SCIENCE

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RADIO-ACTIVITY AND THE PERIODIC SYSTEM[1]

THE periodic system of the elements has for nearly half a century proved a most puzzling and absorbing problem to chemists. It has been called a law, but while there is undoubtedly an underlying law or laws, I doubt whether we have as yet any very clear conception of them. Certainly, the usual statement that the properties of the elements are periodic functions of their atomic weights was never strictly true, even in days of partial knowledge, and is much less true now. It was neither the periodicity "of the geometers," as Mendeleef himself said, nor the function of the mathematician. Indeed, we have now come to a view where, apparently, we must abandon the atomic weight as the only or even the chief determining variable.

The truth is that for many years after its announcement it was more truly a working hypothesis, and a great deal of work had to be and still has to be done before it can attain to its completed form. It contains much that is true, has been most useful as a guiding principle, and has shown a wonderful power of adjustment to new facts and increasing knowledge.

It was in 1895 that the system had to adjust itself to the first severe jolt which it received through the discovery of argon and helium, and three years later, of other inactive, monatomic elements. The necessity for readjustment here had been in part foreseen. The abrupt change in the progression of the elements from strongly electro-negative fluorine to strongly electro-

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[1] Read before the Elisha Mitchell Scientific Society, March 9, 1915.

positive sodium, and, in general, the transition *per saltum* from period to period had been discussed by Reynolds and others. It needed explanation and was impossible mathematically except by passing through zero or infinity. Some, as Sedgwick and de Boisbaudran, seem to have predicted such transition elements, and when argon was discovered it was not difficult for Julius Thomsen and de Boisbaudran to arrange an entire zero group with approximate atomic weights three years before Ramsay's brilliant discovery of the other inactive gases.

There are other anomalies in the system which are difficult to explain with the accepted tabulation. Such, for instance, is the existence of the rare earths, now some sixteen in number, so closely alike chemically and so different from other chemical individuals. The more they are studied, the less possible does it seem to fit them in any vacant places in the table. Meyer has recently suggested that they may form a miniature periodic system in themselves reproducing the relations of the main system. But a more serious breakdown in the supposed fundamental principle of the system comes in the relative position of such elements as argon and potassium, cobalt and nickel, tellurium and iodine. After most exhaustive investigation of their atomic weights it has become evident that these can not be used in deciding the relative order and at the same time have these elements fall into the proper grouping with those elements chemically most nearly related to them. So the order of the atomic weights has been tacitly abandoned and the superior determining power of the chemical characteristics acknowledged. This can only mean that the mass of the atom is not the sole, nor indeed the chief, determining variable, and it would seem that the search for the latter can only be ended by the solution of the problem as to the nature of the atom itself.

Certain clews to this have undoubtedly been in our hands for a long time, but their leading was not clear and the thought of them baffling. Such, for instance, were the facts that by taking an atom of nitrogen and four of hydrogen a grouping of atoms was obtained which behaved in general as an atom and was the analogue of potassium. Or, again, carbon and nitrogen give us an analogue of chlorine—and so with compound radicals in general. But while both building and tearing down again were easy, they seemed to throw no light on how those we could not tear down were once built up.

Still another thought-inspiring fact which would seem to have important bearing on the nature of the atom and hence the meaning of the periodic system is the ease with which certain elements by a change of valence change their chemical character and form distinctive series of salts as if they had been transformed into different elements. This causes some confusion and what would ordinarily be called forcing in the present tabulation of the system, and it will be recalled that Mendeleeff, in his earlier tables, actually placed certain of the metals, as copper and mercury, in two different groups, assigning each two different places. Signs are seen in the work of Barbieri and others of a tendency to place certain of the elements in two or more different groups according to valence.

