

MENDELÉEFF MEMORIAL LECTURE.

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To many of the present generation of English chemists, the commanding, patriarchal figure of Mendeléeff was quite familiar. Though his several visits to London were often connected with official business of the Russian Government Department of Weights and Measures, of which he was the chief official during the later years of his life, he came several times with more purely scientific objects. In 1889 the occasion of his presence in London was the Faraday Lecture which he had been invited to give to the Chemical Society, but which, owing to a sudden and urgent recall to his home, he was unable to deliver in person. His last appearance in this country was in November, 1905, when the Copley Medal was awarded to him by the Royal Society.

The Chemical Society can see his face no more, and all that it can now do is to inscribe high on its roll of honour the name which, more than any other, will be for ever associated with the development of the great generalisation known as the periodic system of the elements.

Dmitri Ivanovitsch Mendeléeff * was the fourteenth and youngest child of his parents, Ivan Pavlovitsch and Maria Dmitrievna, *née* Kornileff. His father, a former student of the Chief Pedagogic Institute of St. Petersburg, obtained the appointment of Director of the Gymnasium at Tobolsk, in Siberia, where he met Maria Dmitrievna, who became his wife. After a few years at Tobolsk, he was transferred to school directorships in Russia, first at Tambov, and afterwards at Saratov. But in order to satisfy the ardent wish of his wife, he took advantage of an opportunity of exchange, by which he became once more Director of the College at Tobolsk, and the family returned to Siberia. Here on January 27th, 1834 (O.S.) was born Dmitri Ivanovitsch, the youngest son.

* For many of the details of Mendeléeff's career and of his home life the writer is indebted to the family chronicle compiled, soon after his death, by his niece, N. J. Gubkina (*née* Kapustina), and published in St. Petersburg, also to pamphlets by A. Archangelsky and P. J. Robinowitsch. He also desires to express his thanks to Mr. D. V. Jéquier, of St. Petersburg, as well as to several Russian friends, for valuable assistance in translation.



ФОТОГРАФИЯ А. Ф. ДРЕССЛЕРА, СПБ.

A. Mandelstam

Soon after his birth the father became gradually blind from cataract in both eyes, and was obliged to resign, the whole family, including eight children, having to subsist on a small pension of 1000 roubles (about £100 per annum). The mother, Maria Dmitrievna, belonged to the old Russian family, Kornileff, settled at Tobolsk. They were the first to establish in Siberia the manufacture of paper and glass. In 1787 the grandfather of Dmitri opened at Tobolsk the first printing press, and from 1789 produced the first newspaper in Siberia, the *Irtysch*. The glass works were situated in the village of Aremziansk, a short distance from Tobolsk.

According to the family tradition, one of the Kornileffs in a previous generation had married a Khirgis Tartar beauty, whom he loved so passionately that when she died he also died of grief. The pure Russian blood thus received a strain of the Mongolian race, and some of their descendants preserved traces of the Oriental type. This, however, was not very noticeable in the features of the chemist.

From her childhood, Maria Dmitrievna was distinguished by her intelligent wish for instruction, and having no other resource when her brother Basile went to school she repeated by herself all his lessons, and thus, unaided, obtained some part of the knowledge so eagerly desired. There can be no doubt she was a woman possessed of remarkable vigour of mind, who exercised great influence over her children. Her activity and capacity are further illustrated by the fact that when her husband became blind she revived the business of the glass works, and carried it on till after his death from consumption in 1847.

Tobolsk was at that time a place of banishment for many political exiles, the so-called Decembrists, one of whom, Bassargin, married Olga, an elder sister of Dmitri. To these Decembrists the boy owed his first interest in natural science. His mother had always cherished the hope that at least one of her children would devote himself to science, and accordingly, after her husband's death and the destruction of the glass works by fire, and spite of failing health and scanty means, she undertook the long and tedious journey from Tobolsk to Moscow, accompanied by her remaining children, Elizabeth and Dmitri Ivanovitsch, with the object of entering the latter, then nearly fifteen years of age, at the University. Disappointed in this object, owing to official difficulties, she removed in the spring of 1850 to St. Petersburg, where ultimately, with the assistance of the Director, Pletnoff, of the Central Pedagogic Institute, a friend of her late husband, she succeeded in securing for her son admission to the Physico-

Mathematical Faculty of the Institute, together with much-needed pecuniary assistance from the Government.

The debt which Dmitri Ivanovitsch owed to his mother he acknowledged later in the introduction to his work on "Solutions," which he dedicated to her memory in the following interesting lines:

"This investigation is dedicated to the memory of a mother by her youngest offspring. Conducting a factory, she could educate him only by her own work. She instructed by example, corrected with love, and in order to devote him to science she left Siberia with him, spending thus her last resources and strength. When dying, she said, 'Refrain from illusions, insist on work, and not on words. Patiently search divine and scientific truth.' She understood how often dialectical methods deceive, how much there is still to be learned, and how, with the aid of science without violence, with love but firmness, all superstition, untruth, and error are removed, bringing in their stead the safety of discovered truth, freedom for further development, general welfare, and inward happiness. Dmitri Mendeléeff regards as sacred a mother's dying words. October, 1887."

In the Pedagogic Institute Dmitri Ivanovitsch was thus able to devote himself to the mathematical and physical sciences under the guidance of Professors Leng and Kupfer in physics, Woskresensky in chemistry, and Ostragradsky in mathematics. Unfortunately, at the end of his course, his health failed, and about this time his mother died. Having been ordered to the South, he fortunately obtained an appointment as chief science master at Simferopol, in the Crimea. The southern climate soon alleviated the serious symptoms of lung disorder, and removal being necessary in consequence of the Crimean War, he was able soon afterwards to undertake a post as teacher of mathematics and physics at the Gymnasium at Odessa. In 1856 he returned to St. Petersburg, and at the early age of twenty-two he was appointed *privat docent* in the University, having secured his certificate as master in chemistry.

At this time he appears to have passed rapidly from one subject to another, but he soon found matter for serious and protracted study in the physical properties of liquids, especially in their expansion by heat. And when, in 1859, by permission of the Minister of Public Instruction Mendeléeff proceeded to study under Regnault in Paris and afterwards in Heidelberg, he devoted himself to this work, communicating his results to Liebig's *Annalen* and the French Academy of Sciences. Returning two years later to St. Petersburg, he secured his Doctorate, and was soon after-

wards appointed Professor of Chemistry in the Technological Institute. In 1866 he became Professor of General Chemistry in the University, Butlerow at the same time occupying the Chair of Organic Chemistry.

As a teacher, Mendeléeff seems to have possessed a special talent for rousing a desire for knowledge, and his lecture room was often filled with students from all faculties of the University. Many of his former students remember gratefully the influence he exercised over them.* One of these writes: "I was a student in the Technological Institute from 1867 to 1869. Mendeléeff was our professor, and in 1868 taught organic chemistry. The previous course by the professor of inorganic chemistry consisted of a collection of recipes, very hard to remember, but, thanks to Mendeléeff, I began to perceive that chemistry was really a science. The most remarkable thing at his lectures was that the mind of his audience worked with his, foreseeing the conclusions he might arrive at, and feeling happy when he did reach these conclusions. More than once he said, 'I do not wish to cram you with facts, but I want you to be able to read chemical treatises and other literature, to be able to analyse them, and, in fact, to understand chemistry. And you should remember that hypotheses are not theories. By a theory I mean a conclusion drawn from the accumulated facts we now possess which enables us to foresee new facts which we do not yet know.' He was considered among the students a most liberal man, and they thought of him as a comrade. More than once during a disturbance between the students and the administration Mendeléeff supported the students, and under his influence many matters were put right." (L. G.) Another former student in the University writes as follows: "I am sorry to say I did not know Mendeléeff personally. I only had the good fortune to follow, in the years 1867-69, his lectures on both Organic and Inorganic Chemistry. The former was an abridged course, which he had the admirable idea to deliver for us students of the mathematical branch of the physico-mathematical faculty. He reduced this course of one lecture a week during one year to a general review of organic compounds and the general laws of their structure. You can imagine what it must have been in the hands of Mendeléeff, thirty-three or thirty-four years old at that time, in the full enjoyment of his mental powers, and just then plunged into the study of his great generalisations. For me it was a revelation, being occupied with the great questions connected with the development of the new system of atomic weights,

