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Chemistry as Rationalised Alchemy

Delivered by PROFESSOR J. R. PARTINGTON, M.B.E., on *7th May*, 1951

When, towards the end of his life, Plato came to write the *Timaeus* he attempted to cover in it the whole scope of created being. After first describing, without conscious difficulty, the origins of the gods and of the heavens, of man and of animals, he comes at last to what is, in effect, the first treatise on theoretical chemistry, for he sets himself the task of the investigation of the nature and affections of the four elements. The dialogue, or more correctly and as often the case with Plato, the monologue, is here conducted, not by Socrates or a member of the school of Plato, but by Timaeus, a Pythagorean. At this point the soaring confidence of the narrative falls to a minor key; the things are difficult, we are to expect only probabilities, and the subject is begun again on this basis. In considering the nature of gold, for example, we might explain it in terms of that division of space into the kinds of triangles which was competent earlier in the treatment to explain so much, but here, says Plato, " it would be by far the safest and most correct to say that it is gold ".

From the very beginning, then, we find that the study of the composition of things is treated as somewhat apart from other branches of learning; its methods are different, and in this study we must " begin again ".

The origins of chemistry, as you well know, are remote and very obscure. In the earliest treatises on the subject, which is there appropriately named the " divine art ", it takes a shape which, at first sight, has little relation to its modern form. In the earlier centuries of the Christian Era, probably in Alexandria and very likely in small, obscure, bodies of students, perhaps with gnostic affinities, a subject completely unlike anything which had preceded it in Greek or Roman learning, came into being. Its sources are only partly Greek and it seems unlikely that its study ever formed a part of the activities of the Museum, or that its works were on the shelves of the great Library. Its most copious author is Zosimos of Panopolis, who is described by a commentator as " the crown of the philosophers, whose language has the abundance of the ocean ". Zosimos gives a definition of chemistry (an alternative name used for the divine art). It is the science which treats of " the composition of waters, movement, increase, taking away and restoration of bodily nature, fixation of spirit on body; the operations of which do not result from the addition of foreign natures taken from without but are due to the proper and unique nature active upon itself, derived from a single species; also the earthy part of metals and the juices of plants; and all this unique and many-coloured system comprises the multiple and very varied discourse of, and research into, the sublunar things of nature, subject to the measure of time, through which nature suffers decay and renews itself continually ".

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Zosimos gives us some practical information, but also some remarkable visions which he had, and the earliest chemistry, and the alchemy which for many centuries was its lineal descendant, offer a strange blend of the practical and the visionary. Besides the description of chemical apparatus and of many new substances not mentioned by earlier writers, we find much of rather obscure and semi-mystical nature, but nothing which would technically be called magic or the black art. The popular exponents of alchemy never fail to adorn themselves, at least metaphorically, in a pointed cap, and would have us believe that alchemy was a branch of magic and largely a matter of hocus-pocus. In actual fact the early treatises are devoid of material of this kind, so fully known in the contemporary magic papyri. There are sometimes dramatic accounts of the finding of books concealed in altars, and the invocation of shades, and the like, but it may well be that this is based on contemporary fiction and added for the sake of effect.

Be this as it may, alchemy always contained a mystical or semi-mystical element. In some cases, as in the attribution of life to what we now think of as inanimate things like metals, which were thought capable of growing like plants and putting forth flowers of diverse colours—forming salts we should say—this view was a commonplace of its time. Just as a seed of wheat put into the earth was thought to die and suffer corruption, and from this dead matter a new life was to arise, so it was thought that if a metal could be killed and its proper nature taken away, its corrupt or primary matter, could, by suitable treatment, like watering the ground, bring it to life in a more perfect form : copper would shine in the splendour of silver or the solar effulgence of gold.

Through the centuries alchemy was studied by great men like Roger Bacon and Albertus Magnus. St. Thomas Aquinas asks whether alchemical gold can lawfully be given as real gold, and affirms that it can if its nature is that of gold ; in fact, as Plato would have said, if it is gold.

Alchemy, which Albertus Magnus called the " beggardly union of genius and fire ", was turned to the service of medicine by Paracelsus, who taught the doctrine of the three alchemical elements, mercury, sulphur and salt. Rather later, Van Helmont, who rejected these and taught that the true elements are air and water, made the first steps towards the founding of chemistry by ridding alchemy of some of its irrational elements, by performing quantitative experiments, and by inventing the new name " gas " for air-like bodies with properties different from those of ordinary air. With Van Helmont, who died in 1644, we are very near the true beginning of scientific chemistry.