I believe that one should keep in mind the idea involved in Patterson-Muir's definition of an element as a collection or group of properties. Thus there are weight, electro-chemical nature, affinity, valence and other properties by which we recognize it and differentiate it from other elements and to which our knowledge of it is necessarily limited. There is a more or less definite gradation in these properties from element to element, showing an inter-relationship, and yet scarcely in itself justifying the conclusion that any one property determines the other or that they are dependent upon it. While it is true that it is hardly possible to dissociate these properties from some conception of matter, such conception has not yet reached its ultimate analysis and until it has we are dealing with the recognized properties alone.

In the same year in which the periodic system was forced to adjust itself to a zero group another discovery was entering upon its marvellous development which was to open up new views as to the nature of matter and radically affect the system. The remarkable and illuminating results obtained in the study of radioactive substances are paving the way for an understanding of the laws on which this system is based.

Radioactivity was regarded by Mme. Curie as an atomic property and this was the guiding thread which led to the discovery of radium. Of course, this preceded by a number of years Rutherford's announcement of his theory of successive transformation or the disintegration of the atom. It is a question whether the fact that an atom is undergoing disintegration is to be regarded as a property in the same sense as the mass, valence, etc., but so long as this change can not be induced, changed or stopped and is known to take place only in the case of a fraction of the elements it is certainly distinctive and may be called a property for lack of a better name. There is, however, undoubtedly a cause for this disintegration and this instability may be due to some inherent property of the atom.

At present there are some thirty-seven radioactive bodies known, with the possibility of still others being identified. Each has distinctive radioactive properties. For a number of these the chemical and physical properties are known. Each is an atom hitherto unknown and must be considered a new element. Of course, the present accepted arrangement of the periodic system does not provide for so many additional elements and indeed is rather hopeless for even the sixteen rare earth elements. What is to be done with this embarrassment of riches?

Soddy's study of the grouping in well-known families of a number of the better known radioactive elements according to their chemical properties, combined with a consideration of the kind of disintegration by which it was produced led him to a generalization which would enable one to place correctly any radioactive element whose source was known, and at the same time give an approximation as to its atomic weight.

Fajans arrived at the same generalization independently from an examination of the electro-chemical evidence, finding that the product of an α ray change was more electro-positive, while that of a β ray change was more electro-negative. Similar conclusions from various evidence were reached by Fleck and Russell.

The generalization is as follows:

> When an α particle is expelled it carries with it two atomic charges of positive electricity and the expulsion of these two positive charges from the atom affects the valency of the product, as Fajans has pointed out, just as in ordinary electro-chemical changes of valency. If the atom were initially in Group IV., for example, its ion is tetravalent and carries four atomic charges of positive electricity. Two such charges having been expelled with the α particle, the product is in the di-valent Group II., non-separable from radium. The mass in this case is four units less. So with the β ray change. The β particle is a negative electron and the loss of this single atomic charge of negative electricity increases the positive valency of the product by one. Radium B, for example (in Group IV.), expels a β particle and becomes radium C (in Group V.). When-

ever two or more radio-elements fall in the same place in the Periodic Table, then, independently of all considerations as to the atomic mass the nature of the parent element, and the sequence of changes in which they result, the elements in question are chemically non-separable and identical. As will later appear, this identity extends also to most of the physical properties such as volatility and spectrum reactions.[2]

To express this "newly revealed complexity of matter," Soddy has suggested the word isotope. A group of two or more elements occupying the same place in the periodic table, differing in atomic weight yet chemically non-separable, is isotopic. There are possibly seven such elements isotopic with lead. Radium is one of four isotopes. The chemistry of thirty-seven radio-elements is thus reduced to a smaller number of about ten types.

Two fundamental changes in the old views as to the system are indicated here. First, the position of an element is not fixed but can be changed in either of two ways—by a change in valence (which, as is well known, can be brought about in various ways), and again by disintegration due to ray-emission. Secondly, more than one element can occupy a given position in the system. This is independent of the atomic weight, but such elements are chemically inseparable. This involves the giving up of all idea of the properties being functions of the atomic weights and necessitates the formulation of the law anew.