* For the following reminiscences, the writer is indebted to Mr. L. Goldenberg and Prince P. Kropotkin respectively.

the mechanical theory of heat, etc. Grove's, Thomson's, Joule's, Séguin's works were then just out, and in these years a sudden blossoming of the natural sciences in all directions seemed to bring us near to the solution of the great problems of the nature of matter and of gravitation. Then I followed Mendeléeff's lectures on Inorganic Chemistry. The 'Principles of Chemistry' was not yet out, but he was evidently writing it at that time. You know how much is said in the footnotes to his 'Principles'; well, imagine each of these notes developed into a beautiful improvisation, with all the freshness of thought of a man who, while he speaks, evolves all the arguments for and against, there on the spot. The hall was always crowded with something like two hundred students, many of whom, I am afraid, could not follow Mendeléeff, but for the few of us who could it was a stimulant to the intellect and a lesson in scientific thinking which must have left deep traces in their development, as it did in mine." (P. K.)

One of Mendeléeff's most remarkable personal features was his flowing abundance of hair. The story goes that, before he was presented to the late Emperor, Alexander III., his Majesty was curious to know whether the professor would have his hair cut. This, however, was not done, and he appeared at Court without passing under the hands of the barber. His habit was to cut his hair once a year in spring, before the warm weather set in. His eyes, though rather deep set, were bright blue, and to the end of his life retained their penetrating glance. Tall in stature, though with slightly stooping shoulders, his hands noticeable for their fine form and expressive gestures, the whole figure proclaimed the grand Russian of the province of Tver.

At home, Mendeléeff always wore an easy garment of his own design, something like a Norfolk jacket without a belt, of dark grey cloth. He rarely wore uniform or evening coat, and attached no importance to ribbons and decorations, of which he had many.

As to his views on social and political questions, many people thought him a rigid monarchist, but he said of himself that he was an evolutionist of peaceable type, desiring a new religion, of which the characteristic should be subordination of the individual to the general good. He always viewed with much sympathy what is called the feminine question. At the Office of Weights and Measures, he employed several ladies, and about 1870 he gave lectures on chemistry to classes of ladies. Nevertheless he considered women inferior to men both in business and in intellectual pursuits, and he thought the chief promoters of the feminine movement aimed, not so much at equality of political position, as at opportunities for work and to escape inactivity. But he thought

the feminine temperament specially suited to all branches of art in the broadest sense of the word, including education.

Mendeléeff held decided views on the subject of education, which he set forth in several publications, especially "Remarks on Public Instruction in Russia" (1901). Here he says, "The fundamental direction of Russian education should be living and real, not based on dead languages, grammatical rules, and dialectical discussions, which, without experimental control, bring self-deceit, illusion, presumption, and selfishness." Believing in the soothing effect of a vital realism in schools, he considered that universal peace and the brotherhood of nations could only be brought about by the operation of this principle. Speaking of the reforms desirable, he says that "for such reforms are required many strong realists; classicists are only fit to be landowners, capitalists, civil servants, men of letters critics, describing and discussing, but helping only indirectly the cause of popular needs. We could live at the present day without a Plato, but a double number of Newtons is required to discover the secrets of nature, and to bring life into harmony with the laws of nature." Mendeléeff was evidently a philosopher of the same type as our own Francis Bacon.

"I am not afraid," he says later, "of the admission of foreign, even of socialistic, ideas into Russia, because I have faith in the Russian people, who have already got rid of the Tartar domination and the feudal system."

Mendeléeff always dined at six o'clock, and liked to entertain his friends and relations, but in his own diet he was extremely moderate. After dinner he enjoyed reading light literature, especially books of adventure, such as those of Fenimore Cooper or Jules Verne. But his literary tastes were peculiar. Though interested in serious literature and appreciating Shakespeare, Schiller, Goethe, Victor Hugo, and Byron as well as the Russian classics, beginning with Zhoucovsky and Pouschkin, his favourites among Russian poets were Maïcoff and Tutcheff, and among the rest Byron. Of the last-named he preferred to all his other works the gloomy poem called "Darkness," and among the rest the "Silentium" of Tutcheff.

He rarely went to the theatre, and did not approve of frequent visits to the theatre by his children, as he considered such distractions tend to destroy concentration and fill the mind with "trifles and foolishness." On the other hand, he was very fond of pictures, and he visited all the exhibitions. That he was interested in questions relating to art, and had given much thought to æsthetic problems, is indicated by a letter* which he addressed in November, 1880, to the well-known Russian daily paper of that time,

* Considerably condensed in the following abstract.

Goloss (*The Voice*), on the subject of a picture by Kouindji, "Night in the Ukraine." Writing of the influence of landscape on different minds, he says, "At first it seemed to me a matter of personal taste, of individual sensitiveness of different persons to the beauty of nature." But, rejecting this simple view, he was led to a conception which he regarded as really satisfactory, and which he wished to share with others. He says, "Landscape was depicted in antiquity, but was not in favour in those times. Even the great masters of the sixteenth century made use of it merely as a frame to their pictures. It was the human form which principally inspired artists of that epoch; even the gods and the Almighty Himself appeared to their minds in human shape. In this alone they found the infinite, the inspiring, the divine. And this was because they worshipped human mind and human spirit. This found expression in science in an exceptional development of mathematics, logic, metaphysics, and politics. Later, however, men lost faith in the absolute and original power of human reason, and they discovered that the study of external nature assists even in the correct appreciation of the nature of the human inner self. Thus nature became an object of study; a natural science arose unknown either to antiquity or to the period of the Renaissance. Observation and experience, inductive reasoning, submission to the inevitable, soon gave rise to a new and more powerful, more productive method of seeking truth. It thus became evident that human nature, including its consciousness and reason, is merely a part of the whole, which is easier to comprehend as such from the study of external nature than of the inner man. External nature thus ceased to be merely subservient to man, and became his equal, his friend. Dead and senseless as it had been, it now became alive. Everywhere it presented motion, stores of energy, natural reason, simplicity, and plan. Inductive and experimental science became a crown of knowledge, royal metaphysics and mathematics had now to be content with modest questioning of nature. Landscape painting was born simultaneously with this change, or perhaps a little earlier. Thus it will probably come to pass that our age will hereafter be known as the epoch of natural science in philosophy, and of landscape in art. Both derive their materials from sources external to man. . . . Man has, however, not been lost sight of as an object of study and of artistic creation, but he now appears, not as a potentate or as a microcosm, but merely as part of a complex whole."

In 1863, when twenty-nine years of age, Mendeléeff married his first wife, *née* Lestshoff, by whom he had one son, Vladimir,* and a daughter, Olga; but the marriage proved unhappy, and after

* Died in 1899, aged 34.

living apart for some time there was a divorce. In 1877 he fell in love with a young lady artist, Anna Ivanovna Popova, of Cossack origin, and in 1881 they were married. This lady exercised considerable influence over his views about art, and the walls of his study were furnished with many products of her pencil, notably portraits of Lavoisier, Descartes, Newton, Galileo, Copernicus, Graham, Mitscherlich, Rose, Chevreul, Faraday, Berthelot, and Dumas, and others of relatives. After his second marriage, Mendeléeff lived first at the University, and afterwards in the apartments built for the Director at the Bureau of Weights and Measures, and here his younger children were born, Lioubov (Aimée), Ivan (Jean), and the twins, Maria and Vassili (Basile).

