"THE PROGRESS OF CHEMICAL THEORY: ITS HELPS AND HINDRANCES."

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Introduction to the Chemical Lecture Course at the FRANKLIN INSTITUTE,
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The PROGRESS of CHEMICAL THEORY: Its HELPS AND HINDRANCES.

By Dr. Persifor Frazer, Prof. of Chemistry.

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It is the ordinary reproach to science of the ignorant and disingenuous, that its conclusions at any given period are no more stable than the wildest speculations of the fanatic and dreamer.

We read continually in the papers some arrant nonsense said to have been pronounced by "one of the eminent scientists" of such-and-such a place and time, which the course of events has disproved; and the public is left to the conclusion that the gains of science are only air castles certain to dissolve when they become unpopular, and certain to lose popularity when the first pleasurable effect of the announcement has passed away. The following extract, from the *St. Louis Republic*, will furnish a case in point:

"It has only been sixty years since a great mathematician demonstrated that a steamship could never cross the Atlantic because it would be impossible for her to carry enough fuel to last during the trip. Before he had hardly deduced his calculations a steamer from America glided into port."

The name of the eminent scientist is not mentioned, and it is safe to conclude that if any man made such an observation he either would have failed of recognition by the class to which he is said to have belonged, or he was false to the fundamental principles of inductive science. It is not the province of inductive science to establish what is impossible, but what is in various degrees likely. Its premises are facts and its conclusions are probabilities; in many cases weak, but in others so strong that they produce the same effect upon the mind as certainties. Nor is it true that the gains of science are evanescent. Parallel with the accumulation of observations run the generalizations upon them. These generalizations are usually passed through the purgatory of hypothesis before they attain the bliss of theory, but no theory is old enough yet to have become more than a theory, though some have stood so many tests of their truth as to carry the conviction of axioms.

In looking over the histories of the sciences one finds the same general course of progress. At the outset in the halcyon days of the old Greeks, it is likely that some wise words will be found to have been spoken concerning them all; words that astound us with the apparent insight they show into problems which it would seem that the last twenty-five centuries were needed to give. But the centuries were not all equally productive. There came across the path of every systematic study of the laws of nature, first the cultured blight of the Aristotelian philosophy, which assuming to know everything, in fact largely contented itself with verbal jugglery, whereas accumulation of facts was the only road to knowledge; obliterating the forward steps that had been made, and substituting in their place the evolution of the universe and its laws from within. It was a philosophy where the distinction between words and things was obscured, and a natural fact was attained by means of a pretty syllogism.*

Vastly worse were the centuries which followed, known as the middle ages. Centuries of ignorance, selfishness, and crime; when the possession of any knowledge but that of an armorer was looked upon with distrust and ascribed to the devil.

The different natural sciences emerged from this barbaric condition one to three centuries ago, and under the liberty of enlightenment, with the stimulus of more general education; have attained an abode in the life of the race from which it is difficult to see how they can be displaced without such a general cateclysm as would nearly destroy the human race itself.

Amongst these sciences, that of chemistry has had such a marvellous career that it is, perhaps, the best example which could be selected of the progress just alluded to. It illustrates aptly not only the methods employed in building up an inductive science, but the things that have helped and those that have hindered a development which, nevertheless, in spite of all hindrances, must fill us with a sense of wonder.

Our reason for this is that from various causes the real growth of chemistry only began in the seventeenth century, and that even then it lost nearly a hundred years in the quagmires of a false hypothesis which not only directed the efforts of chemists into unfruitful fields, but destroyed the value of the conclusions they reached from their work. Yet, even with all these drawbacks, no domain of human investigation has been widened so rapidly and with such advantage to the world.

At the very outset of the subject we find a generalization of old Democritus (who lived 450 B.C.) so astounding in its character and so accurate in most of its statements that only in the past few years have chemists been able to reach these profound thoughts thrown across the ages into the

* It is intended to refer here to the exposition of the Aristotelian philosophy, by its disciples from about the time of the Christian era to the eighteenth century, and not to disparage the marvellous genius of Aristotle.

midst of the civilization of our time, as the legend has it that Bruce's heart was thrown by the Douglas into the hosts of the Saracens to stimulate the ardor of the Christian Knights to charge and recover it. But, unfortunately, no such effect was produced by the good old laughing philosopher; though at a snail's pace, and after a lapse of 2,300 years we have reached the spot. Briefly, as transmitted to Epicurus, and expanded by Lucretius B.C., 99–55, it was this.*

The universe consists of atoms and space. The atoms are of many forms and of different weights, and the number of atoms of each form infinite. Change is only the combination and separation of atoms! Atoms are in constant motion. "First beginnings" or atoms are never destroyed or worn out. The difference between a hard body like iron, and a soft body like air, is that in the first the atoms move to and fro within small distances; in the soft body they move freely or rebound from each other only at long intervals.

Bodies are partly "first beginnings," partly unions of "first beginnings." The properties of the bodies formed of the groupings of "first beginnings" need not be like the properties of the "first beginnings" themselves. "It matters much with what others and in what positions the first beginnings of things are held in union and what motions they do mutually impart and receive."

These views are extraordinary, and, with the exception of the difference in the form of atoms, which is a point beyond what we have been able to reach even now, the above contains a very fair statement of the atomic theory which is held by the most advanced chemists to-day.

How Democritus could have reached such conclusions is a mystery, but his annunciation of these recondite truths very well illustrates the fact that an hypothesis, be it never so beautiful and even true, if unaccompanied by facts to support it in no way helps the progress of natural science. Like every other guess it indicates merely the frame of

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* *Democrit-Aderitæ operum fragmenta.*

mind of the man making it. It is like a floating shadow on the sea of time. Perhaps it defines substance, perhaps only a cloud of fancy.

This seed thrown off by Democritus found no soil of facts on which to grow, from his time until late in the present century, although Gassendi, Canon and Provost at Digue, in France, after ages of ignorance, proposed it again, but without proof; and it is thought to have influenced the minds of Newton and Boyle.

This then is one example of an occurrence in the history of the science which to all appearance neither helped nor obstructed its progress unless in the indirect way of teaching men's minds to grasp large and comprehensive thoughts. All could not have been ignorance and degradation in Abdera (Thrace), or Miletus, or Athens, where a language existed capable of conveying from mind to mind thoughts like these, and where a mind was capable of conceiving such thoughts.

It teaches the student of natural history a lesson in addition to that of the old traveller's speculations, and it may serve to illustrate the difference which the late Prof. Clifford of Cambridge pointed out between accepting those conclusions of natural science which one has been taught, but has not personally investigated, and accepting what is said to have been revealed, but which, it is acknowledged, is not susceptible of any proof. In the one case the way is open to any one to pursue any single direction which has been before taken; measuring and judging of the correctness of the steps of one's predecessor; but in the other case there is no path anywhere, and the correctness of the position assumed cannot be judged. It is the difference between, on the one hand, handing the keys of a hundred trunks to a custom house inspector, who has at best time to examine but one or two, asking him to satisfy himself of the accuracy of your description; and, on the other, telling him that something indescribable ought to convince him more thoroughly of the contents of the trunks which he cannot inspect, than of those which he can. Speaking generally it may be said that a proposition of which the steps which led to its acceptance cannot be indicated and followed, has no place at all in the domain of science, though it may be true.

Such propositions were those of Democritus above given and it is quite just that in the absence of logical proof they should have been excluded from the realm of science, and that to him who first showed reason for believing them should be accorded the honor of their discovery.

