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# THE RELATION OF PHYSICS TO CHEMISTRY[1]

By Dr. N. V. SIDGWICK

OXFORD UNIVERSITY, NON-RESIDENT LECTURER IN CHEMISTRY AT CORNELL UNIVERSITY UNDER THE GEORGE FISHER BAKER FOUNDATION

I AM very grateful to Cornell University and to Professor Dennis for inviting me to join your staff as non-resident lecturer and for the kindness with which you have received me. It is a high honor to have one's name added to the distinguished list of the Baker lecturers. A lectureship of the kind founded by Mr. Baker is, I think, of real service both to the hosts and to the guests. Francis Bacon gives as one of the three chief conditions of scientific progress "conjunction of labor," the intercourse of scientific men, whereby, as he says, "the frailty of man may be supplied." With the progress of knowledge every branch of it becomes more specialized and yet at the same time more dependent on other branches, and the only way in which the workers in any laboratory can get a true sense of the values of the different kinds of chemical work which are being pursued all over the world is by intercourse with chemists from elsewhere. The benefit to the visitors is equally great, especially when it makes them acquainted with so admirable a laboratory and so distinguished a staff of chemists as you have here. I also appreciate greatly the opportunity of studying your methods of teaching and administration; the only way to find out how a university works is to join its staff, and take part in its labors.

I have chosen "The Relation of Physics to Chemistry" as the subject of my introductory lecture, because it seems to me that there is none on which, in

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[1] Introductory public lecture.

the present state of knowledge, it is more necessary that we should have clear views. While it is common to hear men deploring the increase of specialization, through which, they say, one scientific man can scarcely understand what another is doing, it is nevertheless true that the two great sciences of chemistry and physics have now reached a point at which they are attacking identical problems. The task which we as chemists have before us is no light one; neither chemists alone, nor physicists alone, can solve the problems which face us. We must make use of every assistance that we can get, and the most powerful is that of physics. But if we are to use this to the best advantage, we must understand clearly what it is, and in what ways it can help us.

It is a commonplace that all knowledge is one; its division into separate sciences is an unfortunate necessity, arising not so much from the subject-matter as from the limitations of human capacity. The field is so wide that no one can command the whole of it, and its students naturally break up into groups which concentrate on particular provinces and evolve particular methods for dealing with them. This means in practice that the various branches differ quite as much in the methods of attack as in the problems attacked. Among the sciences concerned with non-living matter, there are the three familiar divisions of mathematics, physics and chemistry. Mathematics deals with number, space and time, abstracted from all questions of what it is that is numbered or what occupies the space; physics with the properties of matter, and primarily with those common to various forms of matter; chemistry with the properties of various forms of matter as related to their chemical composition. But these definitions, as you can see, are very imperfect; in fact, no exact boundaries can be laid down. Each science is crossing the frontiers of the next, and in recent years the interpenetration has been very rapid. Mathematics is becoming physicized; the word ether, which, when I was young, was used to distinguish real or physical from ideal or mathematical space, has almost disappeared, not because the concept of ether has been abandoned, but because we are more interested in real space than in imaginary spaces that might exist. Space and time are no longer independent entities, and for a knowledge of their interrelations we appeal not to *a priori* ideas, but to observations of the positions of stars and the wave-lengths of their light.

If the line separating mathematics from physics is blurred, that between physics and chemistry has vanished. Both sciences are now examining the same problems. It is true that they use different methods, but they apply them to the same materials. It is therefore of fundamental importance for us as chemists that the light which the physicists throw on our problems should illuminate them for us as well as for the physicists.

The distinction between different sciences depends on a very obvious fact, that the simpler the problem you are examining, the more precise is the knowledge you can acquire of it—in philosophical language, the less the extension, the greater the intension. The simplest problems of all are those of the mathematician. His materials—number, space and time—are uniform in behavior; he can isolate his problems from all outside interference. Hence he can state his results with the greatest certainty and accuracy, and carry his analysis to the greatest lengths. The physicist has a more complicated task; he has to take account of the differences in behavior of different forms of matter and of the small disturbances to which any actual system, however carefully isolated, is subject; and he must reckon with the imperfection of his measuring instruments. He is therefore often obliged to be content with approximations to the truth. The chemist is faced with still greater complications. While the physicist can restrict his inquiry to simple systems and to the materials which he finds most tractable, the chemist is compelled to extend his work to all forms of matter, or let us say in the first instance to all pure substances. Having this great mass of material to handle, his knowledge of its behavior is necessarily less detailed, less accurate, less deducible from first principles than that of the physicist, and in a still higher degree than that of the mathematician.