In 1890, in consequence of a difference with the administration, Mendeléeff retired from the Professorship in the University. During the disturbances among the students in that year, he succeeded in pacifying them by promising to present their petition to the Minister of Education. Instead of thanks for this service, however, the Professor received a sharp reprimand from the authorities for not minding his own business. The consequence was that Mendeléeff resigned. Independently of the petition, however, there were probably deeper reasons for his being out of favour with the Ministry, connected with his irreconcilable enmity to the classical system of education already referred to (p. 2082). Of this he had made no secret, and it had already brought him into conflict with the authorities. In 1893, however, he was appointed by M. Witte to the office of Director of the Bureau of Weights and Measures, which he retained till his death.

In the earlier part of his life, Mendeléeff was interested in carrying on a series of agricultural experiments on his Tver estate, Boblovo. The peasants, much struck by his success and the abundance of his crops, inquired of him whether this was due to his luck or to his "talent." With a smile and the patois which he always affected in speaking to the country people, he informed them that he certainly had "talent," and, as he said afterwards at home, there is no merit in having luck.

Once during the solar eclipse in 1887 he ascended alone in a balloon with the object of making scientific observations. His assistant, Kovanko, who sat with him in the basket, alighted at the last moment, probably ordered to do so by his chief because the balloon would not rise. When the balloon shot up quickly and disappeared in the clouds, his family was naturally very much alarmed. Fortunately the hero of the adventure was able to descend safely, and a few hours later returned to his family from Moscow. The peasant women thereafter used to tell that Dmitri

Ivanovitsch flew on a bubble and pierced the sky, and for this the authorities made him a chemist!

Mendeléeff was very democratic in his habits, and when travelling from the Capital to his estate, six or seven hours by rail, he always made use of the third class, and on the way talked freely to his fellow-passengers on all sorts of subjects, so that at the end of the journey he was surrounded by all sorts of people. At the railway station, about twelve miles from Boblovo, he was always met by the same driver, Zassorin, who with his troika of greys transported the whole family at full gallop, according to Russian custom.

Such, then, are the chief features of a great personality. If it be admitted that stories are told of his occasional irritability of temper, we can well place on the other side of the account the cordial relations always subsisting between the Professor and his assistants, the confidence and respect between the Master and his servants, the deep affection between the Father and his children, which are known to have persisted throughout his life, and which could be illustrated by many anecdotes. These stories merely serve "to give the world assurance of a man."

For us who live on the other side of Europe, separated as we are by race, by language, by national and social customs, and by form of government, it is not easy to understand completely the texture of such a mind, the quality of such genius, and the conditions, social or political, which may have served to encourage or to repress its activity. The Russian language may be eloquent, expressive, versatile, and harmonious, or it may possess any other good quality that may be claimed for it by those to whom it is a mother tongue, but the fact remains that it is a barrier to free intercourse between the Russian people and the world outside the Russian Empire. This alone creates a condition which must influence the development of thought, and must give to Russian science and philosophy a colour of its own. Mendeléeff was, like many educated Russians, a man of very liberal views on such subjects as education, the position of women, on art and science, and probably on national government. We can hardly guess what would be the influence on such a nature of a rigid administrative *régime* which forbids even the discussion of such questions. We in England are almost unable to imagine such a state of things as would be represented by the closing of, say, University College for a year or more, because the question whether the House of Lords ought to be abolished had been debated in the Students' Union. Imagine the Professor of Chemistry, along with his colleagues, for such a reason deprived of the use of his laboratory

by the police, and only allowed to resume his studies when someone down at Scotland Yard thought proper. Such being the experience of most of the Russian Universities and Technical High Schools, it is not surprising that the output of Russian science, notwithstanding the acknowledged genius of the Russian people, appears sometimes comparatively small. The amount of work done by Mendeléeff, both experimental and theoretical, was prodigious, and all the more remarkable considering the cloudy atmosphere under which so much of it was accomplished.*

In 1882 the Royal Society conferred on Mendeléeff, jointly with Lothar Meyer, the Davy Medal. In 1883 the Chemical Society elected him an Honorary Member, and in 1889 it conferred upon him the highest distinction in its power to award, namely, the Faraday Lectureship, with which is associated the Faraday Medal. In 1890 he was elected a Foreign Member of the Royal Society, and in 1905 he received the Copley Medal. So far as England is concerned, his services to science received full acknowledgment. It is all the more remarkable, therefore, that he never became a member of the Imperial Academy of Sciences of St. Petersburg.

Towards the end of 1906 Mendeléeff's health began to fail. Nevertheless he was able to attend the Minister on the occasion of an official visit in January to the office of Weights and Measures, but he caught cold and, enfeebled as he had been by influenza in the preceding autumn, inflammation of the lungs set in. Retaining consciousness almost to the last, he requested even on the day of his death to be read to from the "Journey to the North Pole," by his favourite author, Jules Verne. He died in the early morning of the 20th January (O.S.), 1907, within a few days of his seventy-third birthday. He was buried in the Wolkowo Cemetery beside the graves of his mother and son.

Turning now to a survey of Mendeléeff's work as a man of science, it will be sufficient if we pass lightly over his first essays. Like so many other chemists, he began by handling simple questions of fact, his first paper, dated 1854, when he was twenty years of age, being on the composition of certain specimens of orthite. It was not till 1859 that he settled down to serious examination of the physical properties of liquids, which led him to a long series of experiments on the thermal dilatation of liquids, of which the

* Professor Walden, at the end of a biographical notice recently published in the *Berichte d. Deut. Chem. Ges.*, April, 1909, gives a list of 262 printed publications by Mendeléeff. These include, not only memoirs on physical and chemical subjects, but books, pamphlets, reports, and newspaper articles relating to exhibitions, to the industries of Russia, to weights and measures, to education, to art, and even to spiritualism.

chief ultimate outcome was the establishment of a simple expression for the expansion of liquids between 0° and the boiling point (Trans., 1883, **45**, 126). This formula is liable to the same kind of modification which has been found necessary in the case of gases. It is, of course, applicable only to an ideal liquid from which all known liquids differ by reason of differences of chemical constitution and consequent differences of density, viscosity, and other properties. Thorpe and Rücker, by applying van der Waals' theory of the general relation between the pressure, volume, and temperature of bodies to Mendeléeff's expression for the thermal expansion, developed a simple method of calculating the critical temperature of liquids from observations of their expansion (Trans., 1884, **45**, 135).

Mendeléeff devoted a large amount of time and of experimental skill to the estimation of the densities of various solutions, especially mixtures of alcohol and water and of sulphuric acid and water, and of aqueous solutions of a large number of salts. In 1889 he embodied the whole in the monograph already referred to. In a paper communicated to the Transactions in 1887 (**51**, 779), he stated his views in the following words: "Solutions may be regarded as strictly definite atomic chemical combinations at temperatures higher than their dissociation temperatures. Definite chemical substances may be either formed or decomposed at temperatures which are higher than those at which dissociation commences; the same phenomenon occurs in solutions; at ordinary temperatures they can be either formed or decomposed." This view was retained by Mendeléeff, and appears in a footnote (p. 64) in the 7th Russian Edition (3rd English Edition) of the *Principles*, 1902, where the following passage occurs: "The conception of solutions as dissociated definite liquid chemical compounds is based on the following considerations: (1) That there exist certain undoubtedly definite crystallised chemical compounds (such as H_2SO_4 , H_2O , or $\text{NaCl} \cdot 2\text{H}_2\text{O}$, or $\text{CaCl}_2 \cdot 6\text{H}_2\text{O}$, etc.), which melt on a certain rise of temperature and then form true solutions; (2) that metallic alloys in a molten condition are real solutions, but on cooling they often give entirely distinct and definite crystallised compounds; (3) that between the solvent and the substance dissolved there are formed in a number of cases many undoubtedly definite compounds, such as compounds with water of crystallisation; (4) that the physical properties of solutions, and especially their specific gravities (a property which can be very accurately determined), vary with a change in composition, and in such a manner as would be required by the formation of one or more definite but dissociating compounds. . . . The increase in specific

gravity (ds) varies in all well-known solutions with the proportion of substance dissolved (dp), and this dependence can be expressed by a formula $ds/dp = A + Bp$ between the limits of definite compounds, whose existence in solutions must be admitted." Applying this method, he concludes that mixtures of alcohol and water may contain several definite compounds, such as $C_2H_6O + 3H_2O$. These views, however, did not prevent his recognising van't Hoff's gas theory as applicable to dilute solutions.

