PRESIDENTIAL ADDRESS.

Delivered at the Annual General Meeting held at the University of Manchester on March 31st, 1938.

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Retrospect and Prospect.

There has probably never been a time when the prospects of chemical science were so splendid as they are to-day. Those who, like myself, began the study of chemistry some forty-seven years ago, are filled with envy of the younger men and the great future of scientific research that lies before them. When I went in 1893 to study organic chemistry under the great Johannes Wislicenus in Leipzig, what I might venture to call the classical period of that science was in a state of high development. Wislicenus was a famous exponent of stereochemical concepts, and I learnt much during the time I spent in his laboratory. I remember proposing to him that I should investigate the action of iodine on the silver salt of hydrazoic acid or azoimide, with the object of obtaining the molecule N_6. Very politely but firmly the great man declined to allow me to try this idea. Probably he foresaw damage to his laboratory and the possibility of having to pay my funeral expenses. Whether I was discouraged by this extremely wise decision or whether it was that many of my friends were engaged on research in a certain rival establishment in the same city, I moved in the late summer of 1894 to the laboratory of Wilhelm Ostwald. No doubt a certain eminent authority in England would have regarded this as a *facilis descensus Averni*. However, being a very young man, I went with the lightheartedness of youth and became a physical chemist of the Arrhenius–Ostwald–van 't Hoff school. The young men of to-day can scarcely realise the state of affairs at that period. To many of the excellent Professors of Chemistry, the solution of a quadratic equation was a somewhat mysterious operation, and a dy/dx a thing which by all the rules of algebra should be simplified by cancelling out the d's. Ostwald was regarded by many good and worthy men in this country as a strange sort of mathematical theorist, whereas in reality he was a very excellent and practical experimenter, who made comparatively little use of mathematics. Indeed, if my memory serves me right, in one of his lectures he accomplished the feat of deducing the second law of thermodynamics from the first. The following example may serve to show the stress that Ostwald laid on practical handicraft in his laboratory. Having to measure the e.m.f.'s of certain cells, I asked the laboratory "Diener" for a potentiometer and galvanometer. The answer was that I had to make such things myself; and so indeed it was, though I doubt if my methods of electrical soldering were of an orthodox character. Instead of a galvanometer one constructed Ostwald's form of "capillary" electrometer.

I need scarcely remind you that at that time the early Arrhenius theory of ionisation in aqueous solutions was one of the chief themes of research in Ostwald's laboratory. For my Ph.D. thesis I had to measure colorimetrically the depth of colour of aqueous solutions of violuric acid containing various colourless organic acids. Fortunately for me, the results agreed fairly well with the simple ionic theory, though neither Ostwald nor I knew that a "tautomeric" equilibrium was also involved. I had, of course, to design and make my own colorimeter. Although the fact did not at all agree with the ionic theory of that period, I noticed that sodium chloride measurably affected the colour of a solution of violuric acid. This was a comparatively early example of a "neutral salt" effect. I could not explain it, but was much cheered by a kindly letter from Arrhenius in which he pointed out that he had observed similar neutral salt effects in some of his own investigations.

After completing my work in Ostwald's laboratory, I had the good fortune to be able to spend a year in van 't Hoff's laboratory in Berlin. Those were the days when he was beginning his famous series of investigations on the conditions of formation and stability of the "Stassfurt" salts. It was a great revelation to me to see how by means of comparatively simple experiments and the simple (essentially thermodynamic) theory of heterogeneous equilibria, van 't Hoff was able to trace out the various regions of stability, the

" crystallisation paths," and the invariant points of the systems he was studying, *i.e.*, the chlorides and sulphates of Na, K, Ca and Mg in presence of water.