The series does not end with the chemist. The relation of the biologist to the chemist is like that of the chemist to the physicist, or of the physicist to the mathematician. He has to deal with structures elaborately built up of a variety of chemical substances, solid, colloidal and liquid; he can penetrate less deeply into these greater complexities.

The truth is that there is a scale of complexities from mathematics to biology—a scale not involving any gradation of moral or intellectual merit; whichever step the man of science stands on, he can rebuke those on one side of him for neglecting the complicating factors which affect all real phenomena, and those on the other for failing to see as deeply into the broader subjects of their inquiry as he himself does into his simpler problems. The accusation is equally true and equally pointless in each case. What we need to learn is not the weaknesses of our allies, which are very like our own, but their strength; we must discover in what ways they can be most serviceable to us.

To this end we may briefly consider how physics has helped chemistry in the past.

In one sense every chemical statement is also physical; it involves a physical background just as every quantitative statement of whatever kind involves a mathematical background. But apart from this general relation we can distinguish three periods in the history of chemistry as related to physics. From the earliest times when any real chemical theory existed—which for practical purposes means from the promulgation of the atomic theory at the beginning of the nineteenth century—down to about 1885, the chief service of physics to chemistry was the establishment of the existence, and the determination of the relative sizes, of molecules. Avogadro's hypothesis was essentially physical, and although chemists as a whole (Faraday is a marked exception) disregarded it for nearly forty years, they lost heavily by doing so, and it was only when Cannizzaro in the fifties demonstrated its importance to chemistry that a real knowledge of molecular composition, the necessary preliminary to a knowledge of structure, became possible. A little later, in 1874, came the definite physical proof of the soundness of the basis of the chemical molecular weights. These were all founded on the assumption that the molecule of hydrogen contained two atoms. Of this there was no positive evidence; the assumption was generally accepted because it was found to explain the facts; but it always remained possible that the hydrogen molecule contained four atoms, and that the number of atoms in all molecules was twice as great as was supposed. In 1874 Kundt and Warburg measured the ratio of the specific heats of mercury vapor at constant pressure and constant volume, and showed that its molecule could not possibly contain more than one atom. It was already known that there were twice as many atoms in a molecule of hydrogen as in a molecule of mercury, and so the final proof of the truth of the molecular theory was supplied.

For twenty-five years after Cannizzaro's paper the energies of chemists were largely devoted to developing the new theory of chemical structure, and to building up on this foundation the great edifice of organic chemistry. On the inorganic side the recognition of Avogadro's principle led to the assignment of the true atomic weights, and as soon as this had been effected the Periodic Classification necessarily and rapidly followed.

Then came in 1885 the second great application of physics, the introduction of thermodynamics into chemistry. The first investigations were indeed some thirty years earlier, and Willard Gibbs had already (1875–1878) published those far-reaching conclusions which were to prove so fruitful in chemistry and physics in later years. But the main development came from van't Hoff. He applied the methods of thermodynamics, based on the general principles of energy, to a large range of chemical phenomena. The most immediately important application was to the behavior of dilute solutions. He realized the great suitability of osmotic pressure for thermodynamic treatment. By means of an ideal engine precisely similar to the classical heat engine of Carnot, but with a solution separated from the solvent by a partition permeable to the solvent alone, he was able to establish, on the experimental basis of Henry's law of the variation of the solubility of a gas with the pressure, the relation between the molecular concentration and the osmotic pressure, and further the relation of this to more easily measurable properties of the solution, the lowering of the vapor pressure, the rise in the boiling point and the fall in the freezing point. He was also able to give a proof of the law of mass action, which had been established empirically some twenty years before. These discoveries initiated the subject of chemical thermodynamics, which has guided so much of the later developments of the science; and they ultimately led, in the hands of Nernst and others of van't Hoff's successors, to the third law of thermodynamics and the chemical constants, and to those investigations of activity which are still in progress.