Of much less importance is the next hypothesis of the nature of things which we find annunciated by Aristotle in his quadrilateral of states: solid, fluid, dry (or warm), and moist (or cold), or what he supposed to be the elements of all bodies, viz: earth, air, fire and water. It was unfortunate, and yet in accordance with the usual march of events that this utterly inadequate and narrow guess should have fettered men's minds for 2,000 years, owing to the mighty hold which Aristotle took of all nations.*

As his historian remarks, Aristotle's works had a prodigious influence in Asia, and Europe, and Africa; among the Persians, Arabs and in Germany where part of his ethics were read in the churches on Sunday instead of the Bible. In the middle ages, too, these elements of Aristotle were imbued with a mysticism more than Platonian.

It was the spirit of that middle age when the ignorant classes being the powers, made patient scientific work difficult and dangerous, that learning was concealed under the mask of paradox and cryptogram as if it were a crime. Whatever Aristotle's view of his elements may have been, it took a new direction, beginning with Geber in the eighth century.

The first chemists were alchemists who sought the transmutation of base metals into gold; the philosopher's stone; and the elixir of life. These were represented by Geber (an Arabian alchemist of 760), Albert von Bollstädt (1193–1280), Roger Bacon (1214–1294), Raymond Lull (1235–1315) Arnald de Villanova, Caletonia (1235–1314), etc. Those who examined physical problems retained the Aristotelian view

* See Aristotle, *A chapter from the history of science*, Lewes.

while the alchemists took more or less modified forms of Geber's doctrine, that the metals were composed of mercury and sulphur. As an instance of the confusion which reigned in the ideas of this time, some believed that these constituents of metals were real sulphur and real mercury, while others believed that qualities were intended by these terms. Geber ascribed to the sulphur the property of giving different colors to the metals.

At the end of the fifteenth century the alchemists had added salt to mercury and sulphur. Many regarded the Aristotelian elements as the ultimate; and mercury, sulphur and salt as the intermediate or proximate elements, as, for example, Basil Valentine, who extended the number of substances of which these were the ultimate elements, from metals to all known matter, but denied that they were the common substances which we know under their names.

In the early part of the sixteenth century the failure to find the philosopher's stone led to the decadence of alchemistical or transmutation chemistry, and the rise of iatro-chemistry or that of healing. Paracelsus (1493–1541) taught that in a burning body the sulphur quality represented the inflammability, the mercury the sublimation, and the salt the ashes.

From this to the end of the seventeenth century disputes as to tenets were numerous, but no real progress was made, Agricola (1490–1555) attacked Paracelsus and fell back upon Aristotle. Libavius wrote the first treatise on chemistry (1595). Van Helmont (1577–1644) denied all Paracelsus' views and sought an universal solvent, which should be a panacea. He first recognized the existence of gases and quantitative relations, and opposed Aristotle's doctrines that fire was a body or earth an element; but believed water and air were such.

Glauber (1603–1668) though possessing variable views, invented better means for separating bodies. Sumert (1572–1637); Willis (1621–1675); Lemery (1645–1715) believed in five first principles—mercury (spirit), sulphur (oil), salt, water (phlegma) and earth. Lemery taught that these were in rapid motion, and thus gave rise to the obvious properties of things. He explained the well-known phenomenon of the calxes of the metals weighing more than the metals themselves, by supposing that in burning they absorbed fire material.

The real philosophy of chemistry commences with Robert Boyle (1622–1691), who denied the accuracy of the doctrines both of Aristotle, and the later alchemical and iatro-improvements upon them. He believed that heat had not the power to transform complex substances into their constituents, but on the contrary, sometimes produced complex out of simple substances, and sometimes was without effect. Other agencies than heat could produce the same effects. He strongly denied that one could predict the number of simple substances as Aristotle and his successors had done. He thought it probable, however, *that the so-called elements consisted of the same kind of matter*, differing only in the size, form, etc., of their respective smallest parts.*

It is well to pause for a moment here to consider these logical and scientific views of Boyle, not alone because they introduced for the first time a rational inductive system of chemistry, emancipated from the mysticism and superstition of the ancients, but also because they are typical of one of the greatest of helps to the progress of chemical theory, independent and fearless criticism.

Except the brilliant guess that the so-called elements consisted of the same kind of matter, Boyle's mission seems to have been to hew down the weeds and undergrowth which had impeded the march of the science; yet his services were invaluable, as without them no further progress could have been made. This fact illustrates also the injustice of the cry so popular in some cases when the fallacy of a proposition has been exposed:

"What have you to set up in its place?"

Surely it cannot be required of him who discovers a flaw in a supposed explanation that he should be always ready with a sound explanation. The two characters of mind,

* See Kopp's *Geschichte der Chemie*.

which are required to accomplish these very different tasks, are entirely unlike.

Plato and Aristotle probably regarded the lightning stroke as a natural phenomenon, and could have refuted the popular belief that it was the missile from Zeus' hand, but it required dozens of centuries of observation before even the most remote approach to an explanation of the phenomenon could be given.

As soon as the ground is cleared of rubbish, other and more rational theories have a chance to grow. Therefore, the iconoclast, if impelled by his sense of truth, and if considerate in his methods, is a necessary pioneer and axeman ahead of the great army of science. It is so much easier, however, to throw down than to build up, that the iconoclast business is often overdone by those who are incapable of any more skilled service to science, and who confound the art of attacking everything with the duty of overthrowing evil. All honor to Robert Boyle for calling a halt in the unbridled fancy of the chemists of his day and clearing the way for a new era! All honor to his deep insight into the workings of nature, that he announced independently what old Democritus had dimly foreshadowed 2,000 years before; and what it was reserved for a great chemist now living to put in words and carry almost to the state of an accepted theory: yet to neither of them will belong the credit of demonstrating the unity of matter, but to some one, it would seem, who shall pass the speculative stage and offer proof. It looks as if this were not to be long delayed.

Both by Boyle's destructions and by his conceptions he aided the progress of chemical theory, as few have done since his time, and chemistry or the study of the most intimate relations of matter, as distinguished from alchemy, magic or the healing art may fairly be said to have started with Robert Boyle.

Singularly enough the first sapling to spring up and occupy the new clearing made by Boyle was an error so gross that it seems to the youngest student of to-day grotesque in its clumsiness, and yet defended by some of the subtlest of sophists, it took 100 years to overthrow it.

And the most instructive part of its history is that it was finally overthrown by an argument which Boyle himself had employed; which had been employed by other sceptics, and explained away by the phlogistonists, and was ultimately and successfully refuted with the same experimental proof by a countryman of Boyle. It is often the case that an attack in front, over the very ground of numberless previous repulses is successful, and it was the case here as shall be briefly shown.

Stahl (1660–1734) was a physician of independent views, who adopted Becher's theory of combustion or the changeability of bodies by heat. He believed that he had settled experimentally this question:

"Is a common quality present in sulphur and carbon? or is one contained in the other?"

The generally accepted view at that time was diametrically opposite to that which Boyle held of combustion, and might be stated thus: Sulphur consists of oil of vitriol and some combustible body, which latter escapes in burning. Stahl combined oil of vitriol with an alkali and heating the combination with carbon obtained an alkaline sulphide similar to that produced by sulphur and an alkali. From this, sulphur (or vitriol) can be separated.

Therefore, the combustible in carbon and sulphur was the same!

Heating calxes of the metals with carbon, there resulted the metals. The metals were then composed of the calxes and this substance. Fats and oils produced the same effect with the calxes, and, hence, in them too was the same combustible substance.

Stahl called this combustible "Phlogiston."