In those far-off days physical chemistry was a comparatively simple branch of science. It was not very much influenced by the kinetic theory of matter as treated by the theoretical physicists. Ostwald once asserted in his lectures that there were only three men in Europe who understood—or thought they understood—this theory. Hence it was not of any use, and unlikely to be true. Indeed, the only part of theoretical physics that really aided chemistry at that time was thermodynamics. As everybody knows to-day, the thermodynamic theory of homogeneous and heterogeneous physico-chemical equilibrium had been completed by J. Willard Gibbs in 1878. His treatment was, however, too difficult for chemists to understand. Probably very few chemists read the books of Duhem and Planck, though the work of Le Chatelier in France and Helmholtz in Germany had a considerable influence. It was, I think, the writings of van 't Hoff and Nernst's celebrated " Theoretische Chemie " (published in 1893) which first made the majority of chemists realise the value of thermodynamical principles and methods.

The period 1883–1913 might, perhaps, be called the " thermodynamic age " of physical chemistry. The development of the American school by Noyes and Lewis, the famous work on heterogeneous equilibria of Roozeboom, Schreinemakers, and van 't Hoff, Haber's great treatise on the thermodynamics of technical gas reactions and Nernst's discovery of his third law were important landmarks of that period. It is amusing nowadays to recollect certain minor extravagances. Ostwald and some of his followers made a graven image of energy and banished atoms and molecules as fictions of the imagination. Chemical reactions were to be described by a sort of operational calculus with sets of simultaneous equations. I remember a certain meeting of the British Association at which Professor Divers gave an address somewhat in that vein, and how this infuriated Professor Dewar. Ostwald, greatly daring, actually gave a lecture at our Chemical Society in which he explained that atoms and molecules could be dispensed with. I fear that the fellow-countrymen of Dalton were both unconvinced and horrified. It is only fair, however, to mention that at a later period—largely due, I think, to the work of Perrin—Ostwald admitted the existence of atoms and molecules as discontinuities of energy.

We know to-day that Einstein's demonstration of the equivalence of mass and energy fully justifies the view that atoms and molecules are special and very high concentrations of energy, so that, from the point of view of the energy concept alone, Ostwald's final conclusion was a correct one. It is also very interesting to observe that in several recent papers and in his recently pubished " Physical Chemistry " Professor Brönsted seems to have put in a precise, novel, and very elegant form a great deal of what the early " Energetiker " strove to express in a vague and imprecise way.

In spite of the enormous development since 1913 of both fact and theory concerning the intimate nature and structure of atoms and molecules, it is a rather striking fact that the period after the war has witnessed the appearance of a number of extremely valuable and important books dealing with " pure " thermodynamics and its application to chemical science. In support of this statement I need only mention the treatises written by Lewis and Randall, Partington, Schottky, Ulich and Wagner, E. A. Guggenheim, and the previously mentioned work of Brönsted. There is, therefore, some justification for the statement that the study of thermodynamics still remains an important part of the training of the young chemist. Fortunately, or perhaps unfortunately, for him the scene is now rapidly changing—developing would be a better word. It is well known that the science of statistical mechanics founded by Maxwell, Boltzmann, and Gibbs provided a statistical basis for thermodynamic laws. For a long time this " rational " basis of thermodynamics remained a sort of caviar to the chemist. Nowadays, however, modern knowledge concerning the nature and possible states of atoms, ions, and molecules and modern quantum mechanics have transformed the older statistical mechanics and combined with it to produce a great statistical science of physico-chemical equilibrium which transcends and at the same time interprets and illuminates the earlier science of thermodynamics. We are fortunate in having in this country, in the person of Professor R. H. Fowler, the greatest exponent of modern statistical mechanics, though probably not many chemists can understand and digest his famous book on that subject. Nevertheless the time has already come when modern quantal statistics must become an essential part of the knowledge of the young chemist who aspires to a fundamental grasp of chemical science as it exists to-day.