Perhaps the most immediately important result of this work was the rise of the theory of electrolytic dissociation. Van't Hoff had shown what was the normal behavior of a solution. Experiment proved that while many solutions behaved as this theory required, those of salts in water did not; and their abnormality was always of the same kind; the salt appeared to form more molecules in the solution than corresponded to its formula. The explanation was given (1887) by Arrhenius, who argued that just as the abnormally low molecular weights indicated by the vapor densities of some gases were assumed, and had been proved, to be due to dissociation, so we must suppose that a salt dissociates in water; and since sodium chloride, for example, can dissociate only into its two atoms, and normal sodium and chlorine atoms can not exist side by side in water, it must form charged ions of the two elements, a conclusion supported by the whole electrical behavior of the solution. The precise form which Arrhenius gave to the theory was, as we now realize, very imperfect; but no one can doubt that by the recognition of a new kind of chemical change, and of a type of molecule peculiarly reactive, he gave an immense impulse to the development of chemistry.

The discoveries of van't Hoff and Arrhenius were immediately followed by two events which are generally taken to mark the birth of physical chemistry as a primary division of chemistry, the call of Ostwald to Leipzig in 1887, and the foundation in the same year of the *Zeitschrift für physikalische Chemie*. Ostwald was in the very first rank as a teacher, if not quite as an investigator, and he was indefatigable in spreading the light of the new science, which yet was really no new science, but as Nernst says,[2] rather the union of two previously separated sciences. The work of the Leipzig school and their followers was largely along lines that had for many years been open for traffic, but had not been used; it consisted in making more precise the physical background which, as I said, underlies every chemical statement. The physical properties of chemical substances and their solutions, and the conditions of their reaction, were measured in detail, and the whole of chemistry assumed a more quantitative aspect. In organic chemistry the enormous variety of new compounds which it was found possible to prepare, and the wonderful success of the structural theory in classifying them, still gave its students plenty of occupation on the qualitative side; but here too the application of the new ideas to explain the behavior of organic compounds was undertaken by Hantzsch with the most illuminating results, and was extended later with great effect by Dimroth and others.

The most important developments of the new science were, however, on the lines of thermodynamics and of the ionic theory. It is a remarkable sign of the predominance of the thermodynamic aspect at this time that Ostwald actually proposed to abandon the idea of atoms altogether; he conceived that he had provided an alternative explanation of the laws of chemical combination, involving no atomic theory but substituting the concept of "equivalent weight," whose meaning was not subject to discussion. By the irony of fate, this doctrine of Ostwald's was propounded exactly at the time when the physicists began their triumphant attack on the problem of the structure of the atom. Van't Hoff had a truer insight into the fundamental problem of chemistry. He pointed out[3] that all natural phenomena may be looked at from two points of view, the thermodynamical and the molecular or atomistic. The nature of a thermodynamic argument is very peculiar. It is based on the fundamental principles of energy, which are as certain as anything we know in science. It lays down conditions of energy change to which a process must conform, granting certain external conditions, whatever its internal mechanism may be. This has the great advantage that, provided the deduction is carried out correctly, which in the simpler instances is not open to doubt, the conclusions are quite certain, and do not depend on the truth of any theory of the process. But for this very reason it does not enable us to decide between two rival mechanisms, provided they can both give the same energy result. And in particular, it takes no account of the time; the ideal processes of a thermodynamic cycle occur reversibly, that is with an infinitesimal driving force, and hence would in fact require an infinite time. So while thermodynamics tells us what the result will be, it does not tell us how we get there or how long it will take.

Of the immense importance of thermodynamics as a calculus there can be no doubt; it lays down conditions to which every true theory must conform, and thus eliminates many false ones; and it has further the great practical use of enabling us to determine a property which it is difficult to measure directly, by observing some thermodynamically related property which is more accessible, as when we determine the osmotic pressure of a solution by observing the change of its freezing point, or the heat of dissociation of a gas from the change of density with temperature. But it only answers half our question; it does not tell us what the molecules are doing in a chemical process; as Ostwald's argument showed, it does not even involve the assumption that there are any molecules. For the proper development of chemistry, the thermodynamic side must be supplemented by the molecular-mechanical.