Professor Dingle, in a recent lucid survey of the significance of science, has said that : " The lines on which science has proceeded since the early seventeenth century were laid down by Galileo and his successors ". He points out that they postulated that the ultimate data for scientific study are our experiences, which form the subject-matter of the various sciences of mechanics, calorimetry, optics, acoustics and the rest. Professor Dingle presumably intends chemistry to be included in the rest, and this is no doubt true if we admit that we obtain knowledge of chemical composition through the senses. Whilst, however, the sense of sight is related to optics, the sense of sound to acoustics, and so on, there is no corresponding chemical sense (if we exclude the sense of smell as not covering all cases). I am not capable of giving any account of the philosophical aspects of the subject which it would be worth your time to listen to, but I intend to invite your attention to the proposition that the development of chemistry followed a rather different line from that envisaged by Professor Dingle for the physical sciences, that it owed

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little or nothing to Galileo, and that the peculiar and complex mode by which chemistry came into being and still progresses, is one which has been neglected by some historians of science more concerned with the origins and development of mathematics, astronomy, and biology.

Chemists have reason to think that the modern trend of their science began with Robert Boyle a little later than Galileo, and if they wish to know if Boyle was guided by any philosophical ideas they have not far to seek, since he himself tells us in so many words that in his method he followed the precepts of a philosophical thinker who is not mentioned by Professor Dingle, nor for that matter by many modern writers, namely Francis Bacon. I am aware of the severe judgments passed on Bacon by many critics, mostly continental, and it is no part of my intention to enter this controversial field. I wish merely to say that Bacon is named as an originator of chemical method, that he does in fact include chemistry among the sciences which must be taken into account in forming an adequate picture of the knowledge of his time, and that he recognised that in this field he was following a long tradition. He realised that chemistry could never hope to develop from a few postulates or laws, which by mathematical methods could lead to consequences which could be tested by experiment, but that it was something more complex and difficult. He recognised that the only workers who had, in his time, anything significant to say about chemistry were the chemists, or as they were then called, the alchemists, and that if progress was to be made in this field it must start with what the experts, the alchemists, had already established, and not from abstract postulates or laws taken over from entirely different fields. Chemistry, he realised, if it was to become a science, would have to be rationalised alchemy.

I have no time to give the passages in Bacon's writings which justify my thesis, but I may direct your attention to the statement of Robert Hooke, who says that " the incomparable Verulam " had shown that " even Physical and Natural Enquiries as well as Mathematical and Geometrical, will be capable also of Demonstration ; so that henceforward the business of Invention will not be so much the Effect of acute Wit, as of a serious and industrious Prosecution ". The distinction drawn by Hooke seems to me significant from the high place he took among those who made important contributions to the development of chemical science in his time. It has been pointed out that most of Bacon's important works on the " advancement of learning " were written in the early years of the seventeenth century, when the main discoveries known to him, such as Gilbert's and Harvey's, were non-mathematical, and the few mathematical works available to him were very specialised. Neither Kepler's laws nor Galileo's laws of falling bodies were known. Of contemporary chemical writings, Bacon shows a competent knowledge. He mentions Paracelsus often, and refers to Isaac Holland and Basil Valentine, long regarded by chemical authorities with great respect. The alchemical-theosophical background of much of Bacon's work was firmly grounded in contemporary England but much more prominent in Germany.

Bacon made chemical experiments himself and his book, *Sylva Sylvarum*, although it has been much derided, was probably the best and most complete collection of its kind available at the time. He was fully aware of the weakness of alchemical practice, saying, for example, that the alchemist fails in " the true proportions and scruples of practice, which makes him renew his trials infinitely and, finding that he lights upon some mean experiments and conclusions on the way, feeds upon them, magnifies them to the most, and supplies the rest in hopes," adding : " not but that the alchemists have made a good many discoveries and presented men with useful inventions ".

He says the development of this science will more probably be effected by "a diligent study of the natures of weight, colour, malleability and extension, volatility and fixedness, and of the first seeds and menstruums of minerals, than that a few grains of an elixir should in a few moments turn other metals into gold".

Bacon was more sceptical of the possibility of alchemy than were Boyle and Newton, both of whom were aware that there was no proof, mathematical or otherwise, of the impossibility of transmutation.

We might surmise that, if Newton had applied his powerful mind to chemistry he would have made advances in that science which could have linked up with the epoch-making work of Lavoisier without the necessity of the slow progress and decline of the theory of phlogiston during the eighteenth century. Yet Newton did apply himself to chemistry, and most assiduously, and the results of his arduous labours were practically negligible. Few historians of chemistry find it necessary to mention Newton at all, and in the history of that science he fills a very modest place. Attempts to extend the theory of attraction to chemistry, and thus put this subject on a mathematical basis, were made by John Keill, who in 1708 stated 30 theorems of the laws of attraction in chemistry; and John Freind in lectures at Oxford in 1704, published in 1709, explained chemical actions on the same principles. These theoretical extensions of Newton's method were almost without influence on the progress of chemistry. On the experimental side, Hales carried out a long series of measurements of the quantities of air extricated in many chemical processes, and by his neglect of what the meanest alchemist would have noticed in the qualities of the materials he had collected, Hales missed important discoveries which many years later were to make the name of Priestley immortal. Even Newton could make no progress without a knowledge of new substances, and these could not be discovered by the use of such methods as had weighed the heavenly bodies and inaugurated a science of mathematical astronomy without parallel in the great achievements of all ages. Prof. Singer has said that Galileo announced the proposition that "science is measurement", but chemistry is a science and it is not all measurement.

