from the Earth at speeds comparable with that of light. Then we shall see that time would appear

* [An effigy on the summit of Mason College. ED.]

to be altered in scale, and even to be reversed, just as optical images of objects may be either larger or smaller than the objects themselves, and may be either the right way up or reversed.

If, for instance, he had started from Sirius twenty years ago and had moved towards the Earth with the velocity of light, he would just be arriving here. As he left Sirius he would just have received the light which left the Earth twenty years before, or forty years ago, and as he approaches the Earth he witnesses the events of 1883. So that into twenty years he has crowded up the events of forty years, and everything will appear to go at twice the speed. Let him move still more rapidly and the hurry would be still greater. Were he only to occupy a few days on the journey, life on Earth would appear to gallop. Children would grow almost visibly, and as soon as they had stopped growing they would rush on to middle age; trains would move at lightning speed—indeed, so fast that he would not see them on their passage, but only notice their disappearance from one station and appearance at the next. But there is another consequence of this motion towards the Earth to be noted. Just as a railway whistle appears to rise in pitch if we are travelling towards it in a train, so would all colours appear to rise in pitch, as it were, to our observer. Our eyes at present are only constituted to make use of vibrations of a certain range of frequency. If by his motion the observer were to quicken up his speed till he received twice as many vibrations in the same time, the present colours would all move quite out of the range of his vision beyond the violet, and would be replaced by those long waves beyond the red which we can now only perceive by their heating-effects. The Earth itself sends out such rays, and it would most likely appear to be a red-hot globe, while boiling water would be of dazzling brightness.

But, perhaps the observer would find life going too fast for him. He would only have to start on his return journey to have a little breathing time, and make life go as slowly as he pleased.

Suppose him to travel from here to Sirius in, say, forty years, facing the Earth all the time. He will arrive in 1923, and be then just overtaken by the faster moving light, leaving twenty years after him in 1903. So that the record of twenty years' events on Earth will be lengthened out into forty. Our life would appear to go at a snail's pace. Our run would be as slow as a walk, our walk a crawl, and our trains would be no better than trams. Children of 20 or 30 would still be at school. At 40 the young people would be entering college or business. At 80 or 100 they would be middle-aged, while rivals to Old Parr would be seen on every hand. Here, then, we have a time-microscope: we need only increase the pace outwards to increase the magnifying power, till each second is lengthened into a day or year. The more nearly the speed of light is approached the greater is the magnification, until equality is reached, when the observer will see nothing, as no fresh waves will enter his eyes. But now suppose him to face about and get up still greater speed. He will now overtake the waves of light, and he will see once more. But the waves will enter his eyes reversed in order. He will overtake first those which left the Earth last. Thus, if he moves with twice the velocity of light towards Sirius, he will arrive in ten years, and there overtake the waves which left the Earth ten years ago. So that on his journey he will see the events of the last ten years, but all in reverse order. The Earth will appear to go the wrong way round the sun, and to rotate the wrong way on its axis. Rivers will steadily flow up towards their sources a far greater achievement than meandering 'level with their fount'; showers of rain will start from the Earth and rise towards the clouds. If he watches the inhabitants and continues his observation long enough he will see old men dying into life, walking backwards all their days, growing younger and younger, until at last they are born out of the world as flourishing babies, at the mature age of three score years and ten.

Let us imagine the observer able to interpret our speech, say, from the motion of the lips. He would soon get accustomed to the inverted order of the sentences, especially if he had studied German in his terrestrial career. He would find mankind talking in general, and in the main correctly, as to what was going to happen, while their references to the past would be hazy, incomplete, and usually quite wrong. He would learn that their memory was only for future events, that they remembered something till it occurred, and that then it went clean out of their minds. It would be interesting to know his opinions on the freewill controversy in studying a race of beings where actions preceded the determination of the will. Would such a race appear to him more or less wise than a forward living race? We call a man wise who can foresee what is going to happen, and act with the future in view. Here he would see beings who would appear to be guided almost entirely by the future, and who would be able to foresee and foretell infinitely better than they could recall the past.

Take an example. In our present mode of life, a man is hanged because he has put a fellow-creature out of the world. The same series of events would appear to our observer, thus: a man would be pulled up into life by a rope, imprisoned for a few weeks till he was more like his fellow-creatures, tried, and set at liberty. He would then proceed to restore a fellow-creature to life by drawing knives or bullets out of him, and thereafter, all would go on happily. From which point of view is the transaction the best that where two men go out of the world, or that where two men enter it?

Were our observer of a scientific turn of mind he might be interested in observing that the mechanics of such a system would coincide with ours while he travelled uniformly. The principle of the conservation of energy would equally apply, and I think that the laws of thermodynamics would be true. But there would be one great change. Instead of, as now, a continual tendency on the part of energy to a uniform and useless level—the so-called dissipation of energy—there would be a continual tendency for it to gather at various levels, and heat would continually be transforming itself into other forms of energy. Stones would suddenly find the ground beneath them grow too hot to hold them, and they would be thrust up into the air to fall again into schoolboys' hands. The rivers would run up-hill, not through defiance of gravitation, but because their banks and the stones in their course would yield them up the energy required.

Here, then, we have what we may fairly compare to the inverted picture in a camera. It would require a Jules Verne to describe life as it would be in such a system. Ordinary mortals could only bring themselves into a fitting state of mind to realise it by some such process as standing on their heads and contemplating the reflection of the rest of the world in a looking-glass.

48.

UNIVERSITY TRAINING IN OUR PROVINCIAL COLLEGES.

[An Address delivered at the Mason Science College, Birmingham, Oct. 2, 1883.]

Those of us who were present at the Annual Meeting last year had the pleasure of listening to an Address by Dr. Tilden upon Technical Education, in which he dwelt especially upon the part which colleges such as ours should take in providing this education. But he also pointed out that they should exercise another function. While they should afford technical education of the highest sort 'the *most important* part of their business is to provide the equivalent of University teaching at the doors of the people.' Dr. Tilden expressed the hope that some successor of his might discuss this part of our work. I have ventured to take up his suggestion, and I propose to-day to address to you some remarks on the possibility of colleges like ours offering to our students some of the advantages of University life and culture—advantages which have till lately only been accessible in England to students who could enter the older Universities of Oxford and Cambridge.

We, as Englishmen, may take these older Universities as types affording the best examples for us of what true University Training should be. For they have grown up with the nation and their systems have in general expressed the highest wishes of the nation in matters educational. When the Universities have seemed to neglect their proper work it has been that the people have lost their interest in higher learning and research. When there has been a widespread enthusiasm for culture the Universities have always been to the fore.

And now that no class of the people is excluded from a participation in their benefits by religious tests they are more than ever national, and are more than ever striving to supply the wants of the nation.

These tests have been so recently removed that their evil effects still remain, in some cases in a critical depreciation of the Universities, in others in a want of interest, and so in ignorance of the life animating them. It may be well, therefore, though it has no immediate connection with my subject, to point out a few signs of the vigour of that life, and to show how eager are the Universities to adjust themselves to the needs of the nation.

We shall so justify the position that they are national and so for us typical Universities, and we shall perhaps at the same time be able to realise a little more clearly what is that University training which such colleges as ours should seek to provide.

I cannot pretend to speak of the literary activity of our Universities in the presence of my colleagues on the Arts side, but it really requires no proof that Oxford and Cambridge take their full share now as in the past in making the literary history of the time. I shall therefore content myself with drawing your attention to their work in other directions.

In science one of the chief characteristics of a University, and the surest sign of its vigour, is the existence of schools of thought and research. Let us see how my own University of Cambridge fosters such schools. It is hardly necessary to mention her mathematical school. That has been famous since the time of Newton, and has long excelled all others. I need only mention the names of Cayley and Sylvester, who are still working, to show that the school retains its ancient vigour. In physics, too, Cambridge is well in the front rank. It would be easy to give names of eminent workers in every branch of physics; but I take, as especially characteristic, the work which is being done in electricity under the influence of Maxwell's theory. Then there is the Physical Laboratory, founded not many years ago by the Duke of Devonshire, and now—under Lord Rayleigh—one of the first, if not the first, in Europe.

The progress of the Physiological School is still more striking. Founded some twelve years ago by Trinity College, it soon became, under the care of Dr. Michael Foster, an example of a school of teaching and research of the highest kind.

And even in technical education, though far removed from the manufacturing centres, Cambridge is doing her best by the establishment of an Engineering School provided with well-appointed workshops.

In another great movement of the time—that for the higher education of women—the Universities are taking their share. Students of Girton and Newnham are now admitted to the tripos examination at Cambridge, and there can be no doubt that they will soon be put on an equal footing with men as members of the University on whom degrees may be conferred.

But the Universities have not confined their energies within their own bounds. We have recently seen the success of the University Extension movement, which has awakened a desire for higher education in so many towns, and has resulted in one or two cases in the establishment of permanent colleges. Another work of incalculable importance has been quietly pursued for more than twenty years in the Local Examinations. These have done much to bring home to us the miserable state of many of our secondary schools, and have also done much indirectly for their improvement.

Our Universities, then, are well aware of the needs of the present, and are able and willing to adapt themselves to the changes which are taking place in educational wants. They are Universities in the highest sense, and we may look to them when we ask what constitutes a true University training, and what it is in their system which makes a residence of three years, or rather fractions of three years, have such an influence upon their students' future.

I have already said that the one chief characteristic of a University is the existence of schools of thought and research like the mathematical, physical, and physiological schools at Cambridge. In such schools the methods of teaching and learning are very different from those which suffice merely to impart knowledge or prepare for examinations. The teachers are no mere connecting links between investigators and learners, passing on the results of research in a diluted form. They are either working themselves to extend the bounds of knowledge, or are so well acquainted with the methods of research that they are able to give sympathy and encouragement to those of their students who are strong enough to question nature for themselves.

Perhaps the most characteristic feature of these schools is the entire absence of authority. The leaders of the school are looked up to as guides, not as infallible directors. Their pupils are taught from the very first to examine the evidence for all they learn, to accept what they hear from their teachers only if it commends itself to their own reasoning faculties; 'to prove all things and to hold fast that which is good.' On the student who is himself capable of original work the effect of such a training is invaluable. He learns how to work; he is inspired by the example of his teacher, and encouraged by the sympathy and advice of his fellow-students.

But the power of research is not given to many. By far the larger number of students cannot hope to advance beyond the territory already conquered by others. They must be content to follow, not to lead. Too much of our teaching and examining for this class of students is like the charging of those secondary batteries of which we have heard so much lately, which only give back what was put into them with some inevitable loss by leakage. So we charge our students with instruction and estimate their value by the percentage they can restore in the examination-room. But a true University training is something very different from this. In a school where research is carried on, even though it be by others, the students gain the habit of independent thought; they learn to arrange their knowledge in their own way, and arrive at results by their own methods. In short, and this is the true University training intellectually, they learn the best of all lessons, the way to teach themselves.

There is another aspect of University life of immense importance, but which I can only touch upon. I mean the social aspect. It would be difficult to exaggerate the gain which results to a man from associating at college with others who have been brought up with widely different experiences from his own. He may often have to give up his old opinions, but those which he keeps and those which he acquires he learns to hold on surer ground. But perhaps one of his best lessons is that he learns to judge of men by what they are. I suppose that nowhere is a man judged more by what he is in himself, and apart from the accidents of class and wealth, than at the Universities.

I go on now to consider whether the colleges which are springing up in our centres of population can hope to supply a University training in this sense.

And first, I shall take that department which the needs of their constituents will render prominent—the higher technical education. It may be unusual to put this on a level with other subjects as a means of the best mental training, but there is technical education and technical education. There is that which is utterly unworthy of the name of education, which turns out pupils whose minds are mere stereotyped rules and tables of the data required in the arts and manufactures at the time when the pupils receive their instruction. With this we have nothing to do. There is the higher technical education which, recognising that life is short, directs the attention of the student to those branches of science which are applied in the arts, which gives him a thorough insight into their principles, and where all that is studied is studied thoroughly.

There seems to be in some minds a fear that the utility of the subjects taught may render them unfit for the highest training. I confess that I do not share this fear in the slightest, and it may be pointed out that this utility has one immensely important advantage. The student sees all around him the practical applications of the principles which he learns at college. Thereby he often has his interest aroused and an enthusiasm awakened in his studies without which he could never profit by them to the fullest. It is hardly sufficiently recognised yet that this interest and enthusiasm which the student should bring to his studies may make almost any subject a means of strengthening the intellect and of giving mental culture of the best kind.

We are too apt to think that two or three subjects are especially capable of affording this culture, forgetting that the fault is not in the subject matter taught but rather in the methods of teaching. Many subjects are still put on a lower level only because we have not yet learnt how to make the best of them in our mode of teaching them and fail to arouse the interest of the pupils. And I think that this is shown by the change which has taken place in the subjects taught at the Universities in the course of time. Two or three centuries ago the logic of the schoolmen was still considered the best instrument of mental training—the teachers knew how to employ the instrument, and their pupils were in some inconceivable way interested in disputations on subjects which to us now seem utterly trivial or ridiculous. But when there was an interest in the subject, and when teachers knew how to make the best of it, it sufficed to train the intellect of a Bacon or a Newton.

Since the real training consists, then, so much in the method of teaching, I believe that we shall find, as we improve in our teaching, that technical education is capable of giving a true University training to students, and at the provincial colleges in our large towns we are in the best position to give this highest technical education. The students will there learn the principles of science not as mere logical exercises but illustrated by their applications to the arts. They will see experiments on a larger scale and more completely carried out than those of the lecture-room. They will thus acquire such a habit of thought that their future work will consist to them of applications of the scientific principles they have learnt; they will be ready to take in new principles and so their college career will be but the commencement of their student life.

It has always been recognised as one of the functions of a University to fit its students for professional careers, for medicine and law especially. Much of what I have said on technical education, and on the advantages we possess in large centres of population, applies to professional education also. Indeed it is not necessary to prove that flourishing medical schools can exist in the provinces; it is rather for the older Universities to show that their schools can rival ours. Here, at Mason College, our arrangement with Queen's College enables us to take a part in providing a thorough training for medical students. I hope that we may look forward to a time when there shall also be a school of law here as there is in London and at Manchester.

Perhaps the provincial colleges find their greatest difficulty in their literary and purely scientific departments. And the difficulty is in obtaining students who will go through a thorough training. The only career open to those who wish to devote their lives to literature or pure science seems to be the teaching profession, and the demand for thoroughly qualified teachers is small. There are few, therefore, who dare run the risk of spending several years in studies which may be after all of no immediate use to them. But in the not distant future all this will be changed. We are at last thoroughly roused to the fact that secondary education is in a bad state, and that we must reform our middle-class schools. Much, of course, has already been done in cases where the possession of endowments has enabled the State to interfere, and there are certainly many private schools excellently conducted. But after all only the fringe has been touched. Too often the pupils at middle-class schools are only taught thoroughly in one respect, viz., in the acquisition of habits of slovenly work and want of concentration.

But, now that we are all convinced of the evils arising from the neglect of secondary education, reform is not far off. With reform there will be a greatly increased demand for thoroughly trained teachers specially qualified in one or two subjects, and local colleges will be called upon to assist in supplying the demand. There will thus, I believe, be a career open to students who will devote themselves to studies which as yet are too often only taken up for a year or two at college and then have to be dropped for subjects not so much to the student's mind but which alone will bring a living.

In a commercial community there is a large class—merchants and so on—who are engaged in the distribution of the wealth produced by others, and who require neither technical nor professional training. It is generally recognised that for those who are afterwards to become members of this class a literary or scientific course of education is the best, if any. But there is a widespread belief that though a college life may make men students it does not make them men of business. There is a suspicion that at college they acquire a dislike to the drudgery of business, habits of unpunctuality, and a casual way of treating matters of importance. It is not necessary, then, to consider whether we can supply a University training for this class, but whether we may expect them to demand it, and whether such a training will be to their advantage.

In considering what gives rise to the adverse opinion I may give some warning of the dangers which beset all students alike when they find themselves released from the discipline of school and have to trust, to a great extent, to their own guidance at college.

I am afraid that we must admit that the year or two at college is for many by no means an unmixed gain. But surely we need not make the melancholy confession that higher culture unfits men for business—that a University training renders a man incapable of acquiring the power of steady work and the knowledge of men which are such important factors in business success. It is not the men with a true University training who fail in business; it is those who have been at college but have missed the training, who have frittered away their time in mere attendance—sometimes even non-attendance at lecture and laboratory, without any serious attempt to study, and who deliberately refuse the training offered to them. For their failure we cannot be held responsible.

But at the same time there are dangers in our system which may render a college course less profitable than it should be even to the studious. One of the greatest dangers arises from that evil of our time, the desire to study too many subjects at once, perhaps in order to pass some examination, or perhaps because not much time can be spent at college. Of course we each of us, as in duty bound, represent our own subjects as of the greatest importance, but it does not follow that a student is wise in attending the lectures of every professor in the college at the same time. This taking up too many subjects at once often only disheartens a student instead of educating him. He may begin the session well. He spends the day in hurrying from class to class and at night he carefully copies out his notes, the rest of his time being taken up with meals and sleep, both perhaps being cut short. But soon he finds that more than note-taking or note-copying is expected. He is told to read the text-books or work out exercises—an impossibility when his time is already fully occupied. He becomes disheartened, ceases to be interested in his work, and too often ceases to work at all. Now such an experience as this—and I fear it is not so uncommon as it should be will show that a student should be careful not to overload himself with classes. He should only undertake those for which he can work thoroughly and keep an interest. His mental training will be far better, and he may acquire habits of industry which will serve him well, whatever his after career may be.

Another danger of our college system lies in the long holidays. The man who finds his four months or so suddenly dwindle down to a fortnight and the bank-holidays is apt to feel the change, and may be tempted to make up for it by laziness throughout the year. But then no one is expected to pass the vacations in utter idleness. One misfortune of our lecture system, especially under present circumstances, is that it has a tendency to make the pupil rely too much on the lecturer, to see everything as he sees it, and with the multiplication of subjects studied this tendency is increased. The student has literally no time to think for himself while the lectures are going on. The vacations are the times for independent study, to gain the power of independent thought. They are times, too, for those who have specialised to keep up some knowledge of other subjects.

I am sure, then, that a college training may be made a fitting preparation for a business career if it is rightly used. And we may take in support of this view the opinion of our founder, one of the ablest men of business that Birmingham has ever produced. Before his death he deliberately changed the function of this college from a merely technical college to one whose aim it is to supply also a general literary or scientific culture, believing that such culture was of importance to, and was needed by, a business community.

It seems to me, then, that with an increasing number of students, who are able to devote their time to literary or scientific studies, we may hope for the development of schools of learning and research in our provincial colleges. These, united with faculties of law and medicine and with technical schools of the best sort, may give an intellectual training which will at least approach that to be gained at the Universities.

But whatever measure of success we may attain in the intellectual training given, no mere collection of students and professors can form a University. The work of the class and the laboratory must have its complement in a vigorous college life outside the lecture-room. The solitary student, no matter what distinctions and prizes he may win, misses almost the best part of a college career. He misses the talks over work with those who are studying the same subjects. He misses the widening of the intellectual sympathies which arises from the association with others who are engaged in different studies. There cannot be the highest University training without this association, and now that specialisation begins so early, it is more than ever important that students studying in different branches should come in contact with each other. Then there are the college societies, the college athletic clubs, and all those features of student life which make us look back to it as such a bright and happy time of our lives. This student life cannot be fully developed all at once; it is necessarily of slow growth; but I think that we at Mason College have already much of it, thanks to the loyalty to the college of our first students.

It is for you new students who are entering on your career here to-day to take your part in promoting the student life, remembering always that you are members of the general body, and that you have duties towards your fellow-students. So may you take your share in making Mason College a true University for Birmingham.

49.

THE GROWTH OF THE MODERN DOCTRINE OF ENERGY.

[Address to the Mason College Physical Society,
March 26, 1884.]

The modern doctrine of Energy may be said to include our knowledge of the modes of action of different portions of matter upon each other, the influence of distance upon that action, and the part played by the intervening medium.

In all histories of science it seems necessary to go back to the Greeks, and show how much of the foundation of the science we owe to them. Let me, then, follow the usual plan. It is especially easy in this case, for the chapter on mechanical science among the Greeks might be made almost as short as the celebrated chapter on the snakes in Iceland viz., there are no snakes in Iceland. With one or two exceptions, the Greeks were absolutely destitute of the physical sense—that sense which observes phenomena, seeks for similarities, and attempts, by experiment, to prove the existence of similarities. Their one great conception which has survived to the present day the atomic theory—was arrived at by a very doubtful process, and was hardly the result of physical observation.

We may really date the growth of mechanical science from the time when Galileo and his contemporaries finally threw off the yoke of Aristotle, and proved the falsity of his law that a body ten times as heavy as another body will fall to the earth ten times as rapidly. While such a belief existed no progress could be made, for its very existence showed that there was no appeal to experiment, and therefore no chance of discovery.

But Galileo had the true scientific spirit. He was not content with the authority of Aristotle, and made the experiment for himself. He found that, making allowance for the retarding effect of the air, all bodies fall from rest at equal rates. Pursuing his investigations, he rendered his greatest service to science in the discovery that bodies which fall freely downwards gain equal velocities in equal times—that is, that the velocity grows uniformly. It is curious to notice that Galileo was still afraid to trust wholly to experiment. To quote Whewell, he took it "for granted the rule must be the simplest possible. 'Bodies,' he says, 'will fall in the most simple way, because natural motions are always the most simple. When a stone falls, if we consider the matter attentively, we shall find that there is no addition, no increase of the velocity more simple than that which is always added in the same manner'—that is, when equal additions take place in equal times." This term 'natural motion' itself arose from a curious misconception that there was a difference in kind between the motion of freely falling bodies, which continually increases, and the so-called 'violent motion' of a stone projected horizontally and gradually coming to rest. Galileo, however, did not content himself with his speculation: he experimented, and found that his guess was confirmed.

Here, then, is our starting point, in the discovery that falling bodies acquire equal velocities in equal times. Our attention is directed to velocity as a physical quantity to be measured, and we have something definite to observe, and to help us to trace out the action of bodies upon each other.

Galileo did further service to science by enunciating what really contains the first law of motion—that a body, if freed from obstacles, would move on a horizontal plane uniformly for ever. Any retardation which is observed is therefore due to obstacles—that is to the presence of other bodies. The magnitude of this step forward may hardly be realised until we consider how the belief in the contrary had misled the earlier physicists. To them the cause of a body's gradual retardation was something inherent in the body itself. It was a difficulty that a body continued to move at all unless urged forward by some other body. Aristotle, for instance, explains the continued motion of a stone thrown from the hand by supposing the air to have a motion communicated to it which urges the stone on.

In the generation succeeding Galileo rapid progress was made in mechanics, and the rapidity of this progress may be measured by the fact that the age was ripe for Newton, who was born just about the time of Galileo's death. Grand as was his work in mechanics, it would never have been taken up as it was, had not the way been prepared for it. It is marvellous that so soon after the darkness of the pre-Galilean age should be elaborated a system of mechanics so advanced as that of Newton—a system which has lasted to our own day as sufficient to describe the motion of bodies.

Newton's system starts from his three Laws of Motion, which sum up our experience of the mutual mechanical actions of bodies. According to these laws, if we fix our attention on a portion of matter, and notice any change in its motion, we may ascribe that change as due to surrounding bodies. If we could only remove these surrounding bodies we should not be able to notice any change in the motion. Further, each body produces its own change in the motion of the observed body, whether other bodies are present or not; and there is a reaction on the acting body equal and opposite to the action.

In fact, Newton directed special attention to the quantity called force—the action of one body upon another. He showed how to measure it, and, in his law of gravitation and its verification he showed that there is in all probability an action between any portion of matter and every other portion.

Here we come to a point on which endless discussion has arisen—the meaning we are to attach to the word 'force.' Newton's Second Law begins by stating that change of motion is proportional to the impressed force. One school says that here force means that metaphysical something which we name when we say that we, as active living beings, are exerting force; and the statement of the law is taken to mean that the physical *effect* is proportional to the metaphysical *cause*. Another school says that force is change of motion, and that this part of the law only tells us the meaning of the word force just as if I were to say that in future I am going to call two shillings and sixpence half-a-crown.

Now the first of these views seems hopelessly wrong, and it is, I think, typical of much incorrect thought which has hindered the progress both of mental and physical science. The metaphysical force is incapable of weight and measurement. To talk of it as a quantity consisting of so many times some unit is absurd. We cannot take our weights and measures into the human mind. When we say that two things in physics are proportional we only mean that each is a physical quantity capable of measurement, and that the numbers expressing the measures always bear the same proportion. But if we state that one quantity, measurable by physical instruments, is proportional to something we cannot measure, and which we cannot express in physical terms, we do not add to our knowledge—we only make a statement which gains meaning in proportion to the confusion of our thought. The only admissible statement is that change of motion is the correspondent of, and accompanies, metaphysical force. But this is not a physical law, and does not help us at all in describing physical phenomena. We have yet to learn thoroughly the lesson that though we use the same words for physical quantities and metaphysical ideas, we are not to hop at will from one meaning to the other. In physics we must carefully keep to the one, and in metaphysics to the other. Physics is concerned solely with the measurable relations between objects, and we must only introduce terms which enable us to describe those relations. I do not mean that we are to be content with such physical descriptions. We are something more than physicists as conscious human beings, and we can describe our experience in terms where the laws of motion have no meaning, and such a description is to us much more real. For instance, when we listen to a great orator, and feel his influence, and mark his power over his hearers, we may say that the charm of his voice, and the passion and fire of his oratory, fascinated us and moved us to applaud; or we may say that a well-constructed source of sound in his throat gave rise to aerial vibrations in which the harmonics had a peculiar nature; that these vibrations were transmitted to a number of receiving instruments in the ears of his hearers, where their energy was modified and transmitted to the brains accompanying the instruments, so that, in a way as yet not understood, the hands were set in motion and a series of sharp shocks of sound were communicated to the air. Our ignorance of many of the steps in either mode of description is so vast that we are continually tempted to fill the gaps, and to try to hide our ignorance by supplementing the one by the other. But what we have to recognise is that the two modes are entirely distinct, and that all we can hope for is to show correspondences. We cannot conclude that because we are ignorant of the correspondences, therefore they do not exist; that because we do not know what corresponds to free will, to our sense of duty, to our sense of responsibility, therefore we are mistaken in supposing that these exist in reality.

But this hardly concerns the doctrine of Energy, and I return to our consideration of Force. Does our rejection of the metaphysical interpretation of the Second Law drive us to say that force is merely a name for change of motion? I think not, and I believe that Newton did not mean this. He appears to me to have looked upon force as being something, as it were, outside the body acted upon, emanating from, or surrounding, or in connection with the body acting, and of which we might have independent measurement. I do not see how otherwise we can talk of the same force, or equal forces acting on quite different kinds of matter. But it would take too long to enter at length into this somewhat difficult matter, and I go on to a consideration which is here suggested to us. Does the action of one body on another take place directly, without reference to the intermediate space? Does the sun, for instance, change the motion of the earth as it swings round from its furthest to its nearest point directly, without its action being communicated through intermediate space, but only with reference to the distance, of which it always seems to have an accurate knowledge? Or does whatever produces the change extend through all the space surrounding the sun? Is the something we call change of motion, when it is seen in the alteration of the earth's movement, in the space before the earth takes it up, or propagated from the sun as centre? May we take the propagation of light as to some extent illustrating this action of force? The light of the sun is filling all space. The earth, when lighted by the sun, is not receiving his rays directly, without passage through the intermediate space, but only reveals the light which would have been then there, whether the earth was there to receive it or not. Newton very decidedly took this view, as is shown most clearly in his often-quoted letter to Bentley: 'That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe that no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers.' (Faraday, *Exp. Res.* vol. 3, p. 532, note.) To Newton, then, there is something surrounding the acting body, or transmitted from it, and this, when communicated to the body acted upon, appears as change of motion. He, therefore, is justified in separating the force from the change of motion, and in looking upon the change of motion as one manifestation of the something which would be in the space even if there was no matter to take it up. Those who believe that we can go no further than the statement that force is the name for change of motion are the representatives of the old action-at-a-distance school. Newton is rather allied to the modern school, who reject action at a distance, and who believe that whenever one body acts upon another there is some machinery in the intervening space, whether evident to our senses or not.

This idea of a something filling all space round bodies seems to have dropped out of sight till the celebrated atomic speculation of Boscovich, an Italian philosopher, who flourished about the middle of the last century. Before his time the theory as to atoms was that they were little hard nuclei filling definite volumes, and with the power of influencing other atoms. But Boscovich saw that as far as we can analyse our knowledge of matter, that knowledge comes to us from force. Thus, I touch the table, or a force acts on my hand to change its downward motion. When you hear me speak, the drums of your ears are rapidly moving under the action of alternating forces. Other senses, we suppose, are acted on by minuter forces, which can single out the individual molecules and change their motions; in all cases our sense-organs appear to be the recipients of the forces exerted upon us by the external world. Starting with the idea that we know of matter only through force, Boscovich, I imagine, joined with it Newton's belief that force does not act at a distance, and so he arrived at a new conception of an atom as a system of force directed to a centre. He entirely rejected the little hard nucleus, and replaced it by an alteration in the force from attraction to repulsion, this latter becoming indefinitely great as we approach the centre. We must not take the ordinary account of this theory, according to which Boscovich did away with atoms, and replaced them by mere mathematical points, without 'parts or magnitude.' At least, to his great follower Faraday, his atoms were something very much more than mere points. Each atom filled all space: wherever the force was there was part of the atom.

I must confess that, as a physicist, I cannot accept this theory, this attempt to abolish the duality of matter and force. To describe any physical action we require at least two terms—the thing, and its rate of motion. Whether we call the thing matter, and the appropriate measure of its change of motion force, or whether we call the thing force, and invent some new name for its change of motion, is of little importance so long as we are all agreed. And so, when trying to form a physical conception of Boscovich's theory, I always find myself merely taking the central nuclei of matter, and spreading them through space with the new name of force. I think that we may go further than this, and say that we cannot be content even with the duality of matter and force, at least as yet. To the physicist there are as many things as admit of independent measurement—matter, force, electricity, heat, and so on. But I imagine that Boscovich regarded his speculation rather from the metaphysical side. His force, I believe, was the physical correspondent of will. His atoms became the centres round which the Divine Will acted. To him there was no dead matter moved hither and thither by other dead matter, but the universe itself was the power of God. Surely this is one of the sublimest speculations that the human mind has ever put forth.

It is somewhat the fashion nowadays with some physicists to sneer at metaphysics as barren in all but confusion of thought. But even a metaphysical speculation may have value as affecting our mode of regarding physical phenomena. This was the case with Boscovich's theory, which had, I believe, the very greatest influence in directing Faraday's experimental work. I have already mentioned that Faraday was a follower of Boscovich. His mind seemed thoroughly saturated with the idea that all the space surrounding a body was filled with what he called the force which that body could communicate to the other bodies, and he fully adopted the idea that the atom extended wherever its action extended. There was to him no such thing as a vacuum. Space was full of force. He was able to form a mental picture of this force from his favourite method of showing the direction of the forces surrounding a magnet by means of iron filings—the so-called lines of magnetic force. In his mind's eye, the space surrounding electric, magnetic, and gravitating bodies was filled with lines of force like those revealed by the iron filings. The question was ever recurring to his mind:—How can I show that this force is present in what appears to our senses empty space? If the action is really transmitted through the space, or is in the space, how can the connecting machinery be revealed? We have an excellent example of his mode of thought in the great paper 'On Static Induction' (*Exp. Res.* vol. 1, p. 360), in which he showed that the action between electrified bodies depends on the nature of the intervening substance, that the force which a given charge of electricity exerts upon another given charge is different when the intervening matter is charged, being greater, for instance, in air than in paraffin or in bisulphide of carbon. He begins with a description of an experiment with a block of ice. Suppose that we have a plate of ice with two pieces of platinum foil on the two sides, and that these are charged with electricity by connection with two wires from a galvanic battery. No current passes, because the ice is a non-conductor, but equal and opposite quantities of electricity will gather on the two sides through *induction.* Is this induction, this action resulting in the gathering of these charges, a direct action at a distance, or is it an action taking place from particle to particle through the ice? If we melt the ice then, since water is a conductor, a current passes, and the charges are, to a great extent, lost. The current, no doubt, is a something in which the intermediate particles are concerned—we have direct evidence of this. But it appears to be only the second step, in which induction is the first. Induction is, as it were, the beginning of a current, and we are therefore almost driven to suspect that it, too, is a something with which the intermediate particles are concerned, or, as Faraday puts it, 'led to suspect that electrical action at a distance never occurred except through the influence of the intervening matter.'

He then sought for a test of the truth of this view, and found it in his grand discovery that the charges gathering on the metal coverings, on the two sides of a plate of given thickness, differed with the material of the plate. As a result of his discovery, everyone now probably believes that electrical actions occur through the influence of the intervening matter, or require connecting machinery.

Another of Faraday's great discoveries—the action of magnetism upon light—bears upon this point, and also illustrates another great move onwards in the progress of thought which took place in the first half of this century. Faraday's account of his discovery opens with these words: 'I have long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their action.' Holding this belief, he was led to seek for some direct relation between light and electricity. In this he failed: the discovery has only been made within the last few years by Dr. Kerr. But he succeeded in showing an action of magnetism on light. If a ray of light which is polarised that is, we may suppose, is made to vibrate in some particular direction, say up and down—and then traverses horizontally a line of magnetic force through some substances such as glass, water, etc., the direction of vibration gradually changes as the ray travels on, and the vibration is found to become more or less sideways, depending upon the length of the line of force traversed, and the intensity of the force. Faraday succeeded, then, in proving a direct relation between magnetism and light. But the experiment had value for him in another way. It showed that the space near a magnet was not ordinary space: it was affected by the presence of the magnetic force, so that it no longer transmitted light in the usual manner. Faraday beautifully described his experiment by saying that he had illuminated a line of magnetic force.

Faraday, then, has made it possible, and almost necessary, for us to think of the action taking place between bodies as depending on the intervening medium; and we must observe that he usually describes this action by his conception of force.

We have now to notice the growth of another idea, the conception of a new quantity to measure, a new quantity to use in our descriptions of phenomena. This quantity we now call energy.

We have already seen that in the early part of the century the attention of physicists was being more and more directed to the so-called relations between the physical forces, and the belief was growing that, as Faraday expressed it, 'the various forms under which the forces of matter are made manifest have one common origin.' At first this belief was very vague, and no exact statement of the relation had been found. But gradually it began to be perceived what should be sought for, and I suspect that we may here trace the direct influence of practical men. At this time the steam-engine was coming into general use, and it was seen that the value of the engine was directly proportional to the work it could do—that is, the distance through which it could move a body against a resisting force. Attention was directed to this quantity of work, measured by the force × distance moved against it, and it was seen that all the so-called physical forces could do work. Was not this something by which they could do work, the something common to all?

Now a moving body was known by virtue of its motion to possess the power of doing work expressed by its mass × half the square of its velocity, or, as we now say, by its kinetic energy. It was also known that in such a case as the projection of a stone upwards, where work is done against gravity, though the kinetic energy disappears, it can all be regained (neglecting the effect of the air) if the motion be reversed, and the sum of the work done and the kinetic energy remains always constant. It appeared probable, then, that the kinetic energy did not actually pass out of existence as the stone rose, but was transformed into a shape not recognised by our senses into that which we now call energy of position, or potential energy. On this assumption we might say that the total energy remained the same, though it might be transferred from kinetic to potential, or the reverse. But in some cases the total quantity of energy in these two forms did not remain the same. Thus, if the stone fell to the ground both kinetic and potential energy disappeared, but at the same time the ground and the stone were heated. Was this heating another form of energy?

That it was so seemed very probable from the new mechanical theories of phenomena which were gaining ground. The old ideas of different physical substances packed into the interstices of bodies were dying out. The corpuscular theory of light was giving way to the wave-theory; the caloric theory of heat to that of vibratory motion of molecules. So it was suspected that, if we only had sufficiently acute senses, we should find that physical phenomena would all be capable of explanation in terms of the position and motion of their molecules and the surrounding medium, whatever that might be. It was natural, then, to suppose that when potential and kinetic energy are apparently lost, as in the case of the fallen stone, these energies are really present in the molecules of the heated body, which are swinging to and fro more violently, and are separated more widely, than before. It was also natural to suppose that the energy was to be measured by the quantity of heat gained, for it was known that, in the case of the steam-engine, the greater the work done the greater was the heat to be supplied. The first careful experiments made to test the correctness of this view were those of Joule, who used various forms of apparatus in which work was done against friction, and the heat developed was measured. He discovered that the heat developed was directly proportional to the work done, and so it was concluded that this heat was merely the equivalent, in a form different to our senses, of the energy which, at first sight, disappeared when the work was done. What was true of heat was supposed to be true of other forms of physical force, as they were then termed, and the general law was enunciated that the quantity of energy remains constant, though it may change its form as far as our sense-perception of it goes.

We may now return to Faraday's ideas, and the development which they have received in the hands of Clerk Maxwell. Maxwell was early thoroughly imbued with Faraday's belief as to electric and magnetic actions taking place in and through the intervening medium, but he was able to go a step further than Faraday, with the conception of energy, which was as familiar to him as a definite measurable quantity, as the force with which Faraday had to be content. Faraday saw the electric and magnetic forces pervading all the space surrounding electric or magnetic bodies. Maxwell saw the energy, that by which the bodies could do work, also pervading this space or stored up in it. Before Maxwell's time energy had already received a name he gave it a local habitation. I imagine that his starting point was some such speculation as this. When a body is acted on by a force, and moves so as to acquire energy, there is no more reason to suppose that this energy is given to it immediately, without reference to the intervening space, than there is to suppose that the force acts at a distance. The energy must enter from the surrounding space. If so, the energy may well be in the space, whether the body is there to be acted upon or not. Wherever then we find that a body is acted on by force, there there is energy, either moving through or stored up in the space, and ready to be imparted to the body. Maxwell was led to investigate how much energy was in the space surrounding electric and magnetic systems, and how it was distributed, and he was able to assign to each portion of the space a quantity depending on the force exerted there, which gives us perfectly consistent results, and enables us to arrange our experimental knowledge so well that it is difficult to doubt his theory.

We are now able to form a somewhat more definite conception of the something extending through all space which surrounds a body capable of exerting force. Faraday thought of it as force—we recognise it as energy.

According to these views, electrical energy is of the potential type—that which we recognise in a compressed or extended spring; and we may provisionally suppose that it is in the form of some sort of strain of the medium, while magnetic energy—the energy round a magnet—is rather of the kinetic type—that of a moving body. Now, in order that energy may move from one body to another, or from particle to particle of the same body, it is necessary that both forms of energy should exist. Thus, we cannot get the energy from a reservoir of water without letting it run down-hill; we cannot get the energy out of a bent spring without letting it unbend and get in motion. So with electric and magnetic energies. We cannot move either of these energies unless they both co-exist. We must allow the strain of the medium to give way—i.e., we must have magnetic energy in order that the energy may be set in motion. Maxwell investigated the rate at which the energy so set in motion would travel in a particular case—where the electrical disturbances were of an alternating character—and he found that this rate coincided with the velocity of light. He was thus led to his great theory that light is only a particular form of electric and magnetic energy. This theory is still on its trial, but it has passed its earlier examinations with credit. From one point of view, the theory may be said to throw us back. Twenty years ago it was supposed that we knew nearly all about light—that it consisted of a vibratory motion of an ether filling all space. Even the direction of vibration we knew, and the only weak point was that we did not know the constitution of the ether. If we are to accept Maxwell's theory, however, we must give this up, and replace the vibratory motion of the ether by an alternating electrical disturbance transmitted onwards as waves of disturbance; but we are, as yet, utterly in the dark as to the true nature of the motion of the ether. But from another point of view we shall have made a great step forward, for whatever explains the phenomena of electricity will explain also the phenomena of light—the number of energies is reduced by one.

In passing, it is interesting to notice how nearly we have come here to the old emission-theory of light. We see now that a something is actually shot out from a luminous body and transmitted through space—that something not material corpuscles, but energy.

It may at first sight appear strange that these ideas of Faraday and Maxwell are all applied to electric and magnetic forces, while that force which to us is most evident and familiar—the force of gravitation—is left untouched. But though it may appear paradoxical, it is nevertheless true that the force of gravitation is so small that it is exceedingly difficult to experiment upon it. The force we are used to is that exerted by the whole earth, and is therefore appreciable; but that exerted between bodies of moderate size, such as could be introduced into our laboratories, is almost insensible. But we can, at least as a speculation, extend the ideas which we have gained from our study of the more manageable electric and magnetic forces, and though it is dangerous to rush in where Faraday and Maxwell feared to tread, I cannot help thinking that Maxwell's investigations as to the distribution of electric energy apply, with very slight alteration, to the case of gravitation-energy, and that it, too, is resident in the surrounding medium. But I have already drawn this paper out to too great a length, and I will now conclude with a short summary of the ideas I have tried to lay before you.

In what I have said I have attempted to trace out how our knowledge of phenomena has become clearer as we have gained one conception after another of measurable quantities by which the phenomena could be described. First came the recognition of velocity, then the idea of a force as measurable by change of motion; then Faraday's conception of a something he called force, extending throughout the space wherein the force can be manifested. Then came energy as something which remains constant in quantity however it changes to our senses; and, last, Maxwell rendered Faraday's conception far more definite by showing that it was probably energy which is the something extending through space. Incidentally we have seen how various physical energies have been related. The next great step will be the discovery of the relation between these energies and gravitation.

50.

THE ELECTRIC CURRENT AND ITS CONNECTION WITH THE SURROUNDING FIELD*.

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I propose in this paper to give some account of a theory of the electric current which is based upon—and, indeed, seems to be an inevitable consequence of—Faraday's views as to the nature of electric action.

In addition to his experimental discoveries, which place him first in the rank of physicists, Faraday has given us a hypothesis as to the nature of physical actions which is now universally held, the hypothesis which regards all actions as transmitted to the body acted on through the surrounding medium. Strictly speaking, Faraday only revived this hypothesis, for it is the most natural one, and it was, I believe, generally accepted before the time of Newton. When in common experience we produce action on other bodies by our own exertion we require some connecting matter to transmit our energy. It was at first felt as a difficulty in Newton's theory of universal gravitation that no mechanism was known to exist which could transmit the action from sun or planet to planet. It was forgotten that Newton's theory was merely a description of the observed motion of the planets, showing how that motion varied with their relative positions; with the addition of a guess that similar actions would be found to take place in all matter varying similarly with relative position—a guess since verified by laboratory experiments. The simplicity with which the theory could be stated in terms only of the distance of the bodies apart, and without reference to any intervening mechanism, seems to have led to the supposition that none existed. Newton, however, was clearly of opinion that the action required a medium for its transmission, and Faraday claims him as supporting the view that in the case of gravitation towards the sun 'the power is always existing around the sun and through infinite space, whether secondary bodies be there to be acted upon by gravitation or not,' and this, 'is, in philosophical respects,

* This paper contains a general account of a hypothesis of which fuller details have already been published in *Phil. Trans.* Part II, 1884, and Part II, 1885, and *Proceedings of the Birmingham Philosophical Society*, 1885–6. [*Collected Papers*, Arts. 10, 11, 12.]

the same as that admitted by all in regard to light, heat, and radiant phenomena; and (in a sense even more general and extensive) is that now driven upon our attention in an especially forcible and instructive manner, by the phenomena of electricity and magnetism, because of their dependence on dual forms of power.' (*Exp. Res.* vol. 3, p. 574.)

I shall begin with some account of Faraday's views regarding the nature of electric action, familiar as they now are, because I wish to show how we are naturally led on from them to the views as to the nature of electric current which will be set forth in this paper.

Whenever any body is electrified, by any process whatever, with one kind of electrification, the opposite electrification is somewhere *in its presence.* If a positively charged body is placed on an insulating stand in the middle of a room an equal quantity of negative electricity is on the surrounding walls—induced, as it is termed, by the positive. Faraday sought to charge the air within a large hollow conducting cube with one kind of electricity alone, but he always found that the other kind was present in equal quantity on the inner surface of the walls of the cube, and when the electrification of the air was discharged against the walls the two exactly neutralised each other, and no balance of either kind remained over. From this and other experiments Faraday concludes that 'bodies cannot be charged absolutely, but only relatively, and by a principle which is the same with that of *induction.* All *charge* is sustained by induction.' (*Exp. Res.* vol. 1, p. 367, § 1178.) This is what he means by 'dual power' in the passage quoted above. The two kinds of electrification always accompany each other. Given one, the other is somewhere in its neighbourhood.

Faraday also showed that the amount of charge gathering on the two opposed surfaces of conductors depended, under given circumstances, on the nature of the insulating medium. Regarding the two charges as inducing each other, he was led to the irresistible conclusion that the induction was propagated by the medium. He thought of it as taking place along the lines of force the curves drawn so that at any point in their length they represent the direction of the force which would act on a small charged body placed at the point.

Let us state his theory in its modern form.

If a conductor A (Fig. 1), charged say positively, is surrounded by another conductor B but insulated from it, the charge on A gathers an equal and opposite charge on the inner surface of B by induction. If we mark out on A plots, each containing one unit of positive electricity, and draw from every point of the boundary of a plot a line of force, we obtain a tube reaching from A to B. Maxwell terms this a unit tube, and it can easily be shown that it will include at its end on B a unit of negative electrification. Drawing all the tubes from all the plots on A we honeycomb the space with tubes.

Not only, then, is the total + equal to the total −, but they correspond element by element, the elements being connected as it were by these tubes of force.

Since the medium is thus propagating, or rather, shall we say, supporting induction, and since the amount of induction depends on the nature of the medium, we must consider it as active in the process. It must be modified in some way corresponding to the existence of the induction. We may think of it as being in some state of strain—a supposition borne out by the fact that it transmits light in a different way when thus supporting induction, a discovery made since Faraday's time. Without attempting to describe the nature of the strain, we may compare the strains at different points, by measuring the forces exerted on small charged bodies placed at those points, or by the amount of negative electricity induced on the surface nearer to A of a small conducting body. Then we may also describe the direction

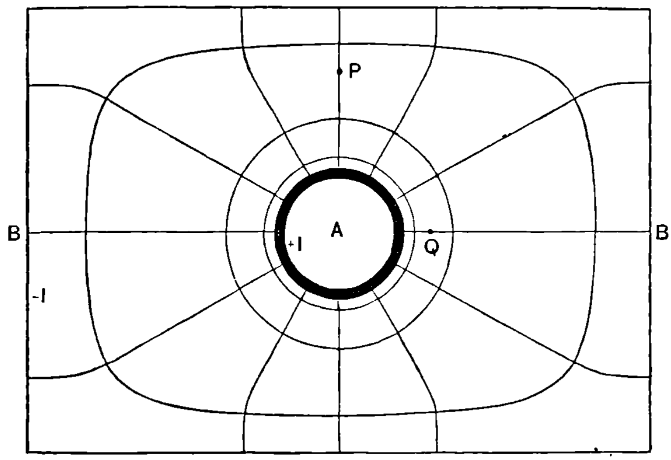


Fig. 1.

of the strain as that in which a small charged body tends to move. If, for example, the force on a small charged body at P is $\uparrow$, and that on the same body removed to Q is $\rightarrow$, and twice as great, we may fairly describe the strain at Q as double that at P, and in a direction at right angles to it. We have here very little beyond the description of experimental facts. Our grain of hypothesis is the statement that the medium is altered in some way. Given this, our experiments enable us to measure the strains in terms of some standard unit strain, and enable us to assign direction to them. In a similar manner we can measure the intensity of light, and assign a direction to it without committing ourselves to any hypothesis as to the nature of light. Now, when a small conductor is placed in the field of force of an electrified body it always becomes charged with opposite electrifications at its two ends by induction, the amount of charge gathering being proportional to the strain. We may, therefore, conveniently term the strain at any point the *induction* in the medium at that point, even though no conductor is present there to show it.

Returning to our charged body A within the conductor B (Fig. 1), we know that a certain amount of work can be got out of the system. If we allow a small conductor to touch A, taking a minute fraction of A's charge, it will tend to move away to B along a line of force, and it can be made to do work. If we let it move along a line of force by steps, so that in each step it gives us an equal quantity of work, we may speak of these steps as equal falls of electrical level, just as when we let a quantity of water fall downhill it will give us equal amounts of work for equal falls of level. While, however, water level may be measured in feet or inches, electrical level can only be measured by work, for the successive steps are not generally of equal length.

If we mark along the line of force points such that in each interval between two points a body charged with one unit of positive electrification gives up one unit of work, the intervals correspond to unit differences of level, and if we draw through the points surfaces surrounding A, and everywhere cutting the tubes of force at right angles, we get electrical level surfaces level because the electrified body does not tend to move along but perpendicular to them.

If now all these level surfaces and all the unit tubes be drawn we have the space entirely cut up into cells, and Maxwell has shown that the number of these cells is just double the number expressing the quantity of work which can be got out of the charge by allowing the small body to perform successive journeys from A to B, or conversely the amount of energy we require to charge the system from its original unelectrified condition.

This energy of charge we may regard as required to strain the medium and throw it into a condition of induction, and we may further regard this energy as residing in the medium wherever the strain exists, just as we may suppose the energy of compressed air as residing in the compressed air. That the energy may be localised is, no doubt, a fresh supposition, but we may adopt it at least as a working hypothesis.

Since the number of cells into which the field is divided is always double the number expressing the energy of charge, assigning to each cell half a unit of energy, we exactly account for all the energy of the system. The mere coincidence of the two numbers is not in itself a sufficient reason for assigning the energy in this proportion. If we found on examining the statistics of all the large towns in England that the number of inhabitants in each town was just six times the number of houses we should certainly not be justified in concluding that exactly six persons dwelt in each house. But we have a further reason for assigning the half unit of energy to each cell. It makes

37—2

the quantity of energy, per cubic centimetre, proportional to the square of the strain in the medium—exactly the law according to which energy is stored up by the ordinary elastic straining of matter. Hence if we have any electrified system, and we draw all the unit tubes and the level surfaces at equal level intervals, we get a complete electrical map of the system, telling us the direction of the force on a small charged body, the variation of the electrical strain from point to point, and the distribution of the energy.

Accepting this account of electrical action our point of view is entirely changed. The old idea of electricity was that it was a something added to or taken away from a conductor or a charged body; now we must regard it as a modification of the surface accompanying the existence of induction in the medium—the manifestation, at the bounding surface, of a peculiar kind of strain in the medium. Let me illustrate this: We regard variation of atmospheric pressure on surfaces exposed to the atmosphere as accompanying changes in the density or motion of the surrounding air, i.e., as surface-manifestations of changes in the medium; but were we utterly without any knowledge of the existence of the atmosphere we should probably have invented 'somethings' existing on the surfaces to account for the pressures and motions of bodies which we now attribute to the air; a layer of a subtle fluid on the surface of the cistern of a barometer with varying attraction on it by the earth; two more subtle fluids, one on the piston and the other on the water, in the common pump, these two attracting each other; another subtle fluid, with a rotary motion, on the sails of a windmill, and so on. Now, till Faraday's time physicists were, in electrical matters, where we should be in regard to atmospheric effects with no thought of an atmosphere, but Faraday has changed all that, though we have not yet fully grasped the consequences of the change. We still use language appropriate to the old ideas, and I fear that the old ideas themselves are still lurking in corners of our minds.

It is important, then, to be quite clear in what sense we shall use the terms 'electricity' and 'induction,' and I will, therefore, briefly recall their meanings. If we have a charged body we apply the term 'electricity' or 'electrification' to the modification of the surface, due to the termination of the strain in the medium there. We apply the term 'induction' to the strain in the medium, though we do not know what the strain is in itself. Faraday himself says—(Letter to Hare, *Exp. Res.* vol. 2, p. 262) that his theory makes no assertion as to the nature of electricity. We can only compare the amount of strain at one point with the amount at another, and we must be very careful not to call this induction electricity.

We now pass on to the consideration of electrical discharge. An electrical discharge is any process by which the energy of the system which we have previously recognised as electrical is wholly or partially transformed into other modifications of electricity.

I shall take first the simple case of two parallel plates separated by an insulator—one positively charged, the other connected to earth and negatively charged 'by induction.' Let us suppose that the insulator is not quite perfect, so that the charges gradually disappear. On the hypothesis that the two electricities are 'somethings' we should have to suppose that they are gradually pulling each other through the insulator, penetrating it, and meeting in the middle to destroy each other. We still use the language of this hypothesis. We speak of a 'leaky' condenser, of a 'current' passing between the plates, of 'electric absorption.' But we have no evidence for this soaking in of the charge, and some evidence exists directly opposed to it. Were it the true explanation we should have the phenomenon of residual charge occurring in all cases, whereas it appears probable that it only occurs with heterogeneous dielectrics. It has been shown that with homogeneous Iceland spar on discharge the whole charge disappears at once, and no residual charge gathers*. (Rowland and Nichols, *Phil. Mag.* vol. 11, 1881, p. 414.) Any previous discharge due to 'leakage' must, therefore, have taken place by the decay of induction all along a tube at the same rate, a breaking down of the state of strain to the same extent throughout, the energy being probably transformed from electrical into heat-energy, and perhaps into energy of chemical combination or separation.

As an illustration of the two modes of viewing the process suppose that a bar of pitch is suddenly twisted, and the two ends held fixed. The pitch will gradually adjust itself to its strained position, the energy of strain gradually dissipating. If the energy of strain died away at the two ends first the process would be analogous to that of the 'leakage' through a dielectric on the electric absorption theory. The actual process of decay, however, goes on all along the bar, which is analogous to the mode of decay of induction which is here advocated.