This hypothesis was rapidly installed into the rights and dignity of a theory and rallied around it as such, some of the brightest and best minds for three generations.

It was not only faulty in its conclusions, but inadmissible in its steps, and should have incurred the opposition of every intelligent man who understood the limitations of inductive philosophy: but it occupied the vacant space left bare by the labors of Boyle, and with a growth as luxuriant as it was pernicious to the attainment of truth, obstructed in many ways all valuable advance of chemical theory, while it lasted.

It may not be amiss before sketching its rise and overthrow, to point out here wherein its inherent fallacies should have condemned this hypothesis from the outset.

Hypothesis means a guess—a temporary structure erected by the employment of the imagination strictly governed by experience, for the purpose of more rapidly reaching a generalization than by waiting for all the facts which in the end will be necessary to sustain a full-fledged theory. After one or two facts bearing on a subject are ascertained, it often happens that the mind is directed towards the possible existence of a law which would explain them both, but numerous unknown and untried experiments must result in a certain way in order that this supposed explanation may stand.

With time and a constantly increasing experience more and more such facts are ascertained. If all fall into their places the hypothesis grows stronger and stronger in probability until by a large accummulation of such corroborations, the hypothesis passes the undefined line which separates it from theory, and becomes a theory.

This theory then goes on increasing in strength by each additional fact which is found conformable to it, until its convincing force is almost as great to the mind as one of the facts which are the bricks of its construction.

But if during this period of probation of an hypothesis or of a theory a single fact is well authenticated which is inconsistent with it, the hypothesis or theory must be abandoned. Of course, in the case of a theory which had been tried and proven hundreds of times and found to apply to newly-discovered facts, its abandonment would be held in abeyance until every effort had been made to prove the authenticity of the fact and its inconsistency with the supposed explanation; but if these were unalterably confirmed the hypothesis or theory must fall.

This constitutes the true principle of inductive philosophy, and only by pursuing this path rigorously can its processes lead to any good result.

The obvious difficulty of the phlogistic theory was that the calx, which was considered the simple body was heavier than the metal which was supposed to be the compound, and this circumstance was explained by supposing that the phlogiston which was absorbed from the fire when the calx was heated therein had the principle of "levity" as opposed to that of "gravity," and that the more of it which was collected in a body the lighter became that body.

Here was the unpardonable error of the phlogistonites in the defence of their theory.

Hofmann (1660–1742), the first to distinguish between magnesia and alumina, asked whether instead of the above explanation of combustion it was not the case that a metal received an "acid" when burned, which "acid" was re-absorbed when the metal was reduced. Besides him many others had noted the increase of weight in combustion, and Boerhave (1668–1738) had doubted the explanation above referred to.

The offence of which as seekers after truth the phlogistonites were guilty was that of calling into existence a substance which was diametrically different in its properties from any then known, and making this imaginary substance and its purely imaginary properties the basis of a theory.

Isaac Newton (1642–1727) had established, by his matchless investigations, commencing 1666, the nature and properties of matter, one of which, gravitation, was common to all bodies, and there was no excuse for an hypothesis after that date, which should deliberately rob matter of its one all-pervading attribute, unless some discovery had been made which seemed to support it.

If it be said that the observations on the combustion of bodies seemed themselves to furnish this proof, it must be admitted that for such an overthrow of all that had been patiently built up, some independent testimony unconnected with the then obscure phenomena of combustion ought to have been sought. This was an unpardonable deflection from the line of calm and dispassionate inquiry, and deserves to be held up for all time to the condemnation of scientific men as a warning, and its fate as an example; the more so because as time went on and the array of obstacles to the acceptation of this theory increased, its supporters were obliged to set up one after another distinct hypotheses to support the first untenable one, until the discussion partook more of the nature of those verbal juggles popular among the schoolmen of the middle ages than the efforts of students of nature to arrive at a knowledge of her laws.

Marggraf (1709–1783) introduced into chemistry the study of reactions in the wet way, and thus laid the foundation of analytical chemistry. He recognized soda as an alkali, and magnesia and alumina as peculiar earths. Against the opinions of his predecessors he held that ammonia was not produced in the distillation of wood, etc., by the union of its constituents, but that it pre-existed in the wood.

J. Black, of Scotland, by his investigation of the alkalies, dealt the first serious blow to the phlogistic theory. He found that magnesia could be changed to "mild" from having been strongly alkaline by exposure to air, or by contact with "mild" alkalies. In other words, that it became carbonate of magnesia by exposure to the carbonic acid of the air or by treatment with the carbonates of the alkalies. Furthermore, he showed that what it lost or gained in these changes was a gas like air, which separated from it under treatment by acids with effervescence, whereas in its caustic condition acids dissolved it without effervescence. He also, with singular astuteness, established for his theory of combustion (which was to finally take the place of the phlogistic theory) the principle of latent heat. The immediate result of Black's discoveries was the rise of pneumatic chemistry or the chemistry of the gases.

Joseph Priestley (1733–1804) followed with more success than any other this line of investigation. He worked sporadically and without system, but with wonderful penetration discovered many things that had escaped the attention of better chemists. He himself called his achievements "sportsman's luck." He found the nitrogen left after combustion in air, and determined its proportion by volume. He regarded it as charged with phlogiston.

In 1774, he discovered oxygen in the oxide of mercury, and recognizing it as the supporter of combustion in the air, made it the measure of the destruction or devitalization of the air. He discovered besides oxygen and nitrogen, the reduction of the calxes by hydrogen. He also observed that by the passage of the electric spark through confined air in contact with moistened litmus, a new acid was produced which colored the latter red. He furnished more material than any other chemist of the day for the destruction of the false phlogistic theory, but he was not only incapable of correctly reasoning on the facts which he had brought to light, but to the day of his death he maintained that his pivotal discovery; the corner-stone of the chemistry of to-day (as it was made by Lavoisier) was nothing but the production of dephlogisticated air.

It was a touching thought for those of us who, in 1874, assembled at his grave in Northumberland, Pa., to do honor to his memory as a great discoverer and a devoted friend of our young States when we threw off the English yoke, and to celebrate the centennial anniversary of the foundation of modern chemistry, in his isolation of oxygen; that this great mind so potent in discovery in the science which he adorned, rejected the obvious fruits of that discovery, and insisted on rejecting the honor which should justly have been his.

H. Cavendish (1731–1810) was a more rigid investigator, who, having discovered hydrogen by the action of sulphuric or hydrochloric acid on iron or zinc, proceeded to examine exhaustively the properties of this new gas. He noted that like weights of metal gave him the same volumes, but that different weights of metals gave him different volumes of the gas. He concluded that hydrogen was either phlogiston or a combination of phlogiston with water, and that dephlogisticated air (oxygen) when combined with phlogiston (hydrogen) produces water.

He discovered the solubility of carbonates of the alkaline earths in excess of carbonic acid water, and discovered nitric acid. He was thoroughly saturated with the phlogistic theory and all the useful deductions which his careful methods would have given him were thrown away by the distortions of the obvious bearing of his experiments which the phlogistic theory necessitated. When Lavoisier had triumphantly overthrown this dragon, Cavendish abandoned chemistry; simply remarking that it was hard to determine which of the rival theories was true.

Passing over the labors of the phlogistonists: Scheele, who discovered chlorine and fluo-silicic acid, and who believed light and heat to be fire and air; the first with more and the second with less phlogiston; and Bergmann, who systematized wet analysis, proved the existence of carbonic acid in the air, and introduced dry qualitative blowpipe analysis; we come to the first of a number of great figures in the history of modern chemical theory whose mission it was to open the door of the exact science of to-day and to indicate the route of those who followed him.