At the time of which I am speaking, the latter years of the nineteenth century, this second side of the matter could not be developed in great detail, owing to the scanty knowledge which we had of the molecule. The relative masses of molecules and the number of atoms which they contained were known with accuracy; but for their absolute masses only the roughest approximations were available, and of the structure of the atoms, and the mechanism which holds them together in the molecule, nothing was known at all. The discovery of the electron in 1897 was the first proof that the atom had any parts.

In the year 1900—an easy date to remember—came the greatest revolution that physics has ever known, the discovery of the quantum by Planck; and this marks the beginning of the third period in the relations of physics to chemistry. Up to that time all physics had been based on what we now call the classical mechanics of Galileo and Newton. This theory had arisen from the observation of the motion of visible bodies on the surface of the earth—weights and pendulums—and had then been extended to the planets, and shown to be equally true of their motions. It had sustained the whole triumphant march of physics through the ensuing two centuries. It was universally assumed, and as it seemed with complete justification, that these principles, which had been

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[2] Lehrb., 1893, 1st ed.

[3] "Lectures on Theoretical and Physical Chemistry," 1898, Vol. I, p. 12.

shown to apply alike to the motions of pendulums and of stars, were equally applicable to all kinds of matter, down to its smallest particles. But towards the end of the last century difficulties had arisen in applying these principles to certain classes of phenomena, especially to those dealing with the relations of radiation and matter. To give only one example, it could be shown on the classical mechanics that the energy of radiation must pass almost entirely into the shortest waves, so that the most intense radiations of a hot body should be in the far ultra-violet; while experiment showed that for ordinary hot bodies the maximum was in the far infra-red, and that even the light of the sun with a surface temperature of 6,000° has its greatest intensity in the yellow, as any one can see by looking at it.

To meet these difficulties Planck put forward the quantum theory, of which the essence is that the interchange of energy does not take place continuously, but in separate steps or quanta, the size of which is not fixed like that of an atom, but is proportional, for radiation at least, to the frequency of the oscillations or waves of which the energy consists. This theory, the truth of which has been completely established by the subsequent development of physics, leads to a remarkable conclusion. It applies of course to all bodies large or small, but in its practical results it leads to one conclusion for large bodies and another for small. The quantum itself is always, in comparison with quantities of energy that we observe in ordinary life, very minute. Thus of the quanta of yellow sodium light it would take $10^{-16}$—ten thousand million million—to heat a milligram of water one degree. It follows that when we are considering masses of matter and quantities of energy such as we can see or handle, the "steps" by which Planck replaced the continuous process of Newton are so small and so numerous as to make no practical difference. Hence for the mechanics of all such "macroscopic" quantities of matter the new theory leads to the same results as the old. This is indeed to be expected; the Newtonian theory has been verified for such bodies, and for them it is true; but for very small bodies, and especially for atoms, the steps become significant, and the theory is not true. This is far from meaning that the new theory is of no practical importance. Our whole lives depend on processes which, although they occur with weighable quantities of matter, really depend on the simultaneous occurrence of an enormous number of atomic interchanges of energy, and these can only be interpreted by means of the quantum theory. It is precisely in chemistry that we have to deal with phenomena of this kind. The first direct evidence of the quantum theory, though of course it was not recognized as such at the time, is Dalton's law of multiple proportions. The "ratio of two small numbers," which we have to introduce in expressing this law, is the fundamental characteristic of the quantum theory. It was no accident, but a basic necessity, that made Ostwald's attempt to eliminate the atom break down when he came up against the laws of multiple and reciprocal proportions.

The recognition of the true mechanics of the atom was a necessary preliminary to any detailed knowledge of atomic structure. The main constituents of the structure had indeed been discovered without the help of the quantum theory, the electron in 1897 and the nucleus in 1911; but their interactions could not be worked out as long as the older mechanics of Newton and Maxwell was used; in fact, on these principles the nuclear model of Rutherford was impossible, and it was only after Bohr had shown how to apply the quantum theory to the atom that further progress could be made. How rapid this progress has been in the last 20 years we all know; it has finally broken down any distinction in subject-matter between physics and chemistry, and the elucidation of molecular structure has now become the task of both sciences.