Now take another case of discharge where the two charged plates A, B are connected with two other plates C, D by two wires (Fig. 2). There will be a partial discharge from A, B, and the charges will be shared between A, B and C, D, and if we follow up in imagination the process of charging C, D we must think of some of the $+$ electrification moving along the wire from A to C, while an equal quantity of $-$ electrification moves along the other wire from B to D, the corresponding amount of induction of the medium being also transferred.

There are several modes in which we may suppose the transfer to take place. We may imagine the electricities travelling along the wires

* [Residual charge has since been shown to occur in Iceland spar. See S. W. Richardson, *Roy. Soc. Proc.* vol. 92, 1915, p. 41. Ed.]

unconnected by any induction while on the journey, packing up their tubes as it were till they arrive opposite to each other on C, D, and then putting them out again into the new insulating space. This supposition is directly at variance with Faraday's fundamental proposition that 'there is no *absolute* charge of matter with one fluid,' for the two electricities, while travelling in the wires, would be entirely disconnected, and would, therefore, be absolute charges unrelated to each other.

Or we may imagine that the strain between A and B, having an outlet provided for it by the wires and C, D, yields, causing a strain to be transmitted along the wires out into the space between C, D, somewhat as a compressed solid between two india-rubber bags filled with water, in the place of A, B, would be partially relieved from strain on connecting the two bags by pipes with two other bags, in the place of C and D, some of the water being forced along the pipes into C and D, and so straining the solid between them. This would, perhaps, be the kind of illustration most appropriate to Maxwell's theory of 'displacement,' if we carried out the idea of displacement in a much more definite manner than Maxwell did. But there are some objections to it. In the first place, it seems to require the medium

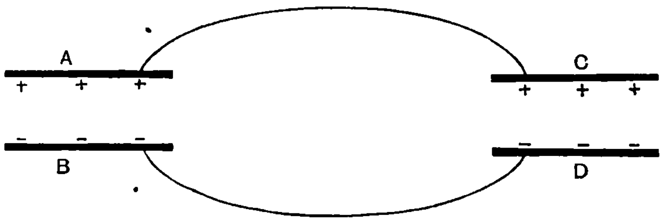


Fig. 2.

within a charged conductor to be subjected to pressure, and thus to differ from the medium in an uncharged body. There is no evidence for this. The inside of the conductor is apparently entirely screened from the outside, and is in exactly the same condition whether the outside is electrified or not. Then it would appear that such a mode of transfer of strain would require the transmission along the tube to be instantaneous, so that as the strain was relieved between A and B it simultaneously appeared in C, D. For, if not, suppose that there is a small interval between the disappearance of some of the strain from A, B, and its reappearance between C and D. We may represent this by the travelling of the electricities along the wires. Now, while the electricities are thus in the wire, they are only connected by induction-tubes passing back through the wires and across the space between A and B. But we are obliged on any theory to admit that induction does not last in a conductor, and therefore the connection between the electricities would decay, and we should have two absolute unrelated charges. Carry the wires away while in this state and we have + and − electricities detached from each other*. The instantaneous transfer would get over this objection, because the connection across C, D would be formed the instant that the connection along the wires broke down. But we know the transfer is not instantaneous, and that it requires a time greater or less according to circumstances.

There is another explanation possible, which, I think, is much more in accordance with facts. This supposes that the induction, instead of being propagated endwise by an end-thrust of the tubes, is propagated *sideways, through the medium.* As the two electrifications travel, one from A to C, the other from B to D, they are still connected by their induction-tubes, which have diverged from the space between A and B, and which sweep through the surrounding medium, converging again in the space between C and D, carrying the strain and energy with them. Or rather we may symbolise the transfer of strain and energy from the one space to the other, by imagining the tubes to move thus, the transfer going on until the strain in C, D balances that in A, B. And we may call in to support this explanation the fact that we have a new phenomenon accompanying the transfer a phenomenon *manifested in the medium*—viz., magnetic action indicating the existence of magnetic energy in the medium.

An illustration may serve to show how this fact supports the explanation that the transfer is through the medium. Whenever fixed machinery is transmitting energy we find a similar coexistence of two energies. When, for instance, some driving power is put in action, tending at one end to turn some machinery against a brake put on at the other end, if the brake is too hard on, the driving power will only work for a very short distance, not moving the piece in contact with the brake, but straining all the intermediate wheels, shafts, and belts. But on gradually lessening the pressure of the brake a point is reached when the machinery begins to move continuously, the strain is transmitted onwards from the driving power to the brake, and the energy with it. But for this transfer it is absolutely necessary that we should have motion, i.e., kinetic energy must exist as well as strain.

So in the transfer of electric energy, we must have the magnetic energy corresponding to the energy of motion of the machinery to transmit the energy from point to point. The magnetic energy in the medium is evidence that the machinery is in motion, and shows that the medium is itself the machinery.

Let us suppose the second insulator between C and D to be imperfect, so that 'leakage' takes place, i.e., so that the induction in it breaks down or

* It might, perhaps, be possible to meet this objection by replying that the opposite electrification is in the same wire at the ends of the tubes of induction, wherever they may be. If, for instance, the + electrification has moved two miles along the wire, and the induction in the first mile has decayed, then the − electrification is at the end of the first mile. But if we accept this explanation what is to prevent these + and − charges neutralising each other in the wire? We should then have the possibility of disappearance of charge merely by conduction along a wire, which is contrary to all our experience. J. H. P. [added June 24].

decays. We shall have a continuous propagation into it of fresh induction from the outside medium, tending to keep up the balance between the strain in *A, B* and that in *C, D.* We therefore have a continuous magnetic action in the medium, and indeed we have all the phenomena of an ordinary current in and round the space between *C* and *D,* and these phenomena we must attribute simply to the sideways propagation into that space of induction or strain, and its subsequent breaking down, with the accompaniment of the necessary magnetic action.

Let us then see whether we can explain an ordinary current in the same manner. Instead of interposing a pair of plates *C, D,* let us connect *A, B* by a single wire. We have in the wire a discharge which is identical in all respects with an ordinary electric current. We have in the surrounding medium magnetic actions, so that if the wire passes near a magnetic needle it is deflected, and in the wire itself heat is developed, and the total heat-energy is the equivalent of that previously existing as electric.

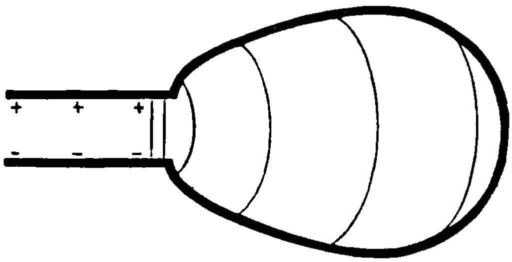


Fig. 3.

It is impossible to point out any difference in kind between this process and that of charging the leaky condenser. We may have all degrees of 'leakage,' from that of an excellent insulator to that of an excellent conductor. As the two electrifications move towards each other along the wire, i.e., as the terminations of the tubes approach each other, the end-parts of the tubes gradually enter the wire. But the wire is incapable of sustaining the strain for more than a moment, and it breaks down, the energy being dissipated as heat. The accompanying figure (Fig. 3) may serve to illustrate the process, successive positions of a tube being given.

If this process be followed out it is clear that if a ring be drawn round the tube the same number of tubes of electric induction must cut it on the way to the wire wherever it may be along the wire, and I think we may naturally connect this with the fact that the work done in carrying a magnetic pole round such a ring is the same wherever it may be, and is proportional to the current or to the number of induction-tubes passing into the wire per second. In fact, we may take the value of the magnetic action as an indication of the rate at which the machinery is carrying in electric induction towards the wire. The magnetic energy itself goes in towards the wire and finally appears there as heat.

We now come to the consideration of the production of a current by an ordinary galvanic battery. At the outset we observe that there is really no difference in kind between the electric discharge here and that occurring between two charged bodies. For if the circuit of the battery is not completed, but it is connected to the two plates of a condenser, charges gather on the opposing faces, + on that connected to the copper, – on that connected to the zinc. The + electrification is transmitted along the wire from the copper, the – along that connected with the zinc. We cannot resist the conclusion that the induction or electric strain is propagated from the battery through the air to the space between the plates, the electrifications travelling along the wires and connected as it were by the induction-tubes.

In ordinary circumstances this charging will only go on until the strain between the plates of the condenser balances that in the battery. But suppose the insulator between the plates is imperfect, so that leakage or, as we may more correctly term it, decay of induction takes place, the charging will be continuous, i.e., the induction will continue to move through the medium, the energy set free from the battery will move through the air into the space between the plates, and there be gradually transformed into heat and perhaps chemical energy.

If we suppose the insulator to become gradually worse and worse, the insulator approaching more and more to an ordinary conductor in its properties, the process must still be the same in kind. Even if we have no insulator at all, but a closed metallic circuit, the only difference we can observe is that we cannot detect any chemical action in the wire, while probably there is chemical action in imperfect insulators. And this is very likely only through our imperfect means of observation. I believe that many electricians suspect that conduction in metals is accompanied by a change of partners in the molecules along the wire—a chemical action, but as all the molecules are alike the final condition of the wire as a whole is not altered.

Tracing, then, the process from the time when the induction has emerged from the battery, we may suppose it somewhat as follows: A tube starts with its + end near the copper terminal, its – end near the zinc. It moves through the medium, the two ends continually moving into the wire and the energy of the strain there breaking down and being transformed into heat. Other tubes will be propagated out in other planes, but they, too, will finally converge on the wire. The magnetic energy existing in the medium is evidence that the machinery is in motion, and the fact that if any two points in the circuit are connected to a condenser, electrical induction exists in the condenser, is evidence that electrical strain exists in the medium.

The chief machinery of the current then exists in the surrounding medium.

It is from the medium that the energy of the current is derived, this energy coming in sideways on to the wire. The wire plays, of course, an essential part, for without the breaking down of the strain in it there would be no relief of pressure, the back-pressure of the medium would soon be equal to the forward pressure of the source of energy, and the machinery would cease working; just as a steam-engine would cease working if the exhaust pipe was stopped up.

The current along the wire must now be regarded in a new light. We may still have electrification moving along the surface of the wire, + near one end, – near the other, these moving in opposite directions; but the essential feature of the current *in* the wire is the continual propagation into it of electrical strain and electrical and magnetic energies, and the breaking down of this strain, with the transformation of the energy into heat. The old question of the velocity of electricity now resolves itself into the question what length of an induction-tube is pushed in sideways into the wire per second, and we can easily imagine cases in which the answer is—an infinite length, or there is no longer any finite velocity; for, take a straight wire in a medium in which the induction outside is all parallel to the wire, the tubes will move parallel to themselves and enter the wire at the same time along their whole length. If we regard this disappearance of a tube as caused by the approach of the ends, we can only make the velocity absolutely infinite.

One point is here worthy of note. One of the laws of current-action is that the current is the same at all points of the circuit. And this is true if we measure it appropriately by heating effect, or by magnetic action. The latter we seek to explain by connecting it with the motion inwards of induction, and it is clear that in this hypothesis the same number of tubes break through any ring surrounding the circuit wherever situated. But as regards passage of electrification, or of tube-ends, the two ends of the wire differ. Along one end, that near the copper, we have + electrification, and near the other end – electrification. If we could only detect any difference between + and – electrified copper, it is probable that we should notice this difference in the circuit.

It may perhaps be interesting to consider one or two cases of electric current in which this hypothesis requires a rearrangement of ideas. Take first the case of an Atlantic cable. Suppose that the zinc terminal of the battery at the sending station is put to earth, the other terminal being connected to the copper core of the cable, and suppose that a magnetic needle is moved as a signal at the receiving station by the energy sent across the ocean. The + electrification from the copper end, and the – from the zinc, accompany each other—the former along the wire, the latter along the earth into the outer metal covering of the wire; the connecting induction-tubes passing from core to sheath through the insulating material. The strain and the energy are propagated onwards through the insulating material, lessening as they go by the amount sent into the wire, the remnant emerging into the open air at the other end, and there being converted into mechanical energy in the motion of the needle. The true cable, then, is the insulating material. The copper core acts, as it were, as a guide by which it finds its way to the right point.

One of the most important steps in the practical application of electricity to telegraphy was the discovery that the earth might serve as the so-called 'return wire,' so that a single wire might be employed from the sending to the receiving station if well connected to earth at the receiving end. The action has been explained in various ways. Sometimes it has been supposed that the battery draws up a store of electricity from the earth and pumps it through the wire to the earth again at the other end, just as a pump might pump up water at one point of a reservoir, force it through a long pipe, and discharge it again into the reservoir at a distant point. I suppose it is this idea which leads some to describe the earth as 'that great reservoir of electricity.'

But now we can give a different interpretation. The earth is a conductor, and when the battery sends its energy out into the medium one kind of electrification travels along the wire and the other along the earth which is a conductor—the two being connected by induction-tubes, which move along, carrying the two charges, both in the same direction, from sending to receiving station. Instead, then, of regarding the earth as a great reservoir of electricity we may look upon it as merely serving as a conductor for the opposite electrification.

As a last example, I may just touch upon the induction of electric currents in secondary circuits. Instead of a secondary induced current being due to something sent out from the primary wire, we must regard it as intercepting some of the induction and energy on its way from the battery to the primary wire.

I cannot here enter into the subject of the battery and what relation there is between the chemical action occurring there and the electrical induction sent out into the medium. It is a subject full of difficulty and full of controversial matter. We can get glimpses here and there of the process, and we can, I think, see that chemical and electrical energies are absolutely identical, but we cannot give any complete explanation until we have some hypothesis as to the nature of electrolysis much more detailed than any yet put forth.

I have here sought to give an account of the electrical current, which may, I think, be accepted by all, whatever views they may hold as to the mode in which the battery acts. Having once accepted Faraday's theory of electric induction, it is, I think, impossible to doubt that the medium plays some such part in sustaining the current as that which I have described. My chief aim has been to show that instead of an endwise action propagated along the wire, we must now suppose a sideways action, a transmission of strain and energy into the wire from the surrounding insulator.

51.

THE FOUNDATIONS OF OUR BELIEF IN THE INDESTRUCTIBILITY OF MATTER AND THE CONSERVATION OF ENERGY.

A CRITICISM OF SPENCER'S 'FIRST PRINCIPLES,'
Part II, Chaps. IV, V, and VI.

[*Midland Naturalist*, **12**, 1889.]

[Read before the Sociological Section of the Birmingham Natural History and Microscopical Society, November 22, 1888.]

I confess that when I accepted the invitation to give a paper on the chapters in Spencer's *First Principles* dealing with the Constancy of Matter, Motion, and Force, I had no idea of the difficulty of the task which I was undertaking. I remembered that when, many years ago, I read the chapters I disagreed with their general drift, and I thought it would be tolerably easy to disagree still. And so I have found it. But it is one thing to disagree with an author, and quite another thing to give clear reasons for your disagreement, especially when the subject is so difficult and your author is so great a master of argument as Spencer. And there is to me another difficulty in that I have never studied Spencer's system as a whole. The chapters I am to deal with form but a part of that whole; one staircase, as it were, in a grand edifice, which you have watched building stone by stone. I am venturing to criticise this particular staircase when I have not studied the plans of the building, and know not whence it springs or whither it leads. I am a mere carpenter venturing to criticise the work of a great architect. I don't know that I am even a carpenter. I have been, for many years, especially engaged in teaching people how to climb this particular kind of staircase, and perhaps you may think that that is hardly a sufficient warrant for a criticism of the nature of its materials and the strength of its supports. But this is the task I have undertaken.

If I may assume that you are acquainted with Spencer's argument, I need only briefly sum it up as follows:

In Chapter IV he maintains that the indestructibility of matter is a necessary truth, one of which we cannot imagine the contrary when we once clearly present to our minds the meaning of the terms 'matter' and 'indestructible.' He argues that the so-called chemical and physical proofs based upon weighings really assume the principle in assuming that the weights used to counterpoise are constant in their value. He concludes that, when analysed, the indestructibility of matter is found to mean the persistence of force. For if we use the chemical proof, the constancy of weight means persistence of gravitative force, and, if we regard the principle as a necessary truth, we again come to persistence of force, for it is by force that we really know matter.

In Chapter V it is argued that the continuity of motion is a truth of the same order, one of which we cannot imagine the contrary. When we contemplate a swinging pendulum, and note the recurring appearance and disappearance of its motion, we cannot suppose that that of which the motion is a sign has been annihilated when the motion ceases at the end of a swing. We must suppose that there is a *continuous existence* now shown by the motion, and now by the pull down which we feel if we hold the pendulum in its highest position. This existence we think of as the objective correlative of muscular effort; we think of it in terms of force. Again, if a moving body is gradually brought to rest, it is stopped by force exerted by some other body or bodies upon it, and this retarding force has a reaction on the acting bodies, handing on to them the motion lost by the body as it slackens speed. Again we think of the motion being communicated by force. If we seek to prove the continuity of motion, our so-called proofs really assume the persistence of force in some form or another, either in the constancy of the masses concerned, or in the constancy of the measuring instruments used. Hence we again come to the same foundation as that on which the indestructibility of matter is built, viz.:—the persistence of force.

Having concluded that the indestructibility of matter and the continuity of motion ultimately imply the persistence of force, Spencer proceeds, in Chapter VI, to examine the warrant we have for the truth of this last doctrine. He asserts that all our measures assume it, and that therefore we cannot by experiment prove it. We cannot show that it rests on any other truth. All reasoned out conclusions must rest on some postulate. We go on merging derivative truths in wider and wider truths, until at last we reach a widest truth which can be merged in no other or derived from no other. And whoever contemplates the relation in which it stands to the truths of science in general will (he says) see that this truth transcending demonstration is the persistence of force.

It is remarkable that so calmly and closely reasoned an argument should have excited so much heat as this has done in certain physicists: all the more remarkable in that physicists have not, for the most part, closely examined the foundations of their great generalisation for themselves, have not clearly realised what is the result of experience and what is metaphysical assumption.

Perhaps it is from this very neglect of the subject that some of their bitterness towards Spencer has arisen. It is not pleasant to have a stranger coming in to set one's house in order. But there is, I think, another reason. The physicists have by toilsome steps been pushing into a hitherto unknown country; they have been drawing careful maps describing the details of its features as they came to them, and now after putting together the results obtained by generations of explorers, they have found the course, as it were, of the great mountain ranges and rivers. But Mr. Spencer seems to say that after all they need not have toiled so much. From the border of the country the general lie of the strata might have been made out, and it might have been seen that the mountains and rivers could not run otherwise than they do. And indeed all their survey depended on a base-line at the border; all the boasted measures were but in terms of that base-line, so that all their maps were but repetitions of it. It is always irritating to be told that if you had only kept your eyes open you might have saved your pains. An implication of unwisdom is always the direst insult.

But after all, Mr. Spencer may not be right. For my own part, I share the view of the physicists, that his arguments are to a great extent unsound. I hold that the field of science cannot be mapped with certainty from its borders, and that our knowledge of its main features is due solely to the explorers. Further, in that these explorers are fallible, I hold it possible that their maps may be wrong, at least to some extent, and that future generations may show that we have been too hasty in assuming that we knew even the position of the main features.

Were I to criticise Mr. Spencer's statements point by point, there would be danger that we should be confused by differences about mere details. I propose therefore to state my own beliefs in these matters, and to give as far as I can what I consider the warrant for them.

The main work of the physicist is the investigation of the resemblances or similarities which he observes in phenomena. The description of these resemblances he embodies in physical laws. For instance, he observes that bodies resemble each other in falling to the ground when no other body intervenes; that they resemble each other in remaining at rest unless there is some other body to whose presence their motion can be ascribed; that they resemble each other in that they require an effort from him to set them moving, an effort which he feels through his muscular sense; and so on. These are mere qualitative resemblances which can be discovered by simple observation, and every intelligent being has through his own observation, through his early instruction and possibly through the observation of his ancestors, become aware of a number of such resemblances or physical laws which he regards as mere common sense. In fact, in this respect, we are all like Molière's M. Jourdain—we have been speaking physics these forty years and never knew it. But the physicist, of course, goes far beyond this classification of simple observations. He makes experiments as well as observations. He calls in the aid of instruments and makes measurements; he discovers that phenomena resemble each other in various ways which can be expressed by numerical relations. Let us take an example.

An experimenter puts a piece of rock-salt and a vessel of water side by side on the one pan of a balance, and counterpoises them by weights on the other pan. He now powders the salt and finds the weight is still the same; putting the salt into the water and stirring till it is all dissolved, the balance is unchanged. Finally, distilling the liquid and collecting the water and the salt which remain behind, he has them separate, and placing them on the balance, they are counterpoised by the same weight as before. This experiment may serve as a type of all the various weighing experiments, chemical and physical, which are taken as proving the indestructibility of matter.

What conclusions does our experimenter draw? Firstly that the salt was in existence throughout the experiment, and secondly that its weight remained the same. But in drawing this conclusion he makes assumptions. He believes that the salt appearing after distillation is the identical salt which disappeared in the water. He could follow it for a time. He saw it change its condition from a lump to a powder. But when it went into the water it ceased to affect his sense of sight. Yet the fact that salt could be obtained from the water again leads him to think that it was in existence all the time. And he ascribes to the salt in its invisible state the change in weight of the water and change in its taste. His belief in the continuity of existence of the salt, in its identity, rests on a postulate which for shortness we may term the continuity or identity postulate. Let us, for the sake of clearness, consider another example of the use of the same postulate.

Suppose that I am with a man whom I know, in a room with a door leading into another empty room, and suppose that shortly before my friend has gone into the other room out of my sight, and has now returned again. I do not suppose that he went out of existence in his absence; I believe that he was in the other room, preserving meanwhile his identity. I may have spoken to him while he was out of sight, and may have received an answer, and this affection of another sense than sight I ascribe to him. I base my belief in his continuity on the same postulate as that on which the experimenter bases his belief in the continuity of the salt. I have not a sufficient knowledge of philosophy to put the postulate in its proper form, but a consideration of cases in which it would not or might not apply may at least give us a working form of statement. If, during the weighing experiment, somebody had been observed introducing fresh salt on to the balance, we could no longer assert identity of the initial and final salt. Or if, in the second example, my friend had a twin brother in the neighbourhood, and if the adjoining room communicated with the street, I might not be sure of the identity of the friend underlying the two appearances. Perhaps, then, we may guard against such cases by the statement that 'if a thing affects us in the same way as a thing has previously affected us, and if we have reason to suppose that no fresh thing has come in from the outside, then the two affections arise from the same thing.'

Secondly, with our experimenter, we assume that the weights used in the counterpoise preserve a constant weight. Mr. Spencer seems to think that this assumption is ultimate or fundamental. But let us examine the assumption a little more closely. To begin with, we assume continuity of existence of weight. We have only direct sense-warrant for the existence of the down-pull while putting the weights on the pan with our hands and while taking them off again. But we apply the continuity postulate and assert that the weight existed while the masses were on the pan. But we go further: we assert constancy in quantity, which is something more than mere continuity of existence, and we have various methods of testing our assertion. We may allow the weights to fall, and time their fall through a given distance in successive trials. All experience tends to show that the time of fall is constant, and we conclude that the weight is constant. It may be argued that we use a clock for the time, and that the clock pendulum may possibly vary in weight, simultaneously and in like proportion with the balance weights. Very well, then; let us use a watch, and we get still the same time of fall in our successive trials. Or let us use a different test, and put the weights on a Salter's Spring balance, and they always stretch the spring equally. If it be argued that possibly the elasticity of the watch-spring and of the balance-spring varies in like proportion with the weight of the weights, then, I say, let us go to the ultimate court of appeal—my own sensations. If I have practised much with my pressure-sense and my muscular sense, I may weigh the weights with my hand and be certain of their approximate constancy. If it be finally argued that my sensations may likewise vary in proportion, then I say that so long as the universe is drawn to a consistent scale, and so long as I am also on that scale, any contraction or expansion of the scale, being beyond my detection, is a matter of perfect indifference to me, and I need not construct my language so as to provide for its possibility. I am content to say that the weight of the salt and the water is constant. But here I think that another postulate has crept in, which in its most general form we may state thus: 'Like sensations imply like objective existences or like physical properties.' We use the particular case that equal sensations imply equal objective existences or equal physical properties. For whatever test of constancy of weight we employ depends ultimately on the equality of two sensations. And indeed this postulate is the basis of all the conclusions as to the outside world which we draw from physical measurements.

Again, though we take weight as our test, the fact that the salt at the end resembles the salt with which we started in other respects than weight—in fact, that it gives us equal sensations leads us to conclude that it is equal in quantity, i.e., that none of it has been destroyed. And on such experiments so interpreted we found the principle of the Indestructibility of Matter.

Passing on from this, let us consider another experiment: that of the swinging pendulum described by Mr. Spencer. And to begin with, let us suppose that I set it swinging with a blow. It starts off in rapid motion, but as it rises up the motion gets less and less and ultimately ceases; only however for a moment. Back it comes on the return journey, and, when once more at the starting point, it is moving as nearly as I can judge at the original rate. Again it rises up, now on the other side, and with speed slackening till it stops; again it returns and so on, the oscillation continuing though the motion is intermittent. As Spencer points out, the motion of the pendulum is the objective correlate of our sense of muscular effort experienced in starting it, not, however, mere effort like the effort of holding a weight in a given position, but of muscular effort combined with motion, for we pushed the hand along in giving the pendulum the blow. It is unfortunate that we have no single sensation which we naturally correlate with the combination of effort and motion, but we all have the idea fixed firmly enough in our minds as work, and this is shown by the common use of the term. To take a familiar example. If bricks have to be carried up a scaffold, the work done is naturally measured by the weight of bricks multiplied by the height of the scaffold; or we think of this product, force $\times$ distance, as describable by the single term Work. Hence we say the motion of the pendulum is the objective correlate of the work done by us. Now as the motion disappears, does it go out of existence? and as it begins again, does it start afresh? Our continuity or identity postulate is ready at our elbows to suggest identity of the motion in succeeding oscillations, and we have a confirmation of the suggestion that it still exists, even when it disappears as motion, in the fact that if at the top of the swing we lay hold of the pendulum it pulls at our hands; it is ready in fact to give back to us work such as we gave to it. We conclude then that there is a continuity of existence, at one time showing itself as motion, and at another manifested only in the pull exerted by the pendulum on the hand. That to which we assign continuity we term energy—kinetic when it shows itself as motion, potential when it is only inferred to exist from the position of the body and the knowledge of the work it will do. We may use as a symbol, to enable us to think of this potential energy, the energy of a stretched india-rubber cord. If a boy projects a ball attached to such a cord, the ball gradually loses its motion but the cord stretches, and in this state of stretch we suppose it to possess the energy previously in the ball. If we think of some invisible connecting machinery between the earth and the pendulum, we may conceive this machinery as stretched when the pendulum rests at its highest point, and as in that state possessing the energy lost as energy of motion by the pendulum.

So far I have closely followed Spencer's masterly analysis of this example, here and there replacing his terms by those more commonly used by physicists, but in his succeeding statements I can no longer go with him. Let us examine one or two of these statements. He argues that the sense of muscular effort is the subjective symbol both for force and for energy, though he recognises that in the latter case the feeling of effort is joined with consciousness of motion. It is true that when we exert mere muscular effort without moving our limbs, we do work and so lose energy and even become tired, but that is due to the particular mechanism employed. If we study the separate muscular fibres instead of the whole limb, we find that they are moving even when we are exerting only a dead pull or push without motion. And so our sense of effort probably accompanies a supply of energy to the muscles, and our feeling of fatigue probably accompanies a loss or absence of energy. The combination of effort with motion uses up a great deal more energy and leads more rapidly to fatigue, but the fatigue is of the same kind in both cases. In the objective world, however, force and energy are entirely distinct. We speak of the steam-pressure in the boiler without confusing it with the horse-power of the engine, one being force per square inch, the other energy per minute. We speak of the weight of a consignment of goods, and we admit the justice of the mileage rate of charge for its carriage by rail, one being force, and the other a charge proportional to energy expended in the carriage.

The physicists, through painful experience, aware of the extreme importance of keeping these two ideas of force and energy distinct, or rather of recognising that the one contains something over and above that which the other does, are repelled by Mr. Spencer's attempt to reduce both to force. They recognise that our muscular sense is misleading inasmuch as it gives us a consciousness of loss of energy when we exert force alone, and only a consciousness of greater loss joined with an inadequate consciousness of motion when we do external work. They say that if effort be correlated with force it is a mistake to correlate it also with energy, and that if we do naturally so correlate it the correlation can only lead us astray.

Another statement which it is difficult to accept, is to the effect that the existence which we have termed energy, must show itself either as motion or as strain—i.e., either as kinetic energy or potential energy. A system in which after any interval the kinetic energy comes back when the bodies are again in their original position is termed a conservative system, and it is of such a system alone that it is strictly accurate to say that the sum of the potential and kinetic energies remains the same. When and only when we have such a system are the forces persistent, i.e., dependent only on the distances of the bodies apart. It is *supposed* (not, as Spencer says, *assumed*) that astronomy furnishes us with a grand example of a conservative system, inasmuch as our proofs of the indestructibility of matter lead us to suppose that the planets have constant masses, and our measures of their distances and motions show that when the distances repeat themselves the velocities recur. The masses being constant the energy must have all returned. But even in this case it is suspected that the forces are not quite persistent, though we have no certain proof of the fact*.

Terrestrial motions are all affected by friction, a sworn enemy to conservation, since by opposing the motions it always ends them without putting any potential energy in their place. Careful examination of cases of friction shows, however, that there is still a sign of the continuity of existence of that which for a time appeared as kinetic energy, and then on vanishing, led us to believe that it still existed as potential or strain energy. This new sign is heat—something affecting a new sense. Further study shows other signs —as light affecting the sense of sight and chemical energy, sometimes perhaps affecting the sense of taste. Then in some cases the phenomena of magnetism and electricity are developed, phenomena which lead us to believe that there is latent energy ready to turn into heat, or kinetic or chemical energy in the electric circuit, latent since we have no electric or magnetic senses to detect it.

All these results lead us to believe in the truth of the principle of the continuity or identity of energy, a principle evidently founded on the identity postulate, since what we observe is that energy passes from one form and that simultaneously energy appears in another form, and that when it passes from this latter form we can obtain energy again in the original form. But this continuity does not necessarily imply constancy in quantity. That is another principle founded on experiment. Determinations like those of Joule tend to show that when energy changes from one form to another there is a fixed rate of exchange. If then, using the known rates of exchange, we suppose all energy converted intó one form, experiment leads us to suppose that the sum total is constant.

We can now see in what sense it is true that energy must show itself either as kinetic or as strain. It is only true if we assume that light, heat, and the rest are either kinetic or strain energies or mixtures of the two.

This brings me to the consideration of another part of the work of the physicist.

* I may here point out an error into which Spencer appears to have fallen, confounding the equality of action and reaction with persistence of force. One is a relation true at any instant, the other a relation true in successive instants.

His main work, as I have said, is the determination of resemblances or similarities, and he groups phenomena according to these. In the course of scientific work many of these groups are shown to resemble each other—one set of phenomena is shown to be a mere combination of phenomena already known, and the phenomena are then said to be explained. Thus Wells showed that in the deposition of dew there is a cooling of the earth's surface, cooling therefore of the moisture-laden atmosphere in contact with it, and deposition of some of the moisture. In other words, he showed that the deposition of dew resembled other depositions of water, and so he explained it. Or again, Faraday explained the formation of electricity by the jet of steam in the hydro-electric machine when he showed that there was friction between the drops of water carried by the steam jet, and the sides of the orifice past which they rushed, that the two were oppositely electrified, and that it was therefore similar to other known cases of electrification by friction. And numberless other instances might be given.

But the physicist is not content with explanations which he can prove. He is an inveterate builder-up of hypotheses for the most part unverifiable, but that hardly troubles him. His hypotheses are always attempts to imagine such a condition of affairs that he may continue the work of explanation, i.e., of detection of hidden similarities. For instance, a solid body is, to our senses, a continuous something entirely filling up a space. If it is heated it expands; if it is soluble in water, it disappears when put in water. If we make no hypotheses, we can go no further. The expansion of a continuous solid is unlike anything else, and is therefore inexplicable; but I hold and here I think Mr. Spencer would consider me quite hopeless—that there is no difficulty whatever in conceiving of the expansion of continuous matter. Again, the disappearance of the continuous salt in continuous water is inexplicable, but I have no sense-warrant that it is not going on, and I may be driven to attempt to conceive it. But now let me introduce the unverifiable, or, at least, unverified, hypothesis that matter is discontinuous, and really consists of separated particles, and I can explain expansion: it resembles the scattering of a crowd. I can explain solution: it resembles the mixing of two crowds, and so on.

Again, we have recognised various forms of energy—kinetic, affecting the sight in one way, or light affecting it in another way, or heat affecting the temperature-sense, but we cannot say that any one of these resembles any other. Without hypothesis they are inexplicable. But, let me suppose that the ultimate particles of matter possess both strain and kinetic energy, and that, when they bump against my skin, they affect my temperature-sense, and I explain heat. I show that a hot body resembles known mechanical systems. Or, let me suppose that even where I cannot see or feel matter there is still something which can be acted on by the ultimate particles of matter and receive energy from them, and I can explain light as being waves sent out in this intangible something by the vibrating atoms. I show that it resembles other cases of waves sent out from vibrating sources in water or in air.

No doubt this longing for explanation which possesses us is in part strengthened by our belief in identity. If energy is continuous in its existence, then we suppose that in itself it must be the same in kind, though now it affects one sense, now another, and now none at all. We go on from this another step and suppose that if we could only train our senses sufficiently we should be able to follow the energy through all its transmigrations, and see it ever the same in kind. The senses used in the investigation of visible motion, the muscular sense, the touch, the eye, are the most thoroughly trained, and work best together. We, therefore, naturally fix on these as the senses which are, in imagination, to follow the energy up, and so our hypotheses are constructed to enable us to explain all phenomena as cases of mechanical action and mechanical motion—to explain all the forms of energy as kinetic and potential.

As yet, our hypotheses are unverified, and, for the most part, they appear likely to remain so, for it is difficult to conceive of any test of their truth. And until they are verified we must ever bear in mind that new hypotheses may at any time be devised, which may explain phenomena even better than the old ones. So, it behoves us to be cautious in committing ourselves to doctrines as to the indestructibility of matter or the continuity of motion, which are based on hypotheses as to the structure of matter and the nature of energy. We need have no fear that without these doctrines science would be impossible. If matter is destructible and motion ceases, there is only the more work for the physicist to do in determining the conditions of annihilation. He can still find resemblances, can still explain the complex unknown as made up of the simpler known. And when his senses fail to guide him, he can still invent hypotheses whereby his imagination may come to their aid. His science will only stop when he comes to the ultimate ideas, the inexplicables, in terms of which all phenomena are to be described inexplicables, in that they can be no further resolved, in that they are utterly unlike each other but not unknowable, for we know them one from the other, and we know them as the threads with which is woven all that we have yet discovered of the pattern of the universe.

But this is not an exposition of Mr. Spencer's chapters. I seem to have travelled so far on a diverging path, that I have almost lost sight of the goal to which he would lead. Let me attempt, in conclusion, to state in a few words where I think we diverged.

While Mr. Spencer holds that common experience of matter and motion, if rightly interpreted, leads to the belief in the indestructibility of the one and the continuity of the other, I hold that common experience only raises a presumption, the belief is only rightly and firmly founded on the results of careful and exact quantitative experiments. While he holds that they are necessary truths, I still think it conceivable that they are false. While he regards them both as leading to the persistence of force as the ultimate postulate, I very much doubt whether any relation between definite ideas is a postulate. The postulates which I have used are both of them conditional propositions. If so and so, then so and so. In fact, I suspect that the mind is provided only with machinery ready to arrange the results put into it by the senses, and that it does not contain any results ready made.

52.

PRESIDENTIAL ADDRESS TO THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION (DOVER), 1899.

[*British Association Report*, 1899, pp. 615–624.]

The members of this Section will, I am sure, desire me to give expression to the gratification that we all feel in the realisation of the scheme first proposed from this chair by Dr. Lodge, the scheme for the establishment of a National Physical Laboratory. It would be useless here to attempt to point out the importance of the step taken in the definite foundation of the Laboratory, for we all recognise that it was absolutely necessary for the due progress of physical research in this country. It is matter for congratulation that the initial guidance of the work of the Laboratory has been placed in such able hands.

While the investigation of Nature is ever increasing our knowledge, and while each new discovery is a positive addition never again to be lost, the range of the investigation and the nature of the knowledge gained form the theme of endless discussion. And in this discussion, so different are the views of different schools of thought, that it might appear hopeless to look for general agreement, or to attempt to mark progress.

Nevertheless, I believe that in some directions there has been real progress, and that physicists, at least, are tending towards a general agreement as to the nature of the laws in which they embody their discoveries, of the explanations which they seek to give, and of the hypotheses they make in their search for explanations.

I propose to ask you to consider the terms of this agreement, and the form in which, as it appears to me, they should be drawn up.

The range of the physicist's study consists in the visible motions and other sensible changes of matter. The experiences with which he deals are the impressions on his senses, and his aim is to describe in the shortest possible way how his various senses have been, will be, or would be affected.

His method consists in finding out all likenesses, in classing together all similar events, and so giving an account as concise as possible of the motions and changes observed. His success in the search for likenesses and his striving after conciseness of description lead him to imagine such a constitution of things that likenesses exist even where they elude his observation, and he is thus enabled to simplify his classification on the assumption that the constitution thus imagined is a reality. He is enabled to predict on the assumption that the likenesses of the future will be the likenesses of the past.

His account of Nature, then, is, as it is often termed, a descriptive account.

Were there no similarities in events, our account of them could not rise above a mere directory, with each individual event entered up separately with its address. But the similarities observed enable us to class large numbers of events together, to give general descriptions, and indeed to make, instead of a directory, a readable book of science, with laws as the headings of the chapters.

These laws are, I believe, in all cases brief descriptions of observed similarities. By way of illustration let us take two or three examples.

The law of gravitation states that to each portion of matter we can assign a constant—its mass—such that there is an acceleration towards it of other matter proportional to that mass divided by the square of its distance away. Or all bodies resemble each other in having this acceleration towards each other.

Hooke's law for the case of a stretched wire states that each successive equal small load produces an equal stretch, or states that the behaviour of the wire is similar for all equal small pulls.

Joule's law for the heat appearing when a current flows in a wire states that the rate of development of heat is proportional to the square of the current multiplied by the resistance, or states that all the different cases resemble each other in having $H \div C^2Rt$ constant.

And, generally, when a law is expressed by an equation, that equation is a statement that two different sets of measurements are made, represented by the terms on the two sides of the equation, and that all the different cases resemble each other in that the two sets have the constant relation expressed by the equation. Accurate prediction is based on the assumption that when we have made the measurements on the one side of the equation we can tell the result of the measurements implied on the other side.

If this is a true account of the nature of physical laws, they have, we must confess, greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the universe. Now we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed.

The old conception of laws as self-sufficing governors of Nature was, no doubt, a survival of a much older conception of the scope of physical science, a mode of regarding physical phenomena which had itself passed away.

I imagine that originally man looked on himself and the result of his action in the motions and changes which he produced in matter, as the one type in terms of which he should seek to describe all motions and changes. Knowing that his purpose and will were followed by motions and changes in the matter about him, he thought of similar purpose and will behind all the motions and changes which he observed, however they occurred; and he believed, too, that it was necessary to think thus in giving any consistent account of his observations. Taking this anthropomorphic—or, shall we say, psychical—view, the laws he formulated were not merely descriptions of similarities of behaviour, but they were also expressions of fixed purpose and the resulting constancy of action. They were commands given to matter which it must obey.

The psychical method, the introduction of purpose and will, is still appropriate when we are concerned with living beings. Indeed, it is the only method which we attempt to follow when we are describing the motions of our fellow-creatures. No one seeks to describe the motions and actions of himself and of his fellow-men, and to classify them without any reference to the similarity of purpose when the actions are similar. But as the study of Nature progressed, it was found to be quite futile to bring in the ideas of purpose and will when merely describing and classifying the motions and changes of non-living matter. Purpose and will could be entirely left out of sight, and yet the observed motions and changes could be described, and predictions could be made as to future motions and changes. Limiting the aim of physical science to such description and prediction, it gradually became clear that the method was adequate for the purpose, and over the range of non-living matter, at least, the psychical yielded to the physical. Laws ceased to be commands analogous to legal enactments, and became mere descriptions. But during the passage from one position to the other, by a confusion of thought which may appear strange to us now that we have finished the journey, though no doubt it was inevitable, the purpose and will of which the laws have been the expression were put into the laws themselves; they were personified and made to will and act.

Even now these early stages in the history of thought can be traced by survivals in our language, survivals due to the ascription of moral qualities to matter. Thus gases are still sometimes said to obey or to disobey Boyle's Law as if it were an enactment for their guidance, and as if it set forth an ideal, the perfect gas, for their imitation. We still hear language which seems to imply that real gases are wanting in perfection, in that they fail to observe the exact letter of the law. I suppose on this view we should have to say that hydrogen is nearest to perfection; but then we should have to regard it as righteous over-much, a sort of Pharisee among gases which overshoots the mark in its endeavour to obey the law. Oxygen and nitrogen we may regard as good enough in the affairs of everyday life. But carbon dioxide and chlorine and the like are poor sinners which yield to temptation and liquefy whenever circumstances press at all hardly on them.

There is a similar ascription of moral qualities when we judge bodies according to their fulfilment of the purpose for which we use them, when we describe them as good or bad radiators, good or bad insulators, as if it were a duty on their part to radiate well, or insulate well, and as if there were failures on the part of Nature to come up to the proper standard.

These are of course mere trivialities, but the reaction of language on thought is so subtle and far-reaching that, risking the accusation of pedantry, I would urge the abolition of all such picturesque terms. In our quantitative estimates let us be content with 'high' or 'low,' 'great' or 'small,' and let us remember that there is no such thing as a failure to obey a physical law. A broken law is merely a false description.

Concurrently with the change in our conception of physical law has come a change in our conception of physical explanation. We have not to go very far back to find such a statement as this—that we have explained anything when we know the cause of it, or when we have found out the reason why—a statement which is only appropriate on the psychical view. Without entering into any discussion of the meaning of cause, we can at least assert that that meaning will only have true content when it is concerned with purpose and will. On the purely physical or descriptive view, the idea of cause is quite out of place. In description we are solely concerned with the 'how' of things, and their 'why' we purposely leave out of account. We explain an event not when we know 'why' it happened, but when we show 'how' it is like something else happening elsewhere or otherwhen—when, in fact, we can include it as a case described by some law already set forth. In explanation, we do not account *for* the event, but we improve our account *of* it by likening it to what we already knew.

For instance, Newton explained the falling of a stone when he showed that its acceleration towards the earth was similar to and could be expressed by the same law as the acceleration of the moon towards the earth.

He explained the air disturbance we call 'sound' when he showed that the motions and forces in the pressure-waves were like motions and forces already studied.

Franklin explained lightning when and so far as he showed that it was similar in its behaviour to other electric discharges.

Here I do not fear any accusation of pedantry in joining those who urge that we should adapt our language to the modern view. It would be a very real gain, a great assistance to clear thinking, if we could entirely abolish the word 'cause' in physical description, cease to say 'why' things happen unless we wish to signify an antecedent purpose, and be content to own that our laws are but expressions of 'how' they occur.

The aim of explanation, then, is to reduce the number of laws as far as possible, by showing that laws, at first separated, may be merged in one; to reduce the number of chapters in the book of science by showing that some are truly mere sub-sections of chapters already written.

To take an old but never-worn-out metaphor, the physicist is examining the garment of Nature, learning of how many, or rather of how few different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come.

How many different kinds of thread does Nature use?

So far, we have recognised some eight or nine, the number of different forms of energy which we are still obliged to count as distinct. But this distinction we cannot believe to be real. The relations between the different forms of energy, and the fixed rate of exchange when one form gives place to another, encourage us to suppose that if we could only sharpen our senses, or change our point of view, we could effect a still further reduction. We stand in front of Nature's loom as we watch the weaving of the garment; while we follow a particular thread in the pattern it suddenly disappears, and a thread of another colour takes its place. Is this a new thread, or is it merely the old thread turned round and presenting a new face to us? We can do little more than guess. We cannot get to the other side of the pattern, and our minutest watching will not tell us all the working of the loom.

Leaving the metaphor, were we true physicists, and physicists alone, we should, I suppose, be content to describe merely what we observe in the changes of energy. We should say, for instance, that so much kinetic energy ceases, and that so much heat appears, or that so much light comes to a surface, and that so much chemical energy takes its place. But we have to take ourselves as we are, and reckon with the fact that though our material is physical, we ourselves are psychical. And, as a mere matter of fact, we are not content with such discontinuous descriptions. We dislike the discontinuity and we think of an underlying identity. We think of the heat as being that which a moment before was energy of visible motion, we think of the light as changing its form alone and becoming itself the chemical energy. Then to our passive dislike to discontinuity we join our active desire to form a mental picture of what may be going on, a picture like something which we already know. Coming on these discontinuities our ordinary method of explanation fails, for they are not obviously like those series of events in which we can trace every step. We then imagine a constitution of matter and modifications of it corresponding to the different kinds of energy, such that the discontinuities vanish, and such that we can picture one form of energy passing into another and yet keeping the same in kind throughout. We are no longer content to describe what we actually see or feel, but we describe what we imagine we should see or feel if our senses were on quite another scale of magnitude and sensibility. We cease to be physicists of the real and become physicists of the ideal.

To form such mental pictures we naturally choose the sense which makes such pictures most definite, the sense of sight, and think of a constitution of matter which shall enable us to explain all the various changes in terms of visible motions and accelerations. We imagine a mechanical constitution of the universe.

We are encouraged in this attempt by the fact that the relations in this mechanical conception can be so exactly stated, that the equations of motion are so very definite. We have, too, examples of mechanical systems, of which we can give accounts far exceeding in accuracy the accounts of other physical systems. Compare, for instance, the accuracy with which we can describe and foretell the path of a planet with our ignorance of the movements of the atmosphere as dependent on the heat of the sun. The planet keeps to the astronomer's time-table, but the wind still bloweth almost where it listeth.

The only foundation which has yet been imagined for this mechanical explanation if we may use 'explanation' to denote the likening of our imaginings to that which we actually observe—is the atomic and molecular hypothesis of matter. This hypothesis arose so early in the history of science that we are almost tempted to suppose that it is a necessity of thought, and that it has a warrant of some higher order than any other hypothesis which could be imagined. But I suspect that if we could trace its early development we should find that it arose in an attempt to explain the phenomena of expansion and contraction, evaporation and solution. Were matter a continuum we should have to admit all these as simple facts, inexplicable in that they are like nothing else. But imagine matter to consist of a crowd of separate particles with interspaces. Contraction and expansion are then merely a drawing in and a widening out of the crowd. Solution is merely the mingling of two crowds, and evaporation merely a dispersal from the outskirts. The most evident properties of matter are then similar to what may be observed in any public meeting.

For ages the molecular hypothesis hardly went further than this. The first step onward was the ascription of vibratory motion to the atoms to explain heat. Then definite qualities were ascribed, definite mutual forces were called into play to explain elasticity and other properties or qualities of matter. But I imagine its first really great achievement was its success in explaining the law of combining proportions, and next to that we should put its success in explaining many of the properties of gases.

While light was regarded as corpuscular—in fact molecular, and while direct action at a distance presented no difficulty, the molecular hypothesis served as the one foundation for the mechanical representation of phenomena. But when it was shown that infinitely the best account of the phenomena of light could be given on the supposition that it consisted of waves, something was needed, as Lord Salisbury has said, to wave, both in the interstellar and in the intermolecular spaces. So the hypothesis of an ether was developed, a necessary complement of that form of the molecular hypothesis in which matter consists of discrete particles with matter-free intervening spaces.

Then Faraday's discovery of the influence of the dielectric medium in electric actions led to the general abandonment of the idea of action at a distance, and the ether was called in to aid matter in the explanation of electric and magnetic phenomena. The discovery that the velocity of electromagnetic waves is the same as that of light-waves is at least circumstantial evidence that the same medium transmits both.

I suppose we all hope that some time we shall succeed in attributing to this medium such further qualities that it will be able to enlarge its scope and take in the work of gravitation.

The mechanical hypothesis has not always taken this dualistic form of material atoms and molecules, floating in a quite distinct ether. I think we may regard Boscovich's theory of point-centres surrounded by infinitely extending atmospheres of force as really an attempt to get rid of the dualism, and Faraday's theory of point-centres with radiating lines of force is only Boscovich's theory in another form. But Lord Kelvin's vortex-atom theory gives us a simplification more easily thought of. Here all space is filled with continuous fluid—shall we say a fluid ether—and the atoms are mere loci of a particular type of motion of this frictionless fluid. The sole differences in the atoms are differences of position and motion. Where there are whirls, we call the fluid matter; where there are no whirls, we call it ether. All energy is energy of motion. Our visible kinetic energy, $MV^2/2$, is energy in and round the central whirls; our visible energy of position, our potential energy, is energy of motion in the outlying regions.

A similar simplification is given by Dr. Larmor's hypothesis, in which, again, all space is filled with continuous substance all of one kind, but this time solid rather than fluid. The atoms are loci of strain instead of whirls, and the ether is that which is strained.

So, as we watch the weaving of the garment of Nature, we resolve it in imagination into threads of ether spangled over with beads of matter. We look still closer, and the beads of matter vanish; they are mere knots and loops in the threads of ether.

The question now faces us—How are we to regard these hypotheses as to the constitution of matter and the connecting ether? How are we to look upon the explanations they afford? Are we to put atoms and ether on an equal footing with the phenomena observed by our senses, as truths to be investigated for their own sake? Or are they mere tools in the search for truth, liable to be worn out or superseded?

That matter is grained in structure is hardly more than the expression of the fact that in very thin layers it ceases to behave as in thicker layers. But when we pass on from this general statement and give definite form to the granules or assume definite qualities to the intergranular cement we are dealing with pure hypotheses.

It is hardly possible to think that we shall ever see an atom or handle the ether. We make no attempt whatever to render them evident to the senses. We connect observed conditions and changes in gross visible matter by invisible molecular and ethereal machinery. The changes at each end of the machinery of which we seek to give an account are in gross matter, and this gross matter is our only instrument of detection, and we never receive direct sense impressions of the imagined atoms or the intervening ether. To a strictly descriptive physicist their only use and interest would lie in their service in prediction of the changes which are to take place in gross matter.

It appears quite possible that various types of machinery might be devised to produce the known effects. The type we have adopted is undergoing constant minor changes, as new discoveries suggest new arrangements of the parts. Is it utterly beyond possibility that the type itself should change?

The special molecular and ethereal machinery which we have designed, and which we now generally use, has been designed because our most highly developed sense is our sense of sight. Were we otherwise, had we a sense more delicate than sight, one affording us material for more definite mental presentation, we might quite possibly have constructed very different hypotheses. Though, as we are, we cannot conceive any higher type than that founded on the sense of sight, we can imagine a lower type, and by way of illustration of the point let us take the sense of which my predecessor spoke last year—the sense of smell. In us it is very undeveloped. But let us imagine a being in whom it is highly cultivated, say, a very intellectual and very hypothetical dog. Let us suppose that he tries to frame a hypothesis as to light. Having found that his sense of smell is excited by surface exhalations, will he not naturally make and be content with a corpuscular theory of light? When he has discovered the facts of dispersion, will he not think of the different colours as different kinds of smell—insensible, perhaps, to him, but sensible to a still more highly gifted, still more hypothetical dog?

Of course, with our superior intellect and sensibility, we can see where his hypothesis would break down; but unless we are to assume that we have reached finality in sense-development, the illustration, grotesque as it may be, will serve to show that our hypotheses are in terms of ourselves rather than in terms of Nature itself, they are ejective rather than objective, and so they are to be regarded as instruments, tools, apparatus only to aid us in the search for truth.

To use an old analogy—and here we can hardly go except upon analogy while the building of Nature is growing spontaneously from within, the model of it, which we seek to construct in our descriptive science, can only be constructed by means of scaffolding from without, a scaffolding of hypotheses. While in the real building all is continuous, in our model there are detached parts which must be connected with the rest by temporary ladders and passages, or which must be supported till we can see how to fill in the understructure. To give the hypotheses equal validity with facts is to confuse the temporary scaffolding with the building itself.

But even if we take this view of the temporary nature of our molecular and ethereal imaginings, it does not lessen their value, their necessity to us.

It is merely a true description of ourselves to say that we must believe in the continuity of physical processes, and that we must attempt to form mental pictures of those processes the details of which elude our observation. For such pictures we must frame hypotheses, and we have to use the best material at command in framing them. At present there is only one fundamental hypothesis—the molecular and ethereal hypothesis in some such form as is generally accepted.

Even if we take the position that the form of the hypothesis may change as our knowledge extends, that we may be able to devise new machinery—nay, even that we may be able to design some quite new type to bring about the same ends—that does not appear to me to lessen the present value of the hypothesis. We can recognise to the full how well it enables us to group together large masses of facts which, without it, would be scattered apart, how it serves to give working explanations, and continually enables investigators to think out new questions for research. We can recognise that it is the symbolical form in which much actual knowledge is cast. We might almost as well quarrel with the use of the letters of the alphabet, inasmuch as they are not the sounds themselves, but mere arbitrary symbols of the sounds.

In this country there is no need for any defence of the use of the molecular hypothesis. But abroad the movement from the position in which hypothesis is confounded with observed truth has carried many through the position of equilibrium equally far on the other side, and a party has been formed which totally abstains from molecules as a protest against immoderate indulgence in their use. Time will show whether these protesters can do without any hypothesis, whether they can build without scaffolding or ladders. I fear that it is only an attempt to build from balloons.

But the protest will have value if it will put us on our guard against using molecules and the ether everywhere and everywhen. There is, I think, some danger that we may get so accustomed to picturing everything in terms of these hypotheses that we may come to suppose that we have no firm basis for the facts of observation until we have given a molecular account of them, that a molecular basis is a firmer foundation than direct experience.