In conjunction with some of his students, Mendeléeff also studied minutely the question of the elasticity of gases, and published several papers on the subject (see Royal Soc. Catalogue), extending over a period of some ten years from 1872. From the earlier researches of Regnault and others, it was known that the law of Boyle and Mariotte is not strictly applicable either to all gases or at all pressures. Mendeléeff and his assistants devoted special attention to the departures from the theoretical requirements of the law exhibited by gases under very greatly reduced pressures. He found that for hydrogen the value of pv diminishes with the pressure down to 20 mm., while for air, carbon dioxide, and some others, pv increases slightly to a maximum.

Another subject to which Mendeléeff gave a good deal of attention was the nature and origin of petroleum. Having already reported in 1866 on the naphtha springs in the Caucasus, in the summer of 1876 he crossed the Atlantic and surveyed the oil fields of Pennsylvania. In the course of these investigations, he was led to form a new theory of the mode of production of these natural deposits. The assumption that the oil is a product of the decomposition of organic remains he rejects on a variety of grounds, which are set forth in a communication to the Russian Chemical Society (Abstract, see *Ber.*, 1877, **10**, 229). Mendeléeff assumes, as others have done, that the interior of the earth consists largely of carbides of metals, especially iron, and that hydrocarbons result from the penetration of water into contact with these compounds, metallic oxide being formed simultaneously. The hydrocarbons are supposed to be driven in vapour from the lower strata, where temperature is high, to more superficial strata, where they condense and are retained under pressure. In 1886, in consequence of rumours as to the possible exhaustion of the Russian oil fields, he was sent by the Government to Baku to collect information, and in 1889 he made a communication on this subject to Dr. Ludwig Mond, which is printed in the Journal of the Society of Chemical Industry (1889, **8**, 753.)

The influence of the great generalisation known as the periodic law can best be estimated by reviewing the state of knowledge

and opinion before the announcement and acceptance of the principle by the chemical world, and subsequently glancing at the influence which, directly or indirectly, it has produced on scientific thought, not only in regard to the great problems to which it immediately relates, but to the whole range of chemical theory.

The use of the expression, "atomic weight," implies the adoption of some form of atomic theory. But forty or more years ago Dalton's atomic theory was by many of the most philosophical chemists and physicists regarded as only a convenient hypothesis, which might be temporarily useful, but could not be accepted as representing physical reality. Since that time, however, a variety of circumstances have contributed to consolidate the Daltonian doctrine. The estimation of the ratios called atomic weights has been the subject of research, attended by more and more elaborate precautions to secure accuracy, from the time of Dalton himself onward through successive generations down to the present day. Though the atomic weights of the majority of the common elements are now known to a high degree of accuracy, the acknowledged errors have been sufficiently great to render abortive various attempts to reduce them to any common scheme of mathematical relationship. As is well known, the most important step toward the systematisation of atomic weights was taken about 1860, mainly on the grounds eloquently and convincingly set forth by Cannizzaro,* in consequence of which the arbitrary selection of numbers for atomic weights was superseded by the practical recognition of the law of Avogadro and the application of the law of Dulong and Petit, so that a common standard was established. No general scheme of atomic weights was previously possible, partial and imperfect efforts in this direction being represented by Döbereiner's triads and the principle of homology made use of by Dumas. Only so soon as numbers representing the atomic weights of calcium, barium, lead, and other metals were corrected and brought into the same category as those of oxygen, sulphur, and carbon was there some chance of determining whether these numbers possessed a common factor or were capable of exhibiting mathematical interrelations which might be regarded as symbolic of physical relations or even directly dependent upon them. The first step in this direction was taken by J. A. R. Newlands, who, after some preliminary attempts in 1864-1865, discovered that when the elements are placed in the order of the numerical value of their atomic weights, corrected as advised by Cannizzaro, the eighth element starting from any point on the list exhibits a revival of the characteristics of the first. This undoubtedly repre-

* 1858, and later, Faraday Lecture, 1872.

sents the first recognition of the principle of periodicity in the series of atomic weights, but whether discouraged by the cool reception of his "law of octaves" by the chemical world or from imperfect apprehension of the importance of this discovery, Newlands failed to follow up the inquiry. It was not long, however, before the matter was taken up by others, and doubtless the improvements in the estimation of atomic weights, following on the work of Stas, then only recently published, inspired greater confidence in the approximate accuracy of the numbers adopted as atomic weights, and thus encouraged inquiry into their relations. The subject is, indeed, an attractive one, for it involves considerations which lie at the foundations of all our notions respecting the physical constitution of matter, and accordingly we find papers by many chemists dealing with the question of these numerical relations. Odling especially seems to have given much thought to the subject, and, ignoring Newlands' previous attempts, he drew up towards the end of 1864 * a table containing a list of all the then well-known elements, arranged horizontally in the order of their generally accepted groups, and perpendicularly in the order of their several atomic weights. He concludes an article in Watts's Dictionary a few months later with these words: "Doubtless some of the arithmetical relations exemplified in the foregoing table are merely accidental, but, taken altogether, they are too numerous and decided not to depend on some *hitherto unrecognised law*." It is important to note the words I have italicised.

Such, then, was the state of knowledge about this time. Evidently the way was being prepared, but the prophet had not made his appearance, the seer who could look with the eyes of confidence beyond the clouds of uncertainty which obscured all ordinary vision.

In March, 1869, Mendeléeff communicated to the Russian Chemical Society an enunciation of the principle of periodicity and a statement of some of the consequences of this recognition of the relation of properties to atomic weight throughout the whole range of the known elements, and this statement was accompanied by a table which, while it bears a close resemblance to Odling's table of 1864, was apparently connected in his mind with an idea which became clearer and more decisive in the modifications which he immediately afterwards introduced into the arrangement.†

* *Quart. J. Sci.*, 1864, 1, 643; and Watts' Dict., Vol. III, 975.

† Subjoined is a translation, as literal as possible, of the German Abstract (*Zeitsch. f. Chem.*, 5, 405). Several obvious misprints have been corrected.

On the Relation of the Properties to the Atomic Weights of the Elements.

By D. MENDELÉEFF.