To those older men who, like myself, have lived through the period **1893—1938**, it seems almost unbelievable how the rapid progress of physical discoveries and theories has in many respects revolutionised the science of chemistry. I remember at the Liverpool Meeting of the British Association in **1896** deserting the chemical section to listen to the discussion in the physical section on the then newly discovered Röntgen rays. Soon afterwards came the discovery of the negative electron by J. J. Thomson, the discovery of radioactivity and radium in France, and the famous work on the radioactive transformation of atoms carried out by Rutherford and his collaborators at Montreal and Manchester. These were great discoveries that the chemists of that period could understand, and we realised, perhaps at first a little slowly, that a vast extension of chemistry was in progress. The award to Rutherford of the Nobel prize for *chemistry* showed very clearly what the Swedish Academy of Sciences thought about the matter. It was understandable, I think, that few chemists realised the importance of Planck's discovery of the quantum of action in **1900**. The distribution of energy in the spectrum of full radiation seemed to have little to do with chemical science. I remember hearing some of my physicist friends talking in the early years of this century about the new Boltzmann–Planck statistics. Thank goodness, I said to myself, that is mathematical physics and not chemistry !

Important events soon followed, mostly due to Einstein and connected with this mysterious and apparently un-chemical quantum : the theory of the specific heats of solids, the photo-electric effect, the theory of light quanta, and Einstein's photochemical law. It became clear that the blessed science of thermodynamics could not cope with these important new discoveries relating to " individual " events and apparent discontinuities, and that a new physical chemistry was rapidly being created. However, it could still be regarded as very " physical " chemistry by most of the members of this Society, something that need not worry a sound practical chemist. Then came the bolts—or rather stars—from Heaven's brightest blue; the famous papers published in **1913** by Niels Bohr. The mysterious quantum had now invaded the atom, that sacred property of *all* chemists. Writing to Chaptal in **1791**, Lavoisier could say (*i.e.*, of his new anti-phlogistic chemistry) " Toute la jeunesse adopte la nouvelle théorie et j'en conclus que la révolution est faite en chimie." One might say the same about Bohr's great discovery of the " quantised " Rutherford–Thomson atom. Soon afterwards came, unhappily, the war and a halt, or at all events a slowing down, of some four to five years' duration in the advance of chemical research. When the last shot was fired in November, **1918**, I think there can have been very few who had any conception of what the following twenty years held in store for our science. The rapid advance of the electron theory of the " Rutherford–Bohr " atom under the influence of the earlier quantum theory brought a vast enlightenment concerning the nature and relationships of the elements. Then, when the physicists were still drawing pictures of the orbits of their revolving electrons, two chemists, G. N. Lewis and Irving Langmuir (and one must not forget Kossel), utilised the electron theory, without worrying much about orbits, in a wonderful attack on the fundamental chemical problems of valency and molecular structure. It is related that after Langmuir had given a lecture on this subject in Cambridge, Mrs. Langmuir turned to Rutherford and asked him what he thought about it. " Oh," said Rutherford, " a very interesting lecture on botany." As you all know, this new electronic theory of valency and the chemical bond was expounded in G. N. Lewis' remarkable book, " Valence and the Structure of Atoms and Molecules," published in **1923**. I think one may say that this particular and most important period of advance in chemical theory reached a certain culminating point when Sidgwick published in **1927** his famous book on " The Electronic Theory of Valency." I must not of course forget to mention that about this time certain eminent organic chemists in this country, *viz.*, Lapworth, Robert Robinson, and Ingold, were using electronic and similar concepts in an important development of the structure, polarity, and reactivity of organic molecules. So far, however, all this was comparatively simple. After a hard day's work, the tired industrial or professional chemist could sink into his armchair of an evening and read about these matters without tears. Sterner events were not far off, for in the years 1924—26 came the new atomic theory and the new quantum mechanics, and already in 1927 Heitler and London had begun work on the chemist's molecule. Since that time there has been a correspondingly rapid advance in chemical science. What does our excellent friend, the hardworking industrial or professional chemist (or for that matter any chemist who has left the University for six or seven years), have to encounter now in his armchair? Wave-functions, eigen values, potential energy curves, resonance, etc., etc.! It is not as if he might perchance say to himself, behold I am not a physical chemist but just a decent organic chemist (if that branch of chemistry forms his special study). I fear that this line of defence will not do, for organic chemistry, offering as it does the greatest wealth of material, is rapidly becoming one of the most important fields for the application of modern physical theories.