Let me illustrate this kind of danger. The phenomena of capillarity can, for the most part, be explained on the assumption of a liquid surface-tension. But if the subject is treated merely from this point of view it stands alone— it is a portion of the building of science hanging in the air. The molecular hypothesis then comes in to give some explanation of the surface tension, gives, as it were, a supporting understructure connecting capillarity with other classes of phenomena. But here, I think, the hypothesis should stop, and such phenomena as can be explained by the surface-tension should be so explained without reference to molecules. They should not be brought in again till the surface-tension explanation fails. It is necessary to bear in mind what part is scaffolding, and what is the building itself, already firm and complete.

Or, as another illustration, take the Second Law of Thermodynamics. I suspect that it is sometimes supposed that a molecular theory from which the Second Law could be deduced would be a better basis for it than the direct experience on which it was founded by Clausius and Kelvin, or that the mere imagining of a Maxwell's sorting demon has already disproved the universality of the law; whereas he is a mere hypothesis grafted on a hypothesis, and nothing corresponding to his action has yet been found.

There is more serious danger of confusion of hypothesis with fact in the use of the ether: more risk of failure to see what is accomplished by its aid. In giving an account of light, for instance, the right course, it appears to me, is to describe the phenomena and lay down the laws under which they are grouped, leaving it an open question what it is that waves, until the phenomena oblige us to introduce something more than matter, until we see what properties we must assign to the ether, properties not possessed by matter, in order that it may be competent to afford the explanations we seek. We should then realise more clearly that it is the constitution of matter which we have imagined, the hypothesis of discrete particles, which obliges us to assume an intervening medium to carry on the disturbance from particle to particle. But the vortex-atom hypothesis and Dr. Larmor's strain-atom hypothesis both seem to indicate that we are moving in the direction of the abolition of the distinction between matter and ether, that we shall come to regard the luminiferous medium, not as an attenuated substance here and there encumbered with detached blocks—the molecules of matter but as something which in certain places exhibits modifications which we term matter. Or starting rather from matter, we may come to think of matter as no longer consisting of separated granules, but as a continuum with properties grouped round the centres, which we regard as atoms or molecules.

Perhaps I may illustrate the danger in the use of the conception of the ether by considering the common way of describing the electromagnetic waves, which are all about us here, as ether-waves. Now in all cases with which we are acquainted, these waves start from matter; their energy before starting was, as far as we can guess, energy of the matter between the different parts of the source, and they manifest themselves in the receiver as energy of matter. As they travel through the air, I believe that it is quite possible that the electric energy can be expressed in terms of the molecules of air in their path, that they are effecting atomic separations as they go. If so, then the air is quite as much concerned in their propagation as the ether between its molecules. In any case, to term them ether-waves is to prejudge the question before we have sufficient evidence.

Unless we bear in mind the hypothetical character of our mechanical conception of things, we may run some risk of another danger the danger of supposing that we have something more real in mechanical than in other measurements. For instance, there is some risk that the work-measure of specific heat should be regarded as more fundamental than the heat-measure, in that heat is truly a 'mode of motion.' On the molecular hypothesis, heat is no doubt a mixture of kinetic energy and potential energy of the molecules and their constituents, and may even be entirely kinetic energy; and we may conceivably in the future make the hypothesis so definite that, when we heat a gramme of water 1°, we can assign such a fraction of an erg to each atom. But look how much pure hypothesis is here. The real superiority of the work-measure of specific heat lies in the fact that it is independent of any particular substance, and there is nothing whatever hypothetical about it*.

* This risk of imagining one particular kind of measure more real than another, more in accordance with the truth of things, may be further illustrated by the common idea that mass-acceleration is the only way to measure a force. We stand apart from our mechanical system and watch the motions and the accelerations of the various parts, and we find that mass-accelerations have a certain significance in our system. If we keep ourselves outside the system and only use our sense of sight, then mass-acceleration is the only way of describing that behaviour of one body in the presence of others which we term force on it. But if we go about in the system and pull and push bodies, we find that there is another conception of force, in which another sense than sight is concerned—another mode of measurement much more ancient and still far more extensively used—the measurement by weight supported. Each method has its own range; each is fundamental in that range. It is one of the great practical problems in physics to make the pendulum give us the exact ratio of the units in the two systems. J. H. P.

Another illustration of the illegitimate use of our hypothesis, as it appears to me, is in the attempt to find in the ether a fixed datum for the measurement of material velocities and accelerations, a something in which we can draw our coordinate axes so that they will never turn or bend. But this is as if, discontented with the movement of the earth's pole, we should seek to find our zero lines of latitude and longitude in the Atlantic Ocean. Leaving out of sight the possibility of ethereal currents which we cannot detect, and the motions due to every ray of light which traverses space, we could only fix positions and directions in the ether by buoying them with matter. We know nothing of the ether, except by its effects on matter, and, after all, it would be the material buoys which would fix the positions and not the ether in which they float.

The discussion of the physical method, with its descriptive laws and explanations, and its hypothetical extension of description, leads us on to the consideration of the limitation of its range. The method was developed in the study of matter which we describe as non-living, and with non-living matter the method has sufficed for the particular purposes of the physicist. Of course only a little corner of the universe has been explored, but in the study of non-living matter we have come to no impassable gulfs, no chasms across which we cannot throw bridges of hypothesis. Does the method equally suffice when it is applied to living matter? Can we give a purely physical account of such matter, likening its motions and changes to other motions and changes already observed, and so explaining them? Can we group them in laws which will enable us to predict future conditions and positions? The ancient question never answered, but never ceasing to press for an answer.

Having faith in our descriptive method, let us use it to describe our real attitude on the question. Do we, or do we not, as a matter of fact, make any attempt to apply the physical method to describe and explain those motions of matter which on the psychical view we term voluntary?

Any commonplace example, and the more commonplace the more is it to the point, will at once tell us our practice, whatever may be our theory. For instance, a steamer is going across the Channel. We can give a fairly good physical account of the motion of the steamer. We can describe how the energy stored in the coal passes out through the boiler into the machinery, and how it is ultimately absorbed by the sea. And the machinery once started, we can give an account of the actions and reactions between its various parts and the water, and if only the crew will not interfere, we can predict with some approach to correctness how the vessel will run. All these processes can be likened to processes already studied—perhaps on another scale—in our laboratories, and from the similarities prediction is possible. But now think of a passenger on board who has received an invitation to take the journey. It is simply a matter of fact that we make no attempt at a complete physical account and explanation of those actions which he takes to accomplish his purpose. We trace no lines of induction in the ether connecting him with his friends across the Channel, we seek no law of force under which he moves. In practice the strictest physicist abandons the physical view, and replaces it by the psychical. He admits the study of purpose as well as the study of motion.

He has to admit that here his physical method of prediction fails. In physical observations one set of measurements may lead to the prediction of the results of another set of measurements. The equations expressing the laws imply different observations with some definite relation between their results, and if we know one set of observations and that definite relation we can predict the result of the other set. But if we take the psychical view of actions, we can only measure the actions. We have no independent means of studying and measuring the motives which preceded the actions, we can only estimate their value by the consequent actions. If we formed equations, they would be mere identities with the same terms on either side.

The consistent and persistent physicist, finding the door closed against him, finding that he has hardly a sphere of influence left to him in the psychical region, seeks to apply his methods in another way by assuming that if he knew all about the molecular positions and motions in the living matter, then the ordinary physical laws could be applied and the physical conditions at any future time could be predicted. He would say, I suppose, with regard to the Channel passenger, that it is absurd to begin with the most complicated mechanism, and seek to give a physical account of that. He would urge that we should take some lower form of life where the structure and motions are simpler, and apply the physical methods to that.

Well, then, let us look for the physical explanation of any motion which we are entitled from its likeness to our own action to call a voluntary motion. Must we not own that even the very beginning of such explanation is as yet non-existent? It appears to me that the assumption that our methods do apply, and that purely physical explanation will suffice to predict all motions and changes, voluntary and involuntary, is at present simply a gigantic extrapolation, which we should unhesitatingly reject if it were merely a case of ordinary physical investigation. The physicist when thus extending his range is ceasing to be a physicist, ceasing to be content with his descriptive methods in his intense desire to show that he is a physicist throughout.

Of course we may describe the motions and changes of matter of any type after the event, and in a purely physical manner. And as Professor Ward has suggested, in a most important contribution to this subject which he has made in his recently published Gifford Lectures*, where ordinary physical

* *Naturalism and Agnosticism*, The Gifford Lectures, 1896–98, vol. 2, p. 71.

explanations fail to give an account of the motions, we might imagine some structure in the ether, and such stresses between the ether and matter that our physical explanations should still hold. But, as Professor Ward says, such ethereal constructions would present no warrant for their reality or consistency. Indeed they would be mere images in the surface of things to account for what goes on in front of the surface, and would have no more reality than the images of objects in a glass.

If we have full confidence in the descriptive method, as applied to living and non-living matter, it appears to me that up to the present it teaches us that, while in non-living matter we can always find similarities, that, while each event is like other events, actual or imagined, in a living being there are always dissimilarities. Taking the psychical view—the only view which we really do at present take—in the living being there is always some individuality, something different from any other living being, and full prediction in the physical sense, and by physical methods is impossible. If this be true, the loom of Nature is weaving a pattern with no mere geometrical design. The threads of life, coming in we know not where, now twining together, now dividing, are weaving patterns of their own, ever increasing in intricacy, ever gaining in beauty.

53.

A HISTORY OF THE METHODS OF WEIGHING THE EARTH.

[Presidential Address delivered to the Birmingham Philosophical Society, October 19, 1893.]

[*Birmingham Phil. Soc. Proc.* **9** (1894), pp. 1–23.]

It has been the custom, followed usually by my predecessors, to select for the subject of the annual address some general scientific or educational topic. My reason for departing from this custom is twofold. In the first place, I feel more competent to address you on a subject which has been my special study for some years, and perhaps I may have the best chance of interesting you in that in which I am most interested; and in the second place, I shall by this course economise the time of the Society. Some months ago the Council honoured me with a request that I should give the Society some account of an experiment which I had recently completed on the weighing of the earth. At that time all the ordinary meetings were fully occupied, and I could not arrange to give the account asked for. It occurred to me that I might appropriately give you, this evening, a history of the many experiments which have been made to weigh the earth, and at the same time comply with the request of the Council by giving you most detail with regard to the method with which I am best acquainted.

When we speak of 'weighing the earth,' it is obvious that we are not to interpret the expression quite literally, for the weight of any substance means primarily the pull of the earth upon it. When we weigh out a pound of tea or a ton of coal we always use *the pull of the earth* to measure the pound or the ton, finding how much brass or iron is equally pulled. The earth cannot be weighed in this sense, for it pulls itself equally in all directions, so that the net result is no pull at all.

But in general we do not want to know the weight of a body except as a step towards knowing its mass, that quality which tells us what effect force has on it, how difficult it is to set in motion.

The weight—the actual earth-pull—varies from point to point of the earth's surface. The mass is everywhere and through all time the same.

It is the mass which really interests us, and which we think of when we make purchases by weight. If we buy a pound of tea in London it is no satisfaction to know that here at home it will be pulled by the earth so that it is a quarter of a grain heavier. For all our purposes it is the same quantity of tea here as there.

So with the earth, it is the mass which we really mean when we speak of its weight. Though we cannot directly weigh it to find the mass, we can think of an imaginary experiment which would give us the mass by weighing, could we only carry it out; and perhaps the imagining of this experiment may render rather more real the idea of the earth's mass. Suppose, then, that we could bring up the earth, a cubic foot at a time, to a certain place, weigh each cubic foot as it was brought up, and then replace it, so that the earth was never diminished by more than a cubic foot at a time; the total weight of all the cubic feet would give us the mass of the earth in pounds or tons.

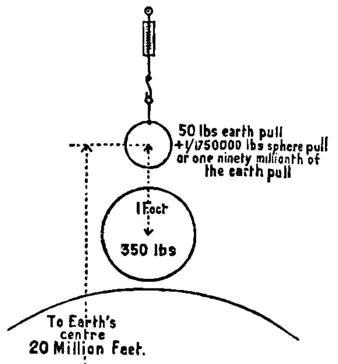
But it is sufficiently evident that no approach to such an experiment is possible, and we must look for some other way to measure the mass. The way is found in the pull, the weight, which the earth gives to bodies on its surface. That pull is, of course, a particular case of gravitation, the attraction which exists, as we know from astronomical and terrestrial observation, between every two parts of matter. And Newton proved that the two pulls exerted on the same piece of matter by two bodies are in proportion to their masses, and inversely as the squares of their distances away from it. If, then, we can hang up a weight and find the pull of the earth on it, and then bring up another body large enough and near enough to exert an appreciable pull upon the hanging weight, by comparing the two pulls and allowing for the different distances, we can find how many times greater is the mass of the earth than that of the body brought up.

To put this in a concrete form—the form it has actually taken in an experiment to be described later—suppose we have a weight of 50 lbs. hung on to a balance (Fig. 1); the earth-pull is its weight of 50 lbs., and the earth on an average is 20,000,000 feet away. Now bring up a sphere weighing about 350 lbs., and at an average distance away of one foot. The 50 lbs. is found to weigh more by about $\frac{1}{250}$ grain, i.e., the sphere-pull on it is $\frac{1}{250}$ grain, or $\frac{1}{1750000}$ lb. Hence the earth-pull is about ninety million times the sphere-pull; but it is twenty million times as far away, so that at the same distance, one foot, the pull would be 400 billion times greater still. Hence the mass of the earth is 400 billion times 90 million times the 350 lb. mass of the sphere. This arithmetic sounds much worse than it is, and the result easily comes out that the mass of the earth is about

12,500,000.000,000,000,000,000,000 lbs.

A large number of this kind is practically very troublesome, and it is much easier to express the result by saying how many times greater is the mass of the earth than that of an equal globe of water. The earth has a radius of about 20,000,000 feet; whence, allowing 62·4 lbs. to each cubic foot, we easily find that its mass is about 2,300,000,000,000,000,000,000,000 lbs., so that the earth weighs $5\frac{1}{2}$ times as much, or its mean density is $5\frac{1}{2}$ times that of water.

The principle of all the methods of weighing the earth is that which I have tried to explain by this example. A weight is hung up, and the pull on it by some body of known mass at a known distance is compared with that exerted by the earth. We may have a specially prepared mass under or at one side of the hanging weight, or we may use a part of the earth itself, such as a mountain, of which, by careful examination, we can estimate the mass. In this case, the hanging weight is placed at one side of the mountain, and is observed to be drawn aside from the line in which it would have hung,



Earth's mass 20 million × 20 million × 90 million × 350 lbs.

Fig. 1.

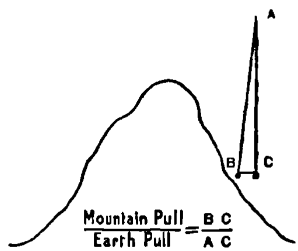


Fig. 2.

towards the mountain (Fig. 2). The distance it is drawn aside bears the same proportion to the length of the string as the pull of the mountain to the pull of the earth.

These methods of experimenting on the subject were clearly indicated by Newton, the discoverer of the law which makes them possible. But he thought that the attraction of a sphere, such as I have described, would be far too small to measure, partly, no doubt, through the fact that he made a mistake in his arithmetic, which led him to estimate the attraction at less than a millionth of its true value. He also calculated what kind of deflection sideways might be expected in a plumb-line on a mountain side, and again, though his arithmetic was correct, he thought the result too small to measure. He merely contented himself with a guess at the mean density. But Newton's guesses were as good as other people's work, for when he estimated that the density was five or six times that of water, the value was, at least, as definite as our experimental knowledge till within the last twenty years.

The first actual measurements were made about 1740, in the course of another celebrated piece of work which first definitely decided the shape of the earth. According to Newton's theory of the earth, its form should be nearly a sphere but slightly flattened at the poles. Certain measurements, made about 1700, had pointed to the shape of a sphere slightly drawn out at the poles, and to settle the question the French Academy sent out two commissions, one to Lapland, and the other to Peru, to measure the length of a degree of latitude in each place. It is easily shown that if the earth is flattened at the poles the degree in northern regions should be longer than at the equator. The Lapland degree proved decidedly longer than the Peruvian one, and so it was definitely settled that the earth is a flattened sphere.

There is a curious story in connection with the Peruvian measurements told by Whymper. They were based on a length carefully measured from one point to another by measuring-rods, and the commission, thinking doubtless that this length should stand as an eternal monument of their work, erected pyramids exactly over the two ends to fix the points. But the Spanish Government, its pride apparently offended by the inscriptions on the pyramids, destroyed them as soon as the commission returned to France. Later they were replaced, and in 1880 Whymper found one of them still in position. A former President of Ecuador had removed the other some hundreds of feet to one side in order that it might be better seen.

In the course of their labours the Peruvian commission made other important scientific investigations, and one member, M. Bouguer, had the happy idea of measuring the variation in weight due to the enormous masses of the Andes. He worked in two ways. In one series of experiments he determined the rate of swing of a pendulum of fixed length at the sea-level, at Quito, a town on a table-land nearly 10,000 feet high, and on the summit of Pichincha, above Quito, and nearly 16,000 feet high. These last are the highest pendulum experiments, I believe, ever made. Now, the time of swing of a pendulum increases as gravity decreases, and, supposing the earth to be a smooth sphere all at sea-level, the decrease due to a rise in height to Quito, nearly 10,000 feet above that level, should be about 89 in 100,000; but the pendulum showed that gravity did not decrease so much. In fact, it was greater than the calculated gravity by about 12 in 100,000. This excess was due to the down pull of the table-land on which Quito stands, or the pull of the piled-up mass was to that of the whole earth as 12 to 100,000, say 1 to 8000. Now, had the table-land been of the same density as the earth, a calculation from its size and distance from Quito showed that its attraction should have been about four times as great as this. Hence, from Bouguer's work, the earth would appear to be four times as dense as the mountain, a result we now know to be much too great. No doubt the means at Bouguer's command were inadequate for accurate work.

Bouguer also used another method, estimating the sideway attraction of Chimborazo, a mountain about 20,000 feet high, on a plumb-line placed at a point on its side. Fig. 3 will show the principle of the method. Suppose that two stations are fixed, one on the side of the mountain due south of the summit, and the other on the same latitude, but some distance westward, away from the influence of the mountain. Suppose that at the second station a star is observed to pass the meridian—we will say, for simplicity—directly overhead, then a plumb-line hung down will be exactly parallel to the observing telescope. At the first station, if the mountain were away, it would also hang down parallel to the telescope when directed to the same star. But the mountain pulls the plumb-line towards it, and changes the overhead point so that the star appears to northward instead of in the zenith. The method simply consists in determining how much the star appears to be shifted to the north.

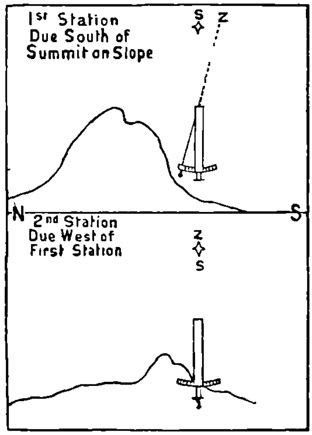


Fig. 3.

To carry out the experiment, Bouguer fixed the first station on the south slope of Chimborazo, just about the perpetual snow line, and the second nearly on the same level, several miles to the westward. He describes how his expedition reached the first station after a most toilsome journey of ten hours over rocks and snow, and how, when they reached it, they had all the time to fight against the snow, which threatened to bury their tent. Nevertheless, they succeeded in making the necessary observations, and a few days later, they were able to move on to the second station. Here they hoped for better things, as they were now below the snow line. But their difficulties were even greater than before, as now they were exposed to the full force of the wind, which filled their eyes with sand and was continually on the point of blowing away their tent. The cold was intense; and so hindered the working of their instruments that they had to apply fire to the levelling-screws before they could turn them. Still they made their observations, and found that the plumb-line was drawn aside about 8 seconds. Had Chimborazo been of the density of the whole earth, Bouguer calculated that it would have drawn aside the vertical by about twelve times this, so that the earth appeared to be twelve times as dense as the mountain, a result undoubtedly very far wide of the truth. But it is little wonder that under such circumstances the experiment failed to give a good result, and all honour is due to Bouguer for the ingenuity and perseverance which enabled him to obtain any result at all. At least he deserves the credit of first showing that the attraction by mountain masses actually exists, and that the earth, as a whole, is denser than the surface strata. As he remarks, his experiments at any rate proved that the earth was not merely a hollow shell, as some had till then held; nor was it a globe full of water, as others had maintained. He fully recognised that his experiments were mere trials, and hoped that they would be repeated in Europe.

Thirty years later his hope was fulfilled. Maskelyne, then the English Astronomer Royal,. brought the subject before the Royal Society in 1772, and obtained the appointment of a committee 'to consider of a proper hill whereon to try the experiment, and to prepare everything necessary for carrying the design into execution.' Cavendish, who was himself to carry out an earth-weighing experiment some twenty-five years later, was probably a member of the committee, and was certainly deeply interested in the subject, for among his papers have been found calculations with regard to Skiddaw, one of several English hills at first considered. Ultimately, however, the committee decided in favour of Schiehallion, a mountain near Rannoch, in Perthshire, 3547 feet high. Here the astronomical part of the experiment was carried out in 1774, and the survey of the district in that and the two following years. The mountain has a short east and west ridge, and slopes down steeply on the north and south, a shape very suitable for the purpose.

Maskelyne, who himself undertook the astronomical work, decided to work in a way very like that followed by Bouguer on Chimborazo, but modified in a manner suggested by him. Two stations were selected, one on the south, and the other on the north slope. A small observatory was erected first at the south station, and the angular distance of some stars from the zenith, when they were due south, was most carefully measured. The stars selected all passed nearly overhead, so that the angles measured were very small. The instrument used was the zenith-sector, a telescope rotating about a horizontal east and west axis at the object-glass end, and provided with a plumb-line hanging from the axis over a graduated scale at the eye-piece end. This showed how far the telescope was from the vertical.

After about a month's work at this station, the observatory was moved to the north station, and again the same stars were observed with the zenith-sector. Another month's work completed this part of the experiment. Fig. 4 will show how the observations gave the attraction due to the hill. Let us for the moment leave out of account the curvature of the earth, and suppose it flat. Further, let us suppose that a star is being observed which would be directly overhead if no mountain existed. Then evidently at S. the plumb-line is pulled to the north, and the zenith is shifted to the south. The star therefore appears slightly to the north. At N. there is an opposite effect, for the mountain pulls the plumb-line southwards, and shifts the zenith to the north; and now the star appears slightly to the south. The total shifting of the star is double the deflection of the plumb-line at either station due to the pull of the mountain.

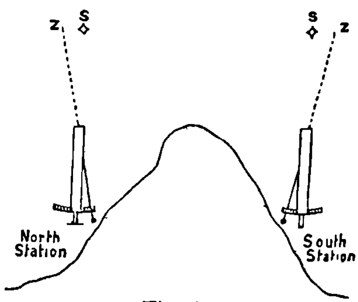


Fig. 4.

But the curvature of the earth also deflects the verticals at N. and S., and in the same way, so that the observed shift of the star is partly due to the mountain, and partly due to the curvature of the earth. A careful measure was made of the distance between the two stations, and this gave the curvature-deflection as about 43″. The observed deflection was about 55″, so that the effect of the mountain, the difference between these, was about 12″.

The next thing was to find the form of the mountain. This was before the days of the Ordnance Survey, so that a complete survey of the district was needed. When this was complete, contour-maps were made, giving the volume and distance of every part of the mountain from each station. Hutton was associated with Maskelyne in this part of the work, and he carried out all the calculations based upon it, being much assisted by valuable suggestions from Cavendish.

Now had the mountain had the same density as the earth, it was calculated from its shape and distance that it should have deflected the plumb-lines towards each other through a total angle of 20·9″, or $1\frac{4}{5}$ times the observed amount. The earth, then, is $1\frac{4}{5}$ times as dense as the mountain. From pieces of the rock of which the mountain is composed, its density was estimated as $2\frac{1}{2}$ times that of water. The earth should have, therefore, density $1\frac{4}{5}$ $2\frac{1}{2}$ or $4\frac{1}{2}$. An estimate of the density of the mountain, based on a survey made thirty years later, brought the result up to 5. All subsequent work has shown that this number is not very far from the truth.

An exactly similar experiment was made eighty years later, on the completion of the Ordnance Survey of the kingdom. Certain anomalies in the direction of the vertical at Edinburgh led Colonel James, the director, to repeat the Schiehallion experiment, using Arthur's Seat as the deflecting mountain. The value obtained for the mean density of the earth was about $5\frac{1}{3}$.

Repetitions have also been made of the pendulum method, tried by Bouguer in the Andes.

The first of these was by Carlini, in 1821. He observed the length of a pendulum swinging seconds at the Hospice on Mont Cenis, about 6000 feet above sea-level, and so obtained the value of gravity there. The value due to mere elevation above the sea-level was easily calculated; but the observed value was greater than that calculated by about 1 in 5000. In other words, the pull of the whole earth was 5000 times greater than that of the mountain under the Hospice. Knowing approximately the shape of the mountain, and estimating its density from specimens of the rock, Carlini found the density of the earth to be about $4\frac{1}{2}$ times that of water.

Another experiment of the same kind was made by Mendenhall, in Japan, in 1880. Here he determined the value of gravity on the summit of Fujiyama, a mountain nearly $2\frac{1}{2}$ miles high. He found it greater than the value calculated from the increased distance from the earth's centre by about 1 in 5000, as Carlini had done on Mont Cenis. Fujiyama, though the higher, is more pointed and less dense than Mont Cenis. Mendenhall estimated the mean density of the earth as 5·77.

Airy applied the pendulum to solve the problem in a somewhat different way, using, instead of a mountain, the crust of the earth between the top and the bottom of a mine. His first attempts were made in 1826, at the Dolcoath copper mine, in Cornwall, which has acquired a melancholy notoriety in the last few weeks. Here he swung a pendulum first at the surface and then at the bottom of the mine. At the point below we may consider that the weight of the pendulum was due to the pull of the part of the earth within the sphere with radius reaching from the earth's centre to the point (Fig. 5). Knowing the value of gravity below, it was easy to calculate what it would have been at the level of the surface had no outer shell existed, and had the change in value merely depended on the greater distance from the earth's centre. The observed value was greater than this through the pull of the outer shell, and it was hoped that the difference would be measured sufficiently accurately to show how much greater is the mass of the earth than that of the crust. The first attempt was brought to an end by a curious accident. As one of the pendulums used was being raised up the shaft, the box containing it took fire, the rope was burnt, and the pendulum fell to the bottom. Two years later another attempt was made, but this was brought to an end by a fall in the mine, which stopped the pump so that the lower station was flooded.

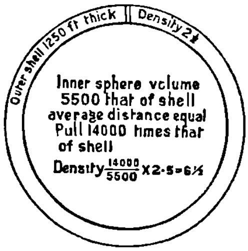


Fig. 5.

Many years later, in 1854, the experiment was again undertaken by Airy, this time in the Harton coal-pit, near Sunderland. The method was exactly the same, a pendulum being swung above and below the surface, and the diminution in gravity above carefully determined. The experiment was carried out with the greatest care and in a most thorough way, two pendulums being swung at the same time—one above and one below—the two being interchanged from time to time. A small army of assistants was occupied in taking the observations, which extended continuously night and day for about three weeks. Now gravity at the surface was greater than it would have been, had no outer shell existed of thickness equal to the depth of the pit, by about 1 in 14,000, so that the pull of the earth was about 14,000 times that of the shell. The density of the shell was determined from specimens of the rocks, and Airy found the density of the earth about $6\frac{1}{2}$.

Some very interesting experiments have recently been made in a similar way by von Sterneck in silver mines in Saxony and Bohemia. I cannot enter into details of his method, but with regard to his results I may say that he obtained different results with different depths of mines, the value of the mean density increasing with the increasing thickness of the shell used. This shows very evidently that there were sources of disturbance vitiating the method. Von Sterneck found, on comparing his observations at the two mines, that the increase in gravity on descending was much more nearly in proportion to the rise of temperature than to the depth of descent. This appears to indicate that whatever disturbs the regularity of gravity disturbs also the slope of temperature.

All the methods which I have so far described use, as you will have noticed, natural masses to compare the earth with, and herein lies a fatal defect as regards exactness. We do not know accurately the density of these masses and what is the condition of the surrounding and underlying strata. We can really only form at the best rough guesses. Indeed, the experiments might rather be turned the other way about, and assuming the value of the mean density of the earth, we might measure the mean density of the mountain or strata of which the attraction is measured.

One, the most recently published experiment, is perhaps to be classed with those I have described, but it avoids, at any rate, this fault. It has only just been completed by M. Berget, at Habay la Neuve, in Luxembourg. He uses a lake of 79 acres area, of which he can vary the depth, and I may perhaps give you a correct notion of his method if I say that in principle he finds the variation in weight by means of a particular kind of spring balance, of a column of mercury, placed above the lake, when the lake is full and when it is lowered about a yard. He performs, in fact, Bouguer's original experiment at Quito, but under the attracted body is an artificial plateau of water, which he can place there or remove at will, instead of the natural plateau on which Quito stands. He obtained for the value of the mean density, 5·41.

We turn now to a different class of experiment, in which the attracting body is altogether on a smaller scale, so that it can be handled in the laboratory. The smallness of the attraction is compensated for by the accuracy with which we know the size and mass of the attracting body.

The idea of such an experiment is due to the Rev. John Michell, a physicist who lived in the last century. He proposed to hang up a light wooden horizontal rod by a fine wire, the rod having two small lead balls at the two ends. The rod would be twisted round if a small force were applied to either ball, until the twist of the wire neutralised the turning-power of the applied force. By making the wire thin enough the rod would turn appreciably with very minute force. This constitutes what is now called a torsion-balance. If a large lead sphere was brought near to one of the balls on one side of it, it would attract it, and twist the rod round. Now, if the time of vibration of such a rod to and fro when disturbed is observed, it is possible to calculate the force applied at one end corresponding to any observed twist. By bringing two spheres up, one on one side at one end and the other on the other side at the other end, and noting the position of the rod, and then moving the spheres each to the other side of the rod, and noting its new position, it is easy to see that the change in position is four times that due to one mass alone brought up to one side of the rod.

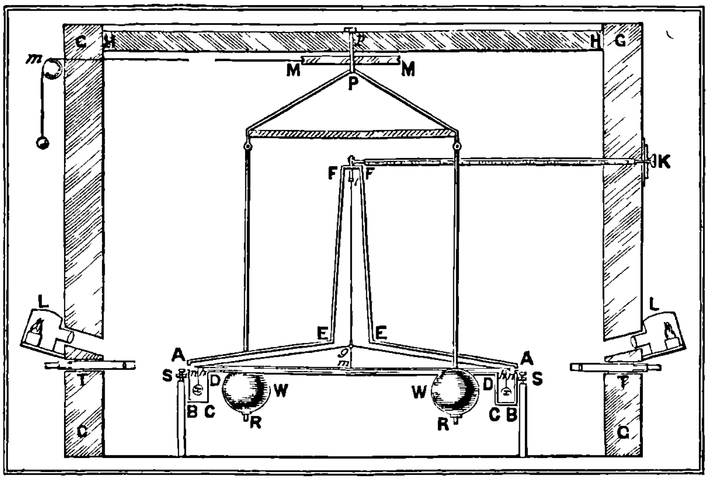


Fig. 6. Cavendish's Apparatus.
hh, torsion-rod. *x*, *x*, balls hung from its ends. *W*, *W*, attracting masses moveable round axis *P*. *T*, *T*, telescopes to view position of torsion-rod.

Michell only prepared the apparatus, and did not actually use it. After his death it came into the possession of Cavendish, who modified and partly re-made it, and in the years 1797–8 carried out the celebrated experiment which bears his name.

The method of working was exactly that proposed by Michell (Fig. 6). Two small lead balls hung from the ends of a light wooden rod suspended horizontally by a long thin wire attached to the centre of the rod. The whole was enclosed in a case. Two large lead spheres were brought up outside the case, so that they acted in opposite directions on the balls, and both tended to twist the rod the same way. They were then moved round so as to produce an opposite twist, and the total twist was measured by a telescope viewing a scale on the rod. The time of vibration was observed, and so it was possible to calculate the attraction of the spheres on the balls, and compare it with their weight or the attraction of the earth. Thus the mass, and therefore the density, of the earth, could be found. The force of attraction of a sphere on a ball was only about $\frac{1}{4000}$ of a grain, so small a quantity that (as you may easily imagine) it was necessary to adopt most careful means to ward off draughts—not merely the coarse draughts such as exist in a room, but the minute and slow currents of air created inside the case by very minute differences of temperature. These minute currents are the great enemy in all measurements of small forces. Cavendish, nevertheless, was fairly successful in preventing them, and obtained a result of 5·448 for the mean density of the earth, a result which deserved far more confidence than any as yet obtained by the 'Natural Mass' method.

His experiment has since been repeated several times. Reich made two experiments in Germany by Cavendish's method, obtaining in 1837 a value 5·49, and about 1849 a value 5·58. In England it was repeated by Baily about 1841 and 1842. Baily's experiment excited great attention at the time, and the result obtained, 5·674, was long supposed to be very near indeed to the truth. But certain discrepancies in the work gradually impaired confidence in the final result, and in 1870 MM. Cornu and Baille, in France, undertook a repetition, with various improvements and refinements. In planning out their own work they succeeded in detecting probably the chief source of error in Baily's work. They are still, I believe, engaged in the experiment, and though they have given an interim result of about 5·5, and have shown that Baily's work, if properly interpreted, should bring out a not very different result, their final conclusion is still to be published.

Meanwhile, Professor Boys, in England, has taken up the experiment, striking out a new line, using for the suspending wire a thread of quartz, which, as he has discovered, possesses splendid qualities for the work. He has shown that, without impairing its sensitiveness, the apparatus may be greatly reduced in size, and so made less liable to disturbances by air currents. He is now carrying on the experiment at Oxford, and with his brilliant skill as an experimenter, and with such well-devised apparatus, we may hope for a degree of accuracy in the result which no one supposed possible a few years ago.

About 1886, Dr. Wilsing, of Potsdam, devised a modified form of Cavendish's experiment, in which a sort of double pendulum is used, i.e., one with a ball below and another at a nearly equal distance above the suspension. The pendulum is then in a very sensitive state, and a very small horizontal force pulls it through a large angle.

It is then just like a torsion-balance, but with a vertical instead of a horizontal rod. If weights are brought up, one to pull the upper ball to one side and the other to pull the lower ball to the other side, the pendulum twists round slightly. From the observed twist and the time of swing the attraction can be measured and compared with the pull of the earth. Wilsing found that the earth had a mean density of 5·579.

About fifteen years ago the idea occurred nearly at the same time to the late Prof. von Jolly and myself to apply the common balance to the problem, though in somewhat different ways. He had a balance fixed at the top of a tower in Munich, and from the scale-pans hung wires supporting two other scale-pans at the bottom of the tower. Imagine that two weights are balanced against each other at the top of the tower. If one is now brought down and put in the lower scale-pan on the same side it is nearer the centre of the earth, and, therefore, heavier. Von Jolly found a gain of about 32 milligrams in 5 kilograms. He now built up under the lower pan a large lead sphere, a yard in diameter, so that its attraction was added to that of the earth. The gain on transferring the weight from the upper to the lower pan now came out about half a milligram more, so that the attraction of the sphere was this half milligram. The earth's attraction was about 10,000,000 times that of the sphere, and its density was calculated to be 5·69.

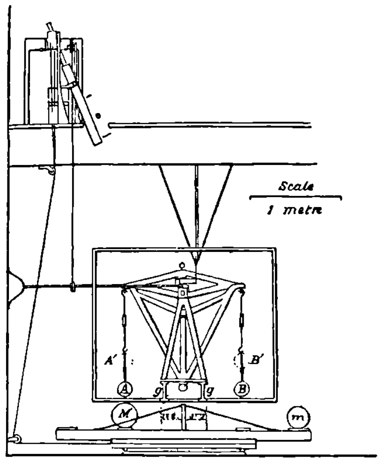


Fig. 7. A, B. weights. each about 50 lbs., hanging from the two arms of balance. M, attracting mass on turn-table. moveable so as to come under either A or B. m, balancing mass. A', B', second positions for A and B. In this position the attraction of M on the beam and suspending wires is the same as before so that the difference of attraction on A and B in the two positions is due to the difference in distance of A and B only, and thus the attraction on the beam, etc., is eliminated.

An experiment very much on this principle, but with great improvements in arrangement, is now being carried out by Drs. Richarz and Krigar-Menzel, at Spandau, near Berlin. Their balance is working with such delicacy that it shows that a change in height of $2\frac{1}{2}$ yards produces less difference in a weight than it should by about one in 25,000,000 parts of the weight, probably because the upper part of the building tends to pull the weight upwards.

My own method will be seen from Fig. 7. *A*, *B* are two lead weights, about 50 lbs. each, hanging down from the ends of a very large and strong balance, inside a protecting wooden case. *M* is a large lead sphere, weighing about 350 lbs., on a turn-table so that it can move round from under *A* till it comes under *B*. The distance between the centres of *M* and *A* or *M* and *B* is about one foot. When under *A*, *M* pulls *A*, and so increases its weight. When moved so as to come under *B* the increase is taken from *A* and put on to *B*. The balance is free to move all the time, so that it tilts over to the *B* side an amount due to double the attraction of *M* on either.

To observe the tilt, a mirror is connected with the balance so that it turns round when the balance tilts, but by the double-suspension method, due to Lord Kelvin, the mirror goes through 150 times as great an angle as the balance. In the room above is a telescope, which views the reflection of a scale in the mirror, and, as the mirror turns round, the scale moves across the field of view. I may say that the tilt observed meant that the beam turned through rather more than 1″, and that the weight moved nearer to the mass by about $\frac{1}{8000}$ of an inch. The weight in milligrammes producing this tilt had to be found. This was done virtually (though not exactly in detail) by moving a centigramme-rider ($\frac{1}{6}$ of a grain) about 1 inch along the beam, which was equivalent to adding to one side a weight of about $\frac{4}{10}$ milligramme ($\frac{1}{160}$ grain). The tilt due to the transfer was observed, and was found to be very nearly the same as that due to the attraction, so that the effect of moving M round from A to B was equivalent to increasing B by $\frac{4}{10}$ milligramme, or $\frac{1}{500000000}$ of its previous weight. The pull on either is half this. In other words, the earth pulled either about 100,000,000 times as much as the mass M, and the earth, which is 20,000,000 times as far away, would, at the same distance, have exerted 400,000,000,000,000 times 100,000,000 times the pull, and is, therefore, so many times heavier. Thus we find that the earth weighs about 125 followed by twenty-three 0's lbs. I need not here enter into detail as to a differential method adopted to eliminate the pull of M on the beam. Perhaps Fig. 7 will explain it.

To make the balance work at all exactly various precautions had to be adopted, especially to ward off disturbing air-currents. It was placed in a closed cellar, and observed from the room above through a small hole in the floor. There were other sources of error which I discovered in the course of the work, some of them leading to delays of years. One of the most serious was due to the attachment of the apparatus used to move the riders to the case containing the balance. I found that when this was used the whole case moved, and that it did not quite return to its original position. Ultimately the rider-moving apparatus had to be quite independent of the case. Then vibration from traffic in the street, or walking in the building, made the mirror quiver, so that I could not see the scale clearly. This was cured by mounting the balance on big blocks of india-rubber, so that while the supports and the case, weighing perhaps a ton, could be made to rock almost with a touch, the india-rubber absorbed all vibration coming from the floor and placed the balance beyond the reach of outside disturbances.

Once I supposed that the apparatus was in working order, and during one year I obtained a series of results. But some curious inconsistencies appeared in these, and at last I found that they were due to a tilting of the floor of the cellar when the large mass M was moved from one side to the other of the balance. The tilt was certainly small perhaps $\frac{1}{3}$″, or say such that in a floor 10 miles long it would amount to 1 inch, yet it was quite enough to alter the result very seriously. I had long before looked for such a tilt, and thought I had proved its non-existence. But it at length asserted its existence, and in the most inconvenient way. If it had only kept the same it would probably, from the differential character of the experiment, have been nearly eliminated from the result. But it gradually grew as time went on, and the floor settled and tilted over more in one piece, and succeeded in making a year's work valueless.

In order to prevent any tilt, a second mass m was introduced, of half the weight and at double the distance. This, of course, complicated the calculation a little, but it quite cured the tilting.

While the apparatus was undergoing the changes necessary for the introduction of the second mass, I discovered another source of error, due to an unsuspected steel core in the inside of the brass wire rope used to pull the turn-table round. This core got slightly magnetised by the earth, and exerted an attraction on the steel knife-edges of the balance, varying with its position. The brass wire was replaced by a gut rope, and the magnetic effect disappeared.

One of the most curious experiences I had occurred one day when I found the balance behaving in a most jerky and irregular way. On going downstairs and opening the case, I discovered some long white hair-like processes, perhaps $\frac{1}{2}$ or $\frac{3}{4}$ inch long, issuing from the little pegs which were used to move the riders on to or off the beam of the balance. These were of wood, covered with gold leaf, and attached to brass holders. The gold leaf had been pasted on. The paste absorbed moisture from the air, and then formed a small galvanic battery with the brass and gold leaf. Hair-like crystals, most likely of sulphate of zinc, were formed, and these grew so long that they touched the balance and interfered with its action.

Ultimately, however, all the most obvious sources of error were detected and stopped, and the balance gave, I think, a fairly good result of 5·493.

It may be interesting to state the accuracy with which it worked. The increase in the weight of the 50 lbs. which was to be measured was about $\frac{1}{50000000}$ of the whole. Measurements of this increase were never wrong by more than 2 per cent. of the amount, usually well within 1 per cent., or $\frac{1}{5000000000}$ of the whole weight, the variation which would occur if the 50 lbs. were moved $\frac{1}{40}$ inch nearer to the centre of the earth. Now these numbers are too large to give us much idea of the smallness of the weights concerned. Suppose, then, we take a rough illustration, in which the small weights are magnified up to be appreciable.

Imagine a balance large enough to contain on one pan the whole population of the British Islands, and that all the population were placed there but one medium-sized boy. Then the increase in weight which had to be measured was equivalent to measuring the increase due to putting that boy on with the rest. The accuracy of measurement was equivalent to observing from the increase in weight whether or no he had taken off one of his boots before stepping on to the pan.

One of the most curious points about this method of weighing the earth is the contrast between the mass to be weighed and the mass in terms of which it is weighed. You will remember that the tilt of the balance is measured by moving a centigramme-rider along the beam. Any inaccuracy in the estimation of the weight of that rider is repeated in the weight of the earth. So that in one sense we may be said to weigh the earth with its twelve billion billion pounds by using a weight of $\frac{1}{50000}$ part of a pound.

At last my long catalogue of experiments is brought to an end, or rather it is brought up to the present time, for such researches have no end. Each generation will try to add another decimal place to the result, or find out the errors of its predecessors. And even now there are many workers in the field, indeed, there is almost an epidemic of earth-weighing. Besides the experiments I have already referred to as now going on, I know of at least two others not yet publicly announced.

But in a research like this, where the result is of such great interest in helping us to a knowledge of the constitution of the earth, and where all sorts of errors lie in wait to deceive the worker, there is every advantage in frequent repetition by different methods. At first the results varied a good deal, but now I think they are tending towards a steady mean. If we take the average of Newton's guess, that the mass of the earth is five or six times that of an equal globe of water, and say that it is $5\frac{1}{2}$ times that mass, I believe that we shall be correct within a very small fraction of the whole.

LIST OF EXPERIMENTS WITH THE RESULTS OBTAINED.

Date	Experimenter	Result
1737–40	Bouguer	Inconclusive
1774–6	Maskelyne and Hutton	4·5 to 5
1797–8	Cavendish	5·45
1821	Carlini	4·7 about
1837	Reich	5·49
1840–1	Baily	5·674
1852	Reich	5·583
1854	Airy	6·565
1855	James and Clarke	5·376
1870–	Cornu and Baille	5·50–5·56
1879–80	Von Jolly	5·692
1883	Von Sterneck	5·77
1885	Von Sterneck	7 about
1886–8	Wilsing	5·579
1890	Poynting	5·493
1893	Berget	5·41
In progress	Cornu and Baille	
	König, Richarz and Krigar-Menzel	
	Boys	
	Laska ?	

54.

THE MEAN DENSITY OF THE EARTH.

[*Nature*, **48**, 1893, p. 370.]

In a note in your issue of August 10, adding to the list of values for the mean density of the earth, which you gave on July 27, it is stated that von Jolly and Poynting obtained the value 5·58. This is, I believe, the value obtained by von Jolly, but my final result, as published in the *Philosophical Transactions** for 1891, is 5·493.

In any account of recent work on this subject I think von Sterneck's experiments at Przibram and Freiberg deserve notice. These were made in the years 1882–5, and were pendulum experiments of the Harton Pit type. The method of comparing the times of swing of the pendulums below and at the surface was, I believe, quite new, and consisted in determining the coincidences with the same clock, which gave simultaneous half-second signals at the two stations by means of an electric circuit. The results unfortunately tend to confirm the conclusion which had, I think, been already drawn from Airy's work—that the mine-method of experiment, though it may add to our knowledge of the constitution of the surface-strata, is useless in determining the mean density of the earth.

Major von Sterneck's papers are published in the *Proceedings of the Militär-Geographisches Institut* of Vienna.

* [*Collected Papers*, Art. 3, p. 88.]

55.

RECENT STUDIES IN GRAVITATION.

[Address: Royal Institution of Great Britain, February 23, 1900.]

[*Roy. Inst. Proc.* **16**, 1900–02, pp. 278 294.]

The studies in gravitation which I am to describe to you this evening will perhaps fall into better order if I rapidly run over the well-beaten track which leads to those studies, the track first laid down by Newton, based on astronomical observations, and only made firmer and broader by every later observation.

I may remind you, then, that the motion of the planets round the sun in ellipses, each marking out the area of its orbit at a constant rate, and each having a year proportional to the square root of the cube of its mean distance from the sun, implies that there is a force on each planet exactly proportioned to its mass, directed towards, and inversely as the square of its distance from the sun. The lines of force radiate out from the sun on all sides equally, and always grasp any matter with a force proportional to its mass, whatever planet that matter belongs to.

If we assume that action and reaction are equal and opposite, then each planet acts on the sun with a force proportional to its own mass; and if, further, we suppose that these forces are merely the sum totals of the forces due to every particle of matter in the bodies acting, we are led straight to the law of gravitation, that the force between two masses M_1, M_2 is always proportional to the product of the masses divided by the square of the distance r between them, or is equal to

$$\frac{G \times M_1 \times M_2}{r^2}$$

and the constant multiplier G is the constant of gravitation.

Since the force is always proportional to the mass acted on, and produces the same change of velocity whatever that mass may be, the change of velocity tells us nothing about the mass in which it takes place, but only about the mass which is pulling. If, however, we compare the accelerations due to different pulling bodies, as for instance that of the sun pulling the earth, with that of the earth pulling the moon, or if we compare changes in motion due to the different planets pulling each other, then we can compare their masses and weigh them, one against another and each against the sun. But in this weighing our standard weight is not the pound or kilogramme of terrestrial weighings, but the mass of the sun.

For instance, from the fact that a body at the earth's surface 4000 miles, on the average, from the mass of the earth, falls with a velocity increasing by 32 ft./sec.^{2}, while the earth itself falls towards the sun, 92 million miles away, with a velocity increasing by about $\frac{1}{5}$ inch/sec.^{2}, we can at once show that the mass of the sun is 300,000 times that of the earth. In other words, astronomical observation gives us only the acceleration, the product of $G \times$ mass acting, but does not tell us the value of G nor of the mass acting, in terms of our terrestrial standards.

To weigh the sun, the planets, or the earth, in pounds or kilogrammes, or to find G, we must descend from the heavenly bodies to earthly matter and either compare the pull of a weighable mass on some body with the pull of the earth on it, or else choose two weighable masses and find the pull between them.

All this was clearly seen by Newton, and was set forth in his *System of the World* (third edition, page 41).

He saw that a mountain mass might be used, and weighed against the earth by finding how much it deflected the plumb-line at its base. The density of the mountain could be found from specimens of the rocks composing it, and the distance of its parts from the plumb-line by a survey. The deflection of the vertical would then give the mass of the earth.

Newton also considered the possibility of measuring the attraction between two weighable masses, and calculated how long it would take a sphere a foot in diameter, of the earth's mean density, to draw another equal sphere, with their surfaces separated by $\frac{1}{4}$-inch, through that $\frac{1}{4}$-inch. But he made a very great mistake in his arithmetic, for while his result gave about 1 month the actual time would only be about $5\frac{1}{2}$ minutes. Had his value been right, gravitational experiments would have been beyond the power of even Professor Boys. Some doubt has been thrown on Newton's authorship of this mistake, but I confess that there is something not altogether unpleasing in the mistake even of a Newton. His faulty arithmetic showed that there was one quality which he shared with the rest of mankind.

Not long after Newton's death the mountain experiment was actually tried, and in two ways. The honour of making these first experiments on gravitation belongs to Bouguer, whose splendid work in thus breaking new ground does not appear to me to have received the credit due to it.

One of his plans consisted in measuring the deflection of the plumb-line due to Chimborazo, one of the Andes peaks, by finding the distance of a star on the meridian from the zenith, first at a station on the south side of the mountain where the vertical was deflected, and then at a station to the west, where the mountain attraction was nearly inconsiderable, so that the actual nearly coincided with the geographical vertical. The difference in zenith distances gave the mountain deflection. It is not surprising that, working in snowstorms at one station, and in sandstorms at the other, Bouguer obtained a very incorrect result. But at least he showed the possibility of such work, and since his time many experiments have been carried out on his lines under more favourable conditions. Now, however, I think it is generally recognised that the difficulty of estimating the mass of a mountain from mere surface chips is insurmountable, and it is admitted that the experiment should be turned the other way about and regarded as an attempt to measure the mass of the mountains from the density of the earth known by other experiments.

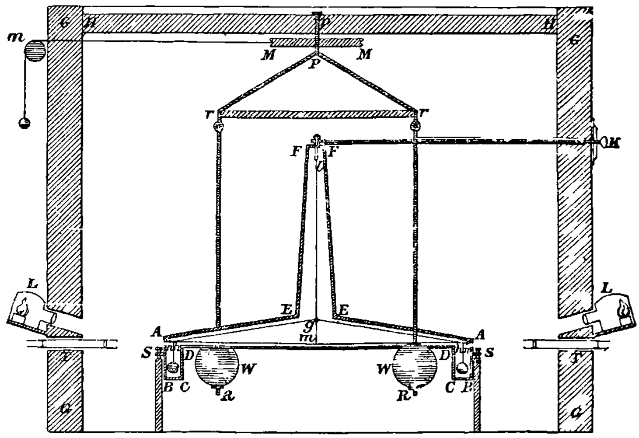


Fig. 1. Cavendish's Apparatus.

These other experiments are on the line indicated by Newton in his calculations of the attraction of two spheres. The first was carried out by Cavendish.

In the apparatus (Fig. 1) he used two lead balls, *B*, *B*, each 2 inches in diameter. These were hung at the ends of a horizontal rod 6 ft. long, the torsion-rod, and this was hung up by a long wire from its middle point. Two large attracting spheres of lead, *W*, *W*, each 12 inches in diameter, were brought close to the balls on opposite sides so that their attractions on the balls conspired to twist the torsion-rod round the same way, and the angle of twist was measured. The force could be reckoned in terms of this angle by setting the rod vibrating to and fro and finding the time of vibration, and the force came out to less than 1/3000 of a grain. Knowing M_1, M_2 and r the distance between them and the force GM_1M_2/r^2, of course Cavendish's result gives G, or knowing the attraction of a big sphere on a ball, and knowing the attraction of the earth on the same ball, that is its weight, the experiment gives the mass of the earth in terms of that of the big sphere, and so its mean density. This experiment has often been repeated, but I do not think it is too much to say that no advance was made in exactness till we come to quite recent work.

By far the most remarkable recent study in gravitation is Professor Boys' beautiful form of the Cavendish experiment, a research which stands out as a model in beauty of design and in exactness of execution (Fig. 2). But as

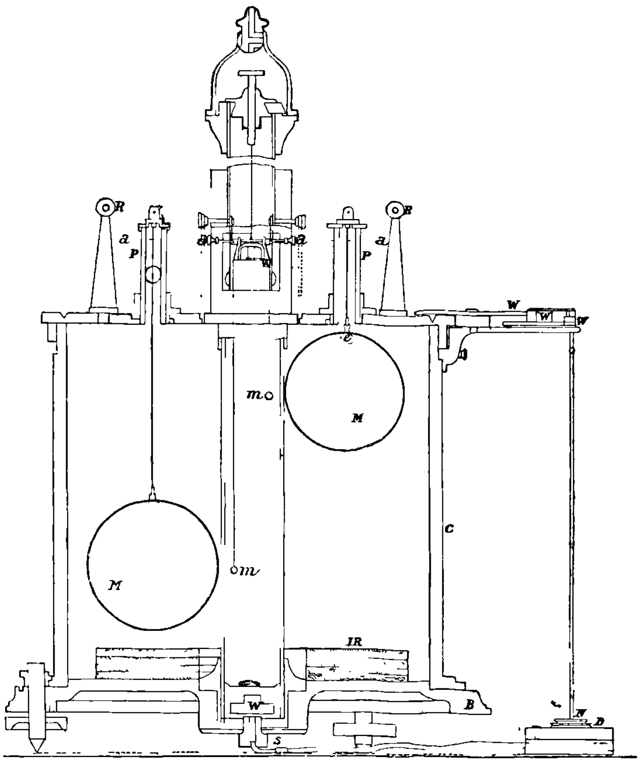


Fig. 2. Boys' Apparatus.