Lavoisier (1743–1794). His first chemical work was the experimental proof that water did not become an earth by boiling, but that the residue observed by boiling in a glass vessel was derived from the glass. His own discoveries are meagre in comparison with those of many of his contemporaries; but he greedily absorbed all that was discovered by others and changed it from crude and disconnected facts into an harmonious and consistent system.

All of his contributions to chemical science were of this character, even that great conception of the difference between the least distinguishable part of a certain kind of matter, and the least part which can take part in chemical reactions—the germ of the future distinction of the then unknown atom and molecule.

The last entrenchment of the phlogistonites was in the observed action of acids on certain metals whereby hydrogen (phlogiston) was produced. "If," they said, "a metal be not a combination of a calx with phlogiston, whence comes the phlogiston produced by the experiment?" The obvious answer was that it came from the acid, but what part? The acid was thought to unite with the metal and dephlogisticate it. Priestley and Cavendish had shown that hydrogen (phlogiston) and dephlogisticated air combined and in great part disappear. What became of them was not satisfactorily settled for a quite surprisingly long time. The fact that a little moisture was observed in the apparatus was probably ascribed to the sudden expansion and contraction of volume of the gases; since these matters and the hygroscopicity of the air were only beginning to be understood. When, therefore, Cavendish announced his discovery of the composition of water, Lavoisier applied it to the theory of combustion with such telling effect that it once and for all overthrew and destroyed phlogiston.

But though this was a glorious service of Lavoisier, it was far from all that chemistry owes to him. The new conceptions required a new language to express them, and Lavoisier with Guyton de Morveau established a system and a nomenclature so perfect as to form the frame work enclosing within itself all systems which followed.

A series of definitions and rules for naming new combinations as well as a partial list of elements were parts of this system.

Lavoisier came back to Robert Boyle's definition of an element as a body which cannot be decomposed into simpler ones. The table of these elements, published by the French colleagues, contains also heat and light, but this was not in conformity with the opinion of Lavoisier, who was far too careful a student of nature to commit himself to any such gratuitous assumption; but rather more probably a concession made to the defeated phlogistonists and a means of avoiding the necessity of explaining that about which the views of scientific men were very conflicting, and nothing was certainly known.

One generalization Lavoisier allowed himself, and that was that all acids contain oxygen (hence the name he gave to that element). It is curious how fate punish s such generalizations whenever they are made in the infancy of our knowledge on any subject. This generalization reflects great credit upon its proposer, and shows a rapid and profound insight into many facts, but it was not many years afterwards pronounced, and is now generally considered, a fallacy, and yet if we interpret it to mean the exhibition of acid characters such as the reddening of litmus and the effect upon the senses, Lavoisier was not entirely wrong; since these characters are inseparable from the presence of water which contains oxygen.

The long chemical war against phlogiston had been fought and won, and the thought of experimenters was turning in a new direction which was to institute a new war lasting only a little less long than the last, but the difference between the two cases was that whereas the phlogistic theory hung like a pall over the whole science, obscuring during its continuance the entire field; in this case the question in dispute was as to the ultimate constituents of matter and none of the many views entertained on these questions interfered with the classification and assimilation of the myriads of facts which experiment and research were eliciting. This war, therefore, while it will serve to illustrate that the most eminent chemists share with the rest of the world the weaknesses of our common humanity, did not materially retard the progress of the theory of chemistry.

Proust (1755–1826) announced the unchangeable proportions by weight in which substances combine together; and that if they combine in more than one proportion it is by leaps and not gradually as the water of the ocean becomes little by little more charged with salts brought down to it by the rivers. This was a great and pregnant discovery which at once led the way to the new field of battle, but the strangest thing about this announcement is that it was vehemently attacked by Berthollet (like Proust, a native of France) in a proposition which a little later seemed nothing but a stupid blunder or obstinate opposition, and yet in Berthollet's contention lay a precious truth only recently recognized and placed in its proper place.

Briefly the skirmish between these two men was this Proust discovered that the relative proportion to each other by weight of carbonic acid and copper in carbonate of copper was constant, no matter in how great excess one or the other of these bodies was present, the weight of the carbonate of copper was the same and the weight of each constitutent in it was invariable. For instance, substituting the accurate weights which better methods and apparatus have enabled chemists to obtain for the inaccurate approximations then made, in 123·4 grams of carbonate of copper, (there were always 63·4 grams of copper and 60 grams of carbonic acid. It made no difference whether these weights of the two elements respectively, or whether two or three times as much of one with the above weight of the other were made to combine: the result was always that 123·4 grams of the compound were found and the excess of either element remained uncombined.

With tin and iron there were two proportions by weight in which each of these elements combined with oxygen, but there were no insensible passages from one to the other. Thus there was a compound of oxygen and tin in which 119 weight-units of the latter combined with 16 of the former; and there was another in which 119 tin combined with 32 of oxygen but there was none in which the 119 weight-units of tin combined with any number of weight-units of oxygen between sixteen and thirty-one.

Berthollet (1748–1822) maintained, on the contrary, that if different masses of two elements are brought together, there will be found in the compound more of that constituent which was in greater quantity before the union. On account of the high position which Berthollet held in the chemical world this view received respectful, though silent attention, for few of the masters of the science were won over by it; because Richter, Klaproth, Vauquelin, and Wenzel had placed the constancy of acid and base in a compound of the two, beyond all question. Proust, however, took up the gauntlet and followed each separate publication of Berthollet by a refutation based upon careful experiment. This lasted for eight years, or from 1799 to 1807, and was settled apparently forever when Proust, by repeating some of Berthollet's own experiments on the successive stages of oxidation showed that his opponent had mistaken a percentage of water for a percentage of oxygen.

But Berthollet's main idea that the mass and the affinity were inseparable factors in the formation of a compound, after having been crushed to earth was to rise again in more recent times by the labors of his countryman of almost similar name, Berthelot and St. Gilles, and by Guldberg and Waage; but they showed, not that the proportion by weight of the compound, but that the rapidity of the reaction was affected by the masses of the constituents.

This dispute and the rise and fall of a theory was only a slight skirmish, which was preliminary to the general engagement. It had an admirable effect on the science, widened men's views, proved that the weapon of the future was to be carefully conducted experiment; and without doubt ripened the next great discovery which was then about to be announced.

John Dalton (1766–1844) was led to the happy thought of taking the data of the weights which Proust had announced as those in which tin, iron, oxygen, etc., combined, and reducing them to their simplest proportions. Proust had found that some arbitrary number of grams of tin (say, for example, 119) combined either with 16 or with 32 grams of oxygen, and with no other weights. Dalton showed that the weights of oxygen in these two compounds were to each other as 1 to 2.

In the same way the different weights of sulphur which entered into combination with a given weight of iron were to each other as 1 to 2. And he found that this held for all cases where two constituents combined with each other in more than one proportion.

Thus, if the amount of hydrogen in olefiant gas or ethylene and marsh gas or methane are compared they are to each other as 1 to 2. By numerous examinations of this kind, in all of which he found this simple relation, he was led to formulate his atomic theory, some of the more important propositions of which may be thus condensed. (1) Every element consists of similar atoms of fixed weight; (2) Chemical combinations are made by the union of the atoms in the simplest proportions. The atomic weight of a compound is equal to the sum of the atomic weights of its constituents. He supposed all the atoms to be spherical and to be surrounded by heat spheres (!)