The advance in both theories and facts is now so rapid and in many respects so novel that it becomes increasingly difficult for a chemist to follow current scientific literature. He has at his disposal, of course, the *Annual Reports* published by this Society, and the excellent *Chemical Reviews* published by the American Chemical Society. Apart from many good textbooks which are now available, I can recommend to him such periodicals as *Die Naturwissenschaften*, *Die Ergebnisse der exakten Wissenschaften*, and *Science Progress*. The most modern branches of physical science of importance to the chemist are treated very fully in the volumes of the "Hand- und Jahrbuch der chemischen Physik," edited by Professors Eucken and Wolf. I hope that the excellent series of monographs begun under the editorship of Professors Mark and Polanyi, and entitled "Die chemische Reaktion," will be continued.

When visiting many years ago the engineering works of Messrs. William Allen & Sons, Bedford, I was very struck with the fact that the famous electrical engineer, Dr. Gustav Kapp, was regularly engaged in giving lectures to the staff on advances in electrical science. I came across a somewhat similar state of affairs when visiting (in 1923) the Research and Development Laboratories of the Bell Telephone Company in New York. I found that Dr. Karl K. Darrow gave regular courses of lectures to the research staff on new developments in physics, especially theoretical physics. A consideration of these matters leads me to make a suggestion which may possibly be regarded as impertinent or unnecessary. The suggestion is this: would it not be a good thing if the larger chemical Companies and Research Associations of this country were to have as permanent members of their staffs some man or men whose duty it was to keep abreast of modern developments in physical and chemical theories and methods, and to impart this knowledge to the workers engaged in experimental research and plant and process development? No doubt much is already done by discussions and colloquia, and by occasional lectures given by outside experts. There may well be room, however, for the work I saw carried out by Dr. Kapp and Dr. Darrow.

I have also another suggestion to make, which I hope will not be resented. The rapid invasion of chemistry during the last twenty years by the new methods and theories of physics has put an extra load on the students and teachers of chemistry at our Universities. I know very well that a considerable proportion of the University teachers of chemistry are well versed in the new advances to which I have referred. But chemists, just like experimental physicists, are essentially experimental philosophers. They carry out research experiments, and are usually much occupied in directing the experimental investigations of groups of young postgraduate students. They cannot be expected to lead the life of a mathematician or theoretical physicist, nor is it in general possible for them to encroach too much on the time of good-natured colleagues in another department. My suggestion is, therefore, that every important University Department of Chemistry should have associated with it, as a member of the staff, a Professor or at least a Reader in Theoretical Chemistry. We all know how extremely fortunate Cambridge is in possessing Professor Lennard-Jones, and I feel sure my former colleagues in the Chemical Department at University College, London, will agree with me that the association of Dr. Eduard Teller with us for a year was of the greatest advantage.

Such a University teacher of theoretical chemistry would be in general what is called a theoretical physicist, although there is no reason why he might not equally well be a chemist who, owing to his special abilities and temperament, had gradually demonstrated his power of advancing knowledge on the theoretical side. I might mention in this respect that Dr. Teller began life as a chemist, and so, I think, did Professor Tolman. Professor Max Born once told me that his earliest work was in the field of physical chemistry.

The older physical chemistry did not appeal to many chemists of that period, because it was not able to deal effectively with what really interested them, namely the intimate structure of molecules and the real nature of valency. One must admit that this point of view had considerable justification. If you told a " really sound " chemist of those days that the kinetics of a particular reaction showed it to be uni- or bi-molecular, he naturally wondered why you worried about such incidental matters. What he wanted to know was where you got to in the end. If you pointed to the successful study of a particular chemical equilibrium, he probably thought to himself, the poor fellow is talking of some badly chosen chemical reaction that he cannot bring to a finish. In extreme cases, the ionisation of electrolytes was regarded as an unpleasant sort of mass phenomenon that had more to do with electricity than chemistry.