Professor Boys has described his experiment already in this theatre* it is not necessary for me to do more than refer to it. It is enough to say that he made the great discovery, obvious perhaps when made, that the sensitiveness of the apparatus is increased by reducing its dimensions. He therefore decreased the scale as far as was consistent with exact measurement of the parts of the apparatus, using a torsion-rod, itself a mirror, only one inch long, gold balls, *m*, *m*, only $\frac{1}{4}$-inch in diameter, and attracting lead masses, *M*, *M*, only $4\frac{1}{4}$ inches in diameter. The force to be measured was less than $1/5 \times 10^6$ grain.

* *Roy. Inst. Proc.* vol. 14, Part II, 1894, p. 353.

The exactness of his work was increased by using as suspending-wire one of his quartz threads. It would be difficult to overestimate the service he has rendered in the measurement of small forces by the discovery of the remarkable properties of these threads.

One of the chief difficulties in the measurement of these small gravitational pulls is the disturbances which are brought about by the air-currents, which blow to and fro and up and down inside the apparatus, producing irregular motions in the torsion-rod. These, though much reduced, are not reduced in proportion to the diminution of the apparatus.

A very interesting repetition of the Cavendish experiment has lately been concluded by Dr. Braun* at Mariaschein in Bohemia, in which he has sought to get rid of these disturbing air-currents by suspending his torsion-rod in a receiver which was nearly exhausted, the pressure being reduced to about $\frac{1}{200}$ of an atmosphere. The gales which have been the despair of other workers were thus reduced to such gentle breezes that their effect was hardly noticeable. His apparatus was nearly a mean proportional between those of Cavendish and Boys, his torsion-rod being about 9 inches long, the balls weighing 54 gms.—less than two ounces—and the attracting masses either 5 or 9 kgms. His work bears internal evidence of great care and accuracy and he obtained almost exactly the same result as Professor Boys.

Dr. Braun carried on his work far from the usual laboratory facilities, far from workshops, and he had to make much of his apparatus himself. His patience and persistence command our highest admiration.

I am glad to say that he is now repeating the experiment, using as suspension a quartz fibre supplied to him by Professor Boys in place of the somewhat untrustworthy metal wire which he used in the work already published.

Professor Boys has almost indignantly disclaimed that he was engaged on any such purely local experiment as the determination of the mean density of the earth. He was working for the Universe, seeking the value of G, information which would be as useful on Mars or Jupiter or out in the stellar system as here on the earth. But perhaps we may this evening consent to be more parochial in our ideas, and express the results in terms of the mean density of the earth. In such terms then both Boys and Braun find that density 5·527 times the density of water, agreeing therefore to 1 in 5000.

There is another mode of proceeding which may be regarded as the Cavendish experiment turned from a horizontal into a vertical plane, and in which the torsion-balance is replaced by the common balance. This method occurred about the same time to the late Professor von Jolly and myself. The principle of my own experiment† will be sufficiently indicated

* *Denkschriften der Math. Wiss. Classe der Kais. Akad. der Wissenschaften*, Wien, vol. 64, 189 .

† *Phil. Trans.* A, 182 (1891), p. 565. [*Collected Papers*, Art. 3.]

by Fig. 3. A big bullion-balance with a 4-foot beam had two lead spheres, *A, B,* each about 50 lbs. in weight, hanging from the two ends in place of the usual scale-pans. A large lead sphere, *M,* 1 foot in diameter and weighing about 350 lbs., was brought first under one hanging weight, then under the other. The pull of the lead sphere acted first on one side alone and then on the other, so that the tilt of the balance-beam when the sphere was moved round was due to twice the pull. By means of riders the tilt, and therefore

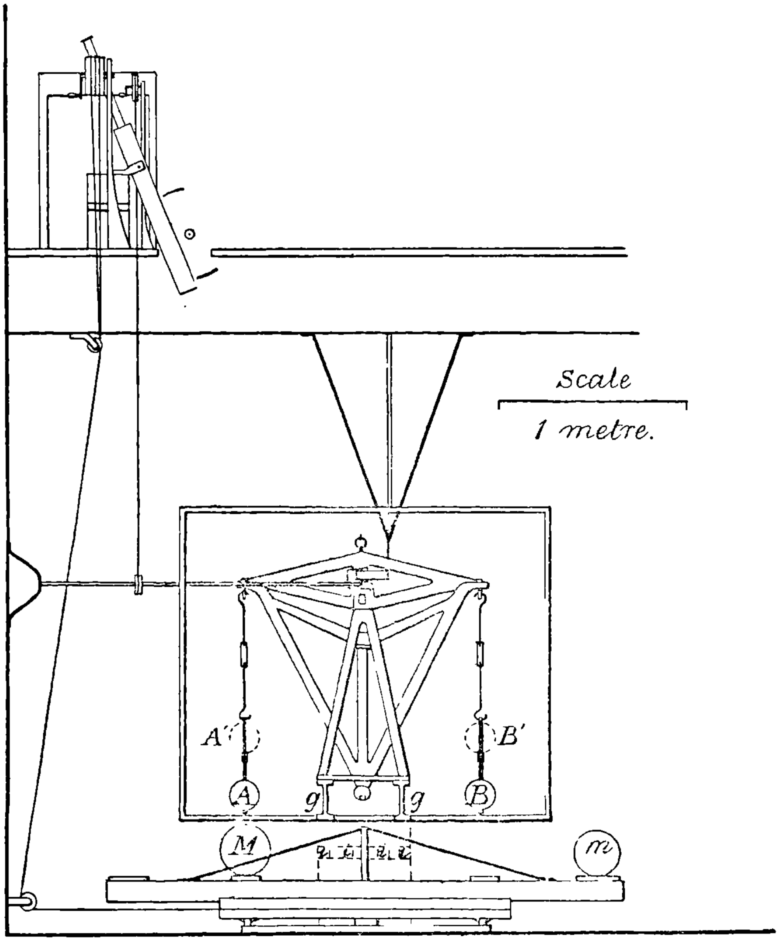


Fig. 3. Common Balance experiment (Poynting).

the pull, was measured directly as so much increase in weight. This increase, when the sphere was brought directly under the hanging weight with 1 foot between the centres, was about $\frac{1}{5}$ mgm. in a total weight of 20 kgm. or about 1 in 100,000,000. If then a sphere one foot away pulls with $1/10^8$ of the earth's pull, the earth being on the average 20,000,000 feet away, it is easy to see that the earth's mass is calculable in terms of the mass of the sphere, and its density is at once deduced. The direct aim of this experiment, then, is not G, but the mass of the earth.

It is not a little surprising that the balance could be made to indicate such a small increase in weight as 1 in 100 million. But not only did it indicate, it measured the increase, with variations usually well within 1 per cent. of the double attraction, or to 1 in 5000 million of the whole weight, a change in weight which would occur merely if one of the spheres were moved $\frac{1}{40}$ inch nearer the earth's centre. This accuracy is only attained by never lifting the knife-edges and planes during an experiment, thus keeping the beam in the same state of strain throughout, and, further, by taking care that none of the mechanism for moving the weights or riders shall be attached in any way to the balance or its case; two conditions which are absolutely essential if we are to get the best results of which the balance is capable.

Quite recently another common balance experiment has been brought to a conclusion by Professor Richarz and Dr. Krigar-Menzel* at Spandau, near Berlin. Their method may be gathered from Fig. 4. A balance of 23 cm., say 9-inch, beam was mounted above a huge lead pile about 2 metres cube, and weighing 100,000 kgm.

Two pans were supported from each end of the beam, one pan above, the other pan below the lead cube, the suspending wires of the lower pans going through narrow vertical tubular holes in the lead. Instead of moving the attracting mass, the attracted mass was moved. Masses of 1 kgm. each were put first, say, one in the upper right-hand pan, the other in the lower left-hand pan, when the pull of the lead block made the right-hand p n heavier and the left-hand pan lighter. Then the weights were changed to the lower right-hand and the upper left-hand, when the pulls of the lead pile were reversed. When we remember that in my experiment a lowering of the hanging sphere by $1\frac{1}{2}$ inches would give an effect as great as the pull I was measuring, it is evident that here the approach to and removal from the earth by over 2 metres would produce very considerable changes in weight, and, indeed, these changes masked the effect of the attraction of the lead. Preliminary experiments had, therefore, to be made before the lead pile was built up, to find the change in weight due to removal from upper to lower pan, and this change had to be allowed for. The quadruple attraction of the lead pile came out at 1·3664 mgm., and the mean density of the earth at 5·505.

This agrees nearly with my own result of 5·49, and it is a curious coincidence that the two most recent balance experiments agree very nearly at, say 5·5, and the two most recent Cavendish experiments agree at, say 5·53. But I confess I think it is merely a coincidence. I have no doubt that the torsion experiment is the more exact, though probably an expe iment

* Anhang zu den *Abhandlungen der Königl. Preuss. Akad. der W ss s laf en z B rl n*, 1898.

on different lines was worth making. And I am quite content to accept the value 5·527 as the standard value for the present.

And so the latest research has amply verified Newton's celebrated guess that 'the quantity of the whole matter of the Earth may be five or six times greater than if it consisted all of water.'

I now turn to another line of gravitational research. When we compare gravitation with other known forces (and those which have been most closely studied are electric and magnetic forces) we are at once led to enquire whether the lines of gravitative force are always straight lines radiating from or to the mass round which they centre, or whether, like electric and magnetic lines of force, they have a preference for some media and a distaste for others. We know, for example, that if a magnetic sphere of iron or cobalt or manganese

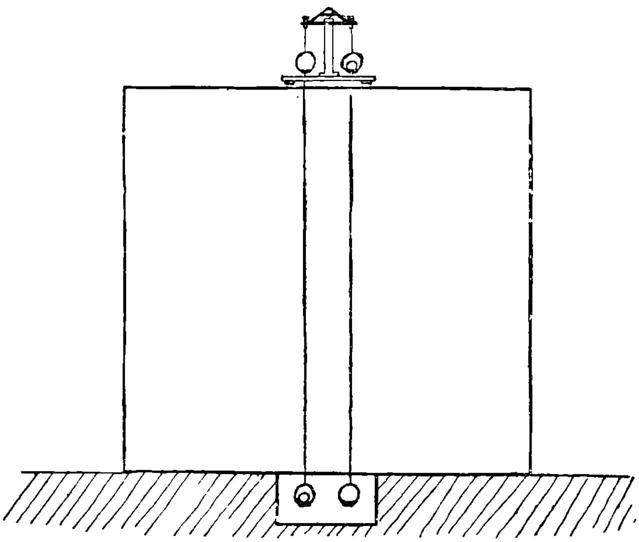


Fig. 4. Common Balance experiment (Richarz and Krigar-Menzel).

is placed in a previously straight field, its permeability is greater than the air it replaces, and the lines of force crowd into it, as in Fig. 5. The magnetic action is then stronger in the presence of the sphere near the ends of a diameter parallel to the original course of the lines of force, and the lines are deflected. If the sphere be diamagnetic, of water, or copper, or bismuth, the permeability being less than that of air, there is an opposite effect, as in Fig. 6, and the field is weakened at the end of a diameter parallel to the lines of force, and again the lines are deflected. Similarly, a dielectric body placed in an electric field gathers in the lines of force, and makes the field where the lines enter and leave stronger than it was before.

If we enclose a magnet in a hollow box of soft iron placed in a magnetic field, the lines of force are gathered into the iron and largely cleared away from the inside cavity, so that the magnet is screened from external action.

Now, common experience might lead us at once to say that there is no very considerable effect of this kind with gravitation. The evidence of ordinary weighings may, perhaps, be rejected, inasmuch as both sides will be equally affected as the balance is commonly used. But a spring balance should show if there is any large effect when used in different positions above different media, or in different enclosures. And the ordinary balance is used in certain experiments in which one weight is suspended beneath the balance case, and surrounded, perhaps, by a metal case, or perhaps, by a water bath. Yet no appreciable variation of weight on that account has yet been noted. Nor does the direction of the vertical change rapidly from place to place, as it would with varying permeability of the ground below. But perhaps the agreement of pendulum results, whatever the block on which the pendulum is placed, and whatever the case in which it is contained, gives the best evidence that there is no great gathering in, or opening out of the lines of the earth's force by different media.

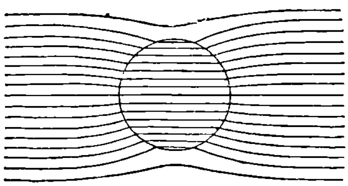


Fig. 5. Paramagnetic sphere placed in a previously straight field.

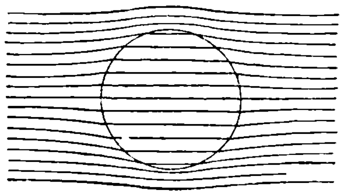


Fig. 6. Diamagnetic sphere placed in a previously straight field.

Still, a direct experiment on the attraction between two masses with different media interposed was well worthy of trial, and such an experiment has lately been carried out in America by Messrs. Austin and Thwing*. The effect to be looked for will be understood from Fig. 7. If a medium more permeable to gravitation is interposed between two bodies, the lines of force will move into it from each side, and the gravitative pull on a body, near the interposed medium on the side away from the attracting body, will be increased.

The apparatus they used was a modified kind of Boys' apparatus (Fig. 8). Two small gold masses in the form of short vertical wires, each ·4 gm. in weight, were arranged at different levels at the ends virtually of a torsion rod 8 mm. long. The attracting masses M_1, M_2 were lead, each about 1 kgm. These were first in the positions shown by black lines in the figure, and were then moved into the positions shown by dotted lines. The attraction was measured first when merely the air and the case of the instrument intervened, and then when various slabs, each 3 cm. thick, 10 cm. wide and 29 cm. high, were interposed. With screens of lead, zinc, mercury, water, alcohol or

* *Physical Review*, vol. 5, 1897, p. 294.

glycerine, the change in attraction was at the most about 1 in 500, and this did not exceed the errors of experiment. That is, they found no evidence of a change in pull with change of medium. If such change exists, it is not of the order of the change of electric pull with change of medium, but something far smaller. Perhaps, it still remains just possible, that there are variations of gravitational permeability comparable with the variations of magnetic permeability in media such as water and alcohol.

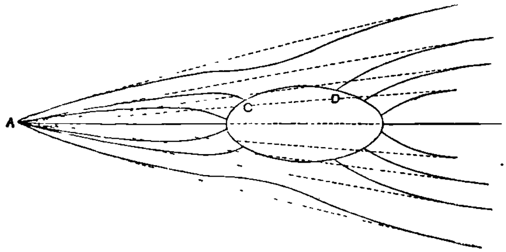


Fig. 7. Effect of interposition of more permeable medium in radiating field of force.

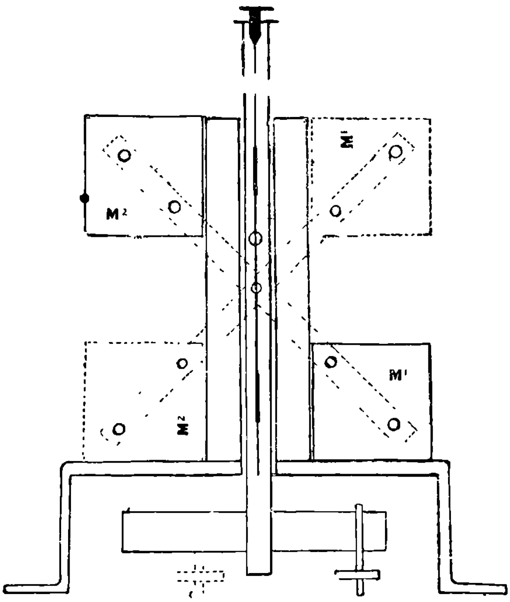


Fig. 8. Experiment on gravitative permeability (Austin and Thwing).

Yet another kind of effect might be suspected. In most crystalline substances the physical properties are different along different directions in a crystal. They expand differently, they conduct heat differently, and they transmit light at different speeds in different directions. We might, then, imagine that the lines of gravitative force spread out from, say, a crystal sphere unequally in different directions. Some years ago, Dr. Mackenzie*

* *Physical Review*, vol. 2, 1895, p. 321.

made an experiment in America in which he sought for direct evidence of such unequal distribution of the lines of force. He used a form of apparatus like that of Professor Boys (Fig. 2), the attracting masses being calc-spar spheres about 2 inches in diameter. The attracted masses in one experiment were small lead spheres about $\frac{1}{2}$ gm. each, and he measured the attraction between the crystals and the lead when the axes of the crystals were set in various positions. But the variation in the attraction was merely of the order of error of experiment. In another experiment the attracted masses were small calc-spar crystal cylinders weighing a little more than $\frac{1}{2}$ gm. each. But again there was no evidence of variation in the attraction with variation of axial direction.

Practically the same problem was attacked in a different way by Mr. Gray and myself*. We tried to find whether a quartz crystal sphere had any directive action on another quartz crystal sphere close to it, whether they tended to set with their axes parallel or crossed.

It may easily be seen that this is the same problem by considering what must happen if there is any difference in the attraction between two such spheres when their axes are parallel and when they are crossed. Suppose, for example, that the attraction is always greater when their axes are parallel, and this seems a reasonable supposition, inasmuch as in straightforward crystallisation successive parts of the crystal are added to the existing crystal, all with their axes parallel. Begin, then, with two quartz crystal spheres near each other with their axes in the same plane, but perpendicular to each other. Remove one to a very great distance, doing work against their mutual attractions. Then, when it is quite out of range of appreciable action, turn it round till its axis is parallel to that of the fixed crystal. This absorbs no work if done slowly. Then let it return. The force on the return journey at every point is greater than the force on the outgoing journey, and more work will be got out than was put in. When the sphere is in its first position, turn it round till the axes are again at right angles. Then work must be done on turning it through this right angle to supply the difference between the outgoing and incoming works. For if no work were done in the turning, we could go through cycle after cycle, always getting a balance of energy over, and this would, I think, imply either a cooling of the crystals or a diminution in their weight, neither supposition being admissible. We are led, then, to say that if the attraction with parallel axes exceeds that with crossed axes, there must be a directive action resisting the turn from the parallel to the crossed positions. And conversely, a directive action implies axial variation in gravitation.

The straightforward mode of testing the existence of this directive action would consist in hanging up one sphere by a wire or thread, and turning the

* *Phil. Trans.* A. 192 (1899), p. 245. *Collected Papers*, Art. 4.

other round into various positions, and observing whether the hanging sphere tended to twist out of position. But the action, if it exists, is so minute, and the disturbances due to air-currents are so great, that it would be extremely difficult to observe its effect directly. It occurred to us that we might call in the aid of the principle of forced oscillations, by turning one sphere round and round at a constant rate, so that the couple would act first in one direction and then in the other, alternately, and so set the hanging sphere vibrating to and fro. The nearer the complete time of vibration of the applied couple to the natural time of vibration of the hanging

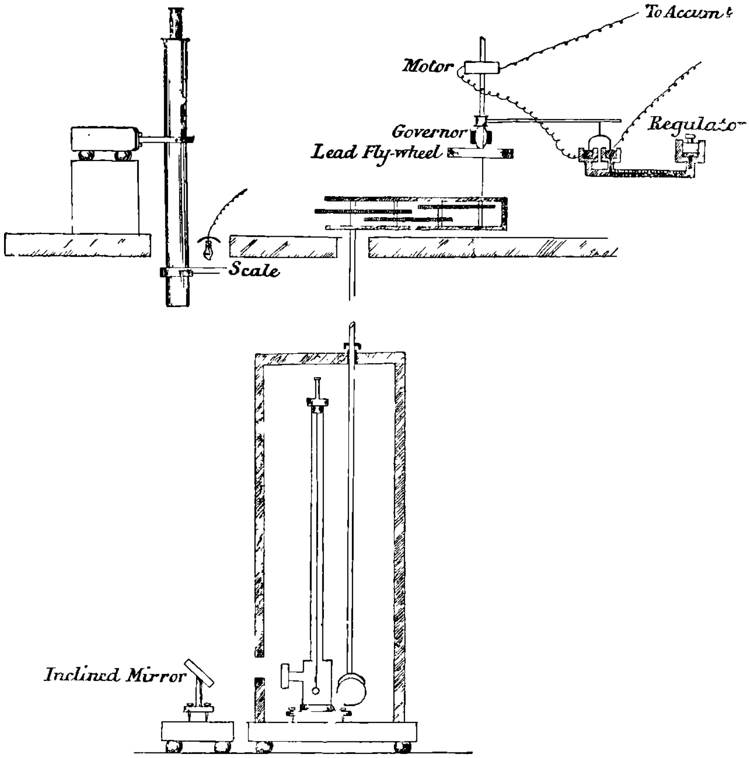


Fig. 9. Experiment on directive action of one quartz crystal on another.

sphere, the greater would be the vibration set up. This is well illustrated by moving the point of suspension of a pendulum to and fro in gradually decreasing periods, when the swing gets longer and longer, till the period is that of the pendulum, and then decreases again. Or by the experiment of varying the length of a jar resounding to a given fork, when the sound suddenly swells out as the length becomes that which would naturally give the same note as the fork. Now, in looking for the couple between the crystals, there are two possible cases. The most likely is that in which the couple acts in one way while the turning sphere is moving from parallel to crossed, and in the opposite way during the next quarter turn from crossed to parallel. That is, the couple vanishes four times during the revolution, and this we may term a quadrantal couple. But it is just possible that a quartz crystal has two ends like a magnet, and that like poles tend to like directions. Then the couple will vanish only twice in a revolution, and may be termed a semicircular couple. We looked for both, but it is enough now to consider the possibility of the quadrantal couple only.

Our mode of working will be seen from Fig. 9. The hanging sphere, ·9 cm. in diameter and 1 gm. in weight, was placed in a light aluminium wire cage with a mirror on it, and suspended by a long quartz fibre in a brass case with a window in it opposite the mirror, and surrounded by a double-walled tinfoiled wooden case. The position of the sphere was read in the usual way by scale and telescope. The time of swing of this little sphere was 120 seconds.

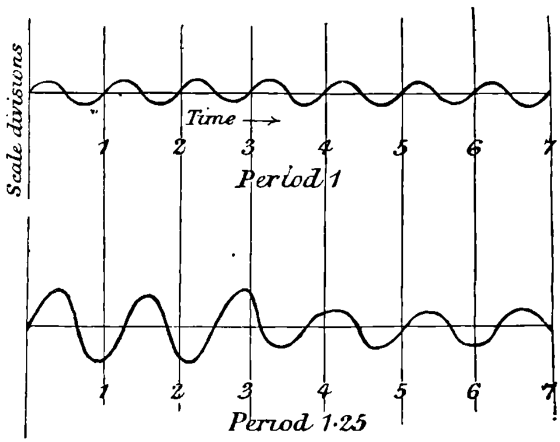


Fig. 10. Upper curve a regular vibration. Lower curve a disturbance dying away.

A larger quartz sphere 6·6 cm. diameter and weighing 400 gms., was fixed at the lower end of an axis which could be turned at any desired rate by a regulated motor. The centres of the spheres were on the same level and 5·9 cm. apart. On the top of the axis was a wheel with 20 equidistant marks on its rim, one passing a fixed point every 11·5 seconds.

It might be expected that the couple, if it existed, would have the greatest effect if its period exactly coincided with the 120-second period of the hanging sphere—i.e. if the larger sphere revolved in 240 seconds. But in the conditions of the experiment the vibrations of the small sphere were very much damped, and the forced oscillations did not mount up as they would in a freer swing. The disturbances, which were mostly of an impulsive kind, continually set the hanging sphere into large vibration, and the latter might easily be taken as due to the revolving sphere. In fact, looking for the couple with exactly coincident periods would be something like trying to find if a fork set the air in a resonating jar vibrating when a brass band was playing all round it. It was necessary to make the period of the couple, then, a little different from the natural 120-second period, and, accordingly, we revolved the large sphere once in 230 seconds, when the supposed quadrantal couple would have a period of 115 seconds.

Figs. 10 and 11 may help to show how this enabled us to eliminate the disturbances. Let the ordinates of the curves in Fig. 10 represent vibrations set out to a horizontal time-scale. The upper curve is a regular vibration of range $\pm$ 3, the lower a disturbance beginning with range $\pm$ 10. The first has period 1, the second period 1·25. Now cutting the curves into lengths equal to the period of the shorter time of vibration, and arranging the lengths one under the other as in Fig. 11, it will be seen that the maxima and the minima of the regular vibration always fall at the same points, so that, taking 7 periods and adding up the ordinates, we get 7 times the range, viz. $\pm$ 21. But in the disturbance the maxima and minima fall at different points, and even with 7 periods only, the range is from + 16 to − 13, or less than the range due to the addition of the much smaller regular vibration.

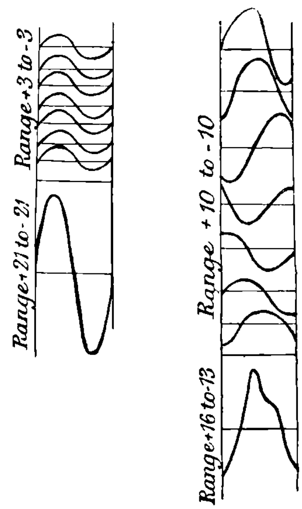


Fig. 11. Results of superposition of lengths of curves in Fig. 10 equal to the period of the regular one.

In our experiment, the couple, if it existed, would very soon establish its vibration, which would always be there and would go through all its values in 115 seconds. An observer, watching the wheel at the top of the revolving axis, gave the time-signals every 11·5 seconds, regulating the speed, if necessary, and an observer at the telescope gave the scale-reading at every signal, that is, 10 times during the period. The values were arranged in 10 columns, each horizontal line giving the readings of a period. The experiment was carried on for about $2\frac{1}{2}$ hours at a time, covering, say, 80 periods. On adding up the columns, the maxima and minima of the couple effect would always fall in the same two columns, and so the addition would give 80 times the swing, while the maxima and minima of the natural swings due to disturbances would fall in different columns, and so, in the long run, neutralise each other. The results of different days' work might, of course, be added together.

There always was a small outstanding effect such as would be produced by a quadrantal couple, but its effect was not always in the same columns, and the net result of about 350 period observations was that there was no 115-second vibration of more than 1 second of arc, while the disturbances were sometimes 50 times as great.

The semicircular couple required the turning sphere to revolve in 115 seconds. Here, want of symmetry in the apparatus would come in with the same effect as the couple sought, and the outstanding result was, accordingly, a little larger.

But in neither case could the experiments be taken as showing a real couple. They only showed that, if it existed, it was incapable of producing an effect greater than that observed.

Perhaps the best way to put the result of our work is this: Imagine the small sphere set with its axis at 45° to that of the other. Then the couple is not greater than one which would take $5\frac{1}{4}$ hours to turn it through that 45° to the parallel position, and it would oscillate about that position in not less than 21 hours.

The semicircular couple is not greater than one which would turn from crossed to parallel position in $4\frac{1}{2}$ hours, and it would oscillate about that position in not less than 17 hours.

Or, if the gravitation is less in the crossed than in the parallel position, and in a constant ratio, the difference is less than 1 in 16,000 in the one case and less than 1 in 2800 in the other.

We may compare with these numbers the difference of rate of travel of yellow light through a quartz crystal along the axis and perpendicular to it. That difference is of quite another order, being about 1 in 170.

As to other possible qualities of gravitation, I shall only mention that quite indecisive experiments have been made to seek for an alteration of mass on chemical combination*, and that at present there is no reason to suppose that temperature affects gravitation. Indeed, as to temperature-effect, the agreement of weight methods and volume methods of measuring expansion with rise of temperature is good, as far as it goes, in showing that weight is independent of temperature†.

So while the experiments to determine G are converging on the same value, the attempts to show that, under certain conditions, it may not be constant, have resulted so far in failure all along the line. No attack on gravitation has succeeded in showing that it is related to anything but the masses of the attracting and the attracted bodies. It appears to have no relation to physical or chemical condition of the acting masses or to the intervening medium.

* Landolt. *Zeit. für Phys. Chem.* vol. 12, 1, 1894. Sanford and Ray, *Physical Review*, vol. 5, 1897, p. 247.

† [See also *Collected Papers*, Art. 5. ED.]

Perhaps we have been led astray by false analogies in some of our questions. Some of the qualities we have sought and failed to find, qualities which characterise electric and magnetic forces, may be due to the polarity, the + and −, which we ascribe to poles and charges, and which have no counterpart in mass.

But this unlikeness, this independence of gravitation of any quality but mass, bars the way to any explanation of its nature.

The dependence of electric forces on the medium, one of Faraday's grand discoveries for ever associated with the Royal Institution, was the first step which led on to the electromagnetic theory of light now so splendidly illustrated by Hertz's electromagnetic waves. The quantitative laws of electrolysis, again due to Faraday, are leading, I believe, to the identification of electrification and chemical separation, to the identification of electric with chemical energy.

But gravitation still stands alone. The isolation which Faraday sought to break down is still complete. Yet the work I have been describing is not all failure. We at least know something in knowing what qualities gravitation does not possess, and when the time shall come for explanation all these laborious and, at first sight, useless experiments will take their place in the foundation on which that explanation will be built.

56.

LE MODE DE PROPAGATION DE L'ÉNERGIE ET DE LA TENSION ÉLECTRIQUE DANS LE CHAMP ÉLECTROMAGNÉTIQUE.

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Introduction.

Avant que Faraday eût découvert le rôle joué par le milieu en électricité, on regardait les actions électriques comme dues aux charges, positives et négatives, répandues à la surface des corps dits *électrisés*. Mais Faraday nous a appris à voir dans ces charges seulement les manifestations, à la surface des conducteurs, d'un certain genre d'altération que nous pouvons appeler une *tension* (strain) *électrique*, existant dans le diélectrique environnant. Il a symbolisé cette altération, cette tension électrique, par les lignes de force s'étendant de conducteur à conducteur, et dont les extrémités correspondent aux charges positives et négatives.

Puis Faraday étendit ses idées au champ magnétique et regarda les lignes de force magnétique comme symbolisant une altération du milieu qu'elles sillonnent.

Quand Maxwell reprit les idées de Faraday, la notion d'énergie avait été développée, et c'est de lui que nous avons appris à regarder les champs électrique et magnétique non simplement comme les sièges d'une altération permettant de rendre compte des forces agissant dans le système, mais comme les sièges d'énergie emmagasinée. Il montra qu'on peut représenter l'énerg'e de tout système électromagnétique de perméabilité et d'inductivité constantes par une certaine distribution dans laquelle la densité de l'énergie est pro portionnelle aux carrés des intensités électrique et magnétique.

Les idées développées ici sur la nature du champ électromagnétique existant autour de conducteurs parcourus par un courant constituent un essai pour étendre les idées de Faraday et de Maxwell, essai d'explication de la façon dont se comportent l'énergie et la tension électrique quand le système n'est pas en équilibre. Dans ces idées, le milieu entourant un conducteur parcouru par un courant électrique est considéré comme le principal siège des actions, le conducteur rendant seulement manifeste leur dernière phase.

Principes généraux.

Localisation de l'énergie. La facilité avec laquelle nous pouvons nous représenter, dans les systèmes électriques et magnétiques, l'énergie comme localisée et comme existant réellement dans chaque centimètre cube en quantité déterminée par une formule de distribution, provient du fait expérimental que nous pouvons mesurer, en chaque point, une quantité bien définie, indépendamment de la disposition du système. Nous pouvons imaginer en un point une paire de plans d'épreuve, les séparer, mesurer leur charge et fixer ainsi la tension électrique et l'intensité électrique en ce point; ou bien nous pouvons imaginer une aiguille aimantée oscillant en un point, compter ses oscillations et connaître ainsi l'induction magnétique et l'intensité magnétique au point considéré. En effectuant ces mesures, nous n'avons pas besoin de savoir comment le champ est produit ou quelle est la distribution des conducteurs ou des aimants.

Il n'en est pas de même pour un système à gravité. On peut, comme l'a montré Maxwell, représenter la distribution de l'énergie potentielle en donnant à sa densité une valeur proportionnelle non à R^2, mais à $C - \frac{R^2}{8\pi}$. où C est une constante et R la force agissant sur l'unité de masse au point considéré. Cette énergie peut être ainsi représentée en fonction d'une quantité mesurable en chaque point du champ, quoiqu'il y ait de grandes difficultés à imaginer un tel mode de distribution.

Quand les corps du système sont en mouvement, l'énergie potentielle est transformée en énergie cinétique, et nous devons supposer aussi pour celle-ci une certaine distribution dans le champ. Mais on ne peut mesurer l'énergie cinétique qu'en observant les changements de position des corps du système, et, même en supposant qu'on imagine un mode de distribution de l'énergie dans le champ qui puisse en représenter correctement l'ensemble, nous n'avons plus cependant, dans ce cas, de quantité mesurable en chaque point qui puisse correspondre à cette distribution.

Au contraire, un système calorifique clos se comporte à ce point de vue comme un système électromagnétique. Nous pouvons mesurer la chaleur qui est mise en jeu dans chaque centimètre cube en observant directement la matière qui s'y trouve, indépendamment de la position des corps environnants, et nous pouvons considérer la chaleur comme résidant dans la matière observée.

Mouvement de l'énergie. Dans un système calorifique, il est naturel d'imaginer que la variation de l'énergie dans un volume donné se produit par l'entrée ou la sortie de chaleur à travers la surface terminale; nous considérons une vitesse de passage de la chaleur à travers une surface, nous traçons des lignes de flux. Si nous supposons que, dans un système électromagnétique, l'énergie est localisée d'une manière analogue, nous serons également autorisés à imaginer que les variations de l'énergie dans un volume donné sont dues aussi à l'entrée ou à la sortie d'énergie à travers la surface, à considérer une vitesse de passage de l'énergie électromagnétique à travers une surface, à mener des lignes de flux. Comme l'expérience montre qu'une variation d'un système électrique entraîne toujours l'existence d'un système magnétique, et qu'une variation d'un système magnétique entraîne toujours l'existence d'un système électrique, nous devons évidemment supposer que tout transport d'énergie électromagnétique à travers une surface est relié à la fois aux conditions électriques et aux conditions magnétiques à la surface.

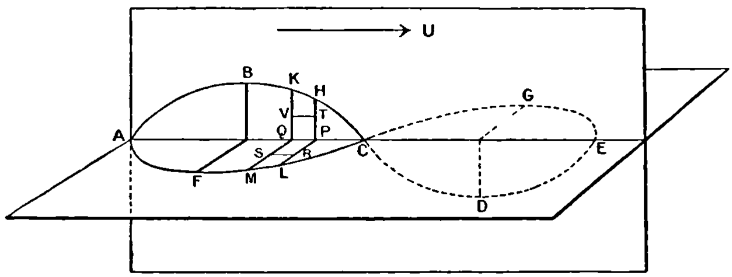

Fig. 1.

Ondes électromagnétiques. Examinons d'abord le cas de la propagation d'ondes électromagnétiques. Pour un tel système, le transport d'énergie d'un point à un autre paraît moins hypothétique que pour tout autre. Il y a intérêt à examiner en détail ce mode de propagation, non seulement au point de vue du transport d'énergie, mais aussi au point de vue de la propagation de la perturbation électrique et de l'induction magnétique: cette étude permettra d'exposer l'idée fondamentale de la théorie de Maxwell: l'équivalence, au point de vue des actions magnétiques, de la variation du déplacement électrique, de la tension électrique, à travers une surface, et d'un courant traversant cette surface, et conduira à une explication de cette équivalence que nous examinerons plus loin.

Supposons un train d'ondes planes polarisées en mouvement dans un milieu de perméabilité μ et de pouvoir inducteur spécifique K, les ondes se déplaçant avec une vitesse uniforme u, sans changer de forme. Représentons par $ABCDE$ (Fig. 1) un train d'ondes de *déplacement électrique* ou de *tension électrique* (strain), cette tension étant dans le plan de la figure, et se déplaçant de gauche à droite avec une vitesse u. D'après la théorie de Maxwell, toute variation dans le nombre des tubes de tension à travers une surface telle que $PRSQ$ est équivalente, au point de vue des actions magnétiques, à un courant à travers cette surface, et si, comme dans la figure, le nombre croît (de bas en haut), il se produit aux limites de cette surface une force magnétomotrice dans le sens inverse du mouvement des aiguilles d'une montre, le long des limites du tube, c'est-à-dire qu'il y a un champ magnétique dans le plan perpendiculaire à la tension électrique. A travers une surface $PTVQ$ située dans le plan de la figure ne passent au contraire pas de tubes de tension, il ne s'y produit pas de variation de tension, et il n'existe pas de champ magnétique dans ce plan. Ainsi, les ondes électriques sont accompagnées d'ondes magnétiques dont le plan est perpendiculaire au leur. Représentons ces dernières par $AFCGE$. Nous pouvons appeler *plan électrique* et *plan magnétique* les plans respectifs des deux ondes. Supposons que $PRSQ$ représente 1 cm.² dans le plan magnétique. Soit $\mathfrak{D}$ le déplacement électrique ou tension électrique en P, et $\mathfrak{D}'$ en Q. Le nombre de tubes de force quittant par seconde $PQRS$ à travers PR, limite antérieure de la surface, est $u \,.\, \mathfrak{D} \,.\, PR$, ou $u \,.\, \mathfrak{D}$, et le nombre de tubes entrant par la limite opposée, par seconde, est $u \,.\, \mathfrak{D}'$. La vitesse d'accroissement de tension électrique à travers cette aire, qui est, au point de vue des actions magnétiques, équivalente à un courant, est $u(\mathfrak{D}' - \mathfrak{D})$. Soient $\mathfrak{H}$ l'intensité magnétique en P et $\mathfrak{H}'$ en Q; l'intégrale linéaire de l'intensité magnétique à travers $PQSR$ est $\mathfrak{H}' - \mathfrak{H}$. Ainsi,

$$\mathfrak{H}' - \mathfrak{H} = 4\pi u(\mathfrak{D}' - \mathfrak{D}). \quad \ldots\ldots\ldots\ldots\ldots\ldots(1)$$

Considérons maintenant la variation de l'induction magnétique à travers 1 cm.² du plan électrique, $PTVQ$; soient $\mathfrak{B}$ l'induction magnétique en P, et $\mathfrak{B}'$ en Q. Le nombre de tubes d'induction magnétique quittant $PTVQ$ par seconde à travers PT est $u \,.\, \mathfrak{B}$, et le nombre de tubes entrant dans $PTVQ$ à travers QV est $u \,.\, \mathfrak{B}'$. L'accroissement de l'induction magnétique à travers cette surface est donc $u(\mathfrak{B}' - \mathfrak{B})$, et elle est égale à l'intégrale de l'intensité électrique autour de $PQVT$; si E est l'intensité électrique en P et E′ en Q, on a ainsi

$$\mathrm{E}' - \mathrm{E} = u(\mathfrak{B}' - \mathfrak{B}). \quad \ldots\ldots\ldots\ldots\ldots\ldots(2)$$

En multipliant (1) et (2) membre à membre, et utilisant les relations

$$K\mathrm{E} - 4\pi\mathfrak{D}, \qquad K\mathrm{E}' - 4\pi\mathfrak{D}',$$

$$\mu\mathfrak{H} = \mathfrak{B}, \qquad \mu\mathfrak{H}' = \mathfrak{B}',$$

on obtient

$$u^2 K\mu = 1$$

ou

$$u = \frac{1}{\sqrt{K\mu}}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3)$$

Ainsi, d'après ce qui précède, on peut regarder à la fois l'intensité magnétique suivant PR comme due au balayage par cette ligne des tubes de tension électrique, et l'intensité électrique suivant PT comme due au balayage par cette ligne des tubes d'induction magnétique.

Si au lieu de considérer 1 cm.² on prend une bande ayant une largeur de 1 cm., avec une extrémité en P et l'autre très loin, dans une région où la perturbation ne se fait plus sentir, on a évidemment

$$\mathfrak{J} = 4\pi u\mathfrak{D} = Ku\mathrm{E}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4)$$

$$\mathrm{E} = u\mathfrak{B} = \mu u\mathfrak{J}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(5)$$

formules qui donnent plus clairement la relation entre l'intensité d'une part, et le balayage des tubes de force d'autre part.

D'après la théorie de Maxwell, la quantité d'énergie par centimètre cube en P, correspondant à la tension électrique, est

$$\frac{K\mathrm{E}^2}{8\pi} = \frac{K\mathrm{E}\mu u\mathfrak{J}}{8\pi} \quad \frac{K\mu u\mathrm{E}\mathfrak{J}}{8\pi},$$

et celle correspondant à l'induction magnétique

$$\frac{\mu\mathfrak{J}^2}{8\pi} = \frac{\mu\mathfrak{J}Ku\mathrm{E}}{8\pi} - \frac{K\mu u\mathrm{E}\mathfrak{J}}{8\pi},$$

c'est-à-dire les énergies électrique et magnétique sont égales en chaque point. Le total par centimètre cube est

$$\frac{K\mu u\mathrm{E}\mathfrak{J}}{4\pi}.$$

Si nous supposons que cette énergie se déplace avec une vitesse u, la quantité d'énergie qui traverse la face antérieure d'un centimètre cube est

$$\frac{uK\mu u\mathrm{E}\mathfrak{J}}{4\pi},$$

ce qui, d'après (3), peut s'écrire

$$\frac{\mathrm{E}\mathfrak{J}}{4\pi}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots 6$$

Loi générale du transport de l'énergie. La forme de cette expression de l'énergie transportée, ou valeur de l'action d'une partie du milieu sur les parties voisines, suggère l'idée qu'elle représente une loi générale pour le cas où les intensités électrique et magnétique sont rectangulaires. Nous pouvons vraisemblablement regarder la tension électrique comme l'analogue de la force élastique, et l'induction magnétique comme l'analogue de la vitesse. La transmission d'énergie mécanique exige à la fois la force et la vitesse, et la valeur de l'énergie transmise dépend du produit de ces deux quantités. Si l'analogie précédente a sa raison d'être, nous pouvons penser que la valeur de l'énergie électromagnétique transportée dépend du produit des intensités électrique et magnétique, ce qui correspond bien au résultat que nous venons d'obtenir. Remarquons que l'énergie se propage dans une direction per pendiculaire au plan des deux intensités, et obtenue à partir de E par un mouvement de vis à droite autour de $\mathfrak{J}$.

Conducteur transportant un courant stationnaire. Voyons maintenant si la loi de transport $\frac{\mathrm{E}\mathfrak{J}}{4\pi}$ peut s'appliquer à l'énergie mise en jeu dans un autre système électromagnétique où E et $\mathfrak{J}$ sont à angle droit, le système étant constitué par un fil parcouru par un courant constant C. Si R est la résistance par centimètre, la chaleur développée par seconde sur une longueur de 1 cm. du fil est $C^2R = C\mathrm{E}$, si E représente l'intensité électrique le long de l'axe du fil. A la surface extérieure du fil, on peut supposer que l'intensité a la même valeur et est parallèle à l'axe; d'ailleurs, l'intensité magnétique à la surface est perpendiculaire au rayon; en décrivant autour du fil un cercle de rayon r, on aura

$$2\pi r\mathfrak{J} = 4\pi C$$

ou

$$\mathfrak{J} = \frac{2C}{r} \qquad \text{.................................(7)}$$

Une surface cylindrique ayant pour base ce cercle et pour hauteur 1 cm. a pour aire $2\pi r$. Partout, sur cette surface, E et $\mathfrak{J}$ sont rectangulaires, et si l'on suppose que l'énergie se propage dans une direction perpendiculaire à E et $\mathfrak{J}$, et avec le même sens que dans le trièdre obtenu dans le cas précédent, on voit que l'énergie se propage vers l'intérieur du fil, la quantité d'énergie qui passe par seconde à travers la surface cylindrique considérée étant

$$\frac{2\pi r\mathrm{E}\mathfrak{J}}{4\pi} = C\mathrm{E},$$

et représentant ainsi la quantité de chaleur apparue par seconde dans 1 cm. de longueur du fil. Ainsi, l'application de la loi rencontrée plus haut, au cas d'un fil parcouru par un courant, conduit au résultat suivant: l'énergie qui est mise en jeu sous forme de chaleur dans le fil entre dans ce fil latéralement, venant du diélectrique environnant, et est la somme des énergies électrique et magnétique; elle n'est pas transmise le long du fil.

Cas général. Il n'est pas nécessaire d'exposer ici le cas général d'un système électromagnétique quelconque. Il suffit de dire que, si le milieu est en repos, l'intégrale cubique de l'accroissement de la somme des énergies électrique et magnétique, du travail effectué par l'action des forces électromagnétiques sur la matière, de la chaleur développée par les courants dans les conducteurs, par seconde, à l'intérieur d'une surface fermée, peut être transformée en une intégrale superficielle dont la valeur élémentaire par centimètre carré est donnée en chaque point par l'expression $\frac{\mathrm{E}\mathfrak{J}\sin\theta\cos\phi}{4\pi}$, où θ représente l'angle de E avec $\mathfrak{J}$ et ϕ l'angle de la normale au plan (E, $\mathfrak{J}$) avec la normale à la surface au point considéré; cette expression doit être comptée positivement si le mouvement de rotation vers la droite effectué autour de $\mathfrak{J}$ à partir de E est dirigé vers l'intérieur de la surface. Cela peut s'interpréter ainsi: les variations de l'énergie du système électromagnétique peuvent être considérées comme provenant d'une action du diélectrique qui se trouve d'un côté de la surface terminale sur le diélectrique situé de l'autre côté, la quantité d'énergie qui est transmise par seconde à travers 1 cm.² de la surface étant représentée par l'expression $\frac{E\mathfrak{J} \sin\theta \cos\phi}{4\pi}$. Si l'élément de surface contient E et $\mathfrak{J}$, l'énergie transmise est $\frac{E\mathfrak{J} \sin\theta}{4\pi}$, et a la plus grande valeur possible pour les éléments de surface menés par le point considéré. Si pour un moment on se représente l'énergie comme un fluide, cela implique que ce fluide coule normalement à l'élément contenant E et $\mathfrak{J}$; et, s'il s'agit d'un système constant avec des surfaces de niveau électriques et magnétiques, les lignes de courant sont les intersections de ces deux familles de surfaces*.

La loi précédente rend compte de la distribution de l'énergie dans un système électromagnétique; cela ne veut nullement dire qu'elle constitue l'unique solution du problème. En effet, il est évident qu'on peut ajouter au flux indiqué par la loi tout flux n'altérant pas la quantité d'énergie existant dans chaque centimètre cube,—mettons la solution d'un problème d'Hydrodynamique où le fluide homogène et continu est remplacé par une énergie homogène. Pour obtenir une solution unique, il serait sans doute nécessaire de faire quelque hypothèse sur la nature des actions électriques et magnétiques.

En l'absence d'hypothèses complètement satisfaisantes, nous devons nous contenter d'analogies. Comme nous l'avons déjà indiqué, la quantité d'énergie est proportionnelle au produit de l'intensité électrique correspondant probablement à la force, et de l'intensité magnétique correspondant probablement au mouvement, de sorte que la loi régissant la transmission de l'énergie dans un système électromagnétique est analogue à celle qui régit la transmission de l'énergie mécanique dans un système soumis à des actions mécaniques. Quoique nous ne puissions pas prouver que, dans le cas actuel, cette loi est la loi naturelle, nous avons cependant de fortes présomptions en sa faveur, et du moins nous pouvons la considérer comme une hypothèse satisfaisante.

Exemples. Nous pouvons maintenant appliquer à quelques exemples familiers les principes qui viennent d'être exposés.

1°. *Décharge lente d'un condensateur.* Un exemple dans lequel nous allons pouvoir analyser le transport d'énergie nous est fourni par le cas de la décharge lente d'un simple condensateur à plateaux parallèles, dont les plateaux sont réunis par un fil de très grande résistance.

* *Phil. Trans.* Part II, 1884, p. 343. [*Collected Papers*, Art. 10.] Le théorème a été étendu, par Heaviside, au cas où le milieu est en mouvement. *Voir* Heaviside, *Electromagnetic Theory*, t. 1, p. 80.

Je puis citer ici une publication antérieure*:

'Soient A et B (Fig. 2) les armatures planes du condensateur, A étant électrisée positivement et B négativement. Avant la décharge, les surfaces équipotentielles seront à peu près comme les indique la figure. La plus grande partie de l'énergie réside dans le diélectrique compris entre les deux plateaux, mais il peut y en avoir partout où se font sentir les actions électriques. Le champ électrique entre A et B est dirigé de A vers B; partout il est normal aux surfaces de niveau. Supposons maintenant qu'on réunisse A et B par un fil de très grande résistance disposé suivant une ligne de force, et dont la résistance est choisie de manière qu'elle soit la même pour la même chute de potentiel, d'un bout à l'autre; nous faisons cette hypothèse sur la distribution de la résistance pour que les surfaces de niveau ne soient pas troublées par le passage du courant; le fil est supposé si fin que la décharge s'opère avec une extrême lenteur.

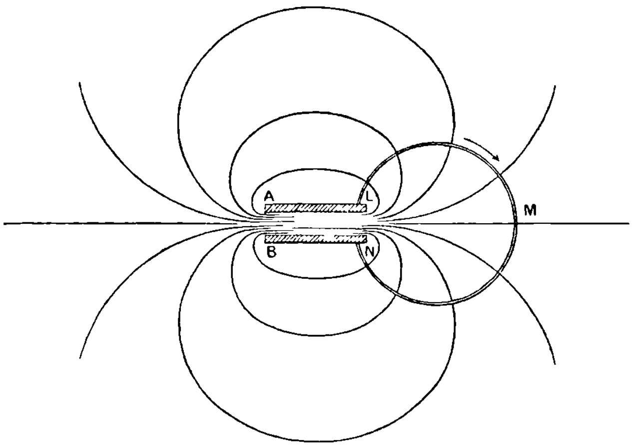

Fig 2.

'Quand la décharge donne dans le fil LMN un courant ayant le sens de la flèche, il se produit en même temps de B vers A un courant de déplacement égal. Le courant peut être indiqué par les lignes de force magnétique, qui forment des lignes fermées embrassant le circuit. Leur direction autour du fil est de droite à gauche en avant, et dans l'espace compris entre A et B, de gauche à droite par devant. La force électromotrice va toujours des surfaces de niveau les plus élevées vers les plus basses, c'est-à-dire de A vers B, aussi bien près du fil que dans l'espace compris entre A et B.

'D'ailleurs, puisque l'énergie se meut toujours normalement à la force électromotrice, elle doit se déplacer suivant les surfaces de niveau. Puisqu'elle se meut aussi normalement aux lignes d'intensité magnétique, elle

* *Phil. Trans.* Part II, 1884, p. 351. [*Collected Papers*, Art. 10, p. 183.]

pénètre, comme nous l'avons déjà vu, latéralement dans l'intérieur du fil, où elle est entièrement convertie en chaleur, si nous supposons la décharge si lente que le courant puisse être envisagé comme constant pendant tout le temps considéré. Mais entre A et B la force électromotrice est de sens inverse à celui du courant, tandis que l'intensité magnétique y a avec le courant la même relation que pour le fil; si nous nous rappelons que la force électromotrice, l'intensité magnétique et la direction du flux d'énergie sont reliées par la relation de vis à droite, nous voyons que l'énergie se meut de l'espace compris entre A et B vers l'extérieur. Ainsi la tension du diélectrique entre A et B se résout graduellement en formant ce que nous appelons le *courant de décharge* le long du fil LMN; l'énergie ainsi mise en jeu se meut vers l'extérieur à travers le diélectrique, suivant toujours les surfaces équipotentielles, et converge graduellement jusqu'à ce qu'elle arrive sur le ci cuit où ces surfaces sont coupées par le fil; elle est alors trai sformée en chaleur. Il est à noter que, si le courant peut être considéré comme constant, l'éne gie se déplace en restant toujours sur les mêmes surfaces de niveau.'

2°. *Circuit contenant une pile.* Si nous considérons un circuit dans lequel une pile maintient un courant constant, nous pouvons regarder la pile comme jouant par rapport au circuit extérieur le même rôle que le condensateur chargé dans le cas précédent, mais avec cette différence que l'énergie est fournie d'une manière continue, et que la décharge n'a pas besoin d'être supposée lente. Faisant abstraction des différentes parties qui constituent la pile et des actions qui s'y produisent, nous pouvons considérer la pile comme un système entretenant, ainsi que le condensateur, un champ élec trique, et comme la source d'une énergie qui se propage continuellement en s'éloignant d'elle, le long des surfaces de niveau, et qui pénètre laté alement dans le fil.

Nous sommes ainsi conduits à regarder le courant comme, au sens propre du mot, un *axe de puissance,* pour employer un terme de Faraday. L'énergie qui se manifeste dans le fil conducteur n'est pas transmise le long du fil, mais y pénètre latéralement, venant du diélectrique environnant. Son entrée dans le fil est seulement son stade final; elle y est dissipée et transformée en chaleur. Le fil conducteur est ainsi au système électromagnétique ce que le réfrigérant est à la machine thermique, un dispositif nécessa re à la mise en jeu de l'énergie, mais recevant de l'énergie qui n'est plus utilisable. Peut-être pouvons-nous trouver une analogie plus complète dans une ma h ne pouvant mettre de l'eau en mouvement au moyen de palettes. Si les p le te sont immobilisées, la machine peut être tendue et accumuler ainsi u e ce ta ne énergie de tension, mais il ne se produit ni mouvement, ni transport, ex e ment de même que dans un circuit électrique incomplet peut exister u e certaine tension électrique, une énergie électrique, mais fixe. M is quand les liaisons qui existaient sont supprimées et que les palettes peuvent tourner, il y a production de tension et d'énergie cinétiques, et le trai sport d'énerg e commence. L'énergie passe continuellement dans l'eau, qui ne peut l'emmagasiner, et s'y transforme en chaleur. De même, quand le circuit électrique est complété, la machine électromagnétique commence à travailler, il y a production simultanée d'énergie électrique et d'énergie magnétique; cette énergie est transportée dans le fil qui ne peut l'emmagasiner sous sa forme électromagnétique, et où elle est transformée en chaleur.