The place occupied by an atom is not solely determined by its mass but by its electrical content as well. According to Soddy, the system represents the chemical character of matter as the function of two variables instead of one. The electrical content is the essential variable in horizontal columns and mass is the essential in vertical columns.

It is somewhat early to raise the question as to whether all elemental atoms are the result of disintegration processes, or, conversely, of synthesis, but in any case the old puzzle remains as to their great irregularity in weight relations if the most accurate chemical determinations are to be relied upon. If the time should arrive when they could be calculated, chemists would naturally return to hydrogen as the standard. Certainly, at present these weights present no simple synthetic relations.

An explanation of this is perhaps at hand if the view of Soddy (and of Crookes at an earlier period and from a different standpoint) is accepted, namely, that in atomic weight determinations it is not a natural constant that is obtained but a mean value of non-homogeneous masses. In other words, the weight may represent the average of various isotopic atoms and not the absolute weight of identical atoms.

It is very fortunate that the simple expedient of arranging the elements in the order of their atomic weights could give the early workers so nearly correct a view of the periodic system. It would probably have remained hidden for a long time if this had not been so prominent a factor in determining the proper sequence. There is undoubtedly a proper sequence. This has been settled hitherto chiefly by consideration of the atomic weight, but also by examination into the relationship existing between the elements. For instance, the order of atomic weights would be iodine and then tellurium, but chemically tellurium belongs to Group VI. and iodine to Group VII. Therefore, the atomic weight order is reversed.

The sequence numbers of the elements, or atomic numbers as they are called, assume a new practical and theoretical importance. Within twenty years after the announcement of the periodic system, some,

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[2] Soddy, "The Radio-elements and the Periodic Law," p. 6.

as Fedaroff, had sought to attach importance to these numbers, but the efforts had little to commend them. Lately it has been suggested by van den Broek that this is a fundamental and important number. Beginning with 1 for K, the numbers would be 2 for He, 3 for Li, 4 for Be, etc. The question then naturally arises, can these numbers be reliably determined without reference to the atomic weights and correcting the manifest mistakes made in following the simple order of these weights?

One method for doing so, though with limitations, lies in the measuring of the scattering of the α particles when passing through different kinds of matter. Geiger found that the angle of the scattering seemed to depend very largely upon the atomic weight of the scattering metal. A very small fraction are scattered through such a large angle that they return on the side of incidence. This deflection is, of course, both a volume and surface effect. For equal thickness of screen calculations based on Rutherford's conception of the atom and his belief that this large angle scattering is due to the near approach of the positively charged α particle to the positive nucleus of the atom of the screen would make the scattering vary as the product of the density by the atomic weight. Thus Rutherford calculated that the scattering by gold should be about fifty times that by aluminium. This has been confirmed by the experiments of Geiger and Marsden, and the relative scattering has been determined for a large number of elements. The phenomenon is manifestly one determined by the electrical content of the atom.

The nuclear charge of the Rutherford atom can be calculated from the α particle scattering at various angles. This charge is found to be one half the atomic weight multiplied by the charge of an electron. The same value was reached by Barkla by observations on X-rays. Soddy concludes that it is the nuclear charge rather than the atomic mass which fixes the position of the element, basing his conclusion largely upon the work of Barkla, Sadler and Moseley, which will be briefly outlined further on. This in reality agrees with the hypothesis of van den Broek that the number of electrons in an atom in the neutral state determines the place of the element if hydrogen has one electron and one nuclear unit charge, helium two electrons and two nuclear unit charges, etc.

The direct method then is a combination of the work of Bragg, Barkla and Sadler, and Moseley. Making use of the work of those first mentioned, Moseley photographed the spectra obtained by the cathode-ray bombardment of a number of elements, the X-rays thus produced being reflected and defined from a crystal face. The frequencies of the vibrations could be determined and this frequency was found proportional to the square of the atomic number. That is, there was a definite shifting in the direction of shorter wavelength in the spectrum of an element from that of the one next above it in the list.