When the elements are arranged in vertical columns, according to increasing atomic weight, so that the horizontal lines contain analogous elements, again according to increasing atomic weight, the following arrangement results, from which several general conclusions may be derived:

			Ti = 50	Zr = 90	? = 180
			V = 51	Nb = 94	Ta = 182
			Cr = 52	Mo = 96	W = 186
			Mn = 55	Rh = 104.4	Pt = 197.4
			Fe = 56	Ru = 104.4	Ir = 198
		Ni = Co = 59	Pd = 106.6	Os = 199	
			Cu = 63.4	Ag = 108	Hg = 200
			Zn = 65.2	Cd = 112	
			? = 68	U = 116	Au = 197 ?
			? = 70	Sn = 118	
			As = 75	Sb = 122	Bi = 210 ?
			Se = 79.4	Te = 128 ?	
			Br = 80	I = 127	
			Rb = 85.4	Cs = 133	Tl = 204
			Sr = 87.6	Ba = 137	Pb = 207
			Ce = 92		
			? = 45		
		?Er = 56	La = 94		
		?Yt = 60	Di = 95		
		?In = 75.6	Th = 118		
H = 1	Be = 9.4	Mg = 24			
	B = 11	Al = 27.4			
	C = 12	Si = 28			
	N = 14	P = 31			
	O = 16	S = 32			
	F = 19	Cl = 35.5			
Li = 7	Na = 23	K = 39			
		Ca = 40			
		? = 45			
		?Er = 56			
		?Yt = 60			
		?In = 75.6			

1. The elements arranged according to the magnitude of atomic weight show a periodic * change of properties.

2. Chemically analogous elements have atomic weights either in agreement (Pt, Ir, Os), or increasing by equal amounts (K, Rb, Cs).

3. The arrangement, according to atomic weights, corresponds with the *valency* of the elements, and to a certain extent the difference in chemical behaviour, for example, Li, Be, B, C, N, O, F.

4. The elements most widely distributed in nature have small atomic weights, and all such elements are distinguished by their characteristic behaviour. They are thus *typical* elements, and the lightest element, hydrogen, is therefore rightly chosen as the typical unit of mass.

5. The magnitude of the atomic weight determines the properties of the element, whence in the study of compounds regard is to be paid not only to the number and properties of the elements and their mutual action, but to the atomic weights of the elements. Hence the compounds of S and Te, Cl and I, show, beside many analogies, yet striking differences.

* Here an error in the German translation does an injustice to the original inasmuch as the Russian word for periodical is rendered "stufenweise" (gradual).

6. It allows the discovery of many *new* elements to be foreseen, for example, analogues of Si and Al with atomic weights between 65 and 75.

7. Some atomic weights will presumably experience a correction; for example, Te cannot have the atomic weight 128, but 123 to 126.

8. From the foregoing table, new analogies between elements become apparent. Thus U appears as an analogue of B and Al, which, as is well known, has long ago been established experimentally.

Previous students of the subject had been, for the most part, struck with the relations obviously subsisting between the members of the several natural families of elements, but had, with few exceptions, failed to perceive that there must be a *general* law binding the whole together. However, Mendeléeff, with that noble sentiment of justice which always animates the truly scientific mind, admits that the idea of a general law had already been foreshadowed by others, and he says (Faraday Lecture, 1889), "I now see clearly that Strecker, de Chancourtois, and Newlands stood foremost in the way towards the discovery of the periodic law, and that they merely wanted the boldness necessary to place the whole question at such a height that its reflection on the facts could be clearly seen."

It may be remarked that Strecker did little more than call attention to the sequence in the values of the atomic weights of certain elements, and states that "we must leave to the future the discovery of the *law* of the relations which appear in these figures" (Theorien u. Experimente zur Bestimmung der Atom Gewichte der Elemente, 1859). De Chancourtois, in his work entitled "Le Vis Tellurique" (1863), devised a geometric method of representing the atomic weights by coiling round a cylinder a helix with an angle of 45° , the cylinder being divided vertically into sixteen equal parts by lines drawn from the circular base. The points of intersection of the helix with these lines were supposed to represent the atomic weights of elements which differed from one another by 16 or by multiples of 16.

Mendeléeff's table of 1869 was subsequently in 1871 modified so as to assume the form with which we have all been so long familiar, and which is to be found in every modern text-book. Thus it may be claimed for Mendeléeff that he was actually the first, not only to formulate a general law connecting atomic weights with properties, but was the first to indicate its character, and, as himself (*Principles*, 1905, II, p. 28) has pointed out, he was the first "to foretell the *properties of undiscovered* elements, or

to alter the accepted atomic weights " in confidence of its validity. The time was, in fact, ripe for the enunciation of this general principle, and, the suggestion once given, the relations embodied in the law could not fail to attract other chemists. Accordingly, in December, 1869, Lothar Meyer, with such knowledge of Mendeléeff's scheme as could be derived from the imperfect German version of his paper of the previous March, proved himself a convinced exponent of the idea by contributing to Liebig's *Annalen* a paper containing a table, substantially identical with that of Mendeléeff, and his famous diagram of atomic volumes, which, more clearly even than the tabular scheme, illustrates the principle of periodicity.

The history of science shows many instances of the same kind. Great generalisations have often resulted from the gradual accumulation of facts which, after remaining for a time isolated or confused, have been found to admit of co-ordination into a comprehensive scheme, and, this once clearly formulated, many workers are found ready to assist in its development. The case is nearly parallel to the recognition of the operation of natural selection by Darwin and Wallace, or it might be compared to the discovery of oxygen by Priestley and Scheele and the utilisation of this knowledge by Lavoisier. In each case much preparatory work had been done, and a body of knowledge had been gradually accumulated which, when duly marshalled and surveyed by the eye of a master, could scarcely fail to reveal to him the underlying principle. The full consequences, however, would appear only to a few.

The law of periodicity was expressed by Mendeléeff in the following words: *

"The properties of the elements, as well as the forms and properties of their compounds, are in periodic dependence on, or (expressing ourselves algebraically) form a periodic function of, the atomic weights of the elements." After a brief historical account of the discovery of the law by himself, Mendeléeff concludes by saying (*Principles*, p. 18): "I consider it well to observe that no law of nature, however general, has been established all at once; its recognition has always been preceded by many presentiments; the establishment of a law, however, does not take place when the first thought of it takes form, or even when its significance is recognised, but only when it has been confirmed by the results of experiment which the man of science must consider as the only proof of the correctness of his conjectures and opinions."

* *Principles*, 1905, Vol. II, p. 17.

I regard it as unnecessary, in the presence of the Fellows of the Chemical Society, to review with any detail the multitudinous applications of the scheme of the elements constructed on the basis of the periodic law. These are the commonplaces of modern theoretical chemistry. They are embodied in every text-book of any importance, and are related by every lecturer and teacher as familiar and indisputably recognised consequences of the system. We may therefore pass lightly over the story of the prediction by Mendeléeff of the properties of undiscovered elements, confirmed so remarkably by the discovery of scandium, gallium, and germanium, and related in dramatic language by Mendeléeff himself (Faraday Lecture). We may also pass over the applications of the system to the correction of atomic weights, illustrated by the case of beryllium, the recognition of previously unnoticed relations, and the discovery of new elements, notably the companions of argon (Ramsay, Presidential Address to Section B, British Association, 1897, and *Proc. Roy. Soc.*, 1898, 63, 437).

It will be more profitable to consider a few of the difficulties which still encumber the application of the law, and which, while limiting our acceptance of it in an unqualified form as applicable to the whole of the elements, tempt the speculative mind to wander in wide fields of conjecture.

Can it be truly said that the elements arranged in the order of their atomic weights show without exception periodic changes of properties? This question has been propounded already, but has never been fully discussed, even by Mendeléeff. An examination of the facts seems, however, to indicate the possibility of some other principle, which, while it does not supersede the periodic scheme, would, if it could be recognised, supplement it. This involves other considerations which we may turn to first.