Mouvement de la tension électrique et des tubes de force électrique.

Passons maintenant de la considération du transport d'énergie dans un champ électromagnétique à celle du transport de la tension électrique*. En nous occupant de la propagation des ondes électromagnétiques, nous nous sommes inspirés de l'idée de Maxwell, qu'un changement dans la densité de la tension électrique à travers une surface est équivalent, au point de vue des actions magnétiques, à un courant traversant cette surface. La loi fondamentale de la distribution électrique dans un système en équilibre est qu'il existe toujours des charges égales et contraires, réunies d'élément à élément par des tubes de tension électrique traversant le milieu et tels que la tension à travers une section droite est constante le long du tube et égale à la charge électrique à chaque extrémité. Si nous supposons que cette loi s'applique encore quand les charges sont en mouvement, par exemple, dans des systèmes tels que ceux que nous avons examinés jusqu'ici, nous pouvons donner une explication de cette équivalence, au point de vue des actions magnétiques, d'un courant électrique et d'une variation de la tension.

Exemples. Appliquons encore ces principes à quelques exemples.

1°. *Charge d'un condensateur.* Supposons une pile de force électromotrice V, dont les pôles ne sont pas reliés entre eux. Un des pôles est chargé positivement, l'autre négativement. Si les pôles se font face, nous pouvons nous les figurer comme reliés par des tubes de tension électrique passant à travers l'air, et allant du pôle positif au pôle négatif. Imaginons les deux pôles reliés aux plateaux d'un condensateur; l'électricité positive passe alors sur l'un des plateaux et l'électricité négative sur l'autre, les charges s'écoulant parallèlement sur les deux fils de jonction, et les tubes de tension qui les joignent s'avançant à travers l'air; à la fin les deux plateaux du condensateur sont chargés positivement et négativement, leur différence de potentiel ayant la même valeur V que celle qui existe entre les pôles. Si C est la capacité du condensateur, devant laquelle on suppose négligeable celle des fils, le flux total qui a passé en chaque point du fil positif est $Q = CV$. Un flux négatif de valeur égale, mais de sens contraire, passe dans l'autre fil et équivaut, au point de vue des actions magnétiques, à un courant positif continuant

* *Voir* pour plus de détails: *Phil. Trans.* Part II, 1885, p. 277 [*Collected Papers*, Art. 11]; et J.-J. Thomson, *Recent Researches*, p. 3: 1893: *Les tubes de Faraday.*

la direction du premier, de sorte que, si l'on imagine autour de chaque fil un circuit circulaire, l'intégrale linéaire de l'intensité magnétique pour chaque circuit est $4\pi Q$. Si nous traçons de plus un circuit annulaire autour des tubes de tension électrique existant entre les armatures du condensateur, nous devons supposer, d'après la théorie de Maxwell, que, pendant la charge, il s'est produit entre les armatures un courant équivalent, de sorte que l'intégrale linéaire de l'intensité magnétique pour le troisième circuit est aussi $4\pi Q$.

D'ailleurs, quand les charges $+ Q$ et $- Q$ se déplacent le long des fils, les tubes de force correspondants doivent se déplacer dans l'air interposé et balayer sur leur chemin le condensateur, et les trois circuits que nous avons imaginés sont coupés par les mêmes tubes. Nous sommes amenés ainsi à supposer que le champ magnétique existant autour des fils et du condensateur est une manifestation de l'action du mécanisme par lequel se propage la tension électrique, et avec elle l'énergie électromagnétique, de la pile au condensateur. L'équivalence, au point de vue des actions magnétiques, d'un courant dans un conducteur et d'un accroissement de tension dans un diélectrique correspond au mouvement de la tension électrique à travers tout circuit environnant. Que cette tension puisse persister et être emmagasinée dans un condensateur, ou qu'elle décroisse lentement, l'énergie correspondante se dissipant dans un fil, peu importe, si l'on ne s'occupe que du diélectrique à travers lequel elle se propage. Si nous acceptons ces idées, nous voyons comment se justifie l'application de l'équivalence d'un courant et d'une variation de tension dans l'étude du mode de propagation des ondes électromagnétiques. L'intensité magnétique autour de l'aire $PQRS$ (Fig. 1) est une manifestation de l'action du mécanisme par lequel la tension électrique se propage dans cette surface.

2°. *Courant stationnaire fourni par une pile.* Revenons maintenant à la pile et supposons que le condensateur n'existe plus et que le circuit soit fermé par un fil à travers lequel circule un courant constant i. Nous devons alors supposer que $+ i$ sort du pôle positif par seconde, $- i$ du pôle négatif, que ces charges sont réunies à travers l'air par des tubes de tension, et que ces charges positive et négative cheminent l'une vers l'autre le long des fils, tandis que les tubes de tension balaient l'air et finalement pénètrent dans le fil. Ce qui caractérise essentiellement le fil, à notre point de vue actuel, est qu'il est capable de recevoir la tension électrique, mais non de l'emmagasiner. Il joue vis-à-vis du diélectrique, par rapport à la tension électrique, le même rôle qu'un liquide vis-à-vis d'un solide, par rapport à l'action d'une scie: on peut scier un liquide, mais le travail effectué en sciant ne subsiste pas, la déformation produite s'évanouit, et l'énergie est dissipée en chaleur.

Ainsi dans le circuit voltaïque la tension électrique se propage du diélectrique environnant dans le fil, où elle disparaît, et une nouvelle tension tend à venir latéralement lui succéder. Si nous imaginons un circuit fermé autour du fil, le même nombre de tubes le balaie, quelles que soient sa forme et sa position, pourvu qu'il soit traversé par le circuit. Nous pouvons regarder l'intégrale de l'intensité magnétique autour du circuit comme mesurant le mouvement du mécanisme, et la relation $\int \mathfrak{J} ds - 4\pi i$ devient ainsi l'expression de la proportionnalité existant entre ce mouvement et le nombre de tubes de tension électrique coupant le circuit fermé par seconde vers l'intérieur.

Le mouvement d'un tube à chaque instant et son mode de propagation à l'intérieur du fil peuvent sans doute dépendre de la forme du circuit et de la disposition des conducteurs et diélectriques environnants. Dans certains cas, nous pourrons nous figurer les tubes comme entrant dans le fil par leurs extrémités et y étant ainsi absorbés graduellement; dans d'autres cas, par exemple celui d'un long fil rectiligne avec ses extrémités équidistantes des pôles, nous pourrons nous les représenter comme se mouvant parallèlement au fil et y pénétrant par tous les points en même temps. Mais les différences qualitatives entre les électricités positive et négative, différences non seulement en signe, dont tant de preuves sont maintenant accumulées, ne nous permettent pas de dire que leurs extrémités positives et négatives sortent de la pile avec la même vitesse.

Il nous faut dire maintenant comment se fait la dissipation de la tension dans le conducteur. Dans les électrolytes cette disparition est accompagnée d'un mouvement des ions suivant les lignes de courant, et, d'après J.-J. Thomson, il y aurait un semblable mouvement d'ions suivant les lignes de courant dans un conducteur. Les ions suivraient le fil en recevant l'énergie qui leur est nécessaire latéralement, du diélectrique.

Nous devons remarquer que nous aurions pu nous occuper aussi des tubes d'induction magnétique, nous les figurer comme se resserrant vers le conducteur, et maintenant par leur mouvement la force électromotrice dans le champ. Mais les tubes électriques existants, que le circuit soit complet ou non, paraissent correspondre à une notion plus fondamentale, et l'idée que nous pouvons nous faire de leur propagation est plus définitive.

3°. *Courant sans champ magnétique dû aux pertes dans un condensateur.* L'idée que le champ magnétique qui entoure un courant est dû au mouvement du mécanisme par lequel la tension électrique se propage vers le conducteur sera peut-être illustrée plus clairement par l'examen d'un cas de courant sans champ magnétique. Lorsqu'un condensateur chargé se décharge lentement par fuite à travers le diélectrique, l'idée ordinaire qu'on se fait de ce phénomène est qu'il se produit de l'armature positive à l'armature négative un courant ordinaire; d'ailleurs, le décroissement graduel de la tension électrique équivaut à un courant de retour de l'armature négative à l'armature positive. Les deux courants s'effectuant suivant les mêmes lignes en sens inverse n'ont aucune action magnétique extérieure. Mais, à notre point de vue, nous n'avons pas du tout à introduire le courant ordinaire, le phénomène consiste en un décroissement de la tension électrique non pas par propagation au dehors, mais par modification sur place. Un circuit environnant n'est nullement balayé par des tubes de tension, et il n'y a pas de champ magnétique autour du condensateur. Il n'y a pas à compenser de courant ordinaire, et nous n'avons à considérer que la destruction graduelle de la tension électrique.

4°. *Courant de retour par la terre.* Un autre exemple de nos idées peut être fourni par le *courant de retour par la terre* dans le télégraphe. Si le pôle positif de la pile est relié à la ligne et le pôle négatif au sol, nous pouvons nous représenter le phénomène ainsi : les tubes de tension balaient l'air entre le fil et le sol, leurs extrémités positives se déplaçant le long du fil et leurs extrémités négatives le long du sol. Ainsi ce qu'on appelle le *retour par la terre* correspond seulement à un moyen simple et commode de permettre à l'extrémité négative de se réunir à l'extrémité positive à l'autre station.

5°. *Courants alternatifs.* Un dernier exemple nous sera donné par la localisation superficielle des courants alternatifs. Dans le cas d'un courant constant, la tension électrique est détruite avec la même rapidité en tous les points de la section du fil. Mais ce régime ne peut s'établir qu'au bout d'un certain temps. Au moment où le circuit vient d'être fermé, les tubes de tension sont dans les couches superficielles et peuvent s'affaiblir ou disparaître avant d'être arrivés à une profondeur notable. Si, immédiatement, l'arrivée des tubes de sens déterminé cesse, qu'à ces tubes succèdent aussitôt des tubes de direction inverse et que cette alternance continue, la tension pourra arriver dans le fil en quantité considérable, tout en y pénétrant peu.

Nous trouvons un phénomène analogue en considérant un liquide dans lequel se meuvent deux plateaux parallèles animés de mouvements de va-et-vient. Si les plateaux se meuvent parallèlement avec une vitesse constante, le liquide est *scié* de la même manière en tous ses points, et la chaleur développée en tous ses points est la même ; mais, si l'un des plateaux est fixe et si l'autre se meut rapidement, la partie sciée est limitée à la couche en contact avec lui. Une autre analogie peut être trouvée dans la conduction de la chaleur : si l'une des extrémités d'un barreau isolé est à une température constante, le flux de chaleur à travers chaque section du barreau est le même. Mais si, à cette extrémité, la température oscille rapidement, les ondes qui voyagent le long du barreau sont d'amplitude rapidement décroissante.

Si donc la localisation superficielle des courants alternatifs trouve une explication dans la théorie ordinaire, elle en trouve une plus simple si l'on regarde le diélectrique comme agissant sur le fil. Dans le cas d'un courant constant, il agit d'une manière uniforme ; dans le cas d'un courant alternatif, il agit par saccades et envoie dans le fil des ondes de tension d'amplitude rapidement décroissante, qui s'annulent pratiquement à une faible profondeur.

57.

THE TRANSFORMATION AND DISSIPATION OF ENERGY.

[*The Inquirer*, 1902, pp. 627–628.]

Autumn is the stock-taking time of Science. Every September the addresses and lectures given at the meeting of the British Association tell us of some of the recent advances made and of new outlooks attained. This year the President, Professor Dewar, gave a most interesting account of one of the greatest advances in Physical Science in our time, the development of the methods of producing low temperatures and the investigation of the properties of matter at those low temperatures, a subject in which he is among the foremost workers. He is engaged in exploring a region not far above the absolute zero of cold, than which we can imagine nothing colder. I propose in this article to give some account of the theory of the transformation of energy which leads us to the belief that there is this absolute zero, this starting point of temperature below which we cannot go. In another article I shall say something of another great advance, the discovery and investigation of the new radiations with which Professor Thomson dealt in the Friday evening lecture, and I shall give some account of the theory of the constitution of the atoms which Professor Thomson has set forth.

Temperatures on the Mercury Thermometer.

An ordinary mercury thermometer gives what we may call an arbitrary scale, since it depends for its indications on the behaviour of two arbitrarily chosen substances, mercury and glass. It depends on the fact that mercury expands more than the glass tube containing it, and dividing the excess expansion between the temperatures of melting ice and boiling water into 100 equal steps, each step is a degree on the mercury-in-glass scale. Most other substances expand nearly equally for each step upwards, each degree, of the mercury thermometer, though, of course, the length of step for one substance may be quite different from that for another. But not quite equally, and some substances, of which water is a very notable example, get hopelessly out of step with mercury. Nor is glass a consistent expander. If we take two mercury thermometers made of different kinds of glass they give slightly different readings when exposed to the same temperature. Then again, mercury freezes at about 40° below melting ice, and at some 400° or 500° above that temperature it evaporates so much that it bursts the containing tube. So that not only is the common scale of temperature arbitrary as depending on the properties of one or a pair of particular substances, but it is very limited—too limited in one direction for use on Arctic expeditions, too limited in the other for manufacturers who wish to measure the temperatures of their furnaces. Further, it affords no sign of a beginning.

Temperatures on the Gas-Scale.

The first idea of a definite general scale of temperature came from the discovery that various gases expand by almost exactly equal amounts for equal rises of temperature. Imagine, for instance, two ordinary thermometers, one containing air instead of mercury, but open at the top of the tube to allow the air to expand and with a little plug or pellet of mercury separating the air inside from the air outside, this plug sliding up and down the tube as the air expands and contracts, and telling the volume of the inside air. Let the other thermometer be just the same size, but let it contain hydrogen instead of air. Then if the two start level at any one temperature, as the temperature is altered, they will advance or recede almost exactly together.

If we imagine the bulbs so big that they hold 273 cubic inches of gas at the temperature of melting ice, each gas will expand to 373 cubic inches at the temperature of boiling water, and if each inch added is taken as marking a degree we have the gas-scale. Oxygen, nitrogen, hydrogen, helium, and others agree very nearly in this equality of expansion, and the gas degrees agree very nearly with mercury degrees between 0 C. and 100 . But the scale can be prolonged upwards as high as any vessel can be made to withstand the temperature and hold the gas, and it can be prolonged downwards, the gases still marching together far away beyond the freezing point of mercury. If we continued to cool the gases they would occupy 272 inches at − 1°, 271 inches at − 2°, and so on, so that if we could only take 273 such steps their volume would vanish, and we should get to the very bottom of the scale. This behaviour of g ses gave the first definite idea of a zero of temperature and pointed to its existence, no great way below the temperatures with which we are familiar. But accurate researches on the properties of gases show that they do not contract quite exactly *pari passu* as they cool. Now too we know that ultimately, and each at a different temperature, every gas will condense to a liquid and then freeze to a solid, and show no sign at all of vanishing away. So that after all the gas-scale is really arbitrary; for as the different gases do not expand or contract exactly alike we must select one particular gas as our standard. In practice too the scale is probably limited by the gas ceasing to be a gas and becoming a liquid.

The Absolute or Work Scale.

About fifty years ago Lord Kelvin, when he was developing the theory of such transformations of energy as we have in the steam-engine, showed that we could imagine a scale of temperature quite definite, quite independent of any particular substance, a scale derived not from the measurement of the expansion of heated bodies but from the work obtained from the heat imparted to produce that expansion. This work-scale has a very definite starting point below which we cannot think it possible to go, and if there are, as usual, 100 steps or degrees from boiling water to melting ice, there are almost exactly 273 steps more down to the absolute zero. The gases then did point in the right direction. How this transformation from heat to work gives us a scale of temperature we may see by considering the corresponding

Transformation of Position-Energy to Work.

If we have a reservoir of water at a height above the general level, that water has potential energy, or, as it is better to call it, position-energy, since it depends on the position of the water above the earth. We can get some of the position-energy out of the water by allowing the water to turn a water-wheel. Water-wheels, as a rule, are not very efficient machines; there is usually a great deal of leak from them, a great deal of useless agitation of the water, and a great deal of friction. But on paper we can make them as perfect as we like. So we will build one with its highest point just level with the reservoir and its lowest just level with the pool at the bottom. It shall take in water quite quietly at the top, be turned round by its load and let it all out quite quietly at the bottom. Evidently by such a wheel we shall get a definite amount of work out of every pound of water passing down it, we shall transform so much of the position-energy into energy of motion, or whatever it may be, in the mill. But we do not get all the position-energy. Some still remains in the water as it flows into the pool at the bottom. The lower this pool, and the greater its depth below the reservoir, the more of the energy do we transform. Still, however, in practice there is always some left in the water turned out. But now imagine that a shaft is sunk, going right down to the centre of the earth, and imagine that we have a series of wheels one below the other, the bottom of one being on a level with and delivering its water to the top of the next, and that this train reaches from the reservoir right down to the centre of the earth. Now we can, at least in imagination, get all the position-energy out of the water by letting it run down the whole train to the centre, where it is equally pulled in all directions and so has no weight left, no tendency to fall any way. The centre of the earth is an absolute zero of level for the water. To go any further we must push the water, when it will take in, not give out, energy of position. If we adjust the diameters of the wheels so that out of each pound of water each wheel extracts the same quantity of work, we may think of each wheel as letting the water down through the same difference of level. This is a 'work' measure of level, and it would not be consistent with the measure by a footrule. For evidently as we go further down the weight of a pound gets less, and the diameter of a wheel which is to extract from it the same amount of work gets larger. To put the idea of these wheels in definite form, let us think of the distance from the earth's surface to its centre as divided into 273 steps-down of level, each giving the same amount of work. If then we put in 273 parts of energy at the surface we can transform one part in each step. Or if we start 100 steps above the surface, and put in 373 parts there, again we can transform one part in each step. Suppose we work only down the first hundred of these steps, between the top and the earth's surface, we get 100 parts transformed out of every 373 put in. And these illustrations serve to show that the proportion transformed in any case depends solely on the levels available and their heights above the zero level.

Energy Transformation depends on Difference of L v l.

The limitation of the energy which can be transformed by the difference of levels available, so obvious in the case of position energy just discussed, exists in all other cases. With each form of energy there is something corresponding to level. With heat-energy it is temperature, with electrical energy it is voltage, and so on.

The theory of the transformation going on in heat engines, such as steam or oil or gas engines, was first given by Carnot and developed afterwards by Clausius, Kelvin and others, and it was this theory of transformation, together with the recognition of the conservation of energy, which made the middle of last century an epoch in physical science.

Case of Transformation of Heat.

Take, for instance, the steam-engine. It works between two levels or temperatures, that of the boiler and that of the condenser, or if there is no condenser that of the air, into which the expanded steam is turned. So much energy is put into the engine at the top level, so much remains in the steam turned out at the lower level, and the working of the engine depends on the difference between the heat put in and the heat turned out. Lord Kelvin saw how this idea of an engine working between two levels would give us a scale of temperature. Imagine a series of engines, the condenser of each serving as boiler to the next, like our train of water-wheels. Let us put some quantity of heat-energy in from the boiler of the first engine, and follow it as it runs down through the series, ever lessening as it goes. Let us adjust the temperature-steps between boiler and condenser in each engine, so that each gives out the same amount of work. On Lord Kelvin's scale each of these temperature-steps is equal or contains the same number of degrees, and from the method of testing this equality it is termed the work-scale of temperature. We can imagine engines which will work at any height or depth of temperature, so that there is no difficulty in thinking of our series continued downwards below freezing-point, and Lord Kelvin showed that making 100 steps from boiling water to melting ice 273 more such steps would yield all the heat-energy put in, and transform it all to work. No one has yet succeeded in imagining the possibility of a step below the lowest of this series, and so it is termed the absolute zero.

Now we can see what is the best we can do with the steam-engine. We can arrange the boiler to be perhaps some 450 degrees or steps above the absolute zero, the condenser some 300 steps above it, so that we can only go down 150 out of the 450 steps imaginable, or only transform one-third of the heat to work. We could transform far more if we could use the temperature of the fire. But the heat falls down enormously in level without undergoing any transformation in passing from the furnace to the water in the boiler. This is a very great defect in the steam-engine. It is as if we worked a water-wheel by allowing the water to fall straight down on to it from a point hundreds of feet above it, thus losing the opportunity of transforming so much more of the energy. If we could use the temperature of the fire as the higher level we could start perhaps at 1000 steps above zero and use some 700 of them. In the gas-engine the ignited gas is its own furnace, and we do use this higher level. Consequently it is far more efficient than the steam-engine, and there is little doubt that when the difficulties in the construction of the gas-engine have been overcome, the steam-engine will be relegated to the museum.

Energy Transformation always accompanies Life.

Regarded merely from the physical point of view every living being is an engine for the transformation of energy. It lives, as it were, between differences of level, and allowing the energy to run down-hill it catches and transforms some of the falling stream. But probably we must regard the living engine as chemical or electrical rather than as one of the heat class.

The Dissipation of Energy.

The transformations of energy depend entirely then on the existence of differences of level. Where all is at one dead level no transformation can happen. Now there is a tendency on the part of energy to reduce its own differences of level. Thus a water reservoir tends to leak and let the water run away down-hill to the level of the sea. An electric current tends to escape from the mains and run 'to earth.' A hot body will certainly share its heat with its cooler neighbours. When this tendency results in action, as a rule some of the energy, if not already heat, is sooner or later transformed to heat, and heat we know will tend to one level. So that we must recognise a tendency on the part of energy to a reduction in transforming power a tendency for each form to come to a dead level, yielding heat as its hills and valleys sink into a plain, and a tendency for heat to be scattered everywhere till it is all at one useless temperature. This scattering is known as the Dissipation of Energy.

Sometimes we are presented with a melancholy picture of the ultimate fate of Nature when this scattering is complete, when each kind of energy has sunk to one level, when all matter is at one temperature, and the universe is a system of dark, worn-out suns and frozen planets hung in a calm, lifeless sea of energy. Such a picture results from applying to the infinite our fragmentary knowledge of the finite. If we could shut up the solar system, say, in an energy-tight box, and leave it to itself, no doubt it would continually approach one dead level of temperature, though it would never in a finite time attain it. But our system is only one bit of a system to which we can set no bounds—a system in which the differences of level may be finite though the stores of energy are infinite. The present store of energy in the sun on which we now depend existed once, according to the best guess we can make, as energy of position of particles scattered through a vast region of space. They drew together and their position-energy was changed, we believe, into the heat now pouring out on all sides from the sun. Possibly the suns may draw together and change their position-energy into new stores of heat, and we may be merely at one stage of a gradual drawing together of matter into larger and larger masses with successive conversions of position energy into heat at centres more and more remote and at epochs further and further apart. This is no doubt wild speculation, but at least it will serve to show that as we know of no beginning in the activity of nature so we need not picture any end.

58.

MOLECULES, ATOMS AND CORPUSCLES.

[*The Inquirer*, 1902, pp. 740–741, 772–773.]

I.

Molecules, atoms and corpuscles are at the present day the letters of the alphabet in which we write our knowledge of Physical Nature. When new phenomena are observed, at once we seek to express them in terms of the motions and actions of the atoms. If a new radiant energy is discovered, we are not content till we can describe it as the disturbance sent out by the clashing of electrified corpuscles against solid bodies. If a new law of solution is put forth, we must show how it would result from the wandering of the atoms of salt among the surrounding crowds of solvent molecules.

Our Senses do not directly reveal Atomic Structures.

Yet to our senses there is no sign of molecule or atom. We feel the steady pressure of a breeze without a thought that the air may be composed of separate little bits hurling themselves against us. We look at a glass of clear water, and it seems absolutely continuous, absolutely filling the space which it occupies in the glass. We run our finger along a plate of polished metal, and it is quite smooth and even to the touch. Even when we beat the metal into a sheet so thin that when we look at it under a microscope we see holes in it, the idea we get is that of a continuous web of metal, with here and there holes and passages in it. But to the imagination of the physicist the air is no more continuous than a swarm of gnats, the water is a pile of grains just as much as a pile of shot or sand, and the metal is a crowd of separate atoms interpenetrated by a continuous web of space. The space is continuous, the atoms are little isolated bits hung in it.

Reasons for imagining the Atomic Structure.

What are the facts, the actual observations, which lead us to imagine this grained or atomic structure of matter, to imagine a constitution utterly beyond direct verification by the senses?

I should put in the first place the ease with which matter, especially when in the gaseous form, can be extended or compressed. The extension or compression is explained, that is to say, is likened to something already known, if the matter consists of atoms separated by spaces. Compression is then only a forcing together of the atoms, extension only a drawing apart.

Another class of facts which leads us to the atomic hypothesis may be illustrated by the simple experiment of putting some salt into water. At once it begins to melt away, and with a little stirring it is soon all gone. Yet it is still all there, distributed through the water, ready to excite our sense of taste, though not our senses of sight or touch till the water has been evaporated off. We have only to think of the molecules of salt as being somehow induced to part company with one another and made to wander about among the molecules of water, and we can form a picture of the processes of solution and diffusion.

But going beyond such solution to definite chemical combination, the uniformity of composition of any one compound, and the simple ratios in which different elements combine to form different compounds, find a simple explanation on the atomic hypothesis, and, at the same time, direct us how to form that hypothesis. We are to suppose that an element consists of little bits or atoms all exactly alike in weight and form. These atoms are invariable as far as our researches go. Temporary exceptions perhaps occur when electric discharges are taking place, but just now we need not consider these. We know of no case in which the atom of one element has been changed or broken up into that of another element. The hydrogen atom remains a hydrogen atom, the gold atom a gold atom. Chemical compounds, we suppose, are made by grouping different atoms together, each group forming a molecule, and consisting usually of a few atoms. Even in elements the atoms are usually grouped in pairs and trios and so on, to form molecules. Here then we have an explanation of the definite combining proportions of the elements. When we find one volume of oxygen combining with two volumes of nitrogen, we think of each molecule of the compound as containing one atom of oxygen to two of nitrogen. Or we may have one volume of oxygen combining with one volume of nitrogen to form another compound, and each molecule contains one atom of oxygen to one of nitrogen. Or three of oxygen combine with two of nitrogen, three atoms of one with two of the other, and so on. In the series there is no case of what are termed incommen surable ratios. We do not find any case of $\sqrt{2}$ times or π times the original quantity of oxygen combining with two of nitrogen. If we did our atomic explanation would utterly break down. Though some physicists will not have anything to do with atoms or molecules, I am not aware that they have attempted any explanation of facts such as these. They have to be contented with the facts without any explanation.

The Use of the Atomic Hypothesis consists in its Explanation of Facts evident to the Senses.

These examples will serve to show the value of the atomic hypothesis in arranging the account of our observations of nature. But the analogy which I have used in saying that atoms and molecules are the letters of our scientific alphabet may be followed further. Just as in writing, the arrangement of letters is really used to express words and sentences which alone have meaning for our minds, so the atoms and molecules are put together to express facts on a scale large enough to be evident to observation, and it is such facts which alone have meaning for our senses. We do not observe the atoms themselves, but, if they really exist, only the effects of enormous numbers of them. What we see, feel and touch are the words and sentences, nay, even chapters and books of which the atoms are the single letters.

The Atomic Hypothesis may possibly be replaced.

There is always danger of pushing an illustration too far. But I will risk the danger to bring out another point. We can use different alphabets to express the same words and sentences. We may even dispense with alphabets altogether and use picture-writing. May it not be the same with our atoms and molecules? May not some other hypothesis be equally successful in explaining the actions of matter? This is a question on which I think we should keep an open mind. For generations now we have been accustomed to atomic ideas; we have been brought up to think in terms of atoms, and it is now almost inconceivable to us that any other explanation could serve to describe what we see. But we must remember, after all, that the atom is a hypothesis, and that it is at least possible that some other, as yet unimagined, hypothesis may displace it. We may possibly learn some new form of writing, some shorthand to replace what will then seem our present clumsy longhand. At present we have not the slightest inkling of such a new alphabet, and I confess that it is to me most difficult to suppose even the possibility of giving up the idea of a grained structure of matter, whatever may be the nature of the grains. But this may be due to a bad early training.

Particular Forms of Atoms which have been imagined.

Even now we may distinguish between the general hypothesis of the existence of atoms, and any particular hypothesis as to their form or structure. Various atomic structures have been imagined. Once they were thought of as little round smooth hard balls, like minute billiard balls, but gifted with forces by which they could pull each other—forces arranged so that they could unite in pairs, trios, and so on. Then came Boscovich's centre-of force idea, in which the smooth hard nucleus was removed and the atom was merely the central point of a system of forces directed to the point—an idea to which I believe Lord Kelvin is now strongly inclined. But Lord Kelvin is the author of another hypothesis—the celebrated vortex-ring theory. In this, space is regarded as full of a fluid, and the atoms are merely little ring-shaped whirls of this fluid turning round as an india-rubber ring turns round when you run it along an umbrella stick. The two ends of half such a ring are seen at the surface of a cup of tea when the tip of a teaspoon is drawn through it. The half-ring is spinning below the surface and connects the two little whirlpools.

Then Dr. Larmor has put forth an elastic-solid theory in which space is full of a solid elastic jelly, and the atoms are little twists or tweaks in the jelly.

A few years ago Professor J. J. Thomson was led to the hypothesis that whatever the form of the atom may be it is a very complex group of corpuscles, the corpuscles being all alike in all atoms. The different atoms differ only in the number and grouping of these corpuscles. The most remarkable part of this hypothesis is that the atoms are not indestructible, and that single corpuscles may be, and indeed are, knocked off in certain electrical phenomena.

Just lately Professor Osborne Reynolds has brought forward a totally new idea. This time space is filled with little round equal grains, piled very close together like a heap of shot, yet with just room enough to move and jostle one another. These grains, however, are not the atoms, but the interspaces. The atoms are empty spaces where so many shot are missing, gaps or holes in the structure, holes surrounded by the round grains.

Each of these hypotheses will explain some one or more of the properties of matter, and Professor Reynolds claims for his empty-hole theory that it will explain more than the rest. For the scoffer there is certainly some support in the multitude of these hypotheses.

General Conception of Atoms.

Fortunately for many uses of the conception of atoms we need not imagine any definite structure. It is enough to think of the atom as a little system possessing mass, capable of motion, and pulling on its neighbours. We must think of the pull between two atoms as being exceedingly minute when the atoms are any sensible distance apart, but as increasing very rapidly as they come nearer, so much so that when they come quite close they can keep together and form molecules.

We must imagine round each molecule a spherical surface drawn at such a distance that when the spherical surfaces of two molecules touch or intersect the molecules become entangled with each other. The sphere thus drawn is called the sphere of action of a molecule, and when we speak of the size of a molecule we really mean the size of this sphere of action.

Without any further supposition than these it is possible in the case of such a gas as air to find the speed at which the molecules are rushing about, the distance which they travel on the average between collisions with each other, the size of their spheres of action, and the number of molecules in a given space. The results obtained are not perhaps very exact. But granted that molecules exist, and granted that they are producing the effects which we observe, they must have very nearly the speed, size, number, and weight which physicists have calculated.

In another article I will attempt to give some account of the methods by which these results have been obtained.

II.

In *The Inquirer* of Nov. 22 I gave some of the reasons for making use of the atomic hypothesis, pointing out that at present it stands alone in enabling us to explain a very large number of phenomena. But, of course, it is still a hypothesis, and wildly improbable as any other explanation may appear to us now, yet in the background of our minds we must keep the idea that it is a hypothesis, and that some other explanation might be possible.

For the present, however, let us take it as established. Let us assume that atoms and molecules do produce the effects ascribed to them, and let us see what conclusions we can draw as to their motion, number, size and weight. I shall try to show that if our preliminary assumptions are granted, then these conclusions are no guess-work, but are inevitable.

The Speed at which Gas-Molecules Travel.

It is much easier to give a molecular description of the gaseous form of matter than of the liquid or solid forms. For in the liquid or solid forms the molecules are packed so close together that they are entangled with each other all the time in all sorts of irregular groups. In a gas, however, the distances between the molecules are on the average so great compared with their size, or, rather, their spheres of action, that they hardly act on each other except during the brief moments when they collide against each other.

To account for the pressure of a gas we are to suppose that its molecules are in rapid motion hither and thither, bombarding and rebounding from any surface exposed to the gas. This bombardment produces the pressure. It is quite an easy problem in mechanics to calculate the speed at which the molecules travel to account for the observed pressure. Thus there is a rain of air-molecules down on the page on which I am now writing, producing a pressure of about 14 lbs. on the square inch. Now we may think of the molecules as moving in six directions—down, up, right, left, to and fro, so that only one-sixth of the air is moving downwards. We know the weight of air in a cubic inch, and one-sixth of that weight. How fast must it be rushing down and bouncing up again to produce a pressure of 14 lbs. on each square inch? The answer is about a quarter of a mile per second, this enormous speed making up for the lightness of the gas. In hydrogen, a much lighter gas than air, the speed is still greater, amounting to over a mile per second. As the temperature rises the speed increases, but very slowly, so that even in the atmosphere of the sun the molecules only move perhaps five or six times as fast as they do in our atmosphere.

Diffusion of Gases.

The rapid diffusion of gases is an obvious consequence of these high speeds. When a gas-tap is turned on without lighting the jet, the gas is soon unpleasantly obvious all over the room. The gas-molecules dart out from the pipe with a speed of, say, half a mile a second, and the wonder is, not that the diffusion is so rapid, but that it is not instantaneous.

Here, however, we must bring into consideration the collisions of the molecules with each other. Let us follow in imagination a molecule which starts from the gas-jet straight across the room. It goes a very little way before it bangs up against another molecule which drives it back. It moves back a little way, when another knocks it to the right. Then a little way and it is knocked upwards, and so on, and its path is an utterly irregular zig-zag. We may illustrate this process of diffusion on a large scale by imagining that a body of people—say here in Birmingham—suddenly make up their minds to journey to Utopia, and that they spread out in all directions in their quest. If they walked steadily on at four miles an hour some of them would arrive in London in about thirty hours. But now suppose that each traveller asks the way to Utopia of everyone he meets, and that he follows the course pointed out to him by the ignorance of his informant. Imagine how he would first be directed north, then south, now backwards, now forwards, up this road, down that lane. It might well be thirty days or even thirty months before any straggler turned up in London.

The Distance which the Molecules Travel between Collisions.

The illustration shows the difficulties with which a molecule has to contend when it wishes to keep to a straight course. Nevertheless, if we could mark the molecules near a given point and watch them, we should find that they would gradually spread from that point, or diffuse, and there are various ways in which their rate of spreading can be observed as for instance, if we heat one part of the gas and measure the rate at which the heated molecules share their extra energy with their neighbours. The slowness of this diffusion as compared with the great speed of the molecules suggests this question:

How often do the molecules collide in each second, and how far do they travel between each two collisions, if moving at the rate of a quarter of a mile per second they only diffuse at the observed slow rates? This is merely a mathematical problem, and the answer is, that an air-molecule makes about five thousand million collisions per second, and moves only about a two hundred and fifty thousandth part of an inch on the average between each succeeding pair of collisions. If the air is rarefied, of course, the collisions decrease, and in the exceedingly rarefied gas in an X-ray tube the molecule may move an inch or so at a time and only make some few thousand collisions per second.

The Number of Molecules in a Cubic Inch of Air.

Knowing the speed and the distance run between collisions it is possible to take another step and to calculate the number of the molecules. But this is only possible if we make another assumption, one, however, which it is difficult to suppose is very wide of the truth. This is that when a gas is liquefied or frozen its molecules are packed close together, or rather that what we have called their spheres of action are all in contact.

Let us imagine a room, ten feet every way, full of air. If the air were all liquefied it would form a puddle on the floor but a tenth of an inch deep. If this were all frozen and gathered into a ball, the ball of solid air would be but little more than a foot in diameter, and in this we imagine all the atoms huddled close together. We have now to put this question: Into how many equal little balls must this big ball be broken so that if they are scattered all through the room any one of them setting off on its travels will meet another every two hundred and fifty thousandth of an inch? Evidently the subdivision will have to be carried very far. For even suppose it is broken up into a million parts there will be one to every cubic inch or so, each about one-tenth of an inch in diameter, and one might wander many inches or even feet before meeting another. Subdivide each of these into a million, and still they will only be about one-thousandth of an inch in diameter and one-hundredth of an inch apart, like a fine mist, and a drop could easily pass among the rest for some tenths of an inch without meeting a neighbour. Exact calculation shows that the big ball would have to be divided into little spheres each about one twenty-millionth of an inch in diameter, and in each cubic inch there would be about a thousand million million million of them. Now we know the weight of a cubic inch of air—it is about one-third of a grain. So that we know the weight of a single molecule of air, at least roughly. It is not likely to be more than two or three times less or two or three times greater than the value found. And it is to be noticed that the only assumptions made are (1) that molecules exist, (2) that by their motion they produce pressure, and (3) that when liquefied they are close together. All the rest is experiment and calculation, allowing no room for supposition.

But the value ultimately found for the weight of a molecule of air is too utterly minute for our understanding. Perhaps its smallness may be brought home to us thus: If we put a molecule of air and a half-pound weight side by side they would bear to each other nearly the same proportion that the half-pound bears to the weight of the whole earth. And a hydrogen atom is twenty-eight times lighter than the average air-molecule!

The Corpuscles of which the Atoms are built.

Minute though the atom is, the researches which were started by the discovery of the X-rays a few years ago have led to the idea that the atom itself can be divided into still smaller bits. When the electric discharge is taking place in a very highly exhausted tube these still smaller bits, the corpuscles, are somehow broken off the atoms, and are flying across the tube. When they hit an opposing surface the tiny shocks produce minute quivers in the surrounding medium, which spread around and form the X-rays. They are of the same nature as light, but too short to affect our sight, though they can act on a photographic plate or a fluorescent screen. The very minuteness of the corpuscles tends to support this explanation, for we are obliged to think of the X-ray waves as exceedingly short, and such as would arise rather from the shock of little corpuscles than of big molecules.

When an electric discharge takes place through a liquid a stream of atoms moves through the liquid, so many atoms for so much electric charge. So definite is the relation that the charge per atom can be calculated, and it is always the same for all atoms which are chemically equivalent. But Professor J. J. Thomson found that in the stream of matter producing the X-rays the matter is much less in proportion to the electricity than the calculated charge per atom would require. Thus he found that in the case of hydrogen it was only necessary to suppose that one five-hundredth of the atom was moving along with the charge. Further, whatever be the kind of gas in the X ray tube, the stream of matter rushing across the tube appears to be the same in kind. The hydrogen atom, the oxygen atom, the nitrogen atom all produce the same kind of material in the electrified stream. When Professor Thomson made this great discovery he was led to put forth the hypothesis that the atoms are all complex, all built up of the same kind of little body or corpuscle, some 500 of them going to make the hydrogen atom, 8,000 to the oxygen atom, 50,000 to the silver atom, and so on. So minute are these corpuscles that they can and do travel through metal. When an X-ray tube is closed at one end by a thin aluminium plate the air cannot get in, but the corpuscles can get out and can be detected in the outside air. In metals generally these corpuscles appear to be very abundant, and Professor Thomson explains the high conductivity of metals for electricity and for heat by their aid. In a heated body, for example, they serve as carriers of energy, and being so minute can pass fairly freely through the interspaces between the bigger molecules, taking their energy from hotter to colder parts of the metal.

But perhaps the most remarkable point in this new hypothesis is the support it gives to the idea that matter and electricity are identical, or rather that electricity is but an aspect of matter, an idea which has long been held in a vague form. Now it becomes precise. I think Professor Thomson would say that negatively electrified matter is matter with an excess of corpuscles, positively electrified matter, matter from which some corpuscles have been torn off. It is the attempt of the negatively electrified matter to get rid of its excess of corpuscles which produces the electric forces it exhibits, and if a positively electrified body comes up and takes off the excess both are satisfied, and we speak of the electricity as discharged. But the corpuscles, the units of electric action, are still there, and it appears probable that the electric forces between them may be used to explain some of the properties of matter, perhaps even gravitation itself. The smaller a body is, however, the larger is the space required for its description, and it would need far more than is at my disposal to give any adequate account of the new views opened out by this remarkable hypothesis. I can only say that it bids fair to take as important a place in the work of the century just beginning as did Dalton's form of the atomic hypothesis, enunciated a hundred years ago, in the century just closed.

59.

THE PRESSURE OF LIGHT.

[*The Inquirer*, 1903, pp. 195–196.]

Within the last few years Professor Lebedew, of Moscow, has succeeded in showing, by some very remarkable experiments, that light presses against any surface on which it falls. Even in the strongest sunlight this pressure is exceedingly minute, less than one two-hundred-thousandth of a grain on the square inch, and the most delicate apparatus is needed for its detection. Little wonder then that it has only now been shown to exist.

Old Attempts to show Light-pressure.

When light was supposed to consist of very little particles, shot out from the source, it was natural to suppose that, moving with a speed of nearly 200,000 miles a second, these particles would press appreciably against any surface which they were bombarding, their enormous speed making up for their infinitesimal mass. In the eighteenth century, when this theory was almost universally held, many experiments were made, which sometimes appeared to show the pressure, but only because various disturbances came into play, and gave an effect which we now know to have been spurious. Some of these experiments are described in Priestley's *Vision*, and Priestley appears to have thought that they conclusively exhibited the pressure. How ever, a few years later Bennett, allowing a ray of sunlight to fall on a paper disc attached to one end of a horizontal straw hung by a spider line in a vacuum, could detect no effect, though a very minute pressure on the disc would have turned the straw a little way round. For two generations Bennett's experiment was taken as disproving the existence of any pressure due to light.

The Wave-theory of Light.

Meanwhile the corpuscular theory disappeared, and the wave-theory reigned in its stead. If light consisted of transverse waves or quivers in an almost immaterial ether it was by no means evident that such waves would bring momentum with them, and press against a surface on which they spent themselves. Indeed, it was rather considered that Bennett's experiment confirmed the wave-theory, inasmuch as waves would account for the absence of pressure.

Forty years ago it seemed almost certain that we knew that light was a quivering of the ether, a jelly-like medium pervading all space. The atoms vibrating in this ether shook it up and down and to and fro, and these up and down and to and fro motions were sent out on all sides as waves of light. The ether was postulated, and then all else was known. But now this form of the wave-theory has entirely disappeared, and in its place we have Maxwell's Electromagnetic theory. Light still consists of waves, but waves of electric strain and magnetic spin. We can only form guesses as to the actual changes of position and the motions in these strains and spins. We can imagine, but only imagine, atomic changes corresponding to them. It would appear as if we had given up a substantial theory for a vague and shadowy one.

But though we cannot with any confidence describe electric strain in terms of atomic and molecular change, yet we do know how to produce such a strain, and we know what it does when produced. It is an old fallacy to suppose that we know more about a thing if we can imagine how it is made up than if we only know what it does. We do not know how the electric strain is made up as we thought we knew how the ether-jelly waves were made up, but we know as a matter of direct experiment how it behaves and how it travels.

The Electromagnetic Theory of Light.

What, then, are the electric strain and the magnetic spin which now we suppose to constitute light? When we hold an electrified stick of sealing-wax near a little bit of paper which it pulls towards it we have every reason to suppose that the action is due to a modified condition of the air round the electrified surface, that the air pulls the wax and paper together, that it is acting as if it were in a state of stretch, like a stretched india-rubber cord. We say that there is electric strain in the air. We picture it by lines of force stretching from surface to surface like thin rubber cords; but these lines of force are mere symbols used to describe the strained condition. Along the lines of force, then, there is a pull shown in the tendency of the two surfaces to draw together. But somewhat as a rubber cord tries to contract, and in so doing tends to bulge out sideways, so the air or any medium which is electrically strained tries to pull together lengthwise and bulge out sideways, pressing against the surroundings.

In like manner, when a magnet pulls a little bit of iron we suppose that the intervening air is altered, and again we symbolise the alteration by lines of force stretching across from surface to surface, trying to shorten and at the same time pressing out sideways. We have some reason to suppose that there is a spinning motion round the lines, perhaps of the atoms or perhaps of the corpuscular constituents of the atoms. Just as a spinning body like the earth tends to draw together along the axis of spin and press out sideways at the Equator, so the medium round a magnet tends to draw together lengthwise and press out sideways.

Let us see how we may have waves of electric strain and magnetic spin. Imagine a wavy line, drawn from left to right across this page, to represent a set of waves of light. Of course this is a purely conventional representation, and does not imply anything as to the actual form and nature of the waves. The kind of electric strain supposed to exist in the waves may be thus described. In each height or crest the strain is that which would be produced by a positively electrified body held underneath the line, and in each depth or trough the strain is that which would be produced by a negatively electrified body held underneath the line. Or to put it in another way, suppose a number of short electrified rods, some of glass electrified positively, some of sealing-wax electrified negatively, and suppose them arranged in two lines thus:

s.-wax	glass	s.-wax	glass
glass	s.-wax	glass	s.-wax,

then the space between the lines will have alternations of electric strain, arranged as we suppose them in waves of light. But while these waves will be each the length of the two words, s.-wax glass, the two alternations in a wave of light are only perhaps a fifty-thousandth of an inch long.

Now let us think of the waves as moving on from left to right. Whenever electric strain moves on we know from experiment and observation that magnetic conditions are produced. In this case it can be shown that there must be magnetic lines of force, perpendicular to the wavy line, perpendicular in fact to the paper on which it is drawn. If the waves are travelling from left to right, then in the crests the magnetic condition will be the same as if a magnetic north pole were held just behind the page, and in the troughs it will be the same as if the pole were held just in front of the page.

Now, as I have said, both the electric strain and the magnetic spin tend to bulge out sideways and produce a pressure sideways, and for both the sideways direction in the waves is the direction in which they travel. That is, the waves press forward against a surface to which they are travelling, and backwards against any surface which they are leaving.

Lebedew's Experiment.

This was all foretold by Maxwell more than thirty years ago. Lebedew has detected the pressure by what we might describe as a refinement of Bennett's experiment. A disc was fixed at the end of a short horizontal arm suspended by an extremely fine fibre and in a vacuum as perfect as possible.

A beam of light was concentrated on the disc, and it moved a little way back. The force was, as nearly as could be measured, that calculated by Maxwell.

Astronomical Consequences.

It is somewhat startling to be told that while the sun is pulling all the planets with the force of gravitation, at the same time he is pushing them away by the light he pours out on them. It is true that this outward push is, for such a planet as the earth, too small in comparison with the inward pull to be taken into account, at any rate at present. On the whole earth it is only about 50,000 tons, which is a mere nothing compared with the sun's pull of gravitation. But as the size of the body diminishes, the gravitation-pull decreases much faster than the light-push. We may imagine particles so small that the pull and push are equal, and for still smaller particles the push may be greater than the pull, and the sun may drive such small particles away from him. Thus a drop of water a twenty-five-thousandth part of an inch in diameter would be equally pulled and pushed by the sun, and smaller drops would be pushed away. But for very small particles the law of light-pressure probably changes, and the pressure diminishes very rapidly. Just as water-waves dashing against a big ship may press it onwards, while a cork, or even a small boat, may ride over them and only be moved up and down, so the very small particles round the sun may ride over the waves of light, as it were, and not be pressed outwards by them. Indeed, if the smallest particles were pressed out according to the same law as the largest ones, the sun would always be driving away the atoms and molecules from his surface and pouring them into space. Perhaps he does drive away those particles which are neither so big as to be drawn in by gravitation, nor so small that they can ride on the light-waves like corks on water; and perhaps in some such action we may find an explanation of the mysterious corona revealed in eclipses, and in the still more mysterious zodiacal light, that enormous belt apparently of thinly-diffused matter which stretches out from the sun all across the earth's orbit, and which may be seen in the west any clear moonless night at this time of the year an hour or two after sunset. But we have only just begun to realise that light-pressure is an actual fact, a fact to be taken into account; and it is too soon yet to say with any certainty what effects it may produce. But, at least, we can say that the proof of its existence is one more triumph for the electromagnetic theory of light, which we owe to the wonderful genius of Maxwell.

60.

MYSTERIES OF MATTER. RADIUM AT THE BRITISH ASSOCIATION.

[*The Inquirer*, 1903, pp. 635–636.]

One of the most interesting features of the recent meeting of the British Association at Southport was the discussion on Radium in Section A, which was initiated by Professor Rutherford of Montreal, a worker who has done much to unravel the tangle of puzzling phenomena presented by this most remarkable substance.

I propose to give some account of the properties of radium as described to the members of the Association, without attempting to follow the actual course of the discussion.

The Discovery of Radium.

Almost immediately after the discovery of the X-rays by Röntgen, came the discovery by Becquerel, that salts of uranium emitted something with properties akin to those of the X-rays. These salts act on a photographic plate in the dark, even through thin sheets of metal; they make certain substances held near them glow with a phosphorescent light, and they make the air around them capable of conducting electricity more or less freely. A little later it was found that salts of thorium possessed these properties in even greater degree, and that they were shared too by certain minerals, notably by pitch-blende, a mineral which is a complicated mixture of salts of uranium and of many other metals. All these substances are classed together as 'radio-active.'

M. and Mme. Curie, two distinguished French physicists, found that, by certain chemical processes, they could extract from pitch-blende a small quantity of something far more radio-active than the original mineral. From this, again, they could extract a still smaller quantity of something still more radio-active, and by repeated actions they ultimately extracted from tons of pitch-blende a few grains of a substance which appeared to be a pure salt of a new metal, which they termed 'radium.' This salt is thousands of times more radio-active than the mineral from which it has been extracted. The metal itself has not yet been obtained, but only its compounds with other bodies such as chlorine, bromine, and nitric acid. It is evidently of the calcium and barium family, but with an atom heavier than these—heavier, indeed, than any other atoms except those of thorium and uranium, the other two radio-active metals. The immense labour in the preparation of radium salts involves great expense, and the value of radium has been expressed as some hundreds of thousands of pounds sterling per pound weight of the substance. But these big figures are only arithmetic. There is probably not yet an ounce of pure radium salt in the whole world. The Curies some time back possessed about a thirtieth of an ounce, and other workers rejoice if they possess a grain of it.

Properties of Radium.

Let us see what are the most evident properties of radium, noting that uranium and thorium share these properties, but in far less degree.

First, and most easily observed, is the property of discharging electrified bodies. Suppose, for instance, that we have a gold-leaf electroscope. This is a simple instrument consisting of two strips of gold leaf which hang down vertically, side by side, from the end of a metal rod held in an insulator. If the metal rod is stroked with flannel or fur, it becomes charged with electricity, which passes down to the gold leaves. The leaves then repel each other and stand out like an inverted V. If, when the leaves are thus charged, a particle of radium is brought near them, they rapidly fall together again, showing that the air has become conducting, and that the charge has passed away through it.

Another most remarkable property is that of making certain substances glow with phosphorescent light. Thus, if zinc sulphide is pasted on to a sheet of cardboard, a small piece of radium brought near the screen so formed will make it glow with an intense green light, giving out sometimes as much light as two or three candles. Even if sheets of metal are interposed, the glow, though fainter, is still apparent. Sir William Crookes discovered that if a particle of radium is held just above the screen, the glow, when examined with a pocket lens, is not continuous, but consists of a series of little flashing points—a most beautiful exaggeration of the twinkling of the stars on a clear night.

Again, the radium itself glows in the dark, and when in air it makes the nitrogen in the air glow, though very faintly, just as that gas does when an electric discharge is being passed through it.

Emission of Heat.

But most remarkable of all the phenomena exhibited by radium is the continual emission of heat discovered by the Curies. Many substances can emit heat as long as they are undergoing spontaneous decomposition and entering into new and less energetic chemical unions. But as yet the radium salts have shown no such changes. Further, all chemical changes hitherto examined take place far less actively when the bodies are extremely cold, whereas radium, so Professor Dewar told us at Southport, gives out more heat when immersed in liquid hydrogen at 250 below the freezing-point of water than at ordinary temperatures. Indeed, if a freshly prepared radium salt is put into the liquid hydrogen, it goes on giving out heat at an increasing rate for several weeks, and no experiment has yet been carried on long enough to show a decrease, though we can hardly suppose that the supply of heat is really endless.

We have, then, the surrounding air made conducting, the glow in the dark, the excitation of glow in neighbouring bodies, and the continuous evolution of heat from no apparent store.

Manifold Activities.

The power of discharging electrified bodies provides some means of examining more minutely what is going on in the space round the radium, and it has been found that there are at least four emissions from it. In the first place it is giving off what appears to be a heavy gas, which Professor Rutherford terms the emanation. This I will return to again. Next it is shooting out positively electrified particles, apparently molecules about as light as those of hydrogen, darting forth with the almost inconceivable velocity of 20,000 miles a second. Thirdly, it is shooting out slower moving negatively electrified particles, apparently far lighter than those of hydrogen, identical indeed with the atomic fragments which Professor Thomson has taught us to see in the Crookes tube; and fourthly, a kind of wave-disturbance of the X-ray sort, but much more penetrating than ordinary X-rays. Probably the flashing on a zinc sulphide screen discovered by Crookes is due to its bombardment by the positively electrified particles, each flash marking the clash of a particle against the screen. Probably, too, the heat evolved in the radium itself is due to the clashing against the molecules of radium of those positively electrified bodies which do not aim straight out from the salt into the air, but bombard their neighbours. Their enormous energy of motion is converted by the clash into heat, just as the energy of motion of a bullet is converted into heat when it strikes a target. Some of the negatively electrified particles also do not get out, but knock against the radium molecules, and so probably give rise to the X-like rays.

Probably the positive and negative particles which escape, and the X-rays, all conspire to knock or shake to pieces the molecules of air round the radium, and in this disorganised state we know that the air will conduct electricity. Probably, too, when they strike their neighbours, instead of escaping, they make those neighbours glow with the phosphorescent light which is seen in the dark. All, *probably*—for it is too soon yet for unqualified assertion.