Such objections cannot be lodged against the most modern developments of chemical theory. Although the theoretical physicist of to-day deals with electrons, atoms and molecules in a statistical manner, the results throw light on that intimate "individual" character of the units which has always been one of the main goals of the chemist. Speaking before such an expert audience, I need scarcely remind you how by the study of X-ray and electron diffraction, atomic and molecular spectra, Raman effects, dielectric constants and dipole moments, magnetic susceptibilities, unimolecular films on liquids, etc., etc., the sizes and shapes of molecules, their internal dimensions and dynamics, the nature and binding energy of valency bonds, the various types of excited states and energy levels of molecules, ions and atoms, and their essential reactivities and reaction possibilities have been greatly elucidated with the help of modern physical theory.

In the stress which I have laid on certain very modern developments, I sincerely hope that our excellent friends the " synthetic," " classical "—or is it the " real " ?—organic chemists will not think I have forgotten them. This great branch of chemical science continues its majestic advance with unfailing vigour. Not only in its own more special field but also in the investigation of substances of biochemical occurrence or importance it can show a progress unequalled in any earlier period. Natural colouring matters and related substances, carbohydrates, complex hydrocarbons, sterols, polynuclear compounds, high molecular polymers and condensation products, vitamins, hormones, phytohormones, and even the prosthetic groups of certain enzymes—I am not competent to attempt to complete the list. In the field of applied and industrial chemistry equally great advances have been made; new and important dyestuffs, emulsifying, wetting and " levelling " agents, synthetic rubbers, antioxidants and vulcanisation accelerators, modern synthetic resins and " plastics," pesticides, fungicides, new and important chemotherapeutic agents—again I do not attempt to complete the list. Here, doubtless, are great regions of thought and fruitful action where the synthetic organic chemist still roams untroubled as yet by electron drifts and stresses, eigen functions, or waves of probability. I advise him, however, to keep his weather eye very bright and open. The time is rapidly approaching, if indeed it is not already here, when the exponents of modern theory, be it called physical, organic or colloid, will pounce upon his preserves.

In the older period of which I have frequently spoken, the principal advances of inorganic chemistry were derived from the ionic theory, the study of heterogeneous equilibria and the investigation of many inorganic reactions by earlier kinetic methods, including much interesting work on catalysis. Then came the important views of Abegg and Bodländer relating to valency, and the famous work of Werner on " complex " compounds. In recent years there has been a great extension in the knowledge of complex (co-ordination) compounds in which full use has been made of X-ray analysis and other modern methods. Thus a great deal of inorganic chemistry has become merged in the general progress of chemical theory relating to valency and molecular structure.

There will always be, I think, a certain marked difference between chemists who are chiefly interested in the laws of phenomena and the generalisation of these laws, and those others whose main interest lies in the nature and behaviour of particular chemical substances or classes of substances. Perhaps I might be permitted to call the first category the physical chemists " par excellence." Their investigations have resulted in enormous advances during the last twenty years. It would be quite unnecessary for me to go into details when addressing such an expert audience. Anyone who calls to mind such subjects as chemical thermo-kinetics, photochemistry and photo-kinetics, the theory of solutions and solubility, weak and strong electrolytes and ionisation, electrochemistry, adsorption phenomena, heterogeneous catalysis, magneto-chemistry and colloid and " surface " chemistry in all their numerous aspects, will realise at once the truly remarkable advances that have been made in these branches of physical chemistry, all of which existed in a vastly less developed form before the war. This list of topics could, of course, be made much longer, and might well include various fields of investigation in which both experimental methods and relevant theories were lacking in the earlier period. I hope that the modest list which I have given will serve to exemplify the interesting point of difference that I desired to make, namely that the physical chemist is more interested in the laws of phenomena than in the qualities of specific substances. Be it well understood, however, that I have no wish to make " invidious distinctions between the major and minor prophets." It takes many prophets to make a good chemistry.