If the whole of the known elements are drawn up in the order of their atomic weights (using the values given by the International Committee for 1908), we find a progression in value from $H=1.008$ to $U=238.5$, with differences between the successive elements which vary from 0.3 (Co-Ni)* to 4.3 (Co-Cu) among the

* Mendeléeff held the view that "in general, cobalt is more nearly allied to iron than nickel, and the latter more nearly to copper" (*Principles*, Eng. Ed., 1905, p. 379). Accordingly, in the first edition of his book, he assigned to cobalt the atomic weight 58.5, and to nickel, the atomic weight 59. In the later edition of 1905, he makes them both 59, and expresses the belief that eventually the atomic weight of cobalt will be found less than that now accepted and less than nickel (Eng. Ed., 1905, II, footnote 25, p. 45). Whatever may be the exact values of the atomic weights of these two elements, there can be no doubt that the atomic weight of cobalt is *greater* than that of nickel. This is proved by the estimations of the specific heats of both these metals purified by methods which preclude the possibility

common elements of which the atomic weights have been most accurately estimated. The large difference, 7·4, between Sb and Te is manifestly due to some error in the atomic weight of tellurium of which no sufficient explanation is yet forthcoming, and it is only when we get to the element Bi that there seems reason for thinking that it must be followed by some hitherto unrecognised elements, since the gap between Bi and the next known element, Ra, is 18·7 units. The atomic weights of the long series of elements beginning with La are confessedly uncertain, but that they all lie between La and Ta seems probable, because although the individual numbers are doubtless inexact, the *average* difference between any two consecutive terms is roughly the same as the average difference between successive atomic weights among the better known elements preceding them. $Ta - La = 181 - 138\cdot9 = 42\cdot1$ for sixteen intervals.

It must also be noted that the differences, approximately three units each, among the three elements with smallest known atomic weights, namely,

H 1·008, He 4, Li 7·03,

are greater than the differences observed among the elements which immediately succeed them, namely,

Li 7·03, Be 9·1, B 11, C 12, N 14·01, O 16, F 19.

It will be seen later that, as regards this part of the scheme, Mendeléeff had put forth a special hypothesis.

If these considerations are to be regarded as having weight, it seems probable that few additional elements are to be expected, except possibly one following Mo and another following W, save in the region already indicated from Bi to Ra. This suggests the remark that, after all, it is not necessary to assume that the materials of which the earth consists should necessarily include a sample of every possible element indicated by such a scheme. Some which are missing from terrestrial matters may perhaps be responsible for phenomena recognisable by the spectroscope in stars or nebulae far distant in cosmical space. The unexpected, however, often happens, and, remembering the discovery of terrestrial of appreciable error or of mutual contamination. The following results were obtained by different observers using different methods:

Temperature.	Cobalt.	Nickel.
From 100° to 15°	0·10303	0·10842
„ 15° to -78·4°	0·0939	0·0975
„ 78·4° to -182·5°	0·0712	0·0719

Tilden, *Phil. Trans.*, 1900, **194 A**, 249.

From 100° to 20° ..	0·104	0·108
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Copaux, *Compt. rend.*, 1905, **140**, 657.

helium, it is permissible to hope that some of the vacant spaces may hereafter be filled by earthly occupants.

There is one important point to be noted here, namely, that if the so-called rare earth metals, praseodymium, neodymium, samarium, gadolinium, terbium, dysprosium, erbium, ytterbium, and others of which the existence is doubtful, do lie in the position indicated, the original statement of the periodic law breaks down at this point. Enough is already known of their properties to show that they are very closely allied together, and cannot fall into separate periods. Mendeléeff says (*Principles*, 1905, Vol. II, p. 45), "This appears to me to be one of the most difficult problems offered to the periodic law." He prefers, however, to leave open the question as to the position of these elements. The discordance of argon and of tellurium with the places assigned to them are also matters which must be left for the consideration of future workers.

One result of the recognition of the periodic law is that theories concerning the genesis of the elements have received a stimulus previously unknown. It is, however, interesting to note the attitude of Mendeléeff toward this question, and the small extent to which this attitude appears to have become modified with the lapse of time. When, in 1889, twenty years after the discovery of the law, he composed the Faraday lecture, he seems to have regarded speculation in this direction as a kind of abuse of the periodic system. He was, of course, fully justified in stating (Faraday Lecture) that "the periodic law, based as it is on the solid and wholesome ground of experimental research, has been evolved independently of any conception as to the nature of the elements; it does not in the least originate in the idea of a unique matter; and it has no historical connexion with that relic of the torments of classical thought." But it is at least questionable how far he was justified in continuing that "therefore it affords no more indication of the unity of matter, or of the compound character of our elements, than the law of Avogadro or the law of specific heats, or even the conclusions of spectrum analysis. None of the advocates of a unique matter have ever tried to explain the law from the standpoint of ideas taken from a remote antiquity, when it was found convenient to admit the existence of many gods and a unique matter." And again, later, "From the foregoing, as well as from the failures of so many attempts at finding in experiment and speculation a proof of the compound character of the elements and of the existence of primordial matter, it is evident, in my opinion, that this theory must be classed among mere utopias."

Fifteen years later, after the discovery of the argon group of elements, of the phenomena of radioactivity, and of radium, it became necessary to consider the relations of these substances to the periodic scheme. In a remarkable article contributed to the new Russian Encyclopædia, and subsequently printed as Appendix III to the *Principles* (English Edition, 1905), Mendeléeff gives a new table of the elements, in which places are found, not only for the argon group and radium, but for two hypothetical elements which are placed before helium and designated x and y .

As this table may be assumed to represent his latest views concerning the relations of the elements, it is here reproduced.*

The y in the table is supposed to be an analogue of helium, and may be identified hereafter with "coronium," which has been recognised in the sun's coronal atmosphere. This gas would have, according to Mendeléeff, a density about 0.2, and therefore a molecular weight about 0.4, or about one-tenth that of helium.

x is the "ether" of the physicist, for which Mendeléeff, disregarding conventional views, supposes a molecular structure. He also assumes that, like the argon group, this element is chemically inert and possesses a very low density and atomic weight, estimated at 0.000,000,000,053.

His views in connexion with this matter are put forward merely as speculations and without dogmatism, but it is clear that he retained his repugnance to the conception of a unique matter to the last. In his essay entitled "A Chemical Conception of the Ether" (translated by Kamensky, 1904), the following passage occurs, p. 32: "Being unable to conceive the formation of the known elements from hydrogen, I can neither regard them as being formed from the element x , although it is the lightest of all the elements. I cannot admit this, not only because no fact points to the possibility of the transformation of one element into another, but chiefly because I do not see that such an admission would in any way facilitate or simplify our understanding of the substances and phenomena of nature."

Chemists and physicists have, however, found it impossible to resist the fascination of this problem, and accordingly there have been many hypotheses as to the origin of the elements and the nature of their connexion with one another. These seem to be inseparable from the periodic scheme itself, which at once provokes the inquiry, Why do these numerical relations occur, and what

* The spaces left vacant in Series I, after hydrogen, are the positions of hypothetical elements having approximately the atomic weights, 1.4, 1.8, 2.2, 2.6, 2.8, 3.0, and 3.4, and standing at the head of groups II to VIII respectively.