The Emanation.

Now let us return to the emanation investigated by Professor Rutherford. This emanation slowly diffuses out from the radium just as if it were a gas with molecules about a hundred times as heavy as those of hydrogen, and it can be carried away by a current of air blown over the radium and led into vessels at a distance. It is far too small in quantity to be weighed or measured, but it splits up slowly and shoots from itself the positively electrified particles which I have just described, and these can be detected by their effects. The portion of each molecule remaining behind is, of course, negatively electrified, but soon a fragment is shot off, it seems, with the negative charge, to constitute one of the negatively electrified stream of particles. The neutral mass remaining may break up again, but here we will leave it. So the emanation appears to be the parent of all the remarkable progeny. The particles shot out from the radium with their positive and negative charges are most likely produced by emanation which has been formed from, and is still entangled in, the mesh of molecules of the radium salt. This emanation can be dissolved out from the salt, which is then quite inactive. But gradually fresh emanation is formed, and the activity rises again.

From the Radium Atom itself.

Now this emanation, this heavy unelectrified gas, as we may regard it, does not appear to be due to a change in the chloride or bromide or nitrate part of the salt, but in the metal part and in that alone, for its rate of issue is the same for the same weight of radium whatever the radium may be combined with. Professor Rutherford believes that it is due to a splitting up of the radium atom itself. The radium atoms, he supposes, are in rather a shaky condition, and every now and then one breaks into two pieces, one piece constituting the emanation and going off, the rest remaining to form a new and smaller atom not yet known. But the emanation is still more shaky and breaks into two, or rather it throws off a positively electrified bit, itself thereby becoming negative. Now the enormous energy of projection of the positive bit implies that in the emanation molecule, and therefore in the parent radium molecule, there is an enormous store of energy, far beyond that in any ordinary chemical union. It is perhaps enough to say that we can imagine an atomic system which shall contain a store of energy not manifested as heat, and which shall fling away bits of itself with some of this store converted into energy of motion, ready in turn to be converted into heat.

Intense Energy.

But, at once, a difficulty arises. If the radium atom is breaking up and forming a new kind of atom, why do we not find the new atom? Why does not the appearance of the radium salt change? Why does not its store of internal energy visibly lessen when its heat expenditure is so lavish? The difficulty lessens and almost vanishes if we look at the Crookes screen flashing under a piece of radium no bigger than a pin's head. There are, perhaps, a hundred or a thousand flashes in a second, a hundred or a thousand positively electrified bits hurled against it in a second, a hundred or a thousand atoms of radium broken up in a second. At the larger figure it would take thirty years for a billion atoms to break up, and there are probably a thousand to a million billion atoms in the pin's head. Looking for signs of change and decay in the radium, then, is like looking for the grey hairs and wrinkles of old age on a new-born child.

Divisible Atoms.

These ideas of the atoms as storehouses of immense quantities of energy, and as systems which can break up and so give out their stores, may seem wild and extravagant. Indeed, so wildly extravagant do they seem to many chemists and to some physicists that they will not consider them with any patience. But the facts are extraordinary, and had anyone described them ten years ago he would have been set down as fit for an asylum.

The discoveries of Lenard, Röntgen and Thomson, however, have shown that they do not stand alone. We are face to face with new phenomena, for which the old scheme of matter, with its indivisible atoms, has so far utterly failed to find a place. All attempts yet made to give an account of these new facts on old lines are, to say the least, more wildly extravagant than anything which Rutherford or Thomson has suggested.

This year we have been celebrating the hundredth birthday of Dalton's atomic theory, a theory which appeared to set the indivisible atom on a sure foundation. It is a curious coincidence that in the same year we should all be discussing this new hypothesis, which would remove that foundation, and shake the atom to pieces.

61.

A CITY UNIVERSITY.

[*The Inquirer*, 1903, p. 660.]

Sir,—In a 'Note of the Week' in your issue of September 19, referring to Sir Norman Lockyer's presidential address at Southport, you say that he enforced his argument for extending, improving, and systematising our national provision for higher education 'by copious quotations from the very men—Mr. Chamberlain, Mr. Balfour, the Duke of Devonshire, and the rest—best placed to carry out themselves the improvements they advocate, but who have instead used their opportunities merely to hamper and disorganise—to use no stronger terms that elementary education, a sure grounding in which is the only possible basis for an efficient course of higher instruction.'

Leaving on one side the question whether elementary education is, or is going to be, hampered and disorganised by the recent Acts, may I point out to you that of 'the very men' Mr. Chamberlain, at least, has not been content to use his opportunities 'merely' to deal with elementary education. As a member of the Staff I have been associated from the beginning with the movement which has led to the foundation and development of the University of Birmingham. I know, as everyone in the University knows, that Mr. Chamberlain has thrown himself heart and soul into the movement and has been our leader throughout. When his duties as a Minister must have been almost overwhelming, he has still found time to give much thought and hard work to the affairs of the University. He has insisted that its equipment, on some lines, at least, should be such as to place it on a level with the best German and American Universities which are being held up to us as models. By his own personal efforts he has made this possible.

Mr. Chamberlain has used his opportunities to found a City University in place of a Federal Institution. The future development of provincial Universities in England has been, thereby, entirely changed—changed I believe entirely for the better.

62.

THE UNIVERSITIES AND THE STATE.

[*The Inquirer,* 1903, p. 779.]

'If our present University shortage be dealt with on battleship conditions, to correct it we should expend *at least* £8,000,000 for new construction, and for the pay-sheet we should have to provide (8 × £50,000) £400,000 yearly for personnel and upkeep.'—*Sir Norman Lockyer's Presidential Address to the British Association, September* 9, 1903.

It is hardly necessary, at this time of day, to justify the claim of higher education for State support. The claim is recognised in the maintenance of the British Museum, of the National Gallery, of the South Kensington Museums. It is recognised by the State decentralised into municipalities, in the formation of libraries and art galleries in all our large towns. Now, too, it is recognised in relation to the special type of higher education given in our universities, in the yearly grants by the Treasury to the various universities and university colleges—grants which are added to by the municipalities.

The State, then, already admits a duty to support higher education. But we can only describe it as a reluctant admission. There is no enthusiasm on the part of governments to encourage such education, no determination to secure that it shall be provided, and shall be efficient. What has been given by the Treasury has been given almost grudgingly. The Treasury has held that higher education is chiefly to be supported by benevolent subscribers. If these are not forthcoming, the State has no call to help, no duty lies on it to see that the education shall be efficient, or even that it shall be provided in any form. This lack of enthusiasm is really shared by the general body of the people. Here and there are wealthy men, willing to contribute large sums to our colleges, but the subscribers form a minute proportion of the population. As a nation, we have little interest in the subject. A generation ago we woke up to the necessity for universal elementary education. Now the new education committees are devising machinery for improving secondary schools. But, still there is no change in the provision for higher education. Newspapers of every party express the pious opinion that Sir Norman Lockyer is justified in his advocacy, that we ought to face the situation and spend more money, but there it stops. No member of Parliament advocates the claims of the universities in season or out of season, no commission is appointed to enquire what a proper equipment would be, no Chancellor of the Exchequer dare suggest a fraction of the expenditure urged by the president of the British Association, for the nation is not interested.

Yet some such expenditure is necessary if higher education is to be general and efficient. At the risk of saying what has been said often and better by others, I am going to attempt to point out briefly why it is important that the nation should be interested, and why it should determine to spend enough to make higher education much more general than it is and much more efficient.

In old days, and right down to the last generation, when either literary subjects or science subjects, taught more or less by literary methods, were the chief vehicles for higher education, the expense was not necessarily very great. A teacher, a room, and a few books only, were needed by the pupils. But we have begun to learn that in a very large proportion of young people, and, I believe, in the vast majority, attention is best aroused, interest is best kindled, and ultimately most culture is attained by putting scientific or professional or technical studies in the first place, and giving literary studies the second place. We have begun to learn that not only pure science, but science applied to manufacture and commerce may be studied as a means of culture, not, perhaps, the highest, but certainly the most efficient for many students. I wish we could cease to compare ourselves with Germany and America, cease to argue that we shall be beaten in the commercial race if we do not improve our education, and take instead the far higher ground that the study for a profession or a trade may be an ennobling study, that every reputable profession or trade may be prepared for and lived as if it were a life worth living. That is the true view. When once the nation is inspired by the idea, students will flock to our universities and adequate State support will not be lacking. For most of the students, the universities will be training grounds for the professions enabling them to prepare worthily for their work in life. Quite clearly, then, it is essential for the higher life of the nation, that what is done at the universities should be done thoroughly; and this involves vast expenditure on teachers, on libraries, and on laboratories.

There is another reason why universities should be expensive to maintain. They are, or should be, seats of learning, not only for pupils, but for teachers. Everyone in a university should be a learner. On the side of letters this means, in the first place, libraries; on the side of science, in the first place, laboratories. But, above all, it means more leisure for the teachers. It means more professors, more assistants, so that all their time and energy should not be given, as it now too often is, to teaching. The advantage of the atmosphere of research round a college or a university is not merely in the discoveries made, which may or may not lead to money and comfort, but it is far more in raising the ideal, in directing the attention of the young student to learning, to the investigation of the past, to the patient study of nature, lifting him above the sordid study of examination methods and the craving for letters to stick after his name. I am inclined to believe that the British worship of degree examinations is very largely responsible for the lack of enthusiasm for higher education. I am a teacher of too many years' experience to wish to abolish examinations. I know how important they are to both pupil and teacher in their proper place and on a proper scale. But they may be made much less formal and much more efficient than they are as a rule in this country. Here the examination to be passed at the end of a course is *the* great consideration all through the course. The one public, all important, final examination is, I am convinced, bad for many men, and thoroughly bad for all women. It concentrates the attention on the wrong issue, leads to an overestimate of the value of a degree, and an underestimate of the culture which the degree should merely attest. This mischievous concentration of attention on examinations and degrees has been forcibly borne in on my mind when I have acted as examiner at the University of London. Year after year I have recognised the same faces at the same examinations; often elderly candidates coming up again and again, each time only to fail. At first sight it looks like a courageous struggle worthily maintained. But look at what it means. Look at the futile waste of the little leisure these men have from their labour as teachers—and they are nearly all teachers—to prepare every year the same set of subjects for examination. Every study has now become merely a 'subject,' for all the life was sucked out of it in the first year. They waste their time in fruitless efforts to put an examination edge on to their knowledge. They spend themselves on a treadmill when they might be out in the fresh air.

A larger equipment of our universities in staff, in libraries, in laboratories, would make them more efficient as seats of learning or research, and would inevitably reduce their importance as examination machines. The examination would sink into its proper place, and no longer be the wretched ideal it now is.

Universities equipped on any such scale cannot be self-supporting. They would only then be accessible to the wealthy, who, at present, do not show any very keen desire for higher education, at any rate of the scientific kind. But they would benefit the nation as a whole, and the nation should support them. It is too important a matter to be left much longer to the chances of private benevolence. As yet, I do not think we want all that Sir Norman Lockyer asks for. We have not yet attracted the students. There is not yet the demand which may encourage such expenditure. But the nation will yet wake up to the need for higher education for all the professions, not that it may keep abreast in the commercial race, not that it may win back the pearl-button trade or the aniline dye manufacture, but that it may live a higher life, a life worthy of a great people.

63.

PHYSICAL LAW AND LIFE.

[*Hibbert Journal*, **1**, 1903, pp. 728–746.]

The conception of the uniformity of Nature doubtless began when our ancestors first realised that they would suffer a repetition of former experience or could repeat former actions when their surrounding conditions were repeated. As mind has grown and experience has widened, so has the belief in this uniformity strengthened, till now it is almost recognised as an axiom that event follows event in orderly sequence, that Nature works by Uniform Laws.

Probably some form of the axiom could be found to which everyone would assent. But while we may agree on the form, agreement will certainly end when we begin to discuss the meaning and extent of each term, when we define what we mean by Law and when we draw or refuse to draw the boundaries of the Nature which works by Law. Behind the mere form of words are ideas which differ as widely as do our outlooks on the Universe and our inlooks on the human mind. In examining these ideas we find ourselves at once brought face to face with the great problem which has been discussed ever since man first attempted to formulate his knowledge and turned his thoughts to philosophy.

I propose in this paper to give some account of the meaning which, as it appears to me, we must ascribe to the term 'Physical Law,' and to enquire how far and in what sense Law is universal in Nature. These are no doubt very ancient questions. Yet they are ever requiring restatement in the light of new knowledge, and our answers are ever needing revision as the questions change their form. I shall try to put the questions as clearly as I can in the form in which they present themselves to the student of Physical Law.

Scientific knowledge as embodied in laws is now generally recognised as being purely descriptive. The aim of science is to formulate in as concise a form as possible an account of *how things happen, how event follows event.* We seek to frame our formulae so that, if we know the conditions prevailing at any one time, we can describe the conditions which will follow. We seek to frame them so that we can forecast the future.

Our descriptions are embodied in laws, which are neither more nor less than statements of similarities or likenesses which we have observed in the happening of events. These laws are not fixed—are not promulgated by Nature herself. They are *our* descriptions of the likenesses which we think we observe when we watch her actions. They are our accounts, not hers - our accounts, if you like, of her ways and habits.

A Law may fail or cease to be true, not because Nature has changed her ways, but because we have failed in our statement of likenesses, or because we learn new details with which our old description does not tally.

Let us take some of the more familiar laws and see how they bear out the statement that they merely describe observed likenesses.

The Law of Gravitation as applied to the planets asserts that they are all like one another in that their rate of change of motion towards the sun multiplied by the square of their distance from the sun gives the same result. Or the law in its most general form asserts that we can assign to each piece of matter a constant number, called its mass, and that the rate of change of velocity of any one piece A towards another piece B is proportional to the mass of B divided by the square of their distance apart. The different cases of gravitational motion of bodies towards each other are like each other, and this law expresses the likeness which we find, whether we observe bodies in the laboratory, or out in the solar system, or even (we believe) among the stars unimaginable distances away. So far, whenever we have observed motions which do not fall in with this description, we have always found some other conditions present which may have masked but have not destroyed the likeness.

Or take the law of interchange between heat and mechanical work. So far as we can observe, every such interchange is like every other. in that if we divide the work done by the heat produced, the quotient is the same wherever and whenever the observation is made, and the statement of the likeness is known as the law of the mechanical equivalence of heat or as the first law of thermodynamics.

Again the law of constancy of chemical composition asserts, to take a single case, that wherever or whenever we decompose eighteen parts of water we obtain sixteen parts of one gas and two parts of another. The heavier gas obtained in any one case is like the heavier gas obtained in any other case in every quality, and we always call it oxygen. The lighter gas in any one case is like the lighter gas in any other case, and we always call it hydrogen. Any specimen of water is like any other in yielding these like products, and the law of constancy of composition expresses the likeness.

Now let us turn to a case in which a law fails. Boyle's Law asserts that if we keep a gas at one temperature and alter its volume by altering its pressure it will be like itself and like all other gases, in that the pressure multiplied by the volume will be constant throughout the change. But though this law sufficed to describe the observations and experiments of physicists for nearly two hundred years after its first statement by Robert Boyle, yet when more exact means of measurement were devised it was found to be an inexact and so far an untrue description. A much more complicated relation has now been devised to express the likenesses we find in squeezing up different gases. It is not a change in Nature but a change in our statement of what is observed, now that we can observe and measure more carefully.

As our study widens, so too does our perception of likenesses widen, and new physical laws are ever being formulated.

But not only do we find new laws. We are constantly finding that some newly-observed process is like one already known, so that a new law is needless, the new observation falling under an old law already registered. Then we say that we have *explained* the newly-found process.

We are, in fact, always seeking to shorten our descriptions of Nature by classifying our observations according to their likenesses, that is, by formulating laws, and we are always seeking to reduce the number of laws by explanations, that is, by recognising new, less obvious, likenesses.

We may put this in another way, by saying that we are always trying to find typical cases to which others may be likened, and from this point of view our laws are statements of typical cases. We are always trying to reduce the number of typical cases by showing that some of them are like others and need no separate statement.

But this process must stop somewhere. Obviously we cannot go on reducing the number of typical cases till none are left. We must have at the least one to which all others may be likened, one which cannot be explained. At present, indeed, we have many which we cannot liken to any other. And when we come to a typical case unlike any other, that must be taken as a simple fact, simple or unique in the sense that it is unresolved, unlike any other. Thus we may show that events *XYZ* are cases of, or are like events *ABC*, already known and registered. We may perhaps go further and show that *C* is like *A* or *B*. But sooner or later we are brought up against cases simple at least for the time being, and ultimately we must have something permanently simple. If we explain *X* and *Y* and *Z* by *A* and *B* and *C*, we cannot turn round and explain *A* and *B* and *C* by *X* and *Y* and *Z*, and then say that all is explained. That is only repeating in a more subtle form the fallacy of the islanders who sought to make a livelihood by washing each other's clothes.

In seeking, then, to reduce the number of typical cases, some must remain which can be no further analysed, which remain to us simple facts. Our explanations liken other cases to these but do not explain, do not account for the simple cases themselves. These simple cases are the raw material, as it were, out of which the complex pattern of the garment of Nature is woven.

I suppose that we may put down the list of most general laws or most widely prevalent likenesses somewhat as follows:

1. We can assign to every piece of matter a constant number denoting its mass, a number always the same, whatever chemical or physical changes that piece of matter undergoes. This is the law of constancy of matter.

2. In any mutual action between two pieces of matter, the one hands on to the other, unchanged, the momentum it loses, so that the sum total of momentum in any direction is unchanged. This is the law of constancy of momentum. We have made some progress in formulating the laws of interchange of momentum between different pieces of matter or the forces with which they act upon each other, but the only case thoroughly worked out is, I think, that of gravitational force. These two laws deal with motion alone.

3. But when we come to investigate all the other ways in which our senses are affected by matter, we have a third law. This states that we recognise several measurable qualities or conditions of matter which we call forms of Energy or Energies. Thus we have Energy of motion, Energy of position, Heat, Light, and so on. We observe that when one of these disappears some other form appears, and in each case there is a fixed rate of exchange from one form to another. If we lose so much energy of position and heat alone appears, the position-energy lost divided by the heat evolved is constant. Or if we use chemical energy to produce heat, the number of heat units obtained for each unit of chemical energy lost is constant. If, then, we follow up any group of energies and note all the interchanges, we find that all the cases which we watch resemble each other in that, when we reckon up the sum total in terms of any one form as standard, that sum total is constant. It is somewhat like the constancy of the sum of money in the possession of the man at the change-giving counter at an exhibition or a theatre. He may change pence for shillings, silver for gold, gold for notes. Yet if he does his work accurately, the sum total reckoned in, say, shillings will always be the same, though at one time it may all be pence and shillings, at another all gold and notes. This is the law of constancy of energy.

4. We have a group of laws expressing the conditions under which the interchanges or transformations of Energy take place, and stating the amounts which will be transformed under given conditions. These laws form the latest born of the physical sciences, the science still often called Thermodynamics, the name of its childhood. But it is rapidly attaining maturity and strength, and is coming to be called by the more dignified name of Energetics.

Under these laws and groups of laws we can arrange all our knowledge of the actions and processes going on in the world of non-living matter. The laws describe what is evident to our senses—what we see, hear, feel, touch. They state how sensible event follows sensible event, and assuming that the future will be like the past, they enable us, at least to some small extent, to foretell the future. They embody our list of typical cases.

But we are not content with what we see, hear, feel and touch, with likenesses which can be verified by our senses. We are always trying to reduce our list of typical or simple cases by imagining likenesses which we cannot directly perceive—in other words, by framing hypotheses as to the constitution of things, beyond the reach of direct verification by our senses.

The most conspicuous of these hypotheses is the atomic hypothesis of the constitution of matter. We imagine that bodies, however continuous they appear to our sense of sight or to our touch, are really made up of small particles called atoms, with separating interspaces. The mutual actions of these atoms across the separating spaces are supposed to be like the mutual actions which we observe between big, evident masses.

If we believed that a piece of matter is as continuous as it seems to the eye, we should have to suppose that contraction and expansion are simple facts, facts unlike any others. This supposition was characterised by Principal Sir Arthur Rücker, in his British Association Address at Glasgow, as unintelligible and absurd in that it leaves expansion and contraction unexplained. This appears to me to be carrying the passion for explanation to excess. To say that any simple fact, any fact which so far stands by itself and is unlike others, *must* have hidden likenesses, *must* be explicable, and that the contrary is absurd, is an *a priori* mode of dealing with Nature which she may at any time resent and refute by bringing our so-called explanations to nought.

But still Sir Arthur Rücker's statement well illustrates our unwillingness to be brought face to face with the simple and ultimate type, and I have no doubt that the atomic hypothesis was first imagined to escape the necessity of taking the expansion and contraction of solid and liquid matter as simple, inexplicable, ultimate facts. Were matter continuous, they would have to be so taken. But imagine that matter consists of a group of separated atoms, and contraction is merely a drawing together of the members of the group, expansion is merely a separating out. We have explained them by likening them to what we observe every day in a crowd of men or a flock of birds.

Further, we know that matter in thin films or in fine streams does not behave like matter in bulk. New properties are observed which are not to be accounted for by the reduction of the old properties in proportion to the scale of reduction. If the structure is atomic, we can imagine how these new properties will come in when the films or streams are but a few atoms thick. If matter is continuous, we have as yet no kind of explanation of such properties.

But the hypothesis is, of course, extended far beyond its use to explain these mechanical phenomena. Long before the law of constancy of energy was put into exact form, the observed interchanges of energies had led to the idea that some of the observed forms might differ from each other only in their effects on our senses. The differences were thought to be in us and not in Nature. If we could only sharpen our powers of observation, magnify our scale of vision, and make our perception of time more minute, the differences in kind would vanish. Here the atomic hypothesis came in to provide explanations or likenesses. When kinetic energy gave way to heat, it was supposed to be only a transfer of motion from big masses to little atoms, and so heat was explained as a mode of motion. When the atoms clashed together in this motion, they were made to vibrate and send out waves, and so light was explained in some degree by being likened to the waves sent out by jangled bells.

One form of energy after another has thus been reduced to energy of motion, or energy of separation of the atoms, and so has been likened to the observed energy of motion, or energy of separation of big masses.

The chemist, above all, has made use of the hypothesis to explain chemical energy as energy of separation of the atoms, so likening it to the energy of separation of a planet from the sun. Imagining some eighty or a hundred different types of atom, he has sought to explain chemical facts by the configurations and mutual actions of groups of these elementary types. He has likened chemical compounds to solar and stellar systems.

Many chemical and physical facts long ago suggested the idea that we may go still further in our explanations by supposing that the atoms are themselves built up of still smaller bits of matter, or corpuscles, all like one another. This idea has been brought very much to the front by recent electrical researches consequent on the discovery of the Röntgen radiation, and now Professor J. J. Thomson is teaching us that one atom differs from another merely in the number and grouping of the finer 'corpuscles' of which each is composed. At the present time, then, the aim of the atomic hypothesis is to show that we need assume only one type of matter, the corpuscle, and give it only one type of action on its fellows, and that we may then explain all the phenomena of physics by the grouping, motions, and mutual actions of these primordial bits of matter.

Thus physics would be a sort of microcosmic astronomy. In place of the telescope we should need a microscope a million times more powerful than any yet made. Instead of a seconds clock, we should need a time-keeper making billions of beats per second. Instead of an astronomer, we should need a being to watch the corpuscles to whom a second seemed a million years.

The celestial astronomer finds that if he knows the masses, positions, and velocities of the heavenly bodies at any instant, and if he watches them long enough to measure the variation of their mutual action as their distance varies, he can then retire to his calculating-room, and not only describe their positions in the past but also prepare a 'Nautical Almanac' foretelling their positions in the future. Similarly, the atomic astronomer believes that if he knew the masses, positions, and motions of the atoms or corpuscles at any instant, and that if, further, he knew the change of mutual action with change of distance apart, he too could prepare an atomic 'Nautical Almanac.' Not only could he give an account of the universe in the past, but he could reach forward into the future.

As Laplace put it in his celebrated idea of the Perfect Calculator (Ward, *Naturalism and Agnosticism*, vol. 1, p. 41):

'An intelligence who for a given instant should be acquainted with all the forces by which nature is animated, and with the several positions of the beings composing it, if, further, his intellect were vast enough to submit these data to analysis, would include in one and the same formula the movements of the largest body in the universe and those of the lightest atom. Nothing would be uncertain for him; the future as well as the past would be present to his eyes.'

But let us consider what must be the actual method of the Laplacean calculator when dealing with atoms and corpuscles and those data which are altogether beyond the range of our senses. First he will take the data in the form given by his senses, the positions, motions, actions and conditions as to light, heat, electricity and so forth, of the visible or otherwise sensible bodies in his universe. But these bodies are far too large for his atomic calculating-machine. Then he must grind up all his data to powder of atomic or even corpuscular fineness to suit the calculating-machine. This powder he will put into the machine. He will turn the handle and extract the product. But it is still in atomic form, and so is useless as far as telling him what his senses will perceive. He must build up the atoms once more into gross matter, translate the atomic energy into the recognised forms which affect our senses, before he can verify his results by sight, or feel or touch. Our senses know nothing of molecules, atoms or corpuscles, of heat as a mode of atomic motion, of waves of light spreading out from clashing molecules. We want to know what hotness we shall feel, what colour we shall see, what matter we shall touch.

And so we see that the ultra-sensible atomic hypothesis is but an imagined bridge to connect one set of sensible events with another. We can see kinetic energy. When it disappears against friction we can feel the heat which takes its place. We connect the two by imagining the atoms which take up the disappearing motion.

There is a growing school of physicists who claim that the trend of science is to do away with such hypothetical bridges, who regard atoms and molecules as needless suppositions. Or at most they regard the hypotheses as merely temporary structures which may perhaps have done good service in their time. Now, they say, we should seek to describe the sensible in terms of the sensible only, we should investigate the laws of the transformation of energy as we actually see it going on, and we should refrain from introducing atoms and the like imagined things whose existence we can never directly verify.

I have no doubt whatever that our ultimate aim must be to describe the sensible in terms of the sensible. But I see, too, what gulfs there still are separating one part of our knowledge from another, and I see no harm in throwing temporary bridges of hypothesis across these gulfs to connect what would otherwise be detached regions. They allow us to pass to and fro with ease, and have been and are of enormous help to us in our exploration of Nature. But we must bear in mind that we may have many types of connecting bridge, many forms of hypothesis, all perhaps equally serviceable. All perhaps to be broken down and abandoned when we have filled in the gulf which they crossed, and have made firm roadways built of sensible fact.

Whether, however, we accept the creed of the atomic philosopher, or whether we agree with the disciples of this newer school, the school of Energetics, the main aim of science is the same, to obtain a description of Nature as concise as possible by classifying all observed likenesses. Here we must distinguish between the method of science that of classifying likenesses—and the result which has followed that method in its application to physics—viz., the reduction of phenomena to typical cases whose actions we can describe. It is this result which enables us to forecast, on the assumption that the typical cases will remain like themselves, in the future as in the past. Wherever, in what at any rate for the present we may call lifeless matter, this method has been applied it has led to similar results, and the wider and more complete our knowledge has become, the more possible has it been to foretell the future from the past. Now the question arises whether the results will still be the same when the method is applied to all Nature, living as well as non-living, whether it will still give us typical cases of known behaviour when applied to the affairs of life and mind, and whether prediction will be just as possible here as in the motions and actions of non-living matter.

In stating this problem it does not signify whether we use the language of the atomic hypothesis or whether we state it in terms which could be used by the newer school who will have no dealings with atoms. The problem is just the same from either point of view. I shall state it, therefore, in the more familiar language of the atomic hypothesis.

Let us suppose that the Laplacean calculator has been found, and that he has been set to work. He has studied, we will say, all the atoms, and knows all their mutual actions. In his laboratory he has found exactly how hydrogen, carbon, oxygen, nitrogen and the rest, behave. He has found the conditions under which they group together to form compounds, he has learned the shapes of the atomic groups, and he knows under what conditions they will fly apart to form new groups. Suppose that he watches certain groups, and that then from their positions and surroundings he calculates their future course. Now let him watch that course. He finds that they enter certain plants and help to build them up. Later they are taken in by some animal, and later still they are taken into the system of a man, and ultimately they find their way to his brain. Would Laplace's calculator find all his predictions verified as his atoms came in contact with living matter and were themselves concerned with life? Suppose the man into whose brain the atoms entered were Laplace's friend and chief, Napoleon. If the calculator took into account every atom in Napoleon's frame, would he be able to calculate all the motions of Napoleon, all his actions on the similar surrounding groups of atoms which we call his generals? Could the calculator foretell the eclipse of Waterloo as surely as the astronomer foretells an eclipse of the sun? Is man, in fact, from the physical point of view, a group of atoms, each of which behaves as it would with the same neighbours were it part of a non-living system? Leaving out of account thought and feeling, which obviously do not come within the range of observation of the physicist as physicist, can a man's motions and actions all be classed under the general laws which sum up our knowledge of the matter of which he is made, when those laws are formulated from the study of non-living matter? Are the typical cases the same? Could the calculator write, even before Napoleon's birth, a complete physical biography of him from the first to the very last phase, stating where he would go, how he would move, what energy he would emit in the form of sound or reflect in the form of light? Could he say how these energies would affect the motions of his surroundings? It may, by the way, be admitted that such a history would make very poor reading.

But let us now ask ourselves another question. Suppose our calculator not only great as a physicist and mathematician, but equally great as a psychologist and moralist. Could he write down in parallel columns a double account of his Napoleon, in the one column a history of him regarded as a group of atoms, in the other a biography of him, setting forth an account of his thoughts and feelings, his intentions and will? And assuming that he could, would he find correspondences in the two columns, such a thought corresponding to such a set of molecular groupings, such a volition to such a set of molecular motions? Would he find the correspondence so complete that he could at any time fill in a gap on what we will call the psychical side from his complete knowledge of the physical side?

Or confining himself to psychology, would he find that mental condition followed mental condition according to laws which he could formulate? Would he be able to make a list of typical cases of mental conditions of which he could state the consequents, so that, resolving Napoleon's mind into these conditions, he could foretell how he would feel and think, as well as act?

If so, he could proceed along either line, the physical or the psychical, and he might use his psychical knowledge to fill in gaps on the physical side.

We have some suggestion of the Laplacean calculator in our great physicists, some suggestion of the perfect psychical calculator in our great mental philosophers. But we have hardly any suggestion as yet of the combined perfect physicist and perfect psychologist who could point out the correspondences between the two sets of conditions, physical and psychical. Our knowledge of such correspondences as may exist is hardly more than beginning. Some progress has been made in showing physical conditions corresponding to disease, when the mind is disordered, when life is impaired, when decay and return to non-living matter are in progress. The pathologist can tell us something of the morbid conditions of the tissues corresponding to pain, he can show that degeneration of the brain corresponds to idiotcy, that intrusion of foreign non-living matter ends in death. But of the physical correspondences to vigorous life, and thought, and will, he can only give the most general and vague account.

Is this ignorance to be set down to want of experience, and to want of proper means of investigation, ignorance which we may naturally expect in the infancy of a science? Or may it not rather be ascribed to the non existence of the correspondences? May not our knowledge and ignorance just correspond to the facts, knowledge where life is ceasing and is giving place to ordinary physical actions, ignorance where life is in full sway and the actions are different in kind from those studied in non-living matter?

I believe that the latter is the true view, and it appears to me that its truth is borne out by the want of analogies between mental conditions and physical conditions, analogies which we should expect to find were there complete correspondence between the two.

At first sight there may appear to be analogies. We may, for instance, think it possible to connect desire with physical attraction, dislike with physical repulsion. But only at first sight. When closely examined the analogy breaks down. For the physical law states that if A attracts B, B equally attracts A, whereas everyone knows that while A may like B, and seek his company, B may be unutterably bored by A, and seek every means to avoid his company.

Or note how utterly without analogy in the physical universe is admiration for the good, hatred for the bad. It is true that we frequently describe qualities of physical objects as good or bad; but this very mode of description proves the point, for when we examine the meaning, we find that the good is serviceable to the describer, the bad unserviceable: the good falling in with his wish or purpose, the bad running counter to it. When we speak of a good conductor of electricity or a bad reflector of light, it is not the physical quality at all, but the adaptability to the desires of the user which we are connoting by the terms. To speak of a praiseworthy molecule or a wicked wave would be utterly ridiculous.

Then observe how different is the relation of past and present and future in the two cases. In physical phenomena we deduce the future from the past. The present and future are, as it were, pushed into being by the past. But on the mental side the present is drawn into being by the future. Indeed, we might almost distinguish the living being from the non-living system by saying that while the latter lives on and by its past, the former lives by trying to realise its future.

And above all the choice of action which is implied in our attempt to realise an imagined future has no correspondent, no analogy whatever in physical actions. Our sense of responsibility when that choice is made is utterly unlike anything in the physical world.

An attempt is made to save the situation, to liken choice to physical action, by saying that our acts are determined by motives, that deliberation is but the competition of all the motives operating, and that ultimately we yield to the strongest as certainly as a body moves under the strongest force. Our will is like a feather fluttering through the air, swayed hither and thither by successive puffs, and finally borne off by the strongest current. Perhaps it is worth while pointing out that even with this idea of motives the analogy fails. A body does not yield to the strongest force. It moves in the direction of the resultant of all the forces from the greatest to the least, every one counting and having its full effect. The will finally takes one course with one aim and the motives prompting to other courses all drop out of action.

But there is yet a greater contrast between physical action and mental action. In a physical system we can make previous observations and experiments, assign quantitative values to the different conditions, and foretell the resulting motion from their combination. In the mind we have no method of measuring motives. We can only judge, after deliberation has resulted in action, which motive was the strongest by assigning strength to that which prevailed. We can, if we like, assign unit value to this and zero to all the rest which have failed to act, but there is no kind of physical measurement.

I hold that we are more certain of our power of choice and of responsibility than of any other fact, physical or psychical, unless it be indeed that we are still more certain of the power of choice and of the responsibility of someone else who does us what we regard as an intentional injury. We are certain, all of us, in everyday life, that this power of choice exists, whatever conclusion we may come to in the quiet of our studies. It appears to me equally certain that there is no correspondence yet made out between the power of choice and any physical action, and there does not seem any likelihood that a correspondence ever will be made out. The freedom of choice, then, is unlike anything else in Nature, it is a simple fact.

Holding this view, I am bound to repudiate the physical account of Nature when it claims to be a complete account. I am bound to deny that the Laplacean calculator can be successful when he takes man and the mind of man into his calculations.

It is not that the scientific method is inapplicable or that it fails. It is still *the* method. We must still classify according to likenesses, whether we are dealing with physical or with psychical, though the results are different in the two cases. In physics we seek to reduce phenomena to a few simple types, of which we know and can foretell the actions. If we adopt the corpuscular hypothesis, we seek to reduce to one single type and its assumed action. But in mind we are, I believe, in each individual life brought up against an individual type which we can no further resolve. Instead of the single corpuscle, or the eighty or one hundred atoms of the chemist, we have as many types as there are conscious beings—perhaps as many as there are living beings of any kind. If, further, we accept our own mental experience, we must grant that we do not and cannot know the conditions and actions of these innumerable simple types. Every time an intention is formed in the mind and a deliberate choice is made, we have an event unlike any previous event. Freedom of will is a simple fact, unlike anything else, inexplicable.

In our search for likenesses we are brought face to face with unlikenesses, and it is just as much a duty of science to recognise these unlikenesses as to catalogue the likenesses.

While, then, the scientific method still applies in the psychical region, in so far as it consists in classing together likenesses and in recognising and separating unlikenesses, the material dealt with is utterly different from that in the physical region, in that no similar quantitative measurement can be made and no explanation in the sense of complete reduction to types of known behaviour appears possible. If we explain our actions by purpose we use the word 'explain' in a sense different from that which it has in physics, where we describe the present in terms of the past, rather than in terms of a hoped-for future.

We must recognise that this view of life will bring us into conflict with the fundamental laws of non-living matter. Undoubtedly, will results in physical motion. To his fellows a man is a portion of matter which can only act on them, so far as we know, through their senses. How, then, do the physical actions going on in him differ from the physical actions going on in non-living matter?

It has often been pointed out that the will may act as a guiding power changing the direction of motion of the atoms and molecules in the brain, and we can imagine such a guiding power without having to modify our ideas of the constancy of matter or the constancy of motion, or even the constancy of energy. We may suppose, for example, that two molecules are making straight for each other in the brain and that the will in some way introduces a constraint which pushes them always at right angles to their direction of motion. So they may be guided to glide past each other instead of clashing together. This constraint will not change the mass, and we can imagine it so put in that it introduces equal and opposite momenta and so does not affect the total motion. The change of direction implies a slight change of spin, which may be compensated for by a slight opposite spin put on the rest of the body. The energy will not be changed, since a merely deflecting force does no work. But the interposition of the guiding power *does* affect the transformation of energy; instead of the clash which the physicist would foretell there would be a new configuration as the molecules glided past each other in their new directions. The resulting transformation would not fall in with those formulated in the science of energetics. To bring in the Laplacean calculator once more before we banish him to the realm of impossibilities. If he is watching the dance of atoms in the brain, he will see every now and then changes of direction of motion, not calculated in his system of transformations of energy, not provided for in his forecast.

I do not lay any great stress on this conception of the physical action of the will as a guiding power, which does not alter the sum total of energy but only alters its transformations. Still the laws of constancy of matter, motion and energy do appear to be more fundamental than those of the transformation of energy. For while the former will hold whether we go forward or backward in time, the latter are essentially affairs of time, they take time to be effected, and if time could be reversed, or if all the motions in the universe could be suddenly reversed, all the transformations would be reversed, and some, at least, of the laws would, I think, require restatement. But it may be said that after all this is only an attempt to evade the point at issue by saying that some physical actions are not so certain or so constant as the rest. It is better to face the situation boldly and claim for our mental experience as great certainty as that which the physicist claims for his experience in the outside world. If our mental experience convinces us that we have freedom of choice, we are obliged to believe that in mind there is territory which the physicist can never annex. Some of his laws may still hold good, but somewhere or other his scheme must cease to give a true account.

64.

RADIATION IN THE SOLAR SYSTEM.

[Afternoon address delivered at the Cambridge meeting of the British Association, August 23, 1904.]

[*Nature*, **70**, 1904, pp. 512–515.]

I propose to discuss this afternoon certain effects of the energy which is continuously pouring out from the sun on all sides with the speed of light, the energy which we call sunlight when we enjoy the brilliance of a cloudless sky, which we call heat when we bask in its warmth, the stream of radiation which supports all life on our globe and is the source of all our energy.

As we all know, this ceaseless stream of energy is a form of wave-motion. If we pass a beam of sunlight, or its equivalent, the beam from an electric arc, through a prism, the disturbance is analysed into a spectrum of colours, each colour of a different wave-length, the length of wave changing as we go down the spectrum from, say, 1/30,000 of an inch in the red to 1/80,000 of an inch in the blue or violet.

But this visible spectrum is merely the part of the stream of radiation which affects the eye. Beyond the violet are the still shorter waves which affect a photographic plate or a fluorescent screen, and will pass through certain substances opaque to ordinary light. Here, for instance, is a filter devised by Prof. Wood which stops visible rays, but allows the shorter invisible waves to pass and excite the fluorescence of a platinocyanide screen.

Again, beyond the red end are still longer waves, which are present in very considerable amount, and can be rendered evident by their heating effect. We can easily filter out the visible rays and still leave these long waves in the beam by passing it through a thin sheet of vulcanite. A piece of phosphorus placed at the focus of these invisible rays is at once fired, or a thermometer quickly rises in temperature. The waves which have been observed and studied up to the present time range over some nine octaves, from the long waves described to the section yesterday by Prof. Rubens, waves of which there are only 400 in an inch, down to the short waves found by Schumann in the radiation given off by hydrogen under the influence of the electric discharge, waves of which there are a quarter of a million in an inch. No doubt the range will be extended.

Radiant energy consists of a mixture of any or all of these wave-lengths, but the eye is only sensitive at the most to a little more than one octave in the nine or more.

This radiation is emitted not only by incandescent bodies such as the sun, the electric arc, or flames. All bodies are pouring out radiant energy, however hot or cold they may be. In this room we see things by the radiation which they reflect from the daylight. But besides this borrowed radiation, every surface in the room is sending out radiation of its own. Energy is pouring forth from walls, ceiling, floor, rushing about with the speed of light, striking against the opposite surfaces, and being reflected, scattered, and absorbed. And though this radiation does not affect our eyes, it is of the utmost importance in keeping us warm. Could it be stopped, we should soon be driven out by the intense cold, or remain to be frozen to death.

As the temperature of a body is raised, the stream of radiation it pours out increases in quantity. But it also changes in quality. Probably the surface always sends out waves of all lengths from the longest to the shortest, but at first, when it is cold, the long waves alone are appreciable. As it gets hotter, though all the waves become more intense, the shorter ones increase most in intensity, and ultimately they become so prominent that they affect our sense of sight, and then we say that the body is red or white hot.

The quality of the stream depends on the nature of the surface, some surfaces sending out more than others at the same temperature. But the stream is the greatest from a surface which is, when cold, quite black. Its blackness means that it entirely absorbs whatever radiation falls upon it, and such a surface, when heated, sends out radiation of every kind, and for a given temperature each kind of radiation is present to the full extent, that is, at a given temperature no surface sends out more of a given wave-length than a black surface.

A very simple experiment shows that a black surface is a better radiator, or pours out more energy when hot, than a surface which does not absorb fully, but reflects much of the radiation which falls upon it. If a platinum foil with some black marks on it be heated to redness, the marks, black when cold, are much brighter than the surrounding metal when hot; they are, in fact, pouring out much more visible radiation than the metal.

It is with these black surfaces that I am concerned to-day. But, inasmuch as it seems absurd to call them black when they are white hot, I prefer to call them full radiators, since they radiate more fully than any others.

For a long time past experiments have been made to seek a law connecting the radiation or energy-flow from a black or fully radiating surface with its temperature. But it was only twenty-five years ago that a law was suggested by Stefan which agrees at all satisfactorily with experiment. This law is: that the stream of energy is proportional to the fourth power of the temperature, reckoned from the absolute zero 273° below freezing-point on the centigrade scale. This suggestion of Stefan served as the starting point of new and most fertile researches, both theoretical and practical, and we are glad to welcome to this meeting Profs. Wien, Lummer, and Rubens, who have all done most brilliant work on the subject.

Among the researches on radiation recently carried out is one by Kurlbaum in which he determined the actual amount of energy issuing from the black or fully radiating surface per second at 100° C., and therefore at any temperature.

Here is a table which gives the amount at various temperatures, as calculated from Kurlbaum's results:

Rate of Flow of Energy from 1 sq. cm. of Fully Radiating or 'Black' Surface.

Absolute Temperature	Calories Grams of water heated 1 per sec.
0°	0·0
100° Air boils	0·000127
300° Earth's surface	0·0103
1000° Red heat	1 27
3000° Arc carbon	103
6000°	1650
6250°	1930

As an illustration of the 'fourth-power law,' let us see what value it will give us for the temperature of the sun, assuming that he is a full radiator, or that his surface, if cooled down, would be quite black.

We can measure approximately the stream of energy which the sun is pouring out by intercepting the beam falling on a surface exposed to full sunlight, measuring the heat given to that surface per second, and then calculating what fraction the beam is of the whole stream issuing from the sun.

This was first done by Pouillet, and his method will serve to illustrate the principle of all other methods.

In his apparatus the sunlight fell full on a box containing water, and the rate at which the water rose in temperature gave the energy in the stream of solar radiation falling on the box.

Simple as the experiment appears, the determination is beset with difficulties, the chief being the estimation of the fraction of the energy intercepted by the atmosphere, and we are still unable to give a very definite value. Indeed, we cannot yet say whether the outflow of energy is constant or whether it varies. In all probability, however, it does vary, and Prof. Langley, who has devoted years of work to the subject, has recently obtained evidence indicating quite considerable variation.

We may, however, assume that we are not very far from the true value if we say that the stream of radiation from the sun falling perpendicularly on 1 sq. cm. outside the earth's atmosphere will heat 1 gm. of water 1/24° C. every second, or will give 1/24 calorie per sec.

Now the area of a sphere round the sun at the distance of the earth is 46,000 times the area of the sun's surface. The energy from 1 sq. cm. of the sun thus passes through 46,000 sq. cm. at the surface of the earth. It is therefore 46,000 × 1/24 cal./sec., or 1920 cal./sec. But from the table already given, a black surface at 6250° absolute, say 6000° C., gives 1930 calories per second, or the temperature of the sun's radiating surface is 6000°—if he is a full radiator, and there is good reason to suppose that no great error is made in taking him to be one.

Let us now take another illustration of the fourth-power law.

Imagine a little black body which is a good conductor of heat placed in full sunlight at the distance of the earth. Let it be 1 sq. cm. in cross-section, so that it is receiving 1/24 calorie per second.

It will soon warm up to such a temperature that it gives out just as much as it receives, and since it is so small, heat will rapidly flow through it from side to side, so that it will all be very nearly at the same temperature. A sphere 1 sq. cm. in cross-section has area 4 sq. cm., so that it must be giving out from each sq. cm. of its surface 1/96 = 0·0104 calorie each second. From the table above it will be seen that this corresponds very nearly indeed to a temperature of 300° absolute or 27° C., say 80° F.

It is to be noted that this only applies to a little round body. A flat plate facing the sun would be about 60° C. hotter, while if it were edgewise to the sun it might be very much colder.

Let us now see what would be the temperature of the small black sphere at other distances from the sun. It is easily seen that, inasmuch as the heat received, and therefore that given out, varies inversely as the square of the distance, the temperature, by the fourth-power law, will vary inversely as the square root of the distance.

Here is a table of temperatures of small black spheres, due to solar radiation:

Distance from Sun's centre	Temperature Centigrade	
3¾ million miles	1200 C.	Cast iron melts
23 million miles	327°	Lead nearly melts
At Mercury's distance	210	Tin nearly melts
At Venus's distance...	85	Alcohol boils freely
At Earth's distance	27°	Warm summer day
At Mars's distance	– 30°	Arctic cold
At Neptune's distance	219°	Nitrogen frozen

We see from this table that the temperature at the earth's distance is remarkably near the average temperature of the earth's surface, which is usually estimated as about 16° C. or 60° F. This can hardly be regarded as a mere coincidence. The surface of the earth receives, we know, an amount of heat from the inside almost infinitesimal compared with that which it receives from the sun, and on the sun, therefore, we depend for our temperature. The earth acquires such a temperature, in fact, that it radiates out what it receives from the sun. The earth is far too great for the distribution of heat by conduction to play any serious part in equalising the temperature of different regions. But the rotation about its axis secures nearly uniform temperature in a given latitude, and the movements of the atmosphere tend to equalise temperatures in different latitudes. Hence we should expect the earth to have, on the average, nearly the temperature of the small black body at the same distance, slightly less because it reflects some of the solar radiation, and we find that it is, in fact, some 10° less.

Prof. Wien was the first to point out that the temperature of the earth has nearly the value which we should expect from the fourth-power law.

Here is a table showing the average temperatures of the surfaces of the first four planets on the supposition that they are earth-like in all their conditions:

Table of Temperatures of Earth-like Planets.

Mercury	196° C.
Venus	79° ,, *
Earth	17° ,,
Mars	– 38° ,,

The most interesting case is that of Mars. He has, we know, a day nearly the same in length as ours; his axis is inclined to the ecliptic only a little more than ours, and he has some kind of atmosphere. It is exceedingly difficult to suppose, then, that his average temperature can differ much from – 38° C. His atmosphere may be less protective, so that his day temperature may be higher, but then to compensate, his night temperature will be lower. Even his highest equatorial temperature cannot be much higher than the average. On certain suppositions I find that it is still 20° below the freezing-point, and until some new conditions can be pointed out which enable him to establish far higher temperatures than the earth would have at the same distance, it is hard to believe that he can have polar caps of frozen water, melting to liquid in his summer and filling rivers or canals. Unless he is very different from the earth, his whole surface is below the freezing-point.

Let us now turn from these temperature-effects of radiation to another class of effects, those due to pressure.

* [This is probably a slip and intended for 69° C., see p. 315. Ed.]

More than thirty years ago Clerk Maxwell showed that, on his electromagnetic theory of light, light and all radiation like light should press against any surface on which it falls. There should also be a pressure back against any surface from which radiation is reflected or from which it is issuing as a source, the value in every case being equal to the energy in a cubic centimetre of the stream. The existence of this pressure was fully demonstrated independently by Lebedew and by Nichols and Hull some years ago in brilliant experiments in which they allowed a beam of light to fall on a suspended disc in a vacuum. The disc was repelled, and they measured the repulsion and found it to be about that required by Maxwell's theory. Nichols and Hull have since repeated the experiment with greater exactness, and there is now no doubt that the pressure exists and that it has Maxwell's value.

The radiation, then, poured out by the sun is not only a stream of energy. It is also, as it were, a stream of pressure pressing out the heavenly bodies on which it falls. Since the stream thins out as it diverges, according to the inverse square of the distance, the pressure on a given surface falls off according to the same law. We know the energy in a cubic centimetre of sunlight at the distance of the earth, since, moving with the velocity of light, it will supply 1/24 calorie per second. It is easy to calculate that it will press with a force of 6×10^{-5} dyne on a square centimetre, an amount so small that on the whole earth it is but 70,000 tons, a mere trifle compared with the three million billion tons with which the sun pulls the earth by his gravitation.

But now notice the remarkable effect of size on the relation between the radiation-pressure and the gravitative pull. One is on the surface and proportional to the surface, while the other penetrates the surface and pulls every grain of matter throughout the whole volume.

Suppose we could divide the earth up into eight equal globes. Each would have half the diameter of the earth and a quarter the surface. The eight would expose twice the surface which the earth exposes, and the total radiation-pressure would be doubled, while the total gravitative pull would be the same as before. Now divide up each of the eight into eight more equal globes. Again the radiation-pressure would be doubled, while gravitation would be the same.

Continue the process, and it is evident that by successive division we should at last arrive at globes so small and with total surfaces so great that the pressure of the radiation would balance the pull of gravitation. Mere arithmetic shows that this balance would occur when the earth was divided up into little spheres each 1/40,000 cm. in diameter.

In other words, a little speck 1/40,000 cm., say 1/100,000 of an inch in diameter, and of density equal to that of the earth, would be neither attracted nor repelled by the sun.

This balance would hold at all distances, since both would vary in the same way with the distance. Our arithmetic comes to this: that if the earth were spread out in a thin spherical shell with radius about four times the distance of Neptune, the repulsion of sunlight falling on it would balance the inward pull by the sun, and it would have no tendency to contract.

With further division repulsion would exceed attraction, and the particles would be driven away. But I must here say that the law of repulsion does not hold down to such fine division. The repulsion is somewhat less than we have calculated owing to the diffraction of the light.

Some very suggestive speculations with regard to comets' tails have arisen from these considerations, and to these Prof. Boys directed the attention of Section A last year. We may imagine that the nucleus of a comet consists of small meteorites. When these come near the sun they are heated and explosions occur, and fine dust is produced not previously present. If the dust is sufficiently fine, radiation may overpower gravitation and drive it away from the sun, and we may have a manifestation of this expelled dust in the tail of the comet.

I do not, however, want to dwell on this to-day, but to look at the subject in another way.

Let us again introduce our small black sphere, and let us make it 1 sq. cm. in cross-section, 1·13 cm. in diameter, and of the density of the earth. The gravitation pull on it is 42,000 times the radiation-pressure.

Now let us see the effect of size on the radiating body. Let us halve the diameter of the sun. He would then have one eighth the mass and one-quarter the surface. Or, while his pull was reduced to one eighth, his radiation-push would only be reduced to one-quarter. The pull would now be only 21,000 times the push. Halve the diameter again, and the pull would be only 10,500 times the push. Reduce the diameter to 1/42,000 of its original value, that is, to about 20 miles, and the pull would equal the push.

In other words, a sun as hot as ours and 20 miles in diameter would repel bodies less than 1 cm. in diameter, and could only hold in those which were larger.

But it is, of course, absurd to think of such a small sun as this having so high a temperature as 6000°. Let us then reduce the temperature to 1/20, say 300° absolute, or the temperature of the earth. Then the radiation would be reduced to the fourth power of 1/20, or 1/160,000, and the diameter would have to be reduced to 1/160,000 of 20 miles, or about 20 cm., say 8 inches, when again radiation would balance gravitation.

It is not very difficult to show that if we had two equal spheres each of the density and temperature of the earth they would neither attract nor repel each other—their radiation-pressure would balance the gravitative pull—when their diameters were about 6·8 cm.*, when, in fact, they were about the size of cricket balls.

It must be remembered that this is only true for spheres out in space receiving no appreciable radiation from the surrounding region.

It would appear that we have arrived at a result of some importance in considering the aggregation of small meteorites. Imagine a thinly scattered stream of small meteorites at the distance of the earth from the sun. Then, even if they be as large as cricket balls, they may have no tendency to move together. If they are smaller they may even tend to move apart and scatter.

In conclusion, let me mention one more effect of this radiation-pressure. You will remember that radiation presses back against any surface from which it issues. If, then, a sphere at rest in space is radiating equally on all sides it is pressed equally on all sides, and the net result is a balance between the pressures. But suppose that it is moving. It is following up the energy which it pours forth in front, crowding it into a smaller space than if it were at rest, making it more dense. Hence the pressure is slightly greater, and it can be shown that it is greater the greater the velocity and the higher the temperature. On the other hand, it is drawing away from the energy which it pours out behind, thinning it out, as it were, and the pressure at the back is slightly less than if the sphere were at rest.