Recently I had occasion to read an interesting book by Dr. Alwin Mittasch, whose name is so well known in connection with his work on technical catalysis. It was entitled "Catalysis and Determinism ; a Contribution to the Philosophy of Chemistry." Although I failed to see what connection exists between catalysis and the rather awesome subject of determinism, the book raises the interesting and perhaps important question : do chemists have a philosophy ? Chemists have usually been regarded as hard-headed robust fellows who deal with real concrete things. No chemist in the 19th century ever bothered his head much about the luminiferous ether because he could not isolate it, *i.e.*, put it in a bottle and shake it. I was brought up in an Irish College founded on an old Scottish tradition, so I had a year of " Logic and Philosophy." I fear I came to the conclusion that philosophers were much like our old friend Paracelsus ; each man set up his own system and metaphorically burnt the works of his predecessors. In this connection the words of a witty and learned Professor of Greek come to my mind. " Philosophy," said he, " is a disease like the measles. If you are going to catch it, do it when you are young and get it over early."

This amusing remark has no application to that great plastic and continuously developing philosophy of science, which has in large measure taken the place of the older type of philosophical study. We owe it chiefly to the work of mathematicians, physicists and biologists, amongst whom might be mentioned in relatively recent times de Morgan, Clifford, Huxley, Mach, Karl Pearson, Whitehead, Russell, Smuts, Haldane, Planck, Bohr, and Eddington, though many other names will occur to you. The special Journal *Erkenntnis*, edited by Reichenbach and Carnap, is devoted to this subject. Needless to say, I have no intention of entering into details on the present occasion. I simply want to make a strong plea for the study of the philosophy of science by chemists. In the hard task of understanding modern atomic and molecular theories, which is now imposed on all chemists, I think it is the philosophical background, rather than the formal mathematics, which offers the most difficulty. In this respect I would recommend chemists to read everything that Niels Bohr writes, for to my way of thinking he is the profoundest thinker and leader in the most modern development of physico-chemical thought. If the chemist wants to grasp the meaning of the fundamental statistical concept and the " double personality " of radiation and matter, he cannot do better than follow Bohr. We have to realise that the new scientific philosophy is not to be described in terms of the familiar concepts derived from our age-long experience of the ordinary world, for new things have been discovered which require quite new types of concept and new ways of thinking. This is indeed the essential characteristic of the philosophy of science, namely a fluidity of thought and a courageous adaptability to new concepts demanded by new regions of experimental discovery. The formidable mathematical " techniques " associated with the theory of relativity and modern quantum theory impose heavy tasks on those who aspire to use them. The important thing for the student of chemistry is to be able to survey this mathematical

" apparatus " from without, and to get at the real nature of the physical and chemical ideas and the new outlook which underlies it.

A great and splendid prospect, a land of " rare and refreshing fruit," lies before the chemists of the present day. Every experimental tool and every theory of the physicist are at their disposal, ready to be applied to an ever-increasing wealth of material. Nuclear physics will soon become nuclear chemistry. In the famous days when Rutherford and his collaborators at Manchester were elucidating the transformations of the naturally existing radioactive atoms, he used to call on chemists to take a hand. If that great man were alive to-day, he would be the first to call on us to enter the great and rapidly developing field of nuclear chemistry. This is nothing less than a reconstruction and a re-integration of the whole " material " world, in which matter and energy play equivalent parts.