Series.	Zero group.	Group I	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.
0 ...	∞	—	—	—	—	—	—	—	—
1 ...	y	Hydrogen, H = 1.008	—	—	—	—	—	—	—
2 ...	Helium, He = 4.0	Lithium, Li = 7.03	Beryllium, Be = 9.1	Boron, B = 11.0	Carbon, C = 12.0	Nitrogen, N = 14.04	Oxygen, O = 16.0	Fluorine, F = 19.0	—
3 ...	Neon, Ne = 19.9	Sodium, Na = 23.05	Magnesium, Mg = 24.1	Aluminium, Al = 27.0	Silicon, Si = 28.4	Phosphorus, P = 31.0	Sulphur, S = 32.06	Chlorine, Cl = 35.45	—
4 ...	Argon, Ar = 38	Potassium, K = 39.1	Calcium, Ca = 40.1	Scandium, Sc = 44.1	Titanium, Ti = 48.1	Vanadium, V = 51.4	Chromium, Cr = 52.1	Manganese, Mn = 55.0	Iron, Fe = 55.9
5 ...	—	Copper, Cu = 63.6	Zinc, Zn = 65.4	Gallium, Ga = 70.0	Germanium, Ge = 72.3	Arsenic, As = 75.0	Selenium, Se = 79.0	Bromine, Br = 79.95	Cobalt, Co = 59
6 ...	Krypton, Kr = 81.8	Rubidium, Rb = 85.4	Strontium, Sr = 87.6	Yttrium, Y = 89.0	Zirconium, Zr = 90.6	Niobium, Nb = 94.0	Molybdenum, Mo = 96.0	—	Nickel, Ni = 59 (Cn)
7 ...	—	Silver, Ag = 107.9	Cadmium, Cd = 112.4	Indium, In = 114.0	Tin, Sn = 119.0	Antimony, Sb = 120.0	Tellurium, Te = 127	Iodine, I = 127	—
8 ...	Xenon, Xe = 128	Cesium, Cs = 132.9	Barium, Ba = 137.4	Lanthanum, La = 139	Cerium, Ce = 140	—	—	—	—
9 ...	—	—	—	—	—	—	—	—	—
10 ...	—	—	—	Ytterbium, Yb = 173	—	Tantalum, Ta = 183.0	Tungsten, W = 184	—	Osmium, Os = 191
11 ...	—	Gold, Au = 197.2	Mercury, Hg = 200.0	Thallium, Tl = 204.1	Lead, Pb = 206.9	Bismuth, Bi = 208	—	—	Iridium, Ir = 193
12 ...	—	—	Radium, Ra = 224	—	Thorium, Th = 232	—	Uranium, U = 239	—	Platinum, Pt = 194.9 (Au)

is the meaning of them if they do not point to a common genesis or the operation of some process of evolution?

Hypotheses concerning the evolution of the elements have hitherto been usually based on the assumption that the successive stages of condensation of elemental matter proceeded from a single primary stuff, which by a process analogous to polymerisation among carbon compounds gave rise to atoms of greater and greater mass, which were stable at the prevailing and any lower temperature. The physical cause of the successive condensations is supposed to be a falling temperature. It is, of course, possible to imagine that if to the stuff of which hydrogen atoms consist are added successive portions of matter of the same kind, stable structures may at intervals result which we know as the atoms of the elements helium, lithium, beryllium, boron, carbon, nitrogen, oxygen, and fluorine, provided the idea of internal structure in these atoms is allowed. Otherwise, from the mere accretion of matter upon a central nucleus, there seems no sufficient reason why there should not have been formed an indefinite number of intermediate masses corresponding to an indefinite number of what would be called elements. Further, it is difficult to understand why simple increase of mass should change, say, oxygen into fluorine, while a further addition of the same kind should change negative fluorine into inert neon or positive sodium. The possibility of the condensation of a single "protyl" so as to produce, at successive though unequal stages of cooling, the elements known to the chemist has been most ably discussed long ago by Sir William Crookes.

This hypothesis, however, was put forward long before the work of Sir J. J. Thomson and his school was given to the world and the electron was accepted as a physical reality. The hypothesis that one elemental stuff may give rise to the whole array of known elements by a process of condensation accompanied by a loss or gain of electrons, the mass of which is approximately one-thousandth of the mass of an atom of hydrogen, forms the subject of a paper by Mr. A. C. G. Egerton in a recent number of our Transactions (1909, 95, 239). The atomic weights calculated by his formula agree closely with the experimental atomic weights of the first fifteen elements, but the hypothesis gives no explanation of the facts observed in the physical properties of the elements arranged according to the Mendeléeff scheme, their alternation of odd and even valency, the transition from positive on one side of the table to negative on the other, the periodicity of properties shown by the sudden change of character in passing from fluorine to the next element, whether it be neon or sodium.

Another paper by Messrs. A. C. and A. E. Jessup (*Phil. Mag.*,

1908, [vi], 15, 21) has recently provided a hypothesis of an entirely different character. From a study of the spectra of the nebulae, these authors have been led to assume the existence of two hitherto unrecognised elements, to which the names protoglucinum and protoboron are assigned. These with hydrogen and helium are supposed to represent *four* initial substances, or protons, which, by condensation directly or indirectly, give rise to all the rest of the elements. The arguments of these authors are ingenious, but rather artificial in view of the fact that the number of groups in the periodic scheme to be provided for is greater than four.

In the Mendeléeff chart of the elements, there is nothing more striking than the gathering of the negative elements toward what may be called the N.E., and the segregation of the positive elements toward the S.W., the centre of the intermediate territory being occupied by elements which play a more or less undecided part. I have elsewhere (Presidential Address, 1905, Trans., 87, 564) drawn attention to the fact that carbon, at any rate, is not directly deposited by electrolysis from any of its compounds, with positive hydrogen on the one hand, or negative chlorine on the other. I believe the same is true of silicon, these two elements standing in a middle position between the extremes occupied by lithium and fluorine respectively.

If we assume that atoms are made up of two parts (protyls), positive and negative, in proportions which determine by the preponderance of one or the other whether the element shall exhibit the positive character of a metal like lithium or the negative character of a halogen, we arrive at a hypothesis which recalls the ideas put forward nearly a century ago by Berzelius. His views are familiar to every student of the history of chemistry, but have long been relegated to the lumber room of worn-out doctrine. The last few years have, however, given us the remarkable experimental investigations of J. J. Thomson already referred to, and the new conceptions concerning the nature of atoms, which revive the fundamental idea that they are made up of two components.*

* Carnelley, in 1885 (*Brit. Assoc. Reports*), brought forward the idea "that the elements are not elements in the strict sense of the term, but are, in fact, compound radicals made up of at least two simple elements, A and B." The element A was supposed to be identical with carbon, while to B was assigned a negative weight, -2, and it was suggested that it might be the ether of space. C. S. Palmer (*Proc. Colorado Scient. Soc.*) assumed the existence of two sub-elements, to which he gave the names "kalidium" and "oxidium," and his views appear to have a general resemblance to the hypothesis suggested in the text. The original article is abstracted in Venable's *Periodic Law* and is referred to in footnotes in Palmer's translation of *Nernst's Theoretical Chemistry*.

Setting out the known elements in the order of the numerical value of their atomic weights, we find that between the first three elements, $H=1$, $He=4$, and $Li=7$, the difference, 3, is greater than would be expected by comparison with the differences noticed between the elements of greater atomic weight which immediately follow them. In order to satisfy the hypothesis just put forward, there appears to be wanting an element which should stand in the same relation to fluorine as hydrogen to lithium. This would have an atomic weight 2.7 approximately. Whether this exists, and whether its existence is indicated by the unappropriated spectral lines of nebulae or corona, can only be a matter of conjecture. Mendeléeff, in his (1905) latest speculations concerning the possibility of still undiscovered elements, has suggested the existence of a new element of the halogen group with an atomic weight about 3.* But, as already sufficiently shown, he accepted no hypothesis which involved any idea of the composite nature of the elements. It would therefore have been foreign to his system to employ this element in any such manner. But the idea seems to me to assist materially conceptions as to the process of condensation hypothetically occurring in the evolution of the known chemical elements. For to suppose that the typical elements, so different as they are in character, forming the first line of Mendeléeff's scheme, have all resulted from the condensation of a single protyl has always seemed to me a difficult proposition. There is comparatively little difficulty in the view that the successive terms of a family of what, by analogy, may be called a homologous series, may have originated in this way. A consideration of all the properties of the alkali metals, for example, coupled with the character of their spectra, suggests quite naturally the passage from lithium to sodium, and so forth, step by step, by the addition of successive accretions of the same matter to the primal element, the character of which, including valency, is not only sustained through the whole family, but becomes more strongly marked in proportion to the gradual increase of atomic weight. At the opposite end of the table, on the other hand, a *reduction* of the negative character of the element, in passing from fluorine to iodine, seems to suggest that the negative protyl which preponderates in the smaller atom is modified in the larger atom by the addition of a certain proportion of the positive protyl.