The net result is a force opposing the motion, a force like viscous friction, always tending to reduce the speed.

Thus calculation shows that there is a retarding force on the earth as it moves along its orbit amounting in all to about 20 kgm., say 50 lb.†. Not very serious, for in billions of years it will only reduce the velocity by 1 in a million, and it will only have serious effects if the life of the earth is prolonged at its present temperature to hundreds of billions of years.

But here again size is everything. Reduce the diameter of the moving body, and the retarding effect increases in proportion to the reduction. If the earth were reduced to the size of a marble, the effect would be appreciable in a hundred thousand years. If it were reduced to a speck of dust a thousandth of a centimetre in diameter, the effect would be appreciable in a hundred years.

Note what the effect would be. Imagine a dust-particle shot out from the earth and left behind to circulate on its own account round the sun. It would be heated by the sun and would be radiating out on all sides. As it journeyed forward there would be a resisting force tending to stop it. But instead of acting in this way the resistance would enable the sun to pull

* [This should be 2·26 cm. (see p. 709). Ed.]

† [For correction of this figure, see p. 708. Ed.]

the particle inwards, and the fall inwards would actually increase the velocity. This increase in the velocity would increase the resistance, and at the same time the approach to the sun would raise its temperature, increase the radiation, and so increase the resistance still further. The particle would therefore move in a more and more rapid spiral orbit, and ultimately it would fall into the sun. Small marble-sized meteorites would fall in from the distance of the earth probably in a few million years. Small particles of dust would be swept in in a few thousand years.

Thus the sun is ever at work keeping the space round him free from dust. If the particles are very minute he drives them forth into outer space. If they are larger he draws them in. It is just possible that we have evidence of this drawing in in the zodiacal light, that vast dust-like ri g which stretche from the sun outwards far beyond the orbit of the earth, and is at once the largest and the most mysterious member of the solar system*.

* [See following letter in correction. Ed.]

65.

RADIATION-PRESSURE.

[*Nature*, **71**, 1904, pp. 200–201.]

On p. 515 of your issue of September 22* I stated that there is a retarding force on the earth as it moves along its orbit amounting in all to about 20 kgm. The calculation was made on the supposition that the earth is a full radiator of uniform temperature. I have found on revising the calculation that there was an error in the arithmetic, and that the force is considerably greater, though still too small to have an effect worth considering. The following is a simple method of obtaining its value. It assumes that the earth may be treated as a black sphere exposed to sunlight, radiating as much as it receives, and with all its surface at one temperature.

If the stream of solar energy falling normally on 1 sq. cm. is S per second, a black sphere, radius a, receives $\pi a^2 S$ per second. If it radiates R per second per sq. cm. its total radiation is $4\pi a^2 R$, and the assumption of equal receipt and expenditure gives $R = S/4$. The total repulsive force exerted by the sun's radiation is $S\pi a^2/U$, where U is the velocity of light. The total retarding force due to velocity u in the orbit is $4\pi a^2 Ru/3U^2$. This is the Doppler effect due to crowding of energy in front and opening out behind (*Phil. Trans.*, A, 202, p. 546†, corrected by final note). Hence we have

$$\frac{\text{Retarding force}}{\text{Solar repulsion}} = \frac{u}{3U}.$$

At the earth's distance u/U is about 10^{-4}, so that the retarding force is about 1/30,000 of the solar repulsion.

If we take S/U as $5{\cdot}8 \times 10^{-5}$ dyne/cm.^{2} (*Phil. Trans.*, *loc. cit.*, p. 539)‡, and the radius of the earth as $6{\cdot}37 \times 10^8$ cm., the total solar repulsion is about 75×10^6 kgm., say 75,000 tons, and the retarding force is about 2500 kgm.

But another effect comes in which will more than counterbalance this. The hemisphere of the earth which is advancing in the orbit is on the whole colder than that which is retreating, owing to the lag in the warming of the

* [*Collected Papers*, p. 706.] † [*Collected Papers*, p. 326.]

‡ [*Collected Papers*, p. 318.]

surface exposed to the sun. I find that if one hemisphere is at 301 A. and the other at 300° A., the greater radiation from the warmer side gives a net push directed from that side to the colder of about 165,000 kgm. Of course this hemispherical distribution of temperature is only a rough approximation to the real condition, and even if the force be as large as 165,000 kgm. only a component of it acts along the orbit tending to accelerate the motion. Still, that component must almost certainly be much greater than the retarding force due to the Doppler effect, and on the whole, therefore, there is probably a small acceleration in the orbit. A force of 2500 kgm. would destroy about $4/10^{18}$ of the earth's momentum in one year. Even if the accelerating force were twenty-five times as great as this it would only generate $1\ 10^{16}$ of the present momentum in one year. This illustrates the insignificance of radiation-pressure on the larger bodies in the solar system.

I take this opportunity of correcting another error in the address in *Nature* of September 22, which has been pointed out to me by Mr. C. T. Whitmell. It arose from some very faulty arithmetic on p. 541 of the paper in the *Philosophical Transactions* already referred to*. Apparently in the formula giving the radius of each of two equal spheres the mutual radiation repulsion of which balances their gravitative attraction, a square root of 10 was omitted, and the value of that radius should be $a = 0{\cdot}69\theta^2/10^4\rho$. A wrong value was also assigned to the density of the sun. Mr. Whitmell has very kindly re-calculated the results depending on this formula, and I have worked them out independently. We now find that two equal spheres will have equal radiation-repulsion, and gravitative attraction with radii as given below :

Temperature absolute	Density	Radius in centimetres
6200	1·375	1930
300	1	6·1
300	11	0·5645
300	5·5	1·13

The last was given previously as 3·4 cm.

The effect of radiation-pressure on terrestrial dust is worthy of considera tion, for it may be quite appreciable when the particles are small and are among surroundings at different temperatures. For simplicity of calculation, let us suppose a very small dust-particle, of density ρ, to be cylindrical with radius a and length a, and let its flat ends be black and let its curved surface be perfectly reflecting. Let it be situated between two indefinitely extended parallel vertical walls, one at a temperature θ_1 A., the other at a lower temperature θ_2° A., and let its ends be parallel to the walls. The two faces

* [*Collected Papers*, p. 320]

of the dust-particle will, if it is small enough, be at very nearly the same temperature, so that we may leave out of account the pressures due to the emitted radiation and consider only those due to that received from the walls. If σ is the radiation-constant $5{\cdot}32 \times 10^{-5}$ erg/cm.^{2} sec. deg.^{4}, and if U is the velocity of light, the difference of pressure on the two sides will be $2\sigma(\theta_1^4 - \theta_2^4)\,3U$, and the acceleration due to this on area πa^2 and mass $\rho\pi a^3$ is $2\sigma(\theta_1^4 - \theta_2^4)\,3U\rho a$. When $\rho = 1$, $a = 10^{-3}$, $\theta_1 = 400°$ A., $\theta_2 = 300°$ A., this acceleration is 0·02 cm. sec.^{2}.

If the law of radiation-pressure can be taken as still holding when the radius is reduced to $a = 10^{-5}$, the acceleration is 2 cm./sec.^{2}. This implies that such a particle of dust, in a vacuum, and between vertical walls respectively at 27° C. and 127° C., would not fall vertically, but would deviate about 2 mm. per metre towards the colder wall.

The effect found by Prof. Osborne Reynolds (*Phil. Trans.*, 2, 1879, p. 770) on a silk fibre exposed to radiation from a hot body, and assigned by him to 'radiometer' action, is far larger than this. The radius of the fibre was 0·000625 cm., and its length was probably about 15 cm. When it was hung up in a test-tube containing hydrogen at atmospheric pressure, and was exposed to radiation from a neighbouring jar filled with boiling water, the lower end of the fibre moved through 0·01 cm. This would imply an acceleration of about 0·7 cm./sec.^{2}, about sixty times the acceleration of a dust-particle of the same radius under the conditions assumed above. The action detected by Reynolds increased, too, very rapidly as the pressure fell, being ten times as great when the pressure was reduced to 1 inch of mercury.

66.

RADIATION-PRESSURE.

[Presidential Address to the Physical Society of London, February 1905.]

[See Part III, Art. 22.]

67.

SOME ASTRONOMICAL CONSEQUENCES OF THE PRESSURE OF LIGHT.

[Discourse delivered at the Royal Institution on May 11, 1906.]

[*Nature*, **75**, 1906, pp. 90–93.]

Just a year ago Prof. Nichols gave here an account of the beautiful experiment carried out by himself and Prof. Hull which, with the similar experiment of Lebedew, proved conclusively that a beam of light presses against any surface upon which it falls. Not only did Nichols and Hull detect the pressure, which is difficult enough, so minute is it, but they measured it with extraordinary accuracy, and confirmed fully Maxwell's calculation that the pressure on 1 sq. cm. is equal to the energy in 1 cubic centimetre of the beam.

Thus we have a new force to be reckoned with. It is apparently of negligible account in terrestrial affairs, partly in that it never has free and uninterrupted play. But out in the solar system, where there is no disturbing atmosphere, and where it may act without interruption for ages, it may produce very considerable results. Even here, so minute is the force that it need only be taken into account with minute bodies. Prof. Nichols in his discourse told how it may possibly account for the formation of comets' tails if these tails are outbursts of finest dust. To-night I shall try to show how it may be of importance with bodies which, though still minute, are yet far larger than the particles dealt with by Prof. Nichols. Such small bodies appear to abound in our system, and to reveal their existence on any star-light night when perishing as shooting-stars.

We are to examine, then, how the pressure of light, or more generally the pressure of radiation, from one end of the infra-red to the other end of the ultra-violet spectrum will affect the motion of these small bodies.

I think we get a clearer idea of the effects of light or radiation-pressure if we realise from the beginning that a beam of light is a carrier of momentum, that it bears with it a forward push ready to be imparted to any surface which it meeta.

Thus, let a source A (Fig. 1) send out a beam to a surface B, and to bring out this idea of carriage of momentum let A only send out light for a short time, so that the beam does not fill the whole space from A to B, but only the length CD. While the beam is between A and B, B feels nothing. But as soon as D reaches B, B begins to be pushed, or it receives momentum in the direction AB, and will continue to feel the push or receive momentum until C has reached B, when the push will cease. The existence of this push on B is definitely proved by the experiments of Lebedew and Nichols and Hull. Now, unless we are prepared to abandon the conservation of momentum, this momentum must have existed in the beam CD and have been

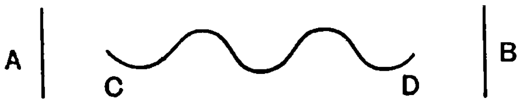

Fig. 1

carried with it, and it must have been put into the beam by A while it was sending forth the waves. A, then, was pouring out forward momentum, and was feeling a back push while it was radiating. This back push against the source has not, I think, been proved to exist by direct experiment, though an indirect proof may perhaps be afforded by the case of reflection. When a beam is totally reflected, the push measured in light-pressure experiments is double that when it is absorbed, that is, there is a push by the incident beam and an equal push by the reflected beam, and we may perhaps regard the reflected beam as starting from the reflector as source, and then we have

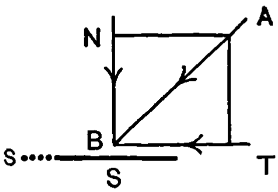

Fig. 2.

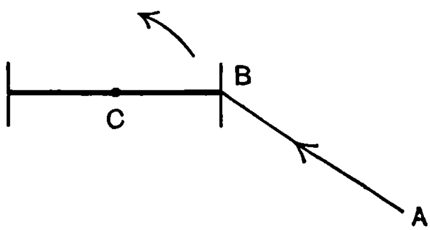

Fig. 3

a push back against the source. But whether this be proof or not, I do not see how there can be the slightest doubt that the pressure against the source exists, and that for the same intensity of beam it is equal to that against a receiving surface.

Some experiments which have been made by Dr. Barlow and myself appear to bring to the front this conception of light as a momentum-carrier. When a beam falls on a black surface it is absorbed extinguished and its momentum is given up to the surface. In a beam of light AB (Fig. 2) the momentum is a push forward in the direction AB, and if it falls on a black surface S it gives up this momentum to S. The total push which is in the direction AB may be resolved into a normal push N and a tangential push T. If S can move freely in its own plane, and only in that plane, T alone comes into play, and S will slide towards s.

To show this effect we fixed two glass discs at the end of a short torsion-rod hung by a fine quartz fibre, the discs being perpendicular to the rod, and the face of one of them being blackened. Fig. 3 shows a plan of the arrangement. The apparatus was enclosed in a glazed case, which was exhausted to about 2 cm. pressure of mercury. On directing a horizontal beam AB at 45° on to the black surface B, the normal force merely pressed B back, but the tangential force turned B, round the point of suspension C, away from AB. It is difficult to make the disc quite symmetrical and the beam quite uniform, and unless these conditions are fulfilled the disturbing forces due to heating of the surface, convection-currents and radiometer-effects may easily have a large moment either way round C. But these disturbing forces take time to develop, as Nichols and Hull showed, while the tangential push of the light acts instantly. Always when the beam is first directed on to B the motion in the first second or two is away from AB.

It has been urged that this experiment is not conclusive in that the lampblack is granular, and the force observed may be due to normal pressure against the sides of the grains. But if the back surface of the disc is blackened, so that the surface is much smoother, the action is as great.

Another form of the experiment which we have lately made is perhaps better. A horizontal disc of mica, about 2 inches in diameter, is suspended in the case by a quartz fibre (Fig. 4). The disc is blackened on its under face. If a beam of light AB is incident at 45° at B, it tends to push the disc one way round. The gas-action due to heating may possibly, and sometimes does, act against this push. But if instead of AB an equal beam CB is sent from the other side, the heating, and therefore the gas-action, is the same, while the tangential push is in the opposite direction, and the deflection now is always less in the direction of the curved arrow than it was before, and the difference gives twice the effect due to the tangential push of either.

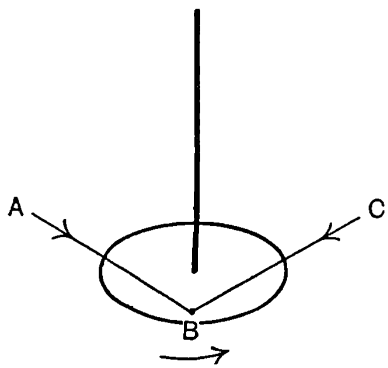

Fig. 4.

Another experiment, rather different in kind, even more clearly shows that light carries a stream of momentum. Two glass prisms B, D (Fig. 5) were fixed at the end of a torsion-arm and suspended by a fibre from C. A beam of light AB was directed horizontally so as to pass through the two prisms and emerge parallel to its original direction along DE. Always the torsion-arm turned as indicated by the curved arrows, just as a pipe would tend to turn if it were bent as the beam of light is bent and carried a stream of water—a stream of forward momentum.

I will not now dwell on the interesting modification of the third law of motion which we must make to reconcile with it these experiments on light. It is enough to say that we must admit the luminiferous medium into momentum-transactions just as long ago we admitted it into transactions with energy.

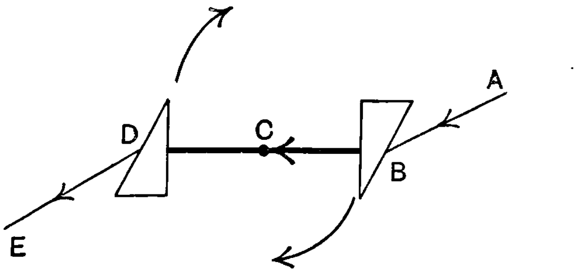

Fig. 5.

Let us now see how this way of regarding a beam of light leads us to expect a modification of the pressure when the receiving or the emitting surface is moving.

First, let us suppose that the receiving surface is moving towards the source. Let A (Fig. 6 a) be the source. Let B be the receiving surface,

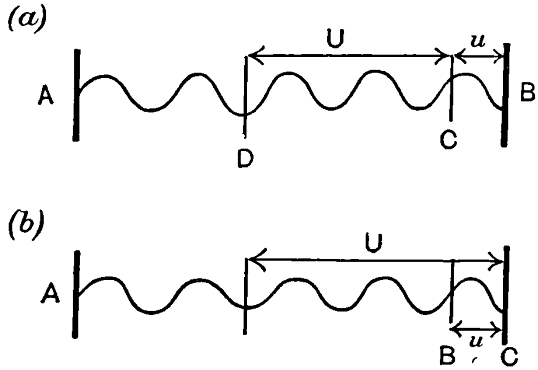

Fig. 6.

moving towards A with velocity u. If B were at rest at C it would receive in one second the radiation and the momentum in length CD U, the velocity of light. But when a given wave starts from D, let the surface start from B, and let them meet at the end of a second. Then B has evidently absorbed the momentum in length $BD = U + u$, and it has received more than it would have done if at rest in the ratio $U + u : U$. The pressure, therefore, is increased, and by the fraction u/U. It is easy to see from Fig. 6 *b* that if B is moving away from the source it receives less momentum, has less pressure than if it were at rest, and the decrease is again by the fraction u/U. We may call this the 'Doppler reception-effect,' 'Doppler' since he was the first to point out the effect of motion on radiation.

If the source is moving there is a nearly equal effect upon it. The pressure is increased if it advances and is decreased if it retreats, but the effect arises in a different way. It is now due to alteration of wave-length. The source crowds up and shortens the waves it sends forward, putting into them more energy and more momentum, and so suffering an increase in pressure, while it draws away from and lengthens the waves it sends backward, putting into them less energy and momentum, and so suffering a decrease in pressure. The alteration of pitch produced in sound by motion of the source is familiar to all.

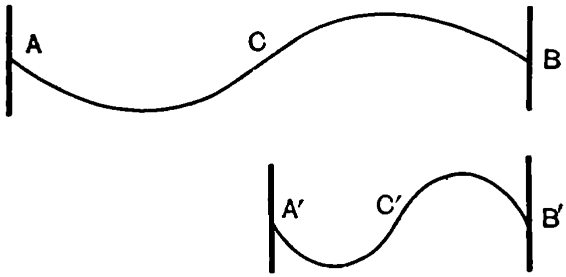

Fig. 7.

We can easily deduce the alteration in pressure if we make the reasonable assumption that the amplitude, the height or depth of the waves sent out from the source, depends on its temperature alone, and not on its motion. Let us imagine, by way of illustration, that the source moves with half the velocity of light, so that a wave which would be ACB (Fig. 7) is packed into half the space $A'C'B'$. With waves of the same height, the energy in a given length is inversely as the square of the wave-length, so that $A'C'B'$ has four times the energy and momentum that ACB has *in the same length*, or the wave $A'C'B'$ has twice the energy and twice the momentum of the wave ACB sent out in the same time, and the pressure against A' is twice that against A.

When the speed of the source u is small compared with that of light. the increase of pressure in forward motion, or decrease in backward motion, is practically the fraction u/U (more exactly it is altered to $U/(U \pm u)$ of the value when at rest). We may call this the 'Doppler emission-effect.'

Coming back to the pressure on a source at rest, that pressure depends on the rate at which the source is pouring out radiant energy, and that rate depends on the temperature of the source. If the body is a black body or a full radiator, the rate of radiation is as the fourth power of the absolute temperature, a law no longer depending on precarious hypotheses, but the result of direct experiment. Here is a table showing the energy radiated and the pressure back against the radiating surface at three important temperatures:

Radiation from and Back Pressure against a Radiating Surface.

Absolute temperature	Energy emitted in ergs per second per sq. cm.	Back pressure in dynes per sq. cm.
0°	0	0
300° (Earth)	$4{\cdot}3 \times 10^{5}$	$9{\cdot}6 \quad 10^{6}$
6000° (Sun)	$6{\cdot}9 \times 0^{10}$	$1{\cdot}5$

A black surface on the earth, then, is pushed back with a force of 1 100,000 mgm. per sq. cm. by its own radiation, while the surface of the sun is pushed back with a force of a milligram and a half on the square centimetre. This table helps us to realise the exceeding minuteness of the forces with which we have to deal.

While we are considering the connection between radiation and temperature, it will be useful to see how the temperature of an absorbing particle depends on its distance from the sun. Take first such a particle at the distance of the earth from the sun. If the sky were completely filled with suns it would be at the temperature of the sun, and give out the corresponding radiation. But the sun only fills 1/200,000 of its sky, so that the particle only receives and gives out 1/200,000 of that radiation. Its temperature is therefore $\sqrt[4]{200,000}$, say about twenty times less than that of the sun. We can form a tolerably good estimate of the temperature of the particle, since the rotation of the earth and its circulating atmosphere make its mean temperature, which is nearly 300° absolute, the same as that of the particle. So that the temperature of the sun is probably about 6000 absolute, or at any rate gives out as much radiation as a full radiator at that temperature.

If we move the particle in to, say, one-quarter the distance, a little within the nearest approach of Mercury, the heat from the sun is sixteen times as great, so that the temperature of the particle is twice as great, say 600 absolute, about the temperature of boiling quicksilver. Out near Jupiter it will be half as great, say 150° absolute, the temperature varying inversely as the square root of the distance.

Now we have the data from which we can trace some of the consequences of light-pressure in the solar system.

The direct pressure of sunlight is virtually a lessening of the sun's gravitation, for, like it, it varies as the inverse square of the distance. As we can by direct measurement find, or at any rate form an estimate of, the energy per c.c. in sunlight, we can calculate the pressure which sunlight exerts on a square centimetre exposed directly to it at the earth's distance, and it works out to about 0·6 mgm. per square metre. On the whole earth it is only about 75,000 tons, a mere nothing compared with the sun's pull, which is forty billion times greater.

But if we halved the radius of the earth we should have one-eighth the gravitation, while we should only reduce the light-pressure to one-quarter, or one would be only twenty billion times the other. With another halving it would be only ten billion times as great, and so on until, if we made a particle a forty-billionth of the radius of the earth, its gravitation would be balanced by the light-pressure if the law held good so far.

This effect of diminution of size applies to the radiating body as well. If we halved the radius of both earth and sun, the gravitative pull would be one sixty-fourth, while the light-pressure would be one-sixteenth, or we should in each halving reduce the ratio of pull to push twice as much, and should much sooner reach the balance between the two, and, of course, the balance would be reached sooner the hotter the bodies. Thus two bodies of the temperature and density of the sun, and about 40 metres in diameter, would neither attract nor repel each other. Two bodies of the temperature and density of the earth would neither attract nor repel each other if a little more than 2 cm., or just under an inch, in diameter.

Suppose, then, a swarm of scattered meteorites 1 inch in diameter and of the earth's density approaching the sun. Out in space their gravitation-pull would be greater than their mutual radiation-push, and there would be a slight tendency to draw together. When they came within 100 million miles of the sun, radiation would about balance gravitation, and they would no longer tend to draw together. As they moved still nearer, repulsion would exceed gravitation, and there would be a tendency—slight, no doubt—to scatter.

It appears possible that this effect should be taken into account in the motion of Saturn's rings if these consist of small particles. Let us suppose that Saturn is still giving off heat of his own in sensible quantity, and, merely for illustration, let us say that his temperature is about that of boiling mercury, 600° absolute. Imagine one of a thinly scattered cloud of particles near the division of the rings. At such a distance from the sun the particle will be receiving nearly all its heat from the planet, which will occupy about one-sixteenth of its sky. If the planet filled the whole sky the particle would be at 600°, and give out corresponding radiation. But filling only one-sixteenth of the sky it gives to the particle, and the particle gives out again, only one-sixteenth of the 600° radiation. It is therefore at $\sqrt[4]{1/16}$, or half the temperature, 300° absolute, the temperature of the earth. Particles in the ring, then, about 1 inch in diameter would neither attract nor repel each other, and each would circle round the planet as if the rest were absent.

Passing on from these mutual actions, let us see how radiation-pressure will affect a spherical absorbing particle moving round the sun. We have already seen that the direct pressure of sunlight acts as a virtual reduction of the sun's pull, and a small particle will not require so great a velocity to keep it in a given orbit as a large body will. A particle 1/1000 inch in diameter, at the distance of the earth from the sun, and of the earth's density, will move so much more slowly than the earth that its year will be nearly two days longer than ours.

In the second place we have the Doppler emission-effect. The particle crowds forward on its own waves emitted in the direction of motion, and draws away from those it sends out behind. There is an increased pressure in front, a reduced pressure behind, and a net force always opposing the motion. This force is a very small fraction of the direct push of the sun, in fact only $\frac{1}{3} \times \frac{\text{velocity of particle}}{\text{velocity of light}}$ of that push.

But, unlike that force, it is always acting against the motion, always dissipating the energy. The result is that the particle, losing some of its energy, falls in a little towards the sun, and moves actually faster in a smaller orbit. The particle we are considering would fall in about 800 miles from the distance of the earth in the first year. Next year it would be hotter, the effect would be greater, and it would move in further. I think it would reach the sun in much less than 100,000 years. As the effect works out to be inversely as the radius, a particle an inch in diameter would reach the sun in much less than a hundred million years.

There is another Doppler emission-effect which must be mentioned. If the whole solar system is drifting along relatively to the ether, there is a Doppler resistance to the drift utterly negligible on the sun and planets, but quite appreciable on meteoric dust. I confess that I am utterly unable to tackle the equations of motion when this force is taken into account, but if we make rough approximations it seems possible that it too would lead to a gradual approach to the sun. The most obvious method of approximation in dealing with a small disturbing force is to omit it. Let us adopt this method here, and turn to another effect which can be tackled a Doppler reception-effect, which only comes into play when a particle is changing its distance from the sun.

Imagine a particle moving in an elliptic orbit to be coming towards the sun. The sun-pressure against it is slightly increased by the motion, or, virtually, gravitation is lessened. When the particle has swung round the sun and is retreating, the sun-pressure is slightly lessened, or, virtually, gravitation is increased. That is, there is always a force tending to resist change of distance from the sun, tending, I take it, to make the orbit less eccentric, more circular.

Now let us see how these forces will act on a comet, supposing a comet to consist of a somewhat thinly scattered cloud of particles of various sizes down to, say, a ten-thousandth of an inch in diameter. Somewhat below that size the particles would be repelled and never tend to approach the sun at all, and would be weeded out of the comet as it first came into our system. Let us suppose that, to begin with, the various sizes are well mixed up. Then at once a sorting action will begin. The direct sun-pressure will lengthen out the year of the finer particles more than that of the coarser, and they will gradually trail behind in the orbit.

Then the Doppler emission-effect will gradually damp down the motion, again more markedly with the finer particles, and they will tend to spiral in towards the sun and shorten the period of revolution. Then the Doppler reception-effect will tend to make the orbit ever less elliptic, and again with the smaller particles the action will be more rapid.

In any single revolution the effect will no doubt be small, even on the smaller particles, but after thousands or millions of revolutions the particles of different sizes may move in orbits so different that they may not appear to have any connection with each other. In course of ages all the smaller particles, and if we have a sufficient balance in the bank of astronomical time even the larger particles, will end their course in the sun itself.

There is one member of our system, Encke's comet, which at first sight looks as if it were manifesting these actions even in the short time, less than a century, that it has been under observation. Its motion is commonly interpreted as a shortening of its period by $2\frac{1}{2}$ hours in each revolution of $3\frac{1}{2}$ years. But Mr. H. C. Plummer has investigated its case, and finds such difficulties, difficulties with which I need not now trouble you, that I fear the obvious explanation that the Doppler resistance is the cause must be abandoned. But though we may not notice the effects in any short time, I see no escape from the conclusion that if comets are clouds of small particles brought into, and made members of, our system, they at once begin to undergo a sorting action. the finer particles drawing inwards more rapidly, and ultimately ending their career in the sun. Possibly the zodiacal light is the dust of long dead comets.

Where our ignorance is complete and unbounded hardly any supposition can be ruled out. Let me, then, in conclusion, make one wild suggestion. Suppose that a larger planet, still so hot as to be a small sun, succeeds in capturing a cloud of cometary dust. Just the action I have been describing should go on. The cloud would gradually spread into a long trail, the larger particles leading, the smaller dropping behind and moving in, and ultimately we might have a ring round the planet, a ring tending to become more and more circular as time went on, with the larger particles outside and the finer particles forming an inner fringe. With different grades of dust we might have different rings. Is it possible that Saturn has been wild enough to have adopted this suggestion?

68.

GEORGE GORE, 1826—1908.

[*Roy. Soc. Proc.* **84**, 1911, pp. xxi–xxii.]

George Gore was born in 1826 at Bristol, where his father had a small business as a cooper. Leaving school at thirteen, he began work as an errand boy. At seventeen he was apprenticed to a cooper and worked at that trade till he was twenty-one, meanwhile studying science and making what experiments he could in his small leisure. He was from the first keenly interested in electro-deposition, and probably it was through his desire to pursue this subject that in 1851 he came to Birmingham, already the chief centre of electroplate manufacture, and here he spent the rest of his life. He appears to have supported himself at first by practising medical galvanism, the apparatus for which he had already improved while at Bristol. Meanwhile he held classes on electroplating and on chemistry and thus began his long career as a teacher in Birmingham. Later he was appointed Science Master at King Edward's School, a post which he held for many years.

In 1854 he published the first of a series of papers on the electro-deposition of metals and soon gained a reputation which led manufacturers in Birmingham and elsewhere to bring their difficulties to him for solution, and from this time onwards he held a leading position in the town as a consulting chemist. Perhaps his most important work consisted in the help which he gave in the early days to the art of electroplating by his numerous discoveries, many of them the basis of present day practice. He wrote several text-books on the subject, which have been widely used here and abroad.

For a time he was chemist to a phosphorus works, and while in that position he discovered the method of bleaching phosphorus by chlorine, which is still in use.

His best known contribution to pure science is his investigation of the properties of anhydrous hydrofluoric acid, which he succeeded in preparing chemically pure. This work occupied him for several years, from 1860 onwards, and was followed by a research on the properties of silver fluoride. Among other researches were investigations on properties of liquid carbonic acid, on ammonia as a solvent of the alkaline metals, and on the thermo-electric action of metals and liquids.

An indefatigable and incessant experimenter, he made many minor discoveries. In 1854 he found that antimony deposited under certain conditions in which it contained a small quantity of antimony terchloride was an unstable form, so that when struck or rubbed or touched with a red-hot wire it suddenly rose in temperature to over 300° C. and changed from a black lustrous body to a greyish powder. This form he termed 'Explosive Antimony.'

In 1858 he invented 'Gore's Sphere,' an interesting modification of the Trevelyan Bar experiment, in which a sphere is set rolling round a pair of circular heated rails and continues to roll round. He further found that the sphere would roll round the rails without other heat than that supplied by an electric current passed from rail to rail through the sphere.

Another discovery was that if a current is passed through a solution of mercuric cyanide and caustic potash between two pools of mercury, a series of crispations appear on the negative pool and humming sounds are given out.

A more important discovery, made in 1868, was that there is a critical point in iron as it cools from a red heat. He found that as a red-hot wire begins to cool it suddenly lengthens and then contracts again. He showed that there was no converse effect on raising the temperature, and that the effect on cooling was accompanied by a change in magnetic permeability.

Dr. Gore was an ardent advocate for the endowment of research, writing in its support at a time when its importance was not recognised as it is to-day. Among his publications is a volume on *The Scientific Bases of National Progress*, in which he urged the value of scientific research to the welfare of the nation. His views were to some extent realised in the foundation, about 1880, of an 'Institute of Scientific Research' by a few citizens of Birmingham. Here Dr. Gore was installed and here he worked for the remainder of his life.

Besides a volume on *The Art of Scientific Discovery*, Dr. Gore occupied his later years, when he was no longer able to experiment so vigorously, in the composition of two works on *The Scientific Basis of Morality* and on *The New Scientific System of Morality*. In these he treated of morals from a materialistic point of view, for which he might have found more sympathy fifty years earlier.

Dr. Gore was elected a Fellow of the Royal Society in 1865, and in 1877 he received the degree of LL.D. from the University of Edinburgh. In 1891 he was given a Civil List Pension in recognition of his contributions to science. He died on December 20, 1908, when nearly eighty-three years of age.

By his will his residuary estate was equally divided between the Royal Society and the Royal Institution for the purpose of assisting original scientific discovery. The share of the Royal Society, amounting to nearly £2,500, has been invested as 'the Gore Fund.'

69.

ATOMIC THEORY (MEDIAEVAL AND MODERN).

[*Encyclopaedia of Religion and Ethics*, **2**, 1909, pp. 203–210.]

1. *History.* If we seek for an explanation of the phenomena of expansion, contraction, solution, and precipitation, we are inevitably led to adopt the hypothesis that matter is, in its minute structure, formed of particles with interspaces. If matter is a continuum, these phenomena are ultimate facts, not to be likened to, and not to be explained by, any other facts. We may conceive the possibility of expansion and contraction of a continuum, but we cannot explain it. If, however, we assume that matter consists of discrete particles, a crowd of particles is like a crowd of people. Expansion is a widening out, contraction is a drawing in, of the crowd. Solution is the thorough mixture of two crowds, precipitation is the expulsion or segregation of certain members of the crowd. Other phenomena, too, find an easy explanation. Thus, evaporation is the passing away of members of the crowd from its boundary. It is no necessary part of the hypothesis that the space between the particles is a vacuum. We know that there is air between the members of a crowd of people. So there may be some kind of material between the particles of matter, displaced when other particles squeeze their way in.

When we trace the history of the speculations about the constitution of matter from mediaeval into modern times, we find that the atomic doctrine of Greek philosophers the doctrine that the minute particles are indivisible has fallen into disfavour. The mediaeval philosophers no doubt thought of matter as composed of minute parts, and they followed the ancient philosophers in regarding the parts as of four kinds, four elements—fire, air, earth, and water. Ordinary bodies were 'mixed bodies,' mixtures of these four, in different proportions in different bodies. But the elements were not always elements in the modern sense. It was sometimes thought that they could be transformed one into another. Roger Bacon, for instance, in the thirteenth century held that the four elements were made of *hyle* (ὕλη), that each could be converted into the nature of another, and everything into anything else. Wheat is a possible man, and man is possible wheat (*de Arte Chymiae*).

These four elements were still dominant when modern science began; and long thereafter they hindered progress, not disappearing indeed till the eighteenth century. At the beginning of the modern period, matter was regarded as consisting of particles, but the particles were not atomic. Thus Francis Bacon in the *Novum Organum* (1620) in several passages mentions the 'atoms' of Leucippus and Democritus only to discard them, and in one passage he says:

'Nor by this are we brought to the (Epicurean) Atom, which presupposes a vacuum, and matter immutable (both of which are false), but to true Particles, as they are found to be' (*Nov. Org.* bk. ii. § 8, Kitchin's tr.).

Bacon made an important contribution to the theory of matter by his clear statement of the doctrine that heat is a mode of motion of the particles:

'Heat is motion, not expansive uniformly in the whole, but expansive through the lesser particles of the body; and at the same time restrained, repelled, and reflected; so that it obtains an alternative motion, ever hurrying, striving, struggling, and irritated by repercussion; whence the fury of fire and heat has its origin' (*ib.* bk. ii. § 20, iii.). 'Heat is motion, expansive, restrained, and struggling through the lesser parts of (a body)' (*ib.* iv.).

These passages clearly imply a crowd of separate particles knocking each other further apart when heated, that is, when thrown into more violent motion. Between the particles Bacon appears to have supposed that there was an intangible, weightless, continuous material which he called 'spirit.' Had he lived later, probably he would have called it 'ether.' He speaks of

'the action and motion of spirit enclosed in tangible bodies. For everything tangible that we know contains an invisible and intangible spirit, and covers and seems to clothe it. Hence that powerful triple source (of effects) and wondrous *process* of spirit in a tangible body. For spirit in a tangible thing, (1) if emitted, contracts and dries up bodies; (2) if retained, softens and melts them; (3) if neither entirely emitted nor entirely retained, models them, gives them limbs, assimilates, carries them out, organises them,' etc. 'Spirit has no weight' (*op. cit.* bk. ii. § 40).

It may be worthy of note that Bacon held that the totality of matter is constant.

The first attempt at a detailed theory of the constitution of matter was made by Descartes in his *Principia Philosophiae* (1644). He held that whatever we can clearly perceive is true, clear perception being of that which is present and manifest to the mind giving attention to it (pt. i. xxx, xlv). Applying this doctrine to the parts of matter, he is led to the rejection of indivisible atoms. For however small we suppose the parts, we are always able in thought to divide any one of them into two or more smaller parts, and may accordingly admit their divisibility. Again, he holds that a vacuum or space in which there is absolutely no body is repugnant to reason. Extension in space is extension in substance. All space, then, is filled with matter. Sensible bodies are composed of insensible particles (pt. iv. cci). These insensible particles are of three elementary forms (pt. iii. lii), though constituted out of the same kind of material. The first element consists of very minute bits, of irregular shape and capable of very rapid motion; chips off the particles forming the second element, and entirely filling the spaces between them. The particles of the second form have become spheres by attrition. They are very minute, and beyond the range of vision. The third form is larger and slower in motion. The sun and stars are composed of the first element, the sky of the second, and the earth and planets of the third. The second and third elements appear to have vortices of the first element round them, and these vortices account for the forces which the particles exert upon each other. They are like the vortices which Descartes supposed to exist round the sun and planets to account for orbital motion.

This theory of the three elemental forms of matter, fanciful in its beginning, becomes more fanciful as he builds it up. It does not appear to have served, except in the mind of Descartes, to co-ordinate facts or to stimulate investigation. But it is important historically, as the first suggestion that it might be possible to consider in detail the ultimate structure of matter and to explain phenomena by this structure. It is important, too, in that it is purely mechanical, that all phenomena are to be explained by configuration and motion of the ultimate particles. Descartes insisted that the sensations are excited by the *motion* of matter only.

'Our mind is of such a nature that the motions of body alone are sufficient to excite in it all sorts of thoughts, without its being necessary that these should in any way resemble the motions which give rise to them, and especially that these motions can excite in it those confused thoughts called sensations' (pt. iv. cxcvii, Veitch's tr.). We perceive nothing outside ourselves 'except light, colours, smells, tastes, sounds, and the tactile qualities; and these I have recently shown to be nothing more, at least so far as they are known to us, than certain dispositions of the objects, consisting in magnitude, figure, and motion' (pt. iv. cxcix).

That is, there is not a separate light principle or substance, smell principle, sound principle. These are in our minds. All the different senses are excited by the size, shape, and motion of the small particles of which matter consists.

Like Bacon, Descartes held that heat is a mode of motion of the particles (pt. iv. xxix).

The most influential writer on the structure of matter in the generation succeeding that of Descartes is Robert Boyle, who in the *Sceptical Chymist* (1661), *Origin of Forms and Qualities* (1666), and other works, laid the foundation of modern chemical theory. He was probably an atomist.

'We may consider,' he says, '(1) that there are in the world great store of Particles of matter, each of which is too small to be, whilst single, sensible; and, being entire, or undivided, must needs both have its determinate shape, and be very Solid. Insomuch that though it be *mentally*, and by Divine Omnipotence divisible, yet by reason of its Smallness and Solidity, Nature doth scarce ever actually divide it; and these may in this sense be called *Minima* or *Prima Naturalia*. (2) That there are also multitudes of corpuscles, which are made up of the coalition of several of the former *Minima Naturalia*, and whose bulk is so small, and their Adhesion so close and strict, that each of these little Primitive Concretions or Clusters (if I may so call them) of Particles is singly below the discernment of Sense, and though not absolutely indivisible by Nature into the *Prima Naturalia* that composed it, or perhaps into other little fragments, yet, for the reasons freshly intimated, they very rarely happen to be actually dissolved or broken, but remain entire in great variety of sensible Bodies, and under various forms or disguises' (*Forms and Qualities*, p. 71).

Yet in the prefatory address to the reader in the same work, he says that he has forborne to use arguments that are grounded on or suppose indivisible corpuscles called atoms. Though not a Cartesian he followed Descartes, as indeed all the world has followed him, in ascribing the qualities of natural bodies to the 'Catholick and fertile Principle *Motion, Bulk, Shape*, and *Texture* of the minute parts of Matter' (*Forms and Qualities*, last page). He, too, regarded heat as a vehement agitation of the parts of the body tending all manner of ways ('Heat and Cold,' *Works*, Shaw's ed. i. 560). Fire, however, is the violent agitation of the particles of a subtle matter. But it is for his clear idea of the nature of chemical combination that we are chiefly indebted to Boyle. In the *Sceptical Chymist* he discards the old elements, air, earth, and water, and says that these three are to be regarded as made up of mixed bodies rather than mixed bodies as made up of them; and by mixed bodies he means ordinary bodies which had been regarded as mixtures of air, earth, and water, and had so come to be called mixed bodies. He dwells on the idea that an enormous number of compounds may be made by various arrangements of corpuscles, and that they differ from each other in nothing but the various textures resulting from the magnitude, shape, motion, and arrangement of the small parts.

'One and the same parcel of universal matter may by various alterations and contextures be brought to deserve the name sometimes of a sulphureous, and sometimes of a terrestrial or aqueous body' (Shaw's ed. iii. 282).

Boyle agreed, then, with Descartes in thinking of matter as one in kind, the differences being due solely to shape, size, and motion of the parts.

Newton does not appear to have concerned himself very much with speculations about the ultimate structure of matter. He was, above all, an experimental philosopher, determining laws by experiment and observation, using mathematics to deduce their consequences, and comparing these consequences with further experiment and observation, thereby verifying or correcting the laws. The framing of atomic hypotheses did not come into this programme. In the *Principia* he hardly uses the hypothetical structure of matter at all. There is a suggestion of it in the determination of the velocity of sound, but a mere suggestion. In the series of queries at the end of the *Optics* (2nd and 3rd editions) he gave himself free play, and among a number of speculations he declared himself an atomist. 'It seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, movable Particles' (3rd ed. p. 375). In the history of the subject Newton is mentioned only as in all probability deflecting attention from it. His immediate successors were occupied so much in following out his methods that theories of matter were apparently little studied or considered by leaders in physical thought.

But the atomic doctrine was probably making way. One very notable contribution to the particle theory was made by D. Bernoulli in his *Hydrodynamica* (1738), § x. He suggested that a gas consists of very minute corpuscles moving with very great velocities in all directions, and that the pressure of a gas against the walls of a containing vessel is due to the bombardment by these corpuscles. He showed that with this constitution the pressure would be inversely as the volume if the volume were changed, which is 'Boyle's Law'; and that if the temperature were raised by increase in the velocity of the corpuscles, the pressure would be proportioned to the square of the velocity. His reasoning is somewhat obscure and unconvincing, and, perhaps on that account, his theory remained almost unnoticed for more than a century, when it was re-discovered and was developed on better lines.

In 1758, Boscovich published his *Theoria Philosophiae Naturalis*. In this work he begins with his celebrated hypothesis of the nature of an atom, and then seeks to show how physical phenomena, such as collision, cohesion, fluid pressure, viscosity, and elasticity may be accounted for if matter is composed of atoms such as he imagines. According to his hypothesis, an atom is a central point to which mass or inertia is assigned, a mass-point let us say, and towards this point force is acting so that another mass-point is urged towards it with an acceleration proportional to the mass of the first point, and varying with the distance in such a way that at a great distance the attraction is inversely as the square of the distance. But when the distance becomes very small the force undergoes one or more alternations of repulsion and attraction, finally ending with a repulsion which is infinitely great when the two points are infinitely near each other. Two central mass-points can therefore never actually coincide. Boscovich did not assign any formula for the force, probably considering that future experiment alone could determine the law, but it is easy to devise expressions which satisfy his general conditions.

For instance, if a mass-point is distant r from a mass-point m, and if its acceleration to m is expressed by $m(r-a)(r-2a)(r-3a)r^5$, the force is attractive, and inversely as the square of the distance if r is very great, it increases as r diminishes to a certain point, and then diminishes to zero when $r = 3a$. Between $r = 3a$ and $r = 2a$ it is a repulsion, again vanishing at $r = 2a$. Between $r = 2a$ and $r = a$ it is an attraction changing again to a repulsion when r is less than a, a repulsion becoming infinite when r is infinitely small. Whatever the velocity of approach, that velocity will be destroyed before the two points coincide, i.e. they never will coincide.

Perhaps we can form an idea of the way in which Boscovich's atoms would act, by imagining that two solid surfaces are brought together. At great distances apart there is merely the inverse square law of gravitative attraction. When, however, they come very near, there is mutual pressure, which we may represent by supposing that the surface atoms have come into each other's first sphere of repulsion, and outside pressure is required to overcome this repulsion. If the outside pressure is made very great, we may force the atoms through this sphere of repulsion into the second sphere of attraction, and we may have equilibrium at the point where the attraction again changes into repulsion. If we can reach this point, the surfaces will adhere even when the outside pressure is removed. Thus the hypothesis explains the adhesion of bodies pressed very closely together.

Boscovich's predecessors had, like Newton, imagined a little hard nucleus round the centre of the atom; Boscovich showed that the nucleus was unnecessary. Its place was efficiently taken by the repulsive force rapidly increasing as the centre was approached. The nucleus had no part to play, and might be discarded.

The hypothesis at once excited attention. It was adopted, for instance, by Priestley (*Vision*, 1772, and *Matter and Spirit*, 1777). In the next century it attracted Faraday, who definitely adopted it (see two letters to R. Phillips, *Researches in Electricity*, 1844–55, ii. 284, iii. 447). Faraday laid stress on the uselessness of the little hard nucleus of finite size an idea which had again gained ground in the form of the atomic hypothesis of Dalton. He urged that the powers of matter, the forces which it exerts, are all that we know about it. These powers or forces *are* the matter, and where they extend the matter also extends. Each atom, then, extends through all space. Faraday symbolised the forces by straight lines extending out on all sides from the atomic centres, and thought of light as tremors or minute jerks carried along these lines—an idea which has been revived and made precise by Sir J. J. Thomson.

In recent years, Lord Kelvin took up 'the inevitable theory of Boscovich,' and sought to show how the grouping of Boscovichian atoms could account for crystalline arrangement and for the phenomena of light.

For purposes of calculation, the Boscovichian atom is indeed inevitable, in some form or other, in any atomic theory in which the forces depend solely on the distance. Mathematically, there is no difficulty in thinking of one point, B, moving towards another point, A, with an acceleration expressed by $mf(r)$. We then define m as the mass of A. If A moves towards B with an acceleration $m'f(r)$, we call m' the mass of B, and we say that the force is $mm'f(r)$. But, physically, there is some difficulty in reducing all to force. We think of force as effort, symbolised by muscular effort; and if we have force alone, it is difficult to assign meaning to effort acting on effort. We must have a duality, at least in thought. If we think of force, we think also of that which the force moves, or perhaps more fundamentally that which resists the force, and we call this matter. If with Boscovich and Faraday we identify the moved with the mover, and say that the matter is but force, then we have a dual aspect of force. In one aspect it acts, in the other it suffers action. One is the symbol of will, the other of sensation. Whatever may be the ultimate fate of Boscovich's atom in physical theory, the conception of the unity of matter and force, and unity as force rather than as matter, is a permanent contribution to philosophical ideas.

We have now come to the time when chemistry began to influence atomic speculation. Up to the latter half of the eighteenth century it was a hindrance rather than a help. Of course, chemical knowledge was continually increasing, so far as concerned the extraction of the metals from ores and the preparation of definite substances. But progress in chemical theory was hopeless while the doctrine of the four elements, fire, air, earth, and water, held sway. About 1700 the phlogiston theory of Stahl came into vogue, and added to the confusion of thought. This theory held that the metals contained a something called 'phlogiston' which they gave off on combining with air, that oxygen was air deprived of phlogiston, that nitrogen was air containing phlogiston, and some chemists even supposed that hydrogen was phlogiston itself. There must have been some expression or symbolisation of fact in a theory which was accepted by discoverers so great as Black, Cavendish, and Priestley. But it is difficult now, so entirely is our point of view changed, to see what facts it expressed. It is obvious only that any advantage in its adoption was far outweighed by the confusion of thought which attended it. Towards the end of the eighteenth century the fog in which chemical theory was enveloped began to lift. The atomic theory had been making way, though in a vague form, and in many minds Boyle's idea that chemical compounds consisted of definite groups of atoms had taken root. The word 'molecule,' originally used as equivalent to particle, was now adopted for atomic groups. The four elements, fire, air, earth, and water, were being gradually disestablished from their supreme position. A number of gases were discovered with definite properties, and obviously not mere modifications of air. Air itself was found to consist almost entirely of two gases which we now call nitrogen and oxygen, and so air ceased to be regarded as an element. Cavendish's discovery that water was a compound of hydrogen and oxygen removed it, too, from the list. Earth was resolved into at least several different earths, and the discovery by Lavoisier of the part played by oxygen in ordinary combustion killed phlogiston. Fire and flame, however, still remained as substances; for the idea that heat was a form of motion, or, as we should now say, of molecular energy, so often put forth in the seventeenth century, had fallen into abeyance, and heat was regarded as a subtle substance. The way was being prepared for the atomic theory of Dalton, and there were several foreshadowings of it, the most notable by William Higgins, who in 1789 published *A Comparative View of the Phlogistic and Antiphlogistic Theories*, in which he says that 'we may justly conclude that water is composed of molecules formed by the union of a single ultimate particle of dephlogisticated air [oxygen] to an ultimate particle of light inflammable air [hydrogen], and that they are incapable of uniting to a third particle of either principle.' But it is doubtful whether he considered this as an example of a general principle, and he does not appear to have considered the 'ultimate particles' of an element as all alike.

In 1804 the modern doctrine of chemical combination was definitely formulated by Dalton, and communicated to his friends. In 1808 he first published it in his *New System of Chemical Philosophy*. According to Dalton, the particles or atoms—he uses the terms indifferently in a simple body are all exactly alike, and in a finite space enormous in number.

'Chemical analysis and synthesis go no farther than to the separation of particles from one another and to their reunion. No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.' Compounds are definite groupings of definite atoms. Thus 1 atom of A + 1 atom of B may form 1 atom of C, binary; or 1 atom of A + 2 atoms of B may form 1 atom of D, ternary, and so on.

Here he uses 'atom' for C and D where we now use 'molecule'; and it is evident that he does not think of an atom as an indivisible particle, but as the smallest particle of a body which has the properties of the body. If this particle is divided, the substance is resolved into different substances.

Dalton goes on to show that the relative weights of two elements entering into, say, a binary compound are proportional to the weights of the atoms themselves, and he gives the first table of atomic weights of the elements, taking the hydrogen atom, the lightest, as having weight 1.

Dalton's great advance appears to consist in the clearness of his conception of an element as consisting of particles all alike, and whether truly atomic or not, yet indivisible by ordinary chemical agency, and in his supposition that compounds consisted in general of small groups of these elementary particles or atoms. This gave an easily pictured hypothesis to account for the chemical fact, by this time established, that the elements combine in definite proportions, which are always integral multiples of the smallest proportions in which they enter into combination. From this time forward, chemists, freed from the burden of the four elements, sought for bodies which were not to be decomposed by ordinary chemical and physical agency. These they termed 'elements,' and they investigated the proportions in which the elements entered into compounds.

Soon after the publication of Dalton's theory, Avogadro put forward the hypothesis that in gases at the same temperature and pressure equal volumes contain equal numbers of molecules a hypothesis then supported by experiments on the combination of gases, and since shown to be in accord with the kinetic theory of gases.

We have seen that with Dalton the atom was not necessarily indivisible, but merely undivided, and in modern times probably no one has held the indivisibility. Indeed, quite early in the development of the atomic theory by chemists, the old idea was revived, that the atoms of the different elements were really made of one kind of material, differing only in the quantity or arrangement of that material. Prout held that all the atomic weights were exact multiples of the atomic weight of hydrogen, a supposition which seems to imply that other elements were really groups of hydrogen atoms not to be divided by known agency. Later, it was suggested that discrepancies with this theory might be accounted for by taking one-half or one-quarter of the hydrogen atom as the unit out of which other atoms were built. But subsequent research on atomic weights has not supported these suggestions. There were, however, other reasons for supposing some community of plan in atomic structure. If weights of different metals be taken, containing, on the atomic hypothesis, equal numbers of atoms, nearly equal quantities of heat are required in many cases to raise these weights through one degree of temperature. In electrolysis, equal quantities of electricity are required to turn equal numbers of atoms out of combination. If the atoms of different elements had, as their only common property, weight or gravitation, and in other qualities were entirely different, we should hardly expect these quantitative relations.

The chemical atomic theory, as set forth by Dalton, is a statical theory. For chemical purposes the atoms may, up to a certain point, be considered as little bodies somehow held together in compounds by forces which need not be specified; and their motion and the motion of the molecules need not be regarded.

By the middle of the nineteenth century the researches of Joule and others had led every one to reject the doctrine of heat as a substance, and had brought to the front the old idea that heat was the energy of motion and of separation of the atoms and molecules. The dynamic aspect of the atomic and molecular theory now began to be seriously studied. We have seen that D. Bernoulli made an early but fruitless attempt to show that gas pressure could be accounted for by molecular motion. Other attempts were made by Herapath in 1821, by Waterston in 1846 (published only in 1892, when Lord Rayleigh disinterred the paper from the archives of the Royal Society), and by Joule.

Joule, in 1848 (*Scientific Papers*, i. 290), was the first to publish a calculation of the velocity with which the molecules are flying about. But the general acceptance of the dynamical, or, as it is now termed, the kinetic, theory began with the work of Krönig, Clausius, and Maxwell about 1856, and from that time onwards the theory has been developed in a series of memoirs by various authors, especially by Clausius, Maxwell, and Boltzmann.