The graphic representation of the system has never been satisfactory in spite of the many efforts to solve it. It is especially difficult to bring out the facts by any representation on a plane surface. The faults of the Mendeleeff table can readily be seen, and they make it very desirable to secure a better mode of expression. And yet it is difficult to use the three dimensions of space so that the average student can grasp the whole. Soddy's lemniscate curve certainly has its good points. This may be compared with the arrangement of Rydberg. It can not be claimed yet, however, that the law or laws underlying this system are known and well understood, and until such time a complete and satisfactory graphic representation is scarcely to be expected. We can agree at least that progress is being made toward such an understanding.

Francis P. Venable

SOME FALLACIES IN THE ARGUMENTS AGAINST FULL-TIME CLINICAL INSTRUCTION[1]

In a recent paper, published in Science, Dr. S. J. Meltzer comments upon two notable facts in connection with the present rather active agitation regarding full-time clinical instructors. The two facts singled out by him are: (1) The appointment of full-time professor of medicine, surgery and pediatrics, by the Johns Hopkins University, and (2) the disparagement of this type of plan by the council on medical education of the American Medical Association. Dr. Meltzer's paper itself constitutes a third notable fact, in that it represents one of the very few unqualifiedly strong appeals that have been made by a clinician in favor of full-time clinical instruction. Although engaged at present in a so-called fundamental research, the current of Dr. Meltzer's life has been clinical to so large a degree, that his conclusions can not be questioned on the ground of academic impracticability. He analyzes the report of the council with logical seriousness; and were it not for the artifice of a single italicized word, one would scarcely feel the flick of Meltzer's lash or realize the seriousness of the attempt of the council to laugh the case out of court. Dr. Meltzer, by rare grace and tact, forges an argument so uncommonly well tempered as to render supportive discussion almost unnecessary. And yet, if there be any force in the plea for full-time heads of clinical departments, it lies in the line of duty of those of us who are clinicians to develop its full strength by discussion.

In such a discussion, as indeed in all such discussions, nothing contributes so much to balance and rationality as does a proper conception of the historical perspective of the problem involved. It is essential to realize at the outset that the question is not a new one involving American medicine alone. Many men would have us believe that suddenly, as a result of this, that, or the other tendency, our clinical instruction in America has been found wanting, and that with typical American impulse we have set to moving in the sacred realm of education, the machinery of experiment. As early as the seventeenth century, Leibnitz attempted to justify his faith in quacks, on the basis that doctors were improperly trained as men of science, and that it was hopeless to look for the development of scientific teachings and methods in a practitioner, *der nichts thut als von einem Patientem zum andern rennen, und wenn er bey dem einen ist, auff den andern schon denket* (who does nothing but run from one patient to another and who, when he is visiting one patient, is already thinking about the next one). Almost a half century ago Billroth anticipated the Flexner report on Medical Education, in his "Ueber Lehren und Lernen," a work necessarily less modern in tone than Flexner's, less broad in the geographical consideration of the subject, but not a whit less emphatic in the assertion of corrective principles. Coming down to more modern times, we have the Report of the Royal Commission on University Education in London (1913) in which it is admitted that "the academic training received by medical students in London has not always been distinguished, and that the scientific spirit has been too often wanting." We in America have also found that, even in our best schools of instruction, the scientific spirit has been too often wanting, and we have found it wanting chiefly in the clinical branches. On this basis rests the agitation for full-time clinical instruction.

The phrase "full-time clinical instruction" signifies that the teaching of each major clinical subject be under the supervision of a properly qualified instructor, who shall serve as the head of his department, who shall devote all his energies during the working

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[1] Read before the twenty-fifth annual meeting of the Association of the American Medical Colleges, Chicago, February 17, 1915.