This brief account of the services which physics has rendered to chemistry in the past may help us to realize the true relation of the sciences to one another. The opposition of extension and intension—the rule that the simpler the problem, the more completely we can solve it—still holds. If we call chemistry molecular physics, we may say that the physicist is applying his more deductive methods to its simpler aspects, while the chemist is simplifying its more complicated phenomena by observation and induction. The practical use of a discussion such as this, which is addressed primarily to chemists and not to physicists, is to get a truer conception of the way in which the chemist should pursue his subject, and of the extent to which he should be influenced by physical conclusions. On the latter point the position is clear; we are bound to make use of any physical weapon that is available for the solution of our problems. On our side we have a duty both to physics and to ourselves. In the first place we have to present to the physicist in a simplified form those questions arising out of our chemical experience which he is best able to solve. The multiplicity of chemical phenomena is so great that only those who have given their whole attention to the subject can really know the facts relevant to a particular chemical question. We have therefore to collect and coordinate the data bearing on the phenomena which are accessible to physical attack. We also have to remember that we chemists are, so to speak, responsible for all chemical compounds. The physicist selects a few compounds peculiarly suitable for his measurements, and acquires detailed knowledge about their structure and behavior. We have to review the whole field of chemistry, and to see how far these conclusions can be extended to chemical substances in general, and if they can not, to find if possible what chemical characteristics limit this application.

At the same time we have to go on with the work of educing general principles out of the great mass of chemical particulars. In the course of this process of simplification it is never possible to proceed very far without forming some idea of the actual mechanism which is at work, or in other words without forming some hypothesis and imagining some model of the molecule. The physicist proceeds in the same way; but his simpler problems, lying nearer to the ideal systems for which the complete dynamics can be worked out, make possible a more detailed theory and a more precise model. This does not mean that they are better than those of the chemist, if by better we mean more suited to the advancing of knowledge; there is a place in the growth of science for both. Every theory and every model is imperfect. As Bohr has pointed out, a model of an atom or molecule is a machine of macroscopic size which is supposed to behave in the same way as an atom or molecule. But such a model, owing to the magnitude of the energies and motions of its parts, will act on the Newtonian laws of mechanics, while the atom is subject to the quantum restrictions. An exact agreement between the model and reality is therefore not to be expected; we can only try to make the differences as small as possible. This is the position of the Bohr model at the present moment. It is built on classical principles, and then a new condition is imposed, forbidding all forms of motion which do not comply with the quantum principles. This new condition is wholly arbitrary, in the sense that it does not follow from the construction of the model; but it is necessary in order to make the model work right.

This model has shown an amazing power of behaving like a real atom; the experimental results can be shown to agree with those predicted even in quite small details. But with the rapid development of atomic physics the model has not been found equal to every demand made upon it. This was partly due to the difficulty of calculating the behavior of such a model in any but its simplest forms; and so far we might hope that the difficulty would be removed by improvements in mathematical analysis. But there were more serious troubles; in certain respects the conclusions derived from the model were shown to be definitely wrong; for example, it represented the hydrogen atom as a disc, while it could be shown experimentally that it was a sphere. This does not necessarily mean that the model is entirely wrong, but only that it is imperfect. In the last few years a new method of attacking atomic structure has been developed, that of wave mechanics. This can hardly be said to involve a model at all; or if it does, it is an elusive form of the Rutherford nuclear atom, in which the stationary states of Bohr are maintained, without their physical meaning being clearly expressed, although it is mathematically defined. But whatever we may think of the new model, the efficacy of the mathematical calculus involved is indisputable; it makes it possible to predict a whole series of properties which were inaccessible by the older method, and which can be verified experimentally. It is obvious that the equations of the new wave mechanics express the truth very closely, and are of immense practical value; and we may hope that as our knowledge increases it will become possible to represent them by a definite model—perhaps some modification of the Bohr model—which will bring the structure more clearly before us.