The great discoveries in chemistry in the eighteenth century, made by Scheele and Priestley, lay in a field amenable only to the use of the chemical method of investigation. The main features of it were well-known to the alchemists. Materials of all kinds were subjected to fire, to acids, to unlikely trials; and from this series of experiments there emerged the new gases, which had been in the hands of chemists before but had not been clearly recognised. It was from this material that Lavoisier was to construct a new science.

Before turning to Lavoisier's contributions I would like to digress for a few moments to consider another matter which is of some importance to us as members of a society concerned with the welfare of the history of science. Some of us may forget at times that there is in existence a deep hostility to the study of that subject. We are made aware of this in many ways. The hostility is noticeable among some teachers in schools, who dislike books which touch upon the historical aspects of their subjects. In the universities we find that the subject of the history of chemistry has disappeared from the syllabus for the degree. When this happened, we were told that the time had come when chemistry must be treated on didactic lines, that the growth and complexity of the science were such that all the energies of students were absorbed in mastering the present state of the science, and that any mention of its origins was not only a waste of time but also could only confuse and repel the student approaching the subject. Although it was seemly and useful to point out the achievements of contemporary chemists; the contributions of their predecessors, outmoded as they were, could well be treated as integral parts of a science which had now reached a stage of development not requiring any enquiry into its remote origins. This view has not been shared by all. Richard Willstätter, for example, emphasised the value of teaching chemistry on an historical basis, even going so far as to say that this method might differentiate its study in a university from that in the more hurried and less fundamental treatment suited to a technical school; but the majority seem to have little sympathy with any study of its history. I believe this attitude goes back at least to Lavoisier. In the *Traité de Chimie* (1789), one of the great historical documents of chemistry, to which the development of the science in the first part of the nineteenth century owes so much, we find it prominent.

In the preface, Lavoisier says he might be reproached with having given no history of the opinions of his predecessors and only presented his own. In an elementary treatise, however, such a long and tedious account would tend to obscure the true object proposed and produce a work the reading of which would be tedious to beginners. The sciences are already difficult enough without bringing in matter foreign to them, and in the interests of clarity everything must be carefully avoided which might distract the attention. It would take at least three or four years of study to learn even the elements of the science, without unnecessary additions. If he had frequently adopted the opinions of his French contemporaries without mentioning their names, this was because they all formed, as it were, a community in which it might be difficult to distinguish what belonged to any individual. The sole method of sound treatment is to preserve only facts given by nature and to seek truth only in the natural sequence of experiments and observations, in the same way as the mathematicians arrive at the solution of a problem. It is with some surprise, therefore, that we find that the book opens with a discussion of caloric, the fluid and material basis of heat, the existence and properties of which are developed by a series of superficial analogies of the soaking up of water by sponges or woods, and the expansive properties of gases are explained by the self-repulsive properties of the caloric existing between their particles. Francis Bacon had hit on the correct nature of heat, perhaps not in a way giving satisfaction to some, but still a way giving a correct result, and an old-fashioned teacher of Lavoisier's time might, in an historical digression, have mentioned this, at the same time pointing out that the idea that heat is a form of motion had been discarded by the modern leaders of the science. Lavoisier says: "We cannot too much help ourselves in abstract things by comparisons with sensible", and hence the water-soaked sponge helps us to reach the view taught throughout the book that heat is a material fluid. He says: "There is a real repulsion between the molecules of elastic fluids", adding as an after-thought, "or at least things behave as if this repulsion occurs". The old-fashioned teacher might have added, as a curiosity, that some had thought the particles of gases did not repel one another, and that Bernoulli in 1738 had shown that the physical properties of gases could be quantitatively explained in terms of the kinetic energies of their non-repulsive particles. In his book Lavoisier, although not often mentioning others, rarely omits to say what he has done. In the section on combustion be begins by saying that in it: "there is hardly anything which is not my property, either because I did it first or because I repeated it under a new point of view". Oxygen gas is: "this air which we, Mr. Priestley, Mr. Scheele and I discovered about the same time", the necessity for brevity in an elementary work leading him to omit the passage in one of his memoirs to the effect that Priestley had discovered the gas "about the same time as I, and I believe even before me".

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The *Traité de Chimie* many times claims to be a much better work than any which preceded it, and in some respects it undoubtedly is. In the introduction to the practical part Lavoisier says it is a mistake to fill an elementary work with minute descriptions of apparatus and illustrations, " which interrupt the flow of ideas and make the reading tedious and difficult ". He gives us, nevertheless, a very detailed description of a gasometer which few apart from Lavoisier could