2. *Kinetic Theory.* The kinetic theory has been studied chiefly with regard to gases. It supposes that a gas consists of an enormous number of molecules, all exactly alike in a given gas, but so small that their average distance apart is very great compared with the size of any one molecule. These molecules are flying about in all directions with very great velocities, continually colliding with each other and with the walls of any containing vessel. The collisions are of such a kind that on the whole the energy of motion remains the same so long as the temperature is constant. The molecules are so far apart that they do not act upon each other except just during the moment of collision. Between collisions, then, they move in straight lines, and the average length of path between two collisions is called the 'Mean Free Path.' The velocities of the molecules are not all equal, but are grouped about a mean value which remains constant, and when the gas is in a constant or steady condition the grouping about this mean value is constant. It can be shown from simple mechanical considerations that if $\bar{v}^2$ is the average of the squares of the velocities, if p is the pressure, and if ρ is the density, then $v^2 = 3p/\rho$. If we suppose that v^2 remains constant so long as the temperature is constant, when the density is changed by changing the pressure, p/ρ is constant. This gives 'Boyle's Law. The mean velocity is not quite the same as v (the square root of the mean of the squares), but the difference is only small. For hydrogen at 0 C. v is about 1800 metres

per second—over a mile per second. For oxygen and nitrogen it is about a quarter of a mile per second. It can be shown that in a mixture of gases it is necessary that the average kinetic energy of each kind of molecule be the same in order that the collisions shall not affect the general condition; hence it can be further shown that the number of molecules in equal volumes of two gases at the same temperature and pressure is equal. This is 'Avogadro's Law.' Experiment shows that the pressure of a gas in a closed vessel increases by equal amounts with equal rises of temperature, and is, in fact, proportional to the temperature reckoned from $-273°$ C. as zero. Then v^2 is also proportional to this temperature. Or the mean kinetic energy of the particles is proportional to the temperature reckoned from $-273°$ C. On this scale of temperature the sun's surface is probably about twenty times as hot as the earth's surface, so that the molecules of hydrogen there have about twenty times as great a v^2 as they have here. Their velocity is thus about $4\frac{1}{2}$ times as great, say, some 5 or 6 miles per second.

So far it is not necessary to enter into the structure of the molecules; but for further developments with regard to the specific heats of gases, structure has to be taken into account, and difficulties arise into which we need not enter.

The mean free path, or the average distance travelled in a straight line between two collisions, plays a very important part in the theory. It is evident that the rate of diffusion of one gas into another, or the diffusion of one part of the same gas into another part, will depend partly on the mean free path, partly on the velocity; and from certain phenomena depending on the rate of diffusion, most simply from the resistance to the travelling of one layer of gas over another, the mean free path can be calculated. For hydrogen at atmospheric pressure it is about 0·00002 cm., or, say, a little less than a hundred thousandth of an inch. For air it is almost half as much. This implies that the hydrogen molecule is effectively less than the air molecule, since it can thread its way farther through its neighbours without collision.

The greatest triumph of the kinetic theory consists in its determination of the size of the molecules and of the number in a given space. By the size of the molecules we are not to suppose some definite solid shape. Imagine two molecules approaching so closely that they just begin to deflect each other, i.e. just begin to collide. Draw equal spheres round each, just touching. Then the radius of either sphere is termed the radius of molecular action, and the sphere round each molecule is taken to be the size of the molecule, even though the atomic centres may be far within the surface of the sphere. It is evident that the mean free path of a molecule depends partly on the number of molecules in a given space, and partly on their size, just as the distance a man can walk straight on in a street without touching another passenger will depend partly on the number of people, partly on their breadth from shoulder to shoulder. A relation can be found connecting the mean free path, the molecular size, and the number present in unit volume; and since we know the mean free path, this gives a relation between size and number. Another relation can be obtained from the supposition that when the gas is condensed to a liquid the contraction is due to all the molecular spheres coming into contact. We may illustrate this by thinking of a cloud of equal water drops in a vessel as representing the molecules. When the cloud sinks to the bottom of the vessel, the volume of water at the bottom will be equal to the volume of each drop multiplied by the number of drops. So the observed volume of liquid into which a given volume of gas condenses may be taken as equal to the volume of each molecule multiplied by the number of molecules. At least it may be taken as nearly equal. We cannot say how closely the molecules are packed in a liquid, and thus some uncertainty is introduced into the results obtained. Thus we have two relations between number and size, and from these two we can calculate approximately both number and size. The result obtained is that the number of molecules in a cubic centimetre of a gas at 0° C. and atmospheric pressure is about 6×10^{19}. The mass of a molecule of hydrogen, which we must suppose to consist of two atoms, is a little less than 10^{-24} of a gramme. The radius of molecular action, differing somewhat for different gases, lies between 10^{-7} and 10^{-8} of a centimetre. The number of molecules in a cubic centimetre of water is about 10^{23}, and the distance from centre to centre about 10^{-8} cm. (These results can be translated into inches if it is remembered that 1 inch = 2·5 cm.) The number of molecules of gas per cubic centimetre has been calculated in other ways, most notably from certain experiments on electric discharge, and the results obtained are in fair accordance with those given above. Unless, then, we reject the theory altogether, we may assume that we know with fair accuracy both the 'size' and the average distance apart of the molecules in a body.

The kinetic theory, when applied to account for the properties of solids and liquids, has made very little progress. In the gaseous condition the molecules, according to the theory, spend most of their time in moving in straight lines, and when the collisions do occur there is no need to consider the forces acting. It is enough to assume that, on the whole, collisions do not alter the general condition. Many properties can then be accounted for. But when we come to solids and liquids, the molecules are supposed to be so closely packed as to be always entangled with each other, and there is no mean free path. Hence we cannot go far without knowing the forces between the molecules; and even if we could specify the forces, the calculations of their effects in such complex systems would probably be exceedingly difficult. In the solid condition, we must suppose that the molecules do not move far from a mean position. Each is held by its neighbours, so that though it may be violently agitated it keeps slackly anchored, as it were, to one spot.

In a liquid, the molecules are still entangled with each other, but they possess more energy, and are able to break away every now and then from their anchorage and get into new surroundings. A liquid may probably be regarded as a cross between a solid and a gas solid-like if time be reckoned by millionths or billionths of a second, gas-like if it be reckoned by seconds.

3. *Atomic and molecular structure.* Since the kinetic theory of matter arose, several attempts have been made to imagine atomic structures which should serve to give some account of phenomena. Rankine (Nichol's *Encyclopaedia, s.v.* 'Heat,' p. 353) assumed that the atoms were little nuclei surrounded by atmospheres whirling round them in vortices—a revival of Descartes's idea. Though Rankine made use of the theory himself, it is obscure in detail, and has not been used by others.

The vortex-atom of Lord Kelvin (*P. R. S. E.* Feb. 1867) is a much more celebrated hypothesis. This atom is founded on Helmholtz's investigations on fluid motion. We are to suppose that space is full of a frictionless incompressible fluid of uniform density throughout. An atom is a vortex-ring in this fluid, a ring-shaped portion of the fluid distinguished from the rest solely by its peculiar whirling motion. The whirling motion may be understood by running an india-rubber umbrella-ring along its stick. The friction makes it move round and round as it travels. The rings which an expert smoker makes are vortex-rings. A spoon drawn sharply across a cup of tea makes half a vortex-ring of which the two cut ends, as it were, appear as little whirlpools, the half ring being below the surface. We can make rings in real fluids, because they possess viscosity or friction. But in a frictionless fluid, the creation and equally the destruction of a ring would require forces to act which the fluid itself does not possess; so that a ring if in existence must have existed in all past time, and will persist in all future time, the same portion of fluid always existing in it. We thus have a suggestion for an indivisible and eternal atom. The difficulty of the mathematical investigation of the mutual actions of vortex-rings is so great, that little progress has been made in the theory. But it possesses value in suggesting the possibility that all energy is of one kind, energy of motion either of the rings themselves or of the fluid outside the rings. In accounting for the eternity of the atom it perhaps goes too far. In recent years the theory has dropped into the background.

Another hypothesis, due to Larmor (*P. R. S.* lxi. (1897), p. 275), supposes that space is filled with an elastic solid, or jelly, capable of vibrating and carrying waves. An atom is the centre of a strain or twist in the jelly. The strain is persistent, but it can move from point to point in the jelly, using new parts as it travels along. It is the form of strain which is the persistent atom. The atom may be likened to a kink on a rope which travels along the rope, new material continually passing into the kink to take the place of the material which passes out of it.

Larmor has developed this hypothesis in several memoirs, showing how electrical and optical phenomena are to be interpreted in terms of it.

The latest atomic hypothesis is one which assigns an electrical structure to the atom. Here we can only give a sketch of the subject. Fuller details will be found in *Electricity and Matter* and *The Corpuscular Theory of Matter*, by Sir J. J. Thomson, on whose researches the hypothesis is chiefly based and in *Electrons*, by Sir Oliver Lodge.

The idea that the forces keeping the atoms together in the molecule are electrical in nature is as old as Davy. It is rendered probable by the fact that electric charges put into a liquid will decompose it, and by the fact that one of the chief sources of electricity is the voltaic cell in which chemical combination occurs and charges are given out. We may suppose that, if a molecule contains two constituent atoms or groups of atoms, one of these is positively electrified and the other negatively electrified, and that it is the attraction between the two charges which binds the atoms together to form the molecules.

Faraday discovered that, when a liquid is decomposed by an electric current, a definite charge of electricity of each kind is required to disengage a definite amount of an element from a compound, and that, if we accept the atomic theory, the charge is proportional to the number of atoms disengaged. The hydrogen atom always requires the same charge. Other atoms require either the same charge as the hydrogen atom or twice the charge or a small multiple of it. There is no evidence for the existence of a smaller charge than that on the hydrogen atom, and apparently all other charges are exact multiples of it. Thus electricity appears to be, as it were, atomic. Any quantity of it consists of multiples of the hydrogen atom charge, which is an undivided unit. We have to suppose that, when we decompose a molecule, each constituent has a charge, one positive the other negative, and we have to put in equal neutralising charges to render the products of decomposition neutral. This interpretation of Faraday's discovery may be regarded as the foundation of the theory of the electrical atom.

The superstructure began with a discovery made by Crookes, when investigating electric discharge in highly rarefied gases. If the discharge takes place between two metal plates in a space sufficiently rarefied, Crookes found that a stream of negatively electrified matter shoots straight out from the plate to which negative electricity is supplied. This stream of matter can be deflected by a magnet. Sir J. J. Thomson found that it can be deflected also by electric force. In a series of brilliant experiments on the deflections by known magnetic and electric forces, he made the immensely important discoveries that the mass carrying a given charge is the same whatever the gas used, and that it is $\frac{1}{1700}$ part of the mass of hydrogen which would carry the same charge. By further experiments he showed that each particle in the stream carried a charge equal to that of the hydrogen atom, so that the mass of each particle is $\frac{1}{1700}$ part of the mass of the hydrogen atom. These minute carriers of negative electricity he termed 'corpuscles.' They are usually now called 'electrons.' We must suppose that the electric forces during the discharge tear away the negatively electrified portions of the atoms at the negative plates, that these are alike for all atoms, and that their mass is but $\frac{1}{1700}$ part of the mass of the hydrogen atom. If the atom was neutral before the corpuscle was torn from it, an equal positive charge remains behind upon what is left.

In building up the electric atom we must bear in mind that all experiment shows that there are equal quantities of the two opposite charges. We do not know of such a thing as a charge of one kind alone. One kind of charge is always accompanied by the other kind of charge—either united to it as in a so-called neutral atom, or separated from it by a greater or less distance, yet connected to it by forces. We imagine, then, that an atom consists electrically of a charge of positive electricity and a greater or less number of electrons, and there are reasons for supposing that the number of electrons is proportional to the atomic weight. The oxygen atom will possess, for instance, 16 times as many electrons as the hydrogen atom. The positive charge is equal to the sum of all the negative charges on the electrons. This positive charge is supposed to be spherical, and to be distributed uniformly within the sphere. In some way it coheres and occupies a definite volume of the order of the atomic volume, say, 10^{-8} cm. radius. The spherical supposition is made for simplicity of treatment. The electrons are moving about inside the globe, and can move without resistance through the positive. With these suppositions, it can be shown that, if there were a single electron moving within the globe of positive, it would have a definite period of revolution round the centre of the globe, the same whatever its distance from the centre. With a number of electrons moving in orbits round the centre, each would affect the time of revolution of the others, but certain arrangements of orbits would persist. Other arrangements might be unstable, and then electrons might be thrown out of the atom. As the electrons circulate in their orbits, they radiate out energy in the form of waves, one wave for each revolution, and the hypothesis thus seeks to account for light and other radiation of like kind. If two such atoms approach each other, one may be so nearly unstable that it parts with one electron, giving it to the other, which may absorb it into its own system. One atom, then, gains a small balance of positive, while the other has acquired an equal small balance of negative, and, these opposite charges attracting each other, the atoms keep together and form a molecule.

The atom possesses an enormous store of energy in the motion of the negative electrons. It may become unstable through mutual action of these in their orbits, and some part may fly off to form a new atom. Thus the hypothesis seeks to account for the breaking down of the atoms which appears to occur in radium and other radio-active bodies.

We have seen that the mass of each electron is $\frac{1}{1700}$ part of that of the hydrogen atom. The hypothesis gives an account of this mass which we owe to J. J. Thomson. If a body with an electric charge upon it is moving, it acts like a short length of electric current, and it is surrounded by a field of magnetic force; that is, there is magnetic energy in the space around as well as electric energy. If the moving body is a sphere with the charge upon its surface, the quantity of magnetic energy is directly proportional to the square of its velocity, and is inversely as its radius. Now, the mass of a body may be measured by the energy required to get up in it a given velocity v, for that energy is $mv^2/2$. If the sphere has mass m and no charge, its energy when moving with v is $mv^2/2$. If it has a charge, then there is in addition the magnetic energy also proportional to v^2, say, $m'v^2$ 2, or the total will be $(m + m')\, v^2/2$. Suppose that when without charge the sphere has no mass, m will be 0. When it has a charge, there will still be mass m', or rather the sphere will behave just as if it had this mass. It is possible, then, that the mass of the electron is entirely due to its charge. Calculation shows that a massless sphere carrying a surface charge equal to that of the hydrogen atom, and about 10^{-13} cm. in radius, would behave as if it had mass $\frac{1}{1700}$ of the hydrogen atom.

If we could suppose that the hydrogen atom contains 1700 electrons, we could thus account for its whole mass. But it appears much more probable that that atom contains but one or a few electrons, so that if we are still to give an electrical account of mass, we must break up the positive globe into very small spheres enormous in number, and each containing a very small charge, far smaller than that on the electron, these being scattered through the globe. But for this supposition there does not as yet appear to be any justification, i.e. it does not account for any observed phenomena.

Certain experiments do seem to show that the mass of the electrons is fully accounted for by the magnetic energy of their charges when in motion; but even if we accept this account it is very doubtful if we have 'explained' mass. Certainly all our measurements of energy involve the idea of mass, and it may perhaps be maintained that the magnetic energy in the space round the moving electron implies the existence of mass in that space to serve as a seat for the energy. If so, the electric theory of mass only takes the mass from the inside of the moving sphere and spreads it through the outside space. Again, we come to the Boscovich-Faraday conception, that an atom is wherever its force acts—not at the centre alone, but spread through all space.

Summary. Present position of the atomic theory. The belief that matter is granular in structure, that it consists of exceedingly minute discrete particles, is irresistible. The particle-structure accounts for a vast host of observed facts, for which no other explanation has been suggested. We must accept it, unless we abandon all explanation by means of hypothesis, and are willing to content ourselves with mere description of phenomena—a frame of mind which is non-existent. When we pursue the particle-structure into detail, we find an excellent explanation for the facts of chemistry in the hypothesis that, in a definite chemical compound, the particles are molecules, all alike, and that each molecule consists of a group of still smaller particles, which, without prejudice to their divisibility, we call atoms. The chemist finds that the compound can be resolved into bodies, each of which is incapable of further resolution by ordinary chemical agency, and which he terms 'elementary.' An element is supposed to consist of atoms all alike in each one kind. No explanation other than this has been devised for chemical phenomena, and it works so well that it has been universally accepted. So far, we cannot even conceive of any other hypothesis. The hypothesis that in a gas the molecules are, on the average, far apart and rushing about leads to a simple explanation of many gas properties, and has suggested properties hitherto unknown which investigation has shown to exist. Again, no alternative hypothesis has been offered; and if we accept this, we are bound to accept the determinations of number and size, determinations arrived at also from other starting-points.

So far, no special assumptions need be made as to atomic or molecular structure. But if we seek an explanation, on the atomic hypothesis, of certain electrical phenomena, we must imagine some definite structure for the atom. The electrical atom, in some such form as that described, is the only atom yet imagined which has any value for the purpose. It has served to explain many of the phenomena, and it has suggested researches which have led to new and important discoveries. It is easy to criticise it, to point out the large assumptions on which it is founded. There is, for instance, the coherent globe of positive electricity held together by unknown agency and allowing the negative electrons to move about freely in it. For this we require properties unlike the properties of any known electrical system. But the chief value of such a hypothesis lies, not in its objective truth, but in its success in accounting for, in co-ordinating, what we actually observe, and in predicting results which are afterwards verified. It is to be regarded as a 'working model' which gives the same results as the actual atom, though, it may be, by quite different machinery. From this point of view the electrical atom is a brilliant success.

We observe phenomena due to matter in large masses. Our senses tell us nothing of ultra-microscopic structure, and perhaps the sparkling of zinc sulphide when exposed to radium is the only phenomenon which can fairly be ascribed to single atoms, each sparkle being assigned to the impact of one atom. When we go behind observed phenomena and assume a minute structure to account for them—a structure far beyond the range of our senses in minuteness, and probably utterly beyond direct verification it is possible to imagine many types of structure which will account for what we observe; just as when we stand in front of a clock which we cannot open, we can imagine many different trains of wheel-work which will account for the motion of the hands. We must, therefore, accept only provisionally any one type of atomic structure which may be offered. It is, on the highest valuation, only one possible solution of the problem. We must be prepared for alteration, for addition, for re-construction, as new phenomena are observed and need to be accounted for. We must be prepared even to abandon it altogether if some better 'working model' is devised. The hypotheses of science are continually changing. Old hypotheses break down and new ones take their place. But the classification of known phenomena which a hypothesis has suggested, and the new discoveries of phenomena to which it has led, remain as positive and permanent additions to natural knowledge when the hypothesis itself has vanished from thought.

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70.

QUELQUES EXPÉRIENCES SUR LA PRESSION DE LA LUMIÈRE.

Conférence faite à la Société française de Physique, le 31 mars 1910.

[*Bulletin des séances de la Société française de Physique*, **1**, 1910.]

Les premiers essais pour mettre en évidence la pression de la lumière ont été faits au XVIII^e siècle par les philosophes qui acceptaient la théorie que la lumière se compose de corpuscules matériels projetés par la source lumineuse. Pour ces philosophes, c'était une conclusion naturelle, que le bombardement d'un corps par les corpuscules qui composeraient la lumière exerce une poussée en arrière ; et, d'après leur théorie, la pression par centimètre carré devait être égale à deux fois l'énergie cinétique des corpuscules par centimètre cube, —ce qui est deux fois la valeur réelle de la pression actuellement mesurée. Si ces anciens expérimentateurs avaient su calculer la densité de l'énergie cinétique dans le faisceau lumineux à l'aide de la chaleur développée par un corps absorbant, ils auraient compris combien la pression cherchée était petite, et ils n'auraient pas été étonnés du peu de succès de leurs efforts.

Le premier essai pour démontrer que *les ondes* exercent une pression a été fait par Euler en 1746*. Je pense que son raisonnement ne s'appuie pas sur des preuves convaincantes, et le peu d'attention que sa théorie a reçu n'est pas surprenante. Mais, il n'est pas sans intérêt maintenant d'observer qu'il croyait que la pression de la lumière du soleil sur les petites particules d'une comète les repoussait du soleil, et formait ainsi la queue de la comète.

Depuis Euler en 1746 c'est Clerk-Maxwell qui, en 1874†, donna la première théorie définitive et raisonnable en prouvant que la lumière exerce une pression sur une surface quelconque, en tombant sur elle.

Il démontra que, si la lumière se compose d'ondes électromagnétiques, elle doit exercer sur une surface sur laquelle elle tombe normalement une pression par centimètre carré égale à l'énergie contenue dans 1 centimètre cube du faisceau. En d'autres termes, la pression égale la densité de l'énergie.

Peu après, Bartholi‡, par une application de la thermodynamique, démontra que des ondes du type lumière doivent exercer une pression. La preuve ne dépend d'aucune hypothèse particulière sur leur nature.

* *Hist. de l'Acad. Roy. des Sciences et Belles-Lettres*, Berlin, 1746, p. 117.

† *Electricity and Magnetism*, vol. 2, § 792.

‡ *Exner Repertorium der Physik*, vol. 21, 1885, p. 198.

Sir Joseph Larmor* a mis cette preuve sous une forme aussi précise que simple, et je me propose de prouver, par sa méthode, comment la radiation issue d'une source doit presser contre elle.

Soit A (Fig. 1) un centimètre carré en repos, qui émet un faisceau de radiation avec énergie E par centimètre cube selon la normale sur A. Soit AC (égal à U) la distance traversée en chaque seconde, et soit n le nombre d'ondes issu par seconde qui occupent l'espace AC. La longueur de chaque onde est λ, si bien que $n\lambda = U$. L'énergie en AC égale EU.

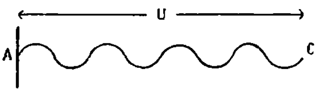

Fig. 1.

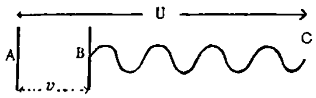

Fig. 2.

Maintenant, supposons que A soit amené en avant avec une vitesse v, de sorte que dans une seconde il parcourt une distance AB égale à v (Fig. 2).

Les n ondes émises par seconde sont maintenant comprises dans la longueur BC égale à $U - v$, et, si la longueur d'ondes actuelle est λ', nous avons:

$$n\lambda' = U - v.$$

Mais $$n\lambda = U,$$

et ainsi $$\frac{\lambda'}{\lambda} = \frac{(U - v)}{U}.$$

Maintenant, nous supposerons que l'amplitude des ondes émises est la même, que A soit en repos ou en mouvement. Avec des ondes du type lumière, la densité de l'énergie est en raison inverse du carré de la longueur d'ondes quand l'amplitude est la même.

Donc, si E' est la densité de l'énergie dans le train BC,

$$E' = \frac{E\lambda^2}{\lambda'^2} = \frac{EU^2}{(U - v)^2},$$

et l'énergie par longueur $U - v$, c'est-à-dire en BC, égale

$$E' (U - v) - \frac{EU^2}{U - v}.$$

Maintenant, nous ferons une supposition de plus, c'est que A verse la même quantité d'énergie qu'il soit en repos ou en mouvement, c'est-à-dire que le flux de son énergie ne dépend que de sa température.

Donc, A émet EU par seconde. Puisque BC contient $\frac{EU^2}{U \quad v}$, et que A a versé seulement EU, il faut qu'une quantité additionnelle d'énergie

$$\frac{EU^2}{U - v} - EU \quad \text{qui est égale à} \quad \frac{EUv}{U - v},$$

* *Encyclopædia Britannica*, vol. 32: *Radiation.*

soit donnée, d'une façon ou d'une autre, au faisceau par le mouvement de A. Cette énergie ne peut être donnée, à moins qu'il n'y ait une force qui fasse résistance au mouvement de A, c'est-à-dire que *le faisceau, en sortant de A, doit presser contre A.*

Si la pression est P,

$$Pv = \frac{EUv}{U - v}$$

et

$$P = \frac{EU}{U - v}.$$

Si v égale zéro (c'est-à-dire si A est en repos):

$$P = E \text{ (la densité de l'énergie).}$$

En négligeant le carré de $\frac{v}{U}$, nous aurons:

$$P = E\left(1 + \frac{v}{U}\right),$$

où P est un peu plus grand quand A avance, et un peu plus petit quand A recule.

La radiation presse en arrière contre la source d'où elle sort, et, conséquemment, la source presse en avant contre le milieu. Autrement dit, elle donne au milieu une quantité de mouvement égale à P par seconde. Cette quantité de mouvement donnée par seconde est répandue sur une longueur U, de sorte que la quantité de mouvement, par centimètre cube des ondes, égale:

$$\frac{P}{U} = \frac{E}{U}.$$

Cette quantité de mouvement est portée en avant par le faisceau, et, quand la lumière tombe sur une surface, la quantité de mouvement se rend à la surface, c'est-à-dire que la lumière presse contre une surface qui la reçoit.

En considérant le cas d'un réflecteur parfait, il est aisé de démontrer que, pour que l'on puisse se rendre compte de l'énergie additionnelle dans le faisceau réfléchi, quand la surface avance, il est nécessaire que le faisceau presse contre elle avec une force $P = E$.

Le raisonnement est tout à fait semblable à celui que je viens d'indiquer pour évaluer la pression contre la source.

Il est un peu plus difficile d'établir ce qui se passe dans le cas plus général, quand la source rayonne dans toutes les directions, mais la pression peut s'évaluer toujours par une modification de la même méthode.

Ainsi nous arrivons à la conception d'un faisceau de lumière comme porteur d'une quantité de mouvement. Il reçoit une quantité de mouvement de la source. Il porte cette quantité à travers l'espace, et il la livre à un corps quelconque qui l'absorbe.

Les ondes lumineuses, donc, possèdent une quantité de mouvement aussi certainement que les corpuscules lumineux des théories de nos ancêtres. Mais cette quantité n'est que la moitié de celle qu'il faudrait attribuer aux corpuscules.

Ce n'est pas mon intention aujourd'hui de décrire les belles expériences de Lebedef* et de Nichols et Hull†. Il suffit de dire qu'ils ont vérifié la théorie en dirigeant un faisceau de lumière sur un disque suspendu, et qu'ils ont prouvé, avec une exactitude surprenante, que la force avec laquelle le disque était pressé en arrière égalait l'énergie par centimètre de la longueur du faisceau,—surprenante si nous nous rappelons que l'énergie par centimètre cube de pleine lumière du soleil, et aussi la pression qu'il exerce, n'est jamais plus que $\frac{6}{10^5}$ d'un milligramme, ou environ 6 grammes par hectare.

Les deux grandes difficultés que l'on rencontre pour mesurer une pression si petite sont les courants de convection et l'action radiométrique. Lebedef suspendait son disque dans un vide si poussé que les courants de convection n'existaient plus, et que l'action radiométrique commençait à disparaître.

MM. Nichols et Hull opéraient avec une pression beaucoup plus grande, une pression de 1 ou 2 centimètres de mercure, cette région remarquable où les courants de convection cessent à peu près, tandis que l'action radiométrique est à peine commencée.

Je désire vous décrire quelques expériences faites par mon collègue, le Dr. Barlow, et moi, dont quelques-unes ont été suggérées par l'idée qu'un faisceau de lumière peut être regardé comme un courant de quantité de mouvement.

La première expérience‡ prouve qu'un faisceau, en tombant obliquement sur une surface qui l'absorbe, exerce une force parallèle à la surface.

Soit AB (Fig. 3) un rayon de lumière qui tombe obliquement sur une surface noire S. Il possède une quantité de mouvement dans la direction AB, et cette quantité est livrée à la surface.

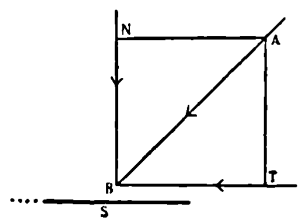

Fig. 3.

Représentons cette quantité de mouvement par la longueur AB, et décomposons là en NB selon la normale et TB selon la surface. NB presse la surface, mais cette pression n'a pas d'effet si la surface ne peut pas se porter en arrière. Mais, si elle est libre de se déplacer dans son propre plan, TB la fera glisser.

* *Annalen der Physik*, 4 Folge, Bd. 6, 1901, p. 433.

† *Proc. American Academy of Arts and Sciences*, vol. 38, 1903, p. 55⁰

‡ *Phil. Mag.* vol. 9, 1905, p. 169. [*Collected Papers*, Art. 21.]

Pour essayer s'il en est bien ainsi, nous avons suspendu deux petits disques, l'un noir, et l'autre argenté. Les disques avaient environ 2 centimètres de diamètre, et la tige qui les portait avait 5 centimètres de longueur. Ils étaient suspendus par un fil de quatre dans une boîte, comme le montre la Fig. 4, et la pression de l'air avait été réduite à environ 1 centimètre.

Un miroir était fixé à la tige, et on regardait l'image de l'échelle dans une lunette.

Quand on dirigeait un rayon de lumière, provenant d'une lampe Nernst sur le disque noir à 45°, il tournait d'un angle à peu près égal à celui calculé d'après l'énergie et la quantité de mouvement dans le faisceau. On évalua cette énergie en faisant tomber le faisceau sur une lame d'argent de poids connu. La lame avait été noircie pour absorber la lumière, et on observait la vitesse avec laquelle sa température montait. Quand le faisceau était dirigé sur la surface argentée, elle se mouvait à peine. Car, quoique le faisceau incident lui donnât une poussée en avant, le faisceau réfléchi donnait une poussée égale en arrière.

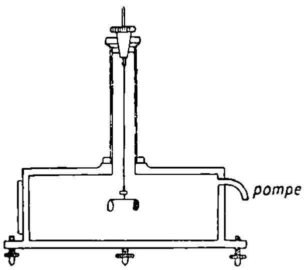

Fig. 4.

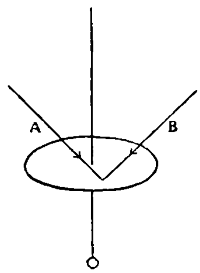

Fig. 5.

Mais cette forme de l'expérience, si simple qu'elle soit en théorie, est très difficile en pratique, à cause de perturbations produites par la convection et par l'action radiométrique. A moins que les deux disques ne soient parfaitement verticaux, et que la normale par le centre de chacun d'eux ne passe exactement par l'axe de suspension, il est très possible que les perturbations tournent l'appareil, d'une façon ou d'autre, et plus que ne le ferait la force due à la lumière. L'appareil exige une construction très précise et un réglage très soigné.

Nous avons fait l'expérience sous une autre forme d'une exécution plus facile*. Nous avons suspendu un disque noir (Fig. 5), d'un diamètre d'environ 5 centimètres, par un fil de quartz, de façon que le disque soit horizontal. On le mit dans une boîte vitrée, et la pression de l'air était de 1 ou 2 centimètres.

Quand un faisceau de lumière A était ainsi dirigé à 45° sur une petite étendue près de la circonférence du disque, la force lumière parallèle à la

* *Nature*, vol. 75, 1906, p. 91. [*Collected Papers*, Art. 67.]

surface tendait à le tourner. Mais la surface s'échauffait par la lumière, et les courants de convection et l'action radiométrique entraient en jeu.

A moins que le disque ne soit parfaitement horizontal, ces forces tendent aussi à faire tourner le disque, et la direction de ce mouvement dépend de la direction de la pente.

Nous avons observé l'angle total dû à l'action de l'air échauffé et à la force lumière horizontale.

Puis, nous avons dirigé le même faisceau sur le même espace à 45 de l'autre côté de la normale (*B*, Fig. 5). La quantité de chaleur absorbée était la même. Donc, l'action de l'air échauffé était la même en grandeur et en direction. Mais la force lumière horizontale s'était renversée. Donc, la différence des deux écarts était le double de celui dû à la force lumière d'un seul faisceau.

Cette méthode nous a donné des résultats très constants, n'importe où que le faisceau tombât sur le disque, même lorsque l'action de l'air échauffé surpassait plusieurs fois celle de la lumière. La force, attribuable à la lumière, se trouva à peu près égale à celle calculée d'après l'énergie dans le faisceau.

Nous mesurions encore celle-ci en la faisant tomber sur un disque d'argent noirci et en mesurant l'échauffement du métal par seconde.

Une autre expérience peut être expliquée par un modèle.

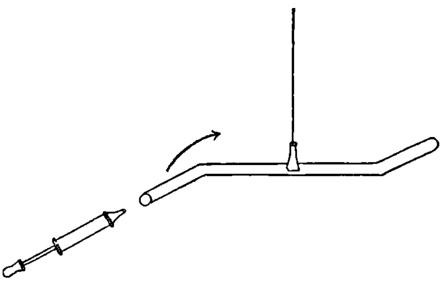

Fig. 6.

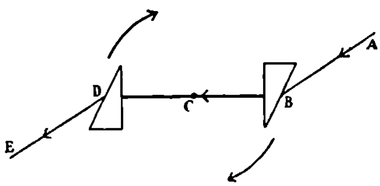

Fig. 7.

Voici un tuyau courbé (Fig. 6) suspendu de façon à être dans un plan horizontal. Si je souffle un courant d'air à travers le tuyau, il doit faire tourner les angles. En faisant tourner chaque angle, il presse vers l'extérieur et par conséquent, le tuyau se met à tourner. Le même effet justement s'observe si nous changeons la direction d'un courant de lumière, et la Fig. 7 montre comment on peut réaliser l'expérience*.

La figure représente en plan deux petits prismes de verre aux bouts d'une tige suspendue par un fil de quartz dans une boîte où l'on a fait le vide. Le rayon est dévié deux fois, puis il sort parallèlement à sa direction primitive.

* *Phil. Mag.* vol. 9, 1905, p. 404. [*Collected Papers*, Art. 22, p. 344.]

Quand la direction de son mouvement primitif vient à changer, une force est exercée vers l'extérieur contre le prisme aussi certainement que s'il y avait un courant de matière.

En envoyant un rayon de lumière à travers les prismes disposés de cette manière, nous avons trouvé qu'ils tournaient, et que, à quelques centièmes près, le déplacement était égal à celui que l'on peut calculer d'après l'énergie et la quantité de mouvement contenues dans le faisceau.

Cette expérience est intéressante parce qu'elle sert à faire comprendre un fait que je me bornerai à indiquer sans démonstration*.

Quand un rayon tombe sur un milieu réfringent plus dense, la force exercée sur le milieu réfringent due au changement de direction est dirigée vers l'extérieur, le long de la normale, et de cette manière on obtient un couple agissant sur les deux prismes.

Une autre forme de l'expérience† est expliquée par la Fig. 8. Un petit bloc de verre rectangulaire était suspendu comme auparavant dans la boîte où on fait le vide, et un rayon de lumière était envoyé à travers le bloc, comme dans la figure. Le bloc, lui aussi, se mettait à tourner. Comme je l'ai dit, la force à l'entrée est dirigée selon la normale, et par conséquent son moment par rapport à l'axe de suspension est nul. De même pour le moment à la sortie. Donc, les forces produisant le couple sont produites aux deux points de réflexion totale à l'intérieur.

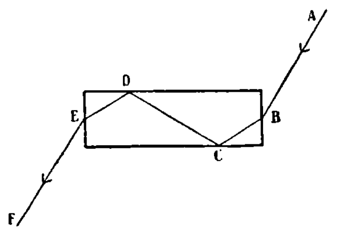

Fig. 8.

Dans chacune de ces deux expériences, nous pouvons raisonner de la manière suivante : Un courant de quantité de mouvement se propage parallèlement à lui-même, et cela ne peut se faire qu'au moyen d'un couple. Le système suspendu fait agir ce couple sur la lumière, et la lumière fait agir un couple égal et contraire sur le système suspendu.

Dans les expériences de Lebedef et de Nichols et Hull, et dans les expériences que je viens de décrire, on cherche la pression de la lumière contre un corps sur lequel elle tombe.

Mais, dans l'essai que je faisais pour prouver que la lumière transporte une quantité de mouvement, je suis arrivé à la conclusion que la lumière, ou, plus généralement, le rayonnement, presse sur la source dont il sort.

La dernière expérience que je décrirai est celle par laquelle nous avons réussi à prouver l'existence de cette pression‡.

* *Phil. Mag.* vol. 9, 1905, p. 401. [*Collected Papers*, Art. 22, p. 341.]

† *Loc. cit.* p. 402. [*Collected Papers*, p. 343.]

‡ *Proc. Roy. Soc.* Bakerian Lecture, March 17, 1910. [*Collect d Papers*, Art. 28.]

Comme un canon recule en sens contraire du projectile qu'il lance, de même un corps lumineux recule devant la lumière qu'il émet.

L'expérience est nécessairement un peu indirecte, parce que nous ne pouvons pas produire la chaleur à l'intérieur du corps lui-même. Seulement nous pouvons l'échauffer en faisant tomber un rayonnement sur lui. L'énergie radiante se convertit en chaleur, la température du corps monte, et puis, la chaleur sort de nouveau sous la forme d'un rayonnement.

Supposons qu'un rayonnement tombe sur une surface d'une lame noircie sur les deux faces, il échauffe la lame. Si elle est très mince, elle est à peu près à la même température partout. En régime permanent, elle émet toute l'énergie incidente, moitié sur une face moitié sur l'autre. Donc, les pressions des rayonnements sortants sont égales et contraires, et ne produisent aucun effet. Ainsi, nous n'avons que la pression, soit P, du rayonnement incident.

Maintenant, prenons une lame noire d'un côté, et parfaitement réfléchissante de l'autre, et faisons tomber un rayonnement sur le côté noir. Au régime permanent de température, la lame émet autant de rayonnement qu'elle en reçoit. Mais le côté postérieur est un réflecteur parfait, et, par suite, n'émet point de rayonnement. Donc, tout le rayonnement est émis par la surface antérieure. S'il était émis entièrement selon la normale, il produirait une pression P égale à celle de la lumière incidente. Mais il sort dans toutes les directions selon la loi des cosinus, et il est aisé de prouver qu'il ne produit qu'une pression égale à $\frac{2}{3}P$.

C'est pourquoi la pression totale est $\frac{5P}{3}$.

Dans l'expérience, nous avons employé quatre disques préparés comme suit: noir et noir, noir et argenté, argenté et argenté, argenté et noir.

Les pressions sur ceux-ci devraient être:

$$P, \qquad \frac{5P}{3}, \qquad 2P, \qquad 2P,$$

supposé que le noir soit parfaitement noir et l'argent parfaitement réfléchissant; mais aucune de ces suppositions n'est permise.

Et puis, bien que nous ayons suspendu les disques dans un flacon dans lequel on a fait le vide aussi parfait que possible, il y avait une petite action radiométrique due au gaz résiduel, et cette action était plus grande sur le disque noir-noir, et tendait à le presser en arrière.

Les disques étaient faits d'une couche d'asphalte fondu et pressé entre deux lames minces circulaires de verre. Ils étaient d'un diamètre de 1,2 cm., et d'une épaisseur d'un dixième de millimètre. L'épaisseur de l'asphalte était aussi environ d'un dixième de millimètre.

Les surfaces argentées se faisaient en déposant de l'argent d'une cathode d'argent.

Puis, les disques furent fixés dans des trous dans une lame de mica, comme on voit dans la Fig. 9, le centre de chacun étant à 1 centimètre de l'axe. Enfin, nous les avons suspendus dans un flacon dans lequel le vide était poussé aussi loin que possible.

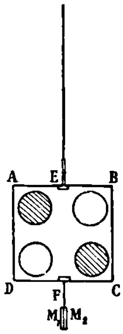

Fig. 9.

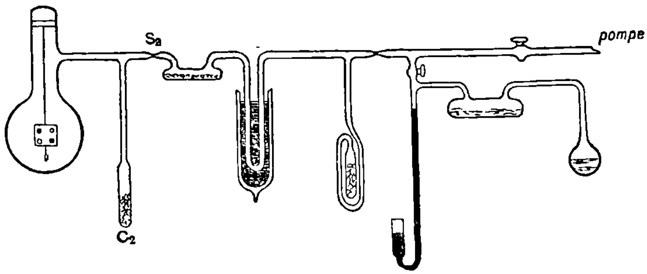

Fig. 10.

Il n'est pas nécessaire de vous expliquer ici tous les détails de la méthode employée pour faire le vide. Il suffit de vous dire que le flacon (Fig. 10) a été rempli d'oxygène et vidé plusieurs fois, qu'il a été scellé en S_2, et vidé définitivement en entourant le ballon C_2, qui contient du charbon, d'air liquide bouillant.

La Fig. 11 représente l'appareil en plan. S est une lampe, L_1 une lentille. La lentille L_2 forme une image de L_1 sur le disque. B est une lampe et C une échelle permettant de mesurer le déplacement par réflexion du miroir M (Fig. 9). La force correspondante a été calculée au moyen du temps d'oscillation et du moment d'inertie.

L'énergie du faisceau incident, qui était maintenue toujours constante, a été mesurée en le faisant tomber sur une lame d'argent noircie, et cette énergie était telle qu'elle devait produire, sur une surface entièrement absorbante, une pression donnant 13,6 divisions sur l'échelle. Les résultats d'un nombre d'expériences ont été les suivants:

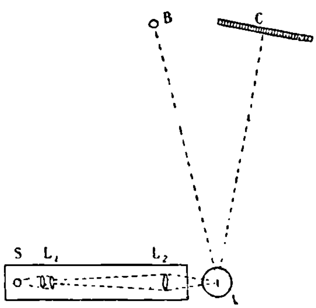

Fig. 11.

Les nombres, dans la première ligne, sont calculés en supposant un pouvoir réfléchissant par N de 5 % et par A de 95 % :

	NN	NA	AA	AN
Calculé ...	14,3	22,0	26,5	26,1
Observé ...	16,1	22,3	28,7	28,0

Sans perturbation par le gaz résiduel, nous aurions dû trouver les valeurs calculées.

Je ne doute pas que l'action radiométrique, qui atteint une valeur maximum avec le disque noir-noir, ne soit la cause de l'excès de 16,1 sur 14,3.

Mais j'espère que nous sommes d'accord, que les résultats prouvent définitivement que le rayonnement issu de la surface noire du second disque le presse en arrière.

S'il n'y avait pas de pression de cette sorte, la force sur ce disque-là serait la même que celle sur le premier disque. Mais les expériences démontrent qu'elle est 1,4 fois de celle sur le premier disque et 0,8 fois celle sur le disque argent-argent, et ce dernier résultat s'accorde étroitement avec la théorie.

En somme, il nous est permis d'affirmer que la lumière reçoit une quantité de mouvement de la source dont elle sort, qu'elle porte cette quantité de mouvement avec elle à travers l'éther (ou quel que soit le milieu qui ondule), et qu'elle la livre à n'importe quelle surface sur laquelle elle tombe.

Les forces dues à la pression de la lumière sont si minimes, de l'ordre d'un cent millième de *dyne* dans les expériences que je viens de décrire, et les perturbations dues au gaz, même dans le meilleur vide, sont si grandes qu'il faut des années de pratique pour vaincre les difficultés et pour produire des résultats certains et décisifs.

Ici, sur la surface de notre terre, et dans notre atmosphère, la pression de la lumière est peu de chose. Mais, dans l'espace entre le soleil et ses planètes, où il y a un vide beaucoup plus poussé que tous ceux que nous savons produire, elle a un libre essor, et elle peut produire sur les corps petits de grands effets à travers les âges, mais négligeables dans le cas de grands corps comme notre terre.

La pression entière de la lumière du soleil sur la terre n'est que 70.000 tonnes. Cela paraît grand, mais la gravitation due au soleil est quarante trillions de fois plus grande.

Pourtant, si le rayon du corps soumis à l'action de la lumière est réduit, la raison de la pression-lumière à la gravitation monte en proportion, de sorte que, sur une sphère d'un diamètre de quarante trillionième de celui de la terre, c'est-à-dire d'environ $\frac{3}{10^5}$ centimètres, et de la même densité que la terre, la pression de la lumière égalerait la gravitation. Si la sphère était encore plus petite, la pression de la lumière serait plus grande que la gravitation, et pousserait la particule hors du système solaire.

Et voilà une des explications des queues des comètes.

Mais je voudrais appeler votre attention sur l'action de la lumière solaire sur des particules un peu plus grandes, des particules telles que celles qui révèlent leur existence par leur mort dans notre atmosphère sous la forme d'étoiles filantes.

Supposons qu'une telle particule, de diamètre, disons de 1 centimètre, de la densité de la terre, et noire de façon à absorber la lumière solaire entièrement, fasse le tour du soleil à la même distance que la terre. La gravitation sera opposée à la pression de la lumière, et la vitesse de la particule nécessaire pour la maintenir dans son orbite sera plus petite que celle de la terre, de sorte qu'elle demandera 7 minutes de plus pour compléter sa révolution, ou bien encore son année sera de 7 minutes plus longue que la nôtre*.

Plus la particule est petite plus l'effet est grand. C'est pourquoi une particule d'un diamètre d'un millième de 1 centimètre exigera 7.000 minutes, ou 117 heures, ou cinq jours de plus par an.

Un autre effet de la pression de la lumière est qu'elle donne lieu à une force qui s'oppose au mouvement de la particule.

La particule est échauffée par le soleil, et, à la distance de la terre, sa température serait à peu près la même que la température moyenne de la surface de la terre, soit 300° absolus, ou 27° C.; donc, elle émet du rayonnement de tous les côtés. Mais les ondes de devant sont plus courtes, tandis que celles de derrière sont plus longues, comme le montre la Fig. 12, dans laquelle les points représentent les positions successives de la particule et les cercles, les ondes successives émises. Ainsi, il y a plus d'énergie par devant que par derrière, plus de pression contre la surface antérieure que contre la surface postérieure; donc, une force qui empêche le mouvement. L'accélération due à cette force est en raison directe de la vitesse, et en raison inverse du rayon de la particule. Il s'ensuit que la particule perd de l'énergie. Elle en rayonne plus qu'elle en reçoit, et elle se transporte vers le soleil.

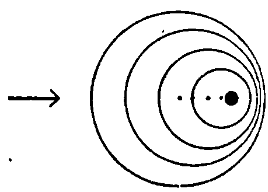

Fig. 12.

Une sphère de la densité de la terre, noire afin d'absorber la lumière du soleil totalement, et d'un diamètre de 1 centimètre, s'approchera du soleil de 1.640 mètres pendant la première année. Ce mouvement continuera, et

* Some arithmetical slips in this paragraph and the next have been corrected. The working-out is given in *The Pressure of Light* (S.P.C.K.). Ed.]

je calcule que, dans quarante-cinq millions d'années environ elle parviendra au soleil.

Avec des particules plus petites, l'action est plus rapide, et une particule d'un diamètre du millième de 1 centimètre, en mouvement d'abord presque dans un cercle à la distance de la terre, décrira une spirale qui finira sur le soleil en quarante-cinq mille ans à peu près.

Le soleil a horreur de la poussière. Avec la pression de sa lumière il repousse les particules les plus fines loin de son système. Avec sa chaleur il échauffe les particules plus grandes. Celles-ci rendent cette chaleur, et avec elle une partie de l'énergie qui les met à même de résister à son attraction.

Peu à peu, il les tire vers lui-même, et enfin elles tombent sur le soleil. Elles sont brûlées. Elles cessent d'avoir une existence séparée.

POSTSCRIPT (1918).

RETARDATION BY RADIATION PRESSURE: A CORRECTION

By Sir JOSEPH LARMOR, F.R.S.

There is a discrepancy, noticed also recently by Prof. Schuster, between the result of Poynting and the one above obtained by me (p. 431) on Maxwellian principles for the retardation of a body arising from its own radiation, which demands consideration. The subject, if it is to be reduced to terms of simple statement of physical principle, without intricate algebra, requires methodical exposition, as the synopsis now submitted may show.

The foundation of Poynting's argument was the postulate that the effect of convection v on a radiating system is, up to the first order of v/c, (i) to retain unaltered the state of the system as determined by the positions and motions of its electrons, (ii) thus to conserve the amplitudes of the transverse optical vibration along all the rays, but to alter the wave-length or period on account of the following-on of the source by the Doppler-Fizeau principle, and therefore to alter the energy-density inversely as the square of the wave-length. This statement proves to be correct: it can be verified by an application of the Lorentz correspondence analogous to the one mentioned *supra*, p. 434. The radiation thus altered thrusts back on its source on account of the momentum it carries away: the thrust of each ray being altered by convection in the same ratio as the density of momentum, or of energy multiplied by c, as Poynting asserted in the text—but also altered in the opposite direction on account of the shortening of the ray along which the momentum is emitted, in accordance with his correction made in an appendix which reduced the result to one-half.

But the thrust deduced by him on these principles for a spherical body, assumed to be a full radiator if that be possible, is only one-third of the value deduced by a complete formal analysis on Maxwellian lines (*supra*, p. 434), namely $-Rv/c^2$ where R is the rate of radiation from the body. Some element must have been overlooked.

In the case of an isolated radiator nothing that could happen can alter the resultant momentum of the entire system, that belonging to the body and that belonging to the radiation that has left it. The momentum in the radiation that is moving out from it over a distant boundary fixed in aether is equal and opposite to its reaction, which is the time-integral of the back thrust of its rays on the boundary. There is also the change of the momentum in the region within the boundary and within the source itself. The latter part fortunately can be determined, and an equation of conservation is established; as follows.

It is easy to verify, by application of the Lorentz transformation in its exact form, that the effect of convection on the electrodynamic energy distributed within any regions that correspond in the two states, convected and unconvected, of the system is expressed by

$$E = E_0\left(1 - \frac{v^2}{c^2}\right)^{-\frac{1}{2}}$$

$$= E_0 + \tfrac{1}{2}\frac{E_0}{c^2}v^2 + \ldots,$$

where E_0 is the energy in the unconvected state of the system. Thus when $(v/c)^4$ is neglected, the change of energy arising from convection is described by the ascription to the system of an extra inertia $E_0\, c^2$, where E_0 is its energy of electrodynamic type. In this interpretation the boundary is supposed to be so far away that the field-energy that is attached to the system itself and gives rise to its electric inertia is practically all included within that boundary; there is also energy of free radiation included within it, but the momentum of that part will come separately into the account.

The momentum attached to the moving source is of form mv, and the force required to alter its motion is $\frac{d}{dt}(mv)$ which is $m\frac{dv}{dt} + \frac{dm}{dt}v$. The latter term in the force, involving the velocity as a factor, is in this case $\frac{d}{dt}\left(\frac{E}{c^2}\right)v$ or $-Rv/c^2$ where R is the radiation per unit time. This agrees with the value that had been directly determined in the discussion reproduced above.

When these items are all collected together the account is balanced, the total momentum not changing with time: this is in accordance with expectation and gives confidence that no element has been omitted. But the change of sign hastily suggested near the end in a footnote appended in the Math. Congress volumes* must be withdrawn, the original text being correct.

To sum up: the total change of momentum in the field of any convected radiator is made up of

(i) the backward thrust of the radiation travelling out across some definite boundary fixed in the aether, which it is desirable to choose so far away that there is sensibly no energy in the aether adjacent to it except free radiation:

(ii) the change of momentum of the free radiation inside this boundary:

(iii) the effect as regards momentum of the change of the inertia of the source owing to the energy of electrodynamic type which it has lost by its own radiation.

* Vol. 1, p. 19, 'On the Dynamics of Radiation.'

The analysis above referred to shows that these parts cancel: and that the force required to change the velocity of the source whose mass is diminishing owing to its radiation, as measured by the rate of change of momentum due to the activity of that force, is simply $m\frac{dv}{dt}$, where m is the mass the system happens to have at the instant. This force has been here determined from the rate of change of momentum over the whole field, which implies that when the source is disturbed the field attached to it has time to attain sensibly to a steady distribution at each instant, and thus to transmit its reactions to the source.

Thus far for an isolated radiator: the loss of radiant energy affects its mass m, but at each instant the effect of an applied force is determined as $m\frac{dv}{dt}$, where m is the mass at that instant.

But for Poynting's particle describing a planetary orbit the radiation from the Sun comes in, which restores the energy lost by radiation from the particle, and so establishes again the retarding force $-\frac{1}{c^2}\frac{dE}{dt}v$. It does more; the thrust of the momentum of solar radiation ϵ scattered and in part absorbed by the particle produces a repulsion from the Sun equal to $\frac{1}{c}\frac{d\epsilon}{dt}$, which is inversely as the square of the distance: but this merely modifies the effective modulus of gravitation, producing once for all a slight permanent change in the orbit.

But there is another interesting effect, which proves to be important. The radiation ϵ from the Sun, considered relative to the orbital motion of the particle, is subject to the astronomical aberration of light: therefore so is the momentum which it carries: coming on to the particle obliquely along with the rays this momentum has a tangential component equal to $\frac{v}{c}$ of the whole, that is to $\frac{v}{c^2}\frac{d\epsilon}{dt}$. Here $\frac{d\epsilon}{dt}$ represents the solar radiation absorbed and also scattered, of which the former part is balanced by the radiation $-\frac{dE}{dt}$ of the particle.

Thus the aberrational effect doubles the previous result, so that for a planetary particle which is a complete absorber the total retarding force is $2Rv/c^2$, which is six times Poynting's result, or three times that result as corrected in his Appendix. However in the astronomical application to explain the transparency of the interplanetary spaces, which was his chief aim, it is only the order of magnitude that is essential: and that remains practically as it was.

But the remarkable result seems to be established that an isolated body cooling in the depths of space would not change its velocity through the aether, the retardation due to the back thrust of the radiation issuing from it being just compensated by increase of velocity due to momentum conserved with diminished mass: it will move on with constant velocity, but with diminishing momentum so long as it has energy to radiate.

The cause of inadequacy of Poynting's form of argument from momentum has yet to be specified. He calculates the part (i) above, the back thrust of the radiation, for a spherical body assumed to be a perfect radiator, by integration over a boundary which coincides with the surface of the body. This procedure is at fault; for so close up to the source there are the electrodynamic fields of the adjacent vibrators as well as the field of the free radiation, and the stress over the boundary due to the former fields is operative and should have been included. He would have attained to the same result for back thrust of the radiation, more simply, by integrating over a very distant boundary; but in that case the changing momentum of the free radiation inside this fixed boundary will also contribute as in (ii), and necessarily to the same degree that the neglected fields of the vibrators would have done on the other plan which he adopted.

The root principles of the thermodynamics of radiant energy, the transmission of momentum to a distance by radiation, and the alteration of its amount by convection of the source or of a reflector, thus stand firm without doubt or ambiguity.

CAMBRIDGE,
October 1918.

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