I have discussed the atomic model at some length, partly because of its intrinsic interest, but largely because an understanding of the conditions and limitations of a physical theory will help us to grasp those of a chemical theory. A chemical theory, dealing with more complicated phenomena, is less accessible to mechanical treatment. It takes account in the first instance of properties which can not be measured quantitatively, but which are clearly shown to exist. It adopts some terminology to express these, without at first making any exact assumptions as to their physical meaning. This after all is what physics has done with the quantum; we don't even know what the quantum limitation really means, although we know what effect it produces. As an example of a chemical theory consider the theory of structural chemistry. This in its original form assumed the existence of linkages between the atoms in a molecule, the nature of which it did not pretend to discuss, though it could make accurate statements as to their number, and as to the order in which the atoms were linked. It was capable of predicting the composition and many of the properties of the substances formed in innumerable organic reactions, and of consistent application to hundreds of thousands of organic compounds. "Chemists," as Helmholtz[4] said in 1891, "must be allowed to form hypotheses after their fashion, since the whole extraordinarily comprehensive system of organic chemistry has developed in the most irrational manner, always linked with sensory images, which could not possibly be legitimate in the form in which they are represented." A direct result of these "sensory images," that is, of the simple model of atoms joined by links of an unspecified physical

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[4] Koenigsberger's ''Life,'' English ed., p. 340.

nature, was the further development begun by van't Hoff, when he extended these ideas to three dimensions, and opened up the new field of stereochemistry. As knowledge increased, new relations were discovered in the behavior of these links. Baeyer in his strain theory assumed that two links of one carbon atom had a "natural" inclination to one another, that given by the tetrahedral model, and that any departure from this involved a proportionate degree of instability in the molecule. This conclusion could not be deduced from the nature of the link, because that nature had not been physically defined; but it was justified by the fact that its consequences agreed with experiment.

During the further study of the reactions of organic compounds, it became possible to classify to some extent the effects which are exerted on the reactivity of certain atomic groups by other atoms or groups present in the same molecule. To express these conclusions new symbols were adopted—plus and minus signs, or thick and thin bonds. All these developments were perfectly legitimate if they made it easier to coordinate the results of experiment. No assumption had been made as to the physical meaning of the valency bond, and the new theories only implied that this force of unknown character is found experimentally to be capable of certain modifications, which are expressed by the new symbols. This is typical of a chemical as opposed to a physical theory; it arrives by induction from experiment at a series of relations between the structures of molecules and their behavior, and shows that these can be simply explained by a small number of assumptions as to the forces between the atoms; but it makes no statement as to the physical meaning of these forces and their modifications.

At the time when these theories of reactivity in organic compounds were being developed, the physicists had arrived at a theory expressing the valency forces in terms of electrons; in particular G. N. Lewis had shown that the non-ionized links, with which the organic chemists were mainly concerned, could be ascribed to the sharing of the valency electrons, two to each link, between the atoms. This theory was itself in some degree symbolic; no one knew precisely what was meant by sharing—we are only now, 15 years later, beginning to learn what it means—but it was possible on the Bohr model to get some general idea how it might happen, and as physicists were by this time able to count exactly the number of electrons in the atom and to determine what groups of electrons were stable, the Lewis theory could be extended very widely; and it was found to give satisfactory results, and to involve no assumptions as to the physical nature and behavior of electrons incompatible with physical experience.

It was quite evident that the explanation of the differences which the organic chemists had detected in the links must ultimately be found in the behavior of the valency electrons; and the organic chemists hastened to look for it there. But at this point we come upon a difficulty. As long as the chemist confines himself to his symbolic representations, he can do what he likes with them, so long as what he does helps him to classify and coordinate his ideas. But as soon as he claims to give them a physical meaning, he must recognize all the implications of a physical statement. Links or bonds may be strained, or thickened, or imperfectly saturated, or classified into primary and subsidiary, and the atoms they join may have a positive or a negative character, because these words correspond to real differences in behavior, and therefore to some change in the forces between the atoms, which we may hope to explain when we know what these forces are. But electrons must behave in certain ascertained ways, and the distribution of positive and negative electricity in a molecule is subject to physical laws and measurable by physical means. It can not be denied that this requirement has sometimes been overlooked. It was said of one well-known theory of molecular structure, which did very good service in its day, that the author's electrons "had so few of the known properties of electrons that it is not immediately clear why they are called electrons at all"; and the same might be said with equal truth of some other theories.

Thus the transition from the chemical to the physical theory needs care. The ultimate object of the chemist is to express his conclusions in physical terms, but he must remember, if he tries to do this, that these terms have already a very elaborate and precise connotation; every concept which he uses involves a series of definitely established properties. That in fact is why it is so important to be able to use them. But it is essential to use them rightly. The chemist must not employ the language of physics unless he is willing to accept its laws. Within these laws a certain latitude of interpretation is left to him, and some tentative physical suggestions may be put forward unsupported by physical evidence, provided the physical evidence does not contradict them. On this last point no exceptions are allowed. The chemist must resist the temptation to make his own physics; if he does, it will be bad physics—just as the physicist has sometimes been tempted to make his own chemistry, and then it was bad chemistry.