The conceptions presented to us in J. J. Thomson's work permit of several supplementary hypotheses, especially the idea that if

* It may also, perhaps, be worthy of note that Mr. Egerton's calculations (*loc. cit.*) lead him to postulate an element of nearly this atomic weight, namely, 2.9844, although his paper gives no indication as to its character.

atoms are really made up of smaller corpuscles these are not thrown together in confusion but, as he has shown, must be distributed within the mass in a definite order, which is determined by the attraction of the electro-positive shell and the self-repulsion of the negative corpuscles included in it. Once the idea of structure within the atom is admitted, the possibility presents itself of there being for the same mass more than one arrangement corresponding to what is called isomerism in compounds. In this way the case of elements with similar properties and identical or nearly identical atomic weights, for example, cobalt and nickel, and even such a case as tellurium, might perhaps be explained. Further, now that the materials which have so long received the unsatisfactory designation of the "rare earths" are found in unexpected abundance, it may be hoped that the study of their chemical characters may be completed. It may turn out that this group may include elements of identical atomic weight, though exhibiting different properties. It does not seem very long ago in the memory of many now living that the nature of the isomerism of the derivatives of benzene was a deep mystery, from which nearly all obscurity cleared away in the light of the then new theory of the constitution of benzene.

I have dwelt at some length on these various hypotheses, because the discussion of the subject to which they relate indicates, in my opinion, one of the consequences of the promulgation and general acceptance of the periodic scheme of the elements. This is, however, not the only result of the recognition of its validity and usefulness by chemists generally. That the elements stand in a definite relation to one another implies that their compounds also fall into their places in an orderly system, and consequently a basis is provided for the complete systematisation of the whole science of chemistry. There is scarcely a treatise on chemistry which does not bear evident witness to this influence. And this is perhaps not the least among the services rendered by this generalisation, for not only is the learner enabled to remember a much larger number of facts than previously, but he is led to perceive a connexion between phenomena and processes which was almost entirely wanting so long as practical chemistry consisted mainly of a bundle of recipes. And here it is fitting that we should glance at the famous treatise by Mendeléeff himself, "The Principles of Chemistry," of which we possess three editions in English, the last of which, issued in 1905, is a rendering of the seventh edition (1903) of the original. An eighth Russian edition began to be issued in 1905, but is incomplete. To this remarkable book it is impossible to do justice in a brief notice or to communicate to

those who have not read it an adequate impression. Clearly it is a work of genius, but such works are not always the most suitable for beginners, though for the advanced student nothing can be more inspiring. The "Principles" embody in reality two distinct treatises, for the text, which is written in an easy style, open to quite straightforward reading, is accompanied by notes which are often more voluminous and usurp entire pages. Even the preface is attended by these commentaries, which are all interesting as showing the spirit of the writer and the restless activity of his mind. A few extracts from the preface will serve to illustrate the truism too often neglected by writers of biographies, that it is impossible to separate a man's work from his life, and that the character and quality of the former are dependent upon the personal characteristics of the man, independently of the opportunities or influences which may have served to assist or to repress his activities.

"If statements of fact," he says, "themselves depend upon the persons who observe them, how much more distinct is the reflection of the personality of him who gives an account of methods and philosophical speculations forming the essence of a science! For this reason there will inevitably be much that is subjective—bearing the stamp of time and locality—in every objective exposition of science. And as an individual production is only significant in virtue of that which has preceded it and that which is contemporary with it, it resembles a mirror, which in reflecting exaggerates the size and clearness of neighbouring objects, and causes a person near it to see reflected most plainly those objects which are on the side to which it is directed, and sometimes even the person holding the mirror. Although I have endeavoured to make my book a true mirror directed toward the whole domain of chemical changes and of the elements taking part in them, yet involuntarily those influences near to me being most clearly reflected and the most brightly illuminated have tinted the entire work with their colouring. In this way the chief peculiarity of the book has been determined. Experimental and practical data and their application in life and industry occupy their place, but the philosophical principles of our science form the chief theme of the work."

Later on he says, "The thought that this book might fall not only into the hands of the beginner for whom it is intended, but also of authorities who might wish to know the views held by an old disciple of science on the current problems of chemistry, greatly complicated the preparation of a new edition, for it necessitated making a selection of the most essential of the vast number of

new researches published year by year and explaining my views on them without greatly enlarging the bulk of the work. After having closely followed all the chief conquests of chemical science since the days of Berzelius, Liebig, Dumas, and Gerhardt, and having seen the triumph of much that lay neglected, and the fall of much that was exalted, I involuntarily acquired a tendency to analyse new facts, and a desire to transmit to my readers the results of such analysis, if it could, in my opinion, help towards a proper explanation and generalisation of the chemical elements. In carefully preparing this edition, I have not lost sight of the fact that I am hardly likely to publish another, and I have therefore in many cases spoken more definitely than formerly. After having been an insignificant but zealous worker in chemistry for almost half a century, I wished my book should retain some traces of how a confirmed disciple of Gerhardt regards the fundamental problems of the theory of the chemical elements at the beginning of the twentieth century. As an example, I may mention that the more I have thought on the nature of the chemical elements, the more decidedly have I turned away from the classical notion of a primary matter, and from the hope of attaining the desired end by a study of electrical and optical phenomena, and the more clearly have I recognised that first and foremost are needed truer conceptions of 'mass' and 'ether' than those in vogue at the present time. The return to electro-chemism which is so evident in the supporters of the hypothesis of 'electrolytic dissociation,' and the notion of a splitting up of atoms into 'electrons,' in my opinion only complicate, and in no way explain, so real a matter (since the days of Lavoisier) as the chemical changes of substances, which led to the recognition of the invariable and ponderable atoms of simple bodies. The definition of mass gave a means for analysing and grasping chemical transformation of substances, and for arriving at the atom, while the mass of the atom was shown by the periodic law to influence all its chief chemical properties. Thus chemistry in its principles stood on the firm foundations laid by Galileo, Newton, and Lavoisier, and in order to gain further insight and knowledge of the atoms themselves, the fundamental conceptions of mass, gravity, and ether will have to be explained by a method of experiment alone, otherwise the realism of science will again open its doors to such metaphysical and 'metachemical' conceptions as phlogiston and other mystical dreams. For my part I endeavour to remain true to the testament of realism left by Newton and Lavoisier, and it is my wish to instil this sentiment into my young readers."

This is very clear, and little more remains to be said. In the seventeenth century Robert Boyle taught us how to distinguish elements from compounds, and how to give to the word "element" a definite connotation clearly distinguishing it from the elusive and fantastic language of the alchemists. In the eighteenth century Lavoisier showed the true nature of the most familiar of chemical compounds, namely, acids, bases, and salts, and helped to lay the foundation of quantitative chemistry. At the beginning of the nineteenth century Dalton gave to chemistry the Atomic Theory, of which it is not too much to say that it provided the scaffold by the aid of which the entire fabric of modern theoretical chemistry has been built up. Sixty years later this conception, developed and adorned by the labours of an army of earnest workers, has been shown to us in a brilliant new light thrown over the whole theory by Mendeléeff.

The views of Boyle, of Lavoisier, and of Dalton have been corrected by experience and broadened by extended knowledge, but the fundamental and essential parts of their ideas remain, and their names are immortal. In like manner the expression of the periodic law of the elements as known to the present generation is destined, we may believe, to be absorbed into a more comprehensive scheme by which obscurities and anomalies will be cleared away, the true relations of all the elements to one another revealed, and doubts as to the doctrine of evolution resolved in one sense or the other. But as with the Atomic Theory itself, there is no reason to doubt that the essential features of the periodic scheme will be clearly distinguished through all time, and in association with it the name of Mendeléeff will be for ever preserved among the Fathers or Founders of Chemistry.