It should be mentioned in passing that Higgins had said in 1789 that chemical smallest particles were united to form compounds in simplest proportions, but as he never adduced any proof of this, the merit of the discovery belongs to Dalton by the law of possession already alluded to before, viz: that in natural science not only must a truth be announced, but some reason for it must be given.

The immediate result of his postulates was that Dalton set out to establish a scale of atomic weights for the elements. Among minor postulates of his which have not lived till our day, but which were natural enough at a time when there were no means of obtaining certainty as to the questions of the number of atoms entering into combination, etc., was this, that if only one proportion by weight of a combination between two elements were known, it must be supposed that the number of atoms entering into this combination was one from each element. If two were known, then that in which the least weight of one combined with the least weight of the other must be considered 1 to 1; when with double this weight of the other the proportion must be 1 to 2, etc. In Dalton's time only one combination between oxygen and hydrogen was known, viz: water, and he assumed this to be composed of one atom of H to one atom of O. As H was and is yet the lightest element known he assumed its weight as one. By the imperfect methods then available, he determined the weight of O which combined with it to form water as 6·5 (in reality it is 7·98 if H = 1).

Ammonia, which was the only compound of H and N known to him, and in which he also assumed one atom of each element, gave him the number 5 for the atomic weight of N. By accurate methods it should be 4·66.) In the lowest compound of carbon and oxygen known, carbonous oxide, he found the atomic weight of carbon 5·4 calling oxygen 6·5 (the right figure is 6).

All his figures were wrong as we now believe because of his false assumption of the constitution of water (not to speak of his imperfect methods of analysis), yet the accuracy which he attained was surprising for his epoch and the invigorating effect on the science was as great as if all his numbers had been absolutely correct.

Humphry Davy (1778–1829), the great discoverer of the alkaline metals and earths, who first announced the elemental character of chlorine, and by his discovery of the halogen acids seemed to have overthrown Lavoisier's dictum regarding the invariable presence of O in all acids: Davy, the discoverer of the safety lamp for miners, first announced his belief that chemical affinity and electricity were the same force. This idea was erected by Berzelius later into the splendid structure which he called the electro-chemical theory. Neither Davy nor Wollaston believed that Dalton's experiments had succeeded in establishing the nature and characters of atoms, but contented themselves with Wollaston's theory of "equivalents," without seeking to define how much matter entered into combination.

Their theory was that the atomic weights of Dalton were merely a series of arbitrary numbers, showing the respective quantities of different elements which were equivalent to each other in combining each with a third.

Wollaston's name of "equivalents" took root later after the apparent failure of Berzelius' theory to account for all the facts, and was the shibboleth of a long period of timidity and vacillation in chemical theory, which marked the reaction of thought when it was feared that the allurements of a beautiful system and the powerful influence of a great authority had drawn the representatives of the science away from sure ground. This period of intellectual cowardice was very tantalizing and very confusing to those who pursued their studies during this period, but in the end it was an advantage to the science by letting the field lie fallow for a time, and making it thus the fitter to receive and develop the seed which finally was sown upon it.

In all cases where the development of a science has been rapid, it is found that the great minds are clustered together, and that the great discoveries occur in succession to supplement each other. It was stated that the discovery by Black originated pneumatic chemistry or the chemistry of the gases. In this field the discoveries of Cavendish, Priestley and Scheele were made, but with the wider view given by Lavoisier to the science, the study of the gases was abandoned for the study of other solid and liquid compounds. But Gay-Lussac (1778–1850) devoted himself to pneumatic chemistry and accomplished in it what supplemented the work of Dalton and prepared the way and assisted the researches of Berzelius.

In 1805, in conjunction with Alex. v. Humboldt, Gay-Lussac established the fact that exactly two volumes of H combine with one volume of O to form water.

He showed the simple relations of the volumes of combining gases to each other and to their compound: he showed the effects of temperature on gases, and how it must be considered in connection with the Boyle-Marriotte law of pressure. His conclusion was that "The specific gravities of gases are proportional to their atomic weights, or are simple multiples of them."

Avogadro, an Italian chemist and physicist, attracted by the discoveries of Gay-Lussac, had, in 1811, deduced from the Boyle-Marriotte law that in equal volumes of two gases at the same pressure and temperature must be contained an equal number of physical particles. It was such a small step from these two beautiful generalizations to the conclusion that the smallest physical parts of elementary gases not being indissoluble must contain more than one atom, and that therefore here was proven the physical smallest parts, and the still smaller chemical smallest parts which are capable of entering into combination. But though Avogadro announced this conclusion in 1811, it was long years before it was taken up and embodied in the theory of the science. Gay-Lussac furthermore, by his work on iodine and cyanogen, laid the foundation of the "radicle;" as his experiments on the action of chlorine on oils did the same for the "substitution theory." He is also the inventor of the method of volume analysis or titrimetry.

J. J. Berzelius (1779–1848) offers a life history such as few have been seen since the beginning of the world. It seemed as if the tangled skeins of nature's most intricate clues were straightened and cleared in his hand as if by magic; and the marvel of the magnificent work which he left as a monument to himself, his country and the science to whose cause he was devoted, is that hardly a line of it needs to be erased, and the corrections of his constants are decimals representing the greater accuracy of apparatus at the present day.

In 1812 he created a new mineral system, in which the combination of the elements in multiple proportion was clearly indicated. His improved methods of analysis served to enable him in 1814 to show that also in organic chemistry this law prevailed. The atomic theory was made by him the guiding principle for the science. He explained the union of elements by the polarities peculiar to their atoms and his electro-chemical theory founded upon this hypothesis brought him to the dualistic view of the combination of matter.

The reason why so little of his work needs to be changed is that he based everything upon investigation and experiment. The results which he achieved here will remain, no matter what theory may be the final outcome of further advance. He saw at once that Dalton's rule for determining the relative number of atoms in a compound was arbitrary and he pronounced it so. With the help of Gay-Lussac's discoveries of the gas volume relations and his own discovery of the oxygen law of relation between the acid and the base, he was enabled to draw correct conclusions as to at least 2,000 bodies which he had personally analysed. He considered the unit volume represented by the atom, and he deduced the constitution of bodies by weight and by volume (as, for instance, water) as we understand them to-day. It is only fair to observe that Berzelius himself, in spite of his strong belief in the power of the volume theory tó assist the investigator to a knowledge of the true atomic relations of a chemical element, recognized its limitations and rejected altogether the efforts to apply it to bodies which could not be studied in the gaseous state.

This is only just to bear in mind, because an onslaught against the splendid structure which his skilful hands had erected was caused by a mistaken notion as to Berzelius' real views of the atom volumes. This onslaught caused the paralysis of the faculty of speculation for many years among chemists, converting one of the most enchanting and exciting fields of discovery into the mechanical record of dry facts, which it was contrary to the fashion of the day to seek to unite under any common cause. Had Berzelius' teachings been properly understood and heeded, there had been no occasion for this panic, and the last days of the grand old Pioneer would not have been embittered by the thought that the labor of his life, which was good work, was doomed to destruction.

The atom values which he had obtained in 1818 are given herewith.