At the other end of the chain of material existence are the living organism and the living cell. In the study of the phenomena of life lies another of the great prospects of physics and chemistry, perhaps the greatest of all. It may well be that in the development of this investigation science will require to form new concepts and adopt new ways of thinking unknown in the physical and chemical theories of to-day. Many biologists hold this opinion and fail to see in existing physico-chemical science any inkling of the finalistic and holistic concepts which they find indispensable for an adequate description of the phenomena of living organisms. Bohr has said that in the fact of life we may have to accept one of those " irreducibles," like the finite velocity of light and the quantum of action. This is, however, no counsel of despair, for the *phenomena* of life appear in many respects to be amenable to our present concepts and ways of thinking. The wonderful development of complex living organisms on this planet and the things they make and do seem at first sight to contradict the entropy principle of an approach to a more random and more " chaotic " state. This principle applies, however, only to an *isolated* system. Our planet is not an isolated system, so that any decrease of entropy (increase of improbability) associated with the organisation and organising actions of life may be over-compensated by the " running down " or increase of entropy associated with the passage of radiation from the hot sun to the cold earth. Maxwell's celebrated demon could cause a diminution of entropy by sorting out the molecules of oxygen and nitrogen in a flask of air. But if the demon could do this without food and without the constant metabolism of food in his inside, and therefore the constant associated increase of entropy, he would be a super-natural ghost. The living organisms we know are not ghosts, but must take in food and metabolise it. Their " improbable " organisation and actions are indissolubly bound up with a running-down to a more probable state of relatively improbable systems produced by the sun's expenditure of a little of its vast store of highly organised energy.

The application of the existing (and developing) principles and methods of physics and chemistry to the study of the intimate processes occurring in living cells and organisms will constitute for a long time to come the most fruitful method of investigation. It is unnecessary for me to mention the very great advances already made in this field by the combined attack of organic chemistry, physical chemistry, and physics. It is undoubtedly the combination of all three disciplines and techniques which leads to the most sure and rapid progress. We see very striking examples of the effects of such combined work in recent investigations on muscles, nerves, enzymes, proteins, and viruses. The lectures on the " Molecular Architecture of Biological Systems," given by Professor Bernal at the Royal Institution during January and February of this year, are highly significant of what is happening to-day, and still more, of what is going to happen to-morrow. Soon the chromosomes and genes of the biologist will become amenable to similar treatment by the methods of physics and chemistry. Whatever the future may hold in store for the investigators of the phenomena of life, the biologists may rest assured that physicists and chemists will always be ready to adopt new concepts and new modes of thought, should the progress of experimental discovery demand such a re-orientation. In the philosophy of science there are no *a priori* principles and no immutable laws of thought (apart, of course, from the exclusion of logical contradictions). As Weyl has pointed out, the world of science is an open world, ever pointing forward and beyond itself. Just before writing this address, I read that wonderful little new book by Einstein and Infeld, entitled "The Evolution of Physics."

There is not a single symbol or mathematical equation in it, though it traces the gradual development of thought in physical science from Galileo and Newton to Einstein, Bohr, and Schrödinger. I recommend it to every member of this audience as a great lesson in the story of physical science, viewed as a gradual and evolutionary process of the formation of ever wider and more abstract concepts adapted to the progress of discovery and the corresponding refinement of our ways of thinking about and describing the world of physics. For the world of physics is also our world, and whether we be chemists, physicists, or biologists, all science is our science. It is, indeed, this drawing together of what were in former days separate " sciences " that offers the greatest hope and the greatest prospect for the future of our own science of chemistry. Whether it be the chemistry of the atomic nucleus, or that of the atom, ion, molecule, micelle or living cell, progress depends more and more on the friendly and close collaboration of all workers in the great domain of science. Who would have thought fifty years ago that an understanding of atoms and molecules would come to be intimately concerned with the theory of differential equations as developed by the pure mathematician? I do not agree with Sir James Jeans that the great Architect of the Universe is probably a pure mathematician, but all of us must allow the human variety a very respectable place in the sun. The dreamer of logical dreams in the study is as indispensable to-day as the worker of magical spells in the laboratory. Science is no hierarchy or oligarchy, but a free democracy open to all the talents.