If these points are clearly realized, the prospects of progress in chemistry are far more favorable now than they have ever been. The ultimate problem of the establishment of the relation between molecular structure and properties is open to attack from both sides, and these attacks are now converging. We have found that the mechanics of the atom is different in many ways from that of large bodies, and we—or they—have found what the mechanics of the atom is, or at any rate how its results can be calculated. Physics has already told us the "empirical formulae" of the atoms, the number of electrons which they contain and their dispositions. It has given us a mechanism of atomic linkage. It has provided us with methods of measuring many of the characteristic properties of the links between atoms, the distance between the atoms, the relative positions in space, the way in which the electrons are shared between the atoms, the work required for their separation. The problems before us are far too complicated to be solved by physicist alone—by deductive reasoning founded on experiments with a few selected compounds. But much of the information we need he has shown us how to obtain; if we cooperate heartily he will provide us with more; and in this way our theories can be tested and amended by physical measurements and physical reasoning at every step. All that is needed is a proper mutual understanding and good-will.

# EDWARD W. MORLEY, CHEMIST, INVESTIGATOR, TEACHER

(Some Personal Notes)

By CHARLES FRANKLIN THWING
PRESIDENT EMERITUS OF WESTERN RESERVE UNIVERSITY

OF the many scholars, scientific, classical, linguistic, historical, philosophic, sociological, who were my college associates for more than thirty years, none was more learned, more illustrious, more devoted, than Morley.

Edward Williams Morley was a child of the manse. He was also a graduate of Andover Theological Seminary. The principles underlying his religious parentage and training were the fundamental and permanent elements of his character. But early in his service as a minister (in Twinsburg, Ohio), he was offered a professorship in Western Reserve College in the neighboring town of Hudson. For in this service he had proved that his interest was rather scientific than theological or clerical. The foundation bore the traditional title of "Natural History and Chemistry." The professorship under this and other titles he held until his retirement in the year 1906. His teaching covered forty years.

Morley united, as not many college professors do unite, great power as a teacher with equally great power as an investigator. His power as a teacher was primarily found in his knowledge, and quite as fundamentally in his devotion to the individual student. His power as an investigator is, of course, illustrated in his devotion to his many and diverse researches. His power as a teacher lives, and lives as long as do the lives of the hundreds of students whom he taught, and to whom he gave intellectual quickening. His work as an investigator relates to at least two fields of nature. In one of these fields his work is completed and is done apparently unto conclusiveness. This work has given him place among the greatest of scientists. In the other field his work still progresses. The first field relates, as says his successor Professor O. F. Tower, to "The densities of oxygen and hydrogen and the ratio in which they combine."[1] The field in which the work is still going on is the field associated with the name of Einstein. In the second field he collaborated with Professor A. A. Michelson "In developing the interferometer, an instrument for measuring lengths in terms of the wave-length of light. They used this instrument to determine the relative motion of the earth and the luminiferous ether."[2] With Professor W. A. Rogers he worked in measuring the expansion of metallic bars; and also with Professor Dayton C. Miller, of the Case School of Applied Science, he experimented upon the "velocity of light in a magnetic field." In all these and other experiments he became associated with his friend, Charles F. Brush, and with Elias Loomis, of Yale, who, long before Morley, was a professor in Western Reserve College. The Michelson-Morley cooperation and the earlier Loomis-Morley cooperation are among the outstanding partnerships in scientific research. Great in his discoveries and inventions, Morley was also great in his associates, and they also were made great through and in him.

These facts both prove and illustrate the breadth of Morley's mind. His interests and devotions were many, his chief interest however lay in the field of the physical sciences. His intellect was at once comprehensive and concentrated. He recognized the differences between a vocation and an avocation. His avocations, however, were several. He knew and loved music. Playing the organ at the chapel service was one of his minor services given to the college at Hud-

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[1] O. F. Tower, "Edward Williams Morley," *Western Reserve University Bulletin*, August, 1923, p. 59.

[2] *Ibid.*, p. 61.