	Berzelius.	*Correct.*
C,	12·12	12·
O,	16·0	16
S,	32·3	32·06
Pb.	416·0	206·95
Hg,	406·0	200·00
Cu.	129·0	63·4
Fe,	109·1	56·0
Na,	93·5	23·05
Ka,	157·6	39·11
Ag,	433·7	107·92

The reason that the values he obtained for the metals were so much higher than ours now, was that he then doubted the occurrence of other oxides than MO, MO_2, MO_3, etc., M standing for the metal. Instead of FeO, Fe_2O_3, he wrote these compounds FeO_2, FeO_3, and consequently his percentage of iron was doubled. For similar reasons the metals K and Na received four times their normal weight since he regarded the compound which we know now as K_2O_4 as KO_3. Some years later he modified this position, admitting the existence of M_2O_3, and his table then conforms nearly to the present. In his further classic work of determining the atom weights, he was assisted by the beautiful discovery of Dulong and Petit in 1819, that the atoms of all elements have the same capacity for heat, or that the product of the specific heat into the atomic weight gives a constant quantity.

Mitscherlich, a student of Berzelius, discovered that compounds of different elements which were similar in the number of atoms, the equivalent of water, etc., were isomorphic in crystallization.

For example, in the phosphorus and arsenic salts only those which had like composition and like number of equivalents of water were isomorphic. Berzelius used both of these as guides but showed a preference for the dictum of his former scholar. In his later table of atomic weights of 1826, Berzelius corrected all but the alkali metals which he still considered united to oxygen in the proportion of 1 to 1.

It will be recalled that Sir Humphry Davy and his school had pronounced chemical inseparable from electrical activity. He had shown that when a compound was decomposed by the electric current and the separate constituents collected at the opposite poles (as for example sulphur and copper) these substances when rubbed or electrically excited showed opposite kinds of electricity; the substance at the negative pole showing positive, and that at the positive pole negative electricity. If the separate constituents (such again as sulphur and copper) were heated, the heat increased their electrical tension up to the point where they combined, and then all electrical activity ceased, the opposite kinds neutralizing each other.

In conducting the current into the compound its constituents received again electric polarity and separated to the respective poles which attracted them.

Berzelius first stated his electro-chemical theory in 1812. His fundamental conception was that the atoms of bodies are electric and therefore have at least two poles which generally are different in strength, and following this difference the elements which these atoms compose are electro-positive or electro-negative. This predominance of one kind of electricity extended to compounds, although necessarily more feeble in these. Compounds, according to this view, are the results of the attraction of the unlike poles of the atoms; and if in the compound there is a preponderance of one kind of electricity, this is because the atoms having this kind were more strongly polar than those having the other. Oxygen as the most electro-negative of all bodies was his criterion of the characters of the elements with which it combined. If the compound containing the least oxygen was basic then the body combining with oxygen was electro-positive. If the oxide were an acid then the element was electro-negative.

On this plan he arranged a table of elements in the order of their polarity. Of course, it followed that any body in this table was electro-positive to all the elements on one side of it and electro-negative to all those on the other.

His dualistic scheme followed as a matter of necessity, the two parts (whether two elements or acid and base) having opposite polarities. Thus:

$\overset{+}{\mathrm{Fe}}\ \overset{-}{\mathrm{O}}$	$\overset{+}{\mathrm{Ba}}\ \overset{-}{\mathrm{O}}$	$\overset{+}{\mathrm{S}}\ \overset{-}{\mathrm{O_3}}$	$\overset{+}{\mathrm{C}}\ \overset{-}{\mathrm{O_2}}$
Ferrous oxide.	Barium oxide	Sulphuric acid.	Carbonic acid.

or for salts.

$\overset{+}{\mathrm{Ba\,O}}\ \overset{-}{\mathrm{S\,O_3}}$	$\overset{+}{\mathrm{Fe\,O.}}\ \overset{-}{\mathrm{C\,O_2}}$
Baryta Sulphate.	Ferrous oxide Carbonate.

and in double salts.

$\overset{+}{\mathrm{KO.\ SO_3}}$	$\overset{-}{\mathrm{Al_2\,O_3.\ SO_3}}$
Potash Sulphate.	Alumina Sulphate.

Potasso aluminum Sulphate.

Berzelius carried his dualism and his electro-chemical theory into organic chemistry. In 1813 he had discovered a simple relation between the elements composing the organic acid and the oxygen of the base, and had proved the combination in multiple proportions in organic chemistry, and it was he who recommended the study of organic bodies in their combinations with inorganic.

He supposed organic, like inorganic, bodies to be binary in constitution, but with compound radicles instead of simple elements. This was Lavoisier's idea. Gay-Lussac had characterized alcohol as consisting of ethylene and water; sugar as carbon and water. Döbereiner called oxalic acid a compound of carbonic acid and carbonic oxide. Berzelius opposed this conception as inconsistent with his electro-chemical theory. He could not acknowledge the existence of oxygen radicles. Yet the probability of such radicles was rendered strong by the discovery of organic bodies containing the same numbers of the same elements, yet exhibiting very different properties. This could only be explained by supposing that the proximate constituents were different.

Wöhler, Liebig and Berzelius, after much hesitation, accepted the fact above referred to and the latter designated the phenomenon as one of isomerism. Liebig and Wöhler had shown that a constituent of the oil of bitter almonds remained unchanged throughout a number of reactions, and this compound, of which the composition was $C_{14}H_{10}O_2$, they called benzoyl. (It is now called dibenzoyl or benzil.)

At first Berzelius was disposed to accept this as an organic compound radicle, but reflecting that it must play the electro-positive *rôle*, although containing oxygen, he finally rejected this hypothesis which he deemed inconsistent with his electro-chemical and dualistic theory; and he was led to the assumption of arbitrary radicles containing no oxygen, of which the formulas, when written together, completely obscured the intimate relationships which existed between classes of salts. This was Berzelius' first fault, not so much due to his vanity, as to the feeling which was well founded that the scheme he had with such infinite pains established was right; that it was being destroyed on theoretical grounds which, although he could not then satisfactorily answer them, with the instinct of a great genius, he felt to be wrong. But his attempts to evade the conclusions only led him into self-contradictions which, when exposed, produced the same effect upon the chemical mind that the reported insolvency of a great banking house produces on the financial world. The failure of a Berzelius shut up the current coin of theorizing the world over.

Berzelius believed that the radicles were unchangeable. Liebig and Dumas were not convinced of their entire unchangeability. The two sets of views separated more and more. Liebig finally defined a radicle as one which must be—(1) an unchangeable unit in a number of com pounds. (2) It must be replaceable in these by simple bodies. (3) It must allow the simple bodies with which it is combined to be replaced by others, to form with it new compounds. This was the old radicle theory.

In 1827, J. B. Dumas (1800–1884) commenced a series of researches on the vapor densities of many substances which are solid or liquid at ordinary temperatures, and showed that if Gay-Lussac's law of the identity of atom-weight with gas-volume-weight be true, then some of Berzelius' atom-weights were double and some were half what they should be. This cast doubt on the truth of the law as well as on Berzelius' work, but the latter held fast to his numbers (which were right), and simply confined his employment of the volume theory to permanent gases. The effect upon the minds of chemists, however, was disastrous; for even Gay-Lussac, Liebig and others, despaired of getting any information as to the atoms, and fell back on the equivalents which were shortly afterwards more precisely defined by Faraday as the contemporaneous quantities of the various constituents of compounds which an electric current of given intensity would disengage. This he called the law of constant electrolytic action.

Under the unfortunate fear of having been misled, all attempts at theory were suspended. Gmelin in the colossal work which bears his name went back to the apparent weights of combination of Lavoisier's time, and the soul was taken out of the science. But Dumas dealt Berzelius a heavier blow still in the field of organic chemistry in his substitution theory, which he called "metalepsie," of which the two propositions were:

(1) If a hydrogen compound is subjected to the action of chlorine, iodine or bromine, it takes for every atom of hydrogen lost an atom of one of these elements to replace it.

(2) If the body contain water, it loses this without replacement.

Auguste Laurent (1807–1853) went further and asserted that the congeries of atoms called radicles which had suffered this substitution of H by Cl, or the loss of water remained practically the same in properties. This was called the nucleal theory which was short-lived, but before it had quite disappeared from the field Dumas came forward with his discovery of chloracetic acid, and declared himself for Laurent.

He maintained that the newly-formed body in such substitutions must resemble the old and he classed such original and substituted products together as derivable from the same type. This is called the old-type theory to distinguish it from the new-type theory subsequently set up by Laurent and Gerhard. Dumas showed that his trichloracetic acid in spite of the substitution of six atoms of chlorine for six atoms of hydrogen was similar in its behavior and characteristics. He stated that "In organic chemistry there are certain types which remain constant even when one substitutes an equal volume of Cl, I, and Br for their H." Acetic and trichloracetic acid, aldehyde and chloral; marsh gas and chloroform, belong to the same chemical type. He went so far as to assert that all bodies derived from each other by equivalent substitutions belonged to the same "mechanical type." This was followed by the following declaration of war against Berzelius' dualistic theory. "Every chemical compound forms a complete whole, and does not consist of two parts. Its chemical character depends principally upon the number and arrangement of its atoms, and secondarily upon their chemical nature."

Berzelius was obliged to account for the facts discovered and to conform his explanation to the new discovery of Melsens in 1842, that chloracetic acid was reduced to acetic acid by potassium amalgam. He finally decided to do this by supposing that

$$C_2\,(H_2)_3 + C_2O_3 . H_2O = \text{Acetic acid.}$$
$$C_2\,(Cl_2)_3 + C_2O_3 . H_2O = \text{Chloracetic acid.}$$

In other words that acetic acid consisted of a radicle (C_2H_6), and trichloracetic acid of the corresponding radicle (C_2Cl_6), each respectively paired with oxalic acid ($C_2O_3 \cdot H_2O$).

But in doing this he gave up the very principle that he had been contending for, viz: that the radicles were unalterable: since it was evident that in the first two symbols of the formulas a radicle C_2H_6 had been changed by substitution into C_2Cl_6.

This admission weakened the faith of his most admiring followers and threw the blight of timidity over the speculations of chemists for thirty years. And yet withal, Berzelius was right in almost every important generalization which he made, and right (within the resources of his time) in every constant he established. Chemistry without his contributions would yet be a mere scaffolding.

The history of this controversy, the most important which has taken place since the commencement of the modern science, is useful as an illustration of another kind of hindrance to the progress of intelligent theory. It is a too blind devotion to one man, and despair of attaining the object which he strove for if he has failed. It is true that if ever hero worship were justifiable that of Berzelius was, but to sweep aside the whole fabric of his labors, because part of his theory was proved untenable, was the veriest cowardice and injustice to him.

How many a chemist in the last forty-five years has struggled through a jungle thick planted with the pretty crochets of mediocre men, wondering what all this jargon meant and whither it was tending; deprived of the vivifying influence of a high and noble thought, and condemned to delve and grub without reasoning, because one of the greatest geniuses of original research the world has ever seen was worsted in an encounter on a small part of his field, and a part to which he had not devoted the best of his thought!

Among the most innocent looking hindrances to the development of sound chemical theory must be reckoned an hypothesis, which was given anonymously to the world in 1815, and which was later referred to its true author, Dr. Prout. This was the supposition that the atomic weights of all elements were even multiples of that of hydrogen.*

* It should be stated that Profs. Crookes, F. W. Clarke and other chemists of the first rank still show a leaning towards the acceptance of something like Prout's "law."

No argument was offered to show why this should be true, but it had the specious and enticing appearance which captivates that sense of order which is a human instinct.

Prout did not hesitate to add the obvious conclusion that hydrogen was the original material, or as one of the greatest of living chemists has put it, the protyle.

By the usual rule which exists in such cases Prout should not have had credit for anything more than a pretty idea which besides was only a modification of a similar thought of Thomson the collaborator of Dalton, who believed that the atomic weights of the elements were multiples of that of oxygen. Prout was more nearly right, of course, because the weight of hydrogen is only about one-sixteenth as great as that of oxygen, and is a more general divisor. But in spite of all, this pretty thought strongly influenced many admirable chemists, even those like Dumas and Stas and Marchand and Erdmann, whose labors in accurately determining atomic weights did more than all else to prove the baselessness of Prout's assumption.

Even to-day the tendency is manifested to get back to the even numbers which Leopold Gmelin adopted in his great work, in entire disregard of the accurate results of Berzelius. It must be added that some are prevented from falling into this snare through the hope that the real protyle will be some day discovered, and that being lighter than hydrogen it will provide a table of atomic weights less disfigured by fractions than the present one.

The moral of this theory is that healthy scientific work must not be influenced by premature attempts to put a finish on it and make it appear symmetrical.

The discovery of an element lighter than hydrogen and possessing properties which, as it will be seen, we can to a certain degree predict, might render very desirable those ugly decimal affixes to the present atomic weights in order to show that experiment supported theory. On the whole Prout's "law" as it is called is a hindrance and quite an annoying one.

The downfall of dualism and the establishment of unitism in compounds was followed by a storm of conflicting notions of chemical union into all of which it is not necessary to enter.

To Laurent and Gerhardt is due the praise for unifying in the new theory of types, the opinions which had been previously thought to be diametrically opposite. It has been said that the uncertainty which prevailed upon the overthrow of the dualistic theory of Berzelius caused most chemists to go back to the old equivalent weights O = 8, but Gerhardt opposed this weakness and strongly maintained the correctness of the numbers of Berzelius, merely correcting his early atomic weights for the alkaline metals in conformity with the then acquired knowledge of the existence of two atoms of the metal in the alkalies. He showed that the quantities of H_2O,CO_2 etc., separated in the reactions of organic compounds did not represent one equivalent each, but two. His aim was to reduce all formulas to one criterion which in the case of gaseous substances was two volumes. His view was that, when two bodies react, one constituent of one (H), uniting with one constituent of the other (O), leaves two residues which unite. Hofmann discovered the substitution products of ammonia and thus established the ammonia type. Williamson established the water type and referred alcohols, ethers, acids, bases and salts to this type. Gerhardt recognized the hydrogen type, the water type, and the ammonia type, but considered the compounds under them units and not couples.* Laurent explained the atom as the least part of matter that can take part in chemical combination; the molecule as the least part which can exist alone, and the equivalent of elements as those quantities which would perform the same amount of chemical work.

Thus the new-type theory was a compromise skilfully

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* Dr. Wolcott Gibbs, in 1858, after referring to his attribution of the theory of water types to Gerhardt and Williamson, says: "I have not done justice to T. Sterry Hunt, to whom is exclusively due the credit of having first applied the theory to the so-called oxygen acids and the anhydrides; and in whose earlier papers may be found the germs of most of the ideas on classification usually attributed to Gerhardt and his disciples."—*Chem. and Geol. Essays*, p. 468.

constructed to save the Berzelians from chagrin while embodying all of value in their system. It took the idea of type from Dumas, that of radicle from Berzelius, but with the difference that it did not presuppose the groups of atoms called radicles to be necessarily capable of existing alone. Gerhardt admitted that the elements substituted in radicles did change the character of the compound.

Kekulé, in the year of Gerhardt's death (1857), added the conception of mixed types.

It is unnecessary to pursue this branch of the theory farther. An entirely new discovery appeared which was a welcome light and enabled many of the obstacles in the way of progress to be seen and surmounted.

Frankland, after an investigation of the alcohol radicles, first announced his views of valence in 1853.

Kolbe, in 1855, accepted his theory and declared that the fatty acids, if imagined free of water, were derivatives of CO_2 or C_2O_4 in which one atom of oxygen is replaced by C_2H_3.

Frankland's discovery was really a corollary of multiple proportions, and had been vaguely foreshadowed by Wöhler, who said that one atom of antimony was equal to three of hydrogen.*

It is simply, as Hofmann expresses it, chemical value in exchange. Some elements, of which H is a type, can only combine one atom to one atom; some, like oxygen, can hold two atoms of the first-named kind in union. Some, like nitrogen, can hold three, and some, like carbon, four. Kekulé, in 1858, determined this valence of carbon, added another to the list of types which led to the establishment of the ring structure of some organic compounds.

Elenmeyer found that every element had a highest valence but might not use all of its combining powers. Wurts and Naquet believed the valence changeable, Kekulé believed it fixed, and that only compounds using all the

* Von Meyer's admirable and impartial work on the history of chemistry (Leipzig 1889), a most valuable contribution to the science and a fitting supplement to Kopp's, has been largely drawn upon.

valences of every constituent were real chemical compounds. It appears to be the general opinion nowadays that the valence of elements is not only variable but that it does not even vary as was supposed by the suppression of two affinities at once (which might be explained as the loss or suppression of two opposite polarities); but that an element may be now an artiad and now a perissad, *i. e.*, even or odd in the number of its bonds.

The subject is not well understood, but it presents a good opportunity for acting on the lesson taught by Prout's "law," not to be captivated by a beautiful idea, but to stick closely to facts.

It would be impossible at this time to follow all the subsequent contributions to the theory of chemistry, but the most superficial sketch of the subject would be incomplete without studying an extraordinary discovery of which the advent had been preparing long before its announcement by Lothar Meyer and D. Mendelejeff. The latter pointed out that if the atomic weights of the light elements (or those with atomic weights from one to thirty-six) be arranged in two lines of seven each, a natural grouping is effected thus:

Li–7.	Be–9·4.	B–11.	C–12.	N–14.	O–16.	F–19
Na–23.	Mg–24.	Al–27·3.	Si–28.	P–31.	S–32.	Cl–35·5.

In these two lines two periods are passed over. From left to right in each, with the increase of atomic weight is a change from the most electro-positive to the most electro-negative, while the elements in the centre are nearly neutral. Again from the beginning of the second period, elements of the same kind come to stand under one another. The same might be shown to be true in their behavior in forming compounds with oxygen. Again the metals are on the left and the non-metals on the right. The specific gravities increase, and the atom-volumes (or the atomic weights divided by the specific gravities), and all other physical and chemical properties which have been examined, change by regular gradations in vertical or horizontal lines. Without pursuing this subject into its curious details it is apparent that there has been some law exemplified in the production of these elements; a condition of things which is cyclical in its action or, as it is called, periodic. From the data obtained it was possible to construct a table which while containing all the known elements, contained gaps in certain places where (according to the analogy of the periods) elements ought to have been but none had been discovered. From the position of these gaps it was even possible to prophesy from the analogy of elements in similar positions of other periods, that if discovered the new elements filling these gaps would have properties intermediate between those occupying places before and after it, in regard to fusing point, solubility, specific heat, positive or negative polarity, etc.

After the announcement of this periodic law, chemists were impatient for a verification of its accuracy, and in the discovery of the newest metals, Gallium and Germanium, those predictions were found to have been well founded.

As no better test of a theory can be had than its use as a basis of prediction, the periodic law may be said to have been proved, and to be evidence of a profound and intimate connection between the elements.

What the connection is has not been certainly ascertained, but a bold and beautiful hypothesis was enunciated in 1886, by one of the greatest of the master minds of our age, in his presidential address before the Chemical Section of the British Association for the Advancement of Science, William Crookes.*

A skeleton of the views in this paper is as follows:

Norman Lockyer said that a terrestrial element is an exceedingly complex thing, broken up into simpler things at the temperature of the sun.

Sir Benjamin Brodie in 1867 said, "We may conceive that in a remote time or in remote space there existed formerly or may exist now certain simpler forms of matter than we find on the surface of our globe."

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* See address of William Crookes, President of the Chemical Section Brit. Ass'n for the Adv. of Science, at the Birmingham meeting, 1886. (R'pt B. A. A. S., 1886, p. 558.)

Professor Stokes, in referring to a line in the spectrum of the nebulæ remarks: "It may possibly indicate some form of matter more elementary than any we know on earth."

Crookes holds that the unequal distribution of elements in the earth's crust is evidence in favor of their composite nature.

The probability of such rare metals as Yttrium, Ytterbium, etc., having been brought together by chance in a few uncommon minerals and in a few localities, is very small. They would rather seem to have been formed severally from some common material placed in conditions in each case nearly identical; and the existence of other groups of metals, as Nickel, Cobalt, and the two Platinum groups furnishes additional ground for this supposition. Another argument in favor of their composite nature is that of the organic compound radicles.

Dr. Carnelly at the previous meeting (1885) had shown that on the supposition of two chemical elements; one with an atomic weight of twelve and the other of two—all the features of peroidicity in Mendelejeff's series could be produced by their combination, and every well-known element reproduced except hydrogen.

Dr. E. J. Mills considered the elements we now have as the result of successive polymerizations.

All these observations Crookes has thrown into a diagram by which is represented the hypothetical condensation of the original "fire mist" out of which the universe was derived and which contained, not matter but the potentiality of matter. In this condensation due to the gradual lowering of the temperature, another force than that of heat was at work—a force which he supposed not very different from electricity. As the temperature was lowered the elements congealed one by one out of the protyle in the form of atoms, of which the weight depended upon the temperature at the time of this formation, and the properties due to the electrical phase at the time of their birth. But the potential energy of the atom was greatest in the first that were thus condensed and more sluggish in the last when the temperature was lowest and the electrical force least.

We must be warned by the very beauty of this conception that it is but the effort of a superior mind to materialize for us the successive phenomena which it sees in imagination, and not a register of observed phenomena or a proven genesis. Yet it is impossible to conceive of the regularity and accuracy with which the labor of a century is fitted into that scheme without feeling that it contains more than a figment of the fancy.

At least nothing greater and nobler has been attempted in our science since its inception, and whether it be finally assigned its place among the great theories of the world or not, it will, undeniably, through its broadening of our views and its enlarging of our conception, have been a help to the progress of chemical theory.

In reference to the supposed steam engine prediction, mentioned at the beginning of this lecture, Mr. W. P. Tatham calls my attention to the fact that the actual observation misquoted by the N. Y. *Herald* for sensational or other satisfactory journalistic reasons, and repeated ever since (in spite of countless corrections), after the manner of the average erroneous newspaper paragraph, was made by Dr. Dionysius Lardner and referred to the steamer of that epoch, which, according to his calculation, could only carry coals for a journey of 2,000 miles with due allowance for accident and delay. That he never entertained such an opinion as that above referred to is evident from the following language: "We are even now upon the brink of such improvements as will probably so extend the powers of the steam engine as to render it available as the means of connecting the most distant parts of the earth."

The steam engine familiarly explained and illustrated, etc., by the Rev. Dionysius Lardner, LL.D., F.R.S., pp. 241–242, etc., etc. E. L. Carey and A. Hart, Philadelphia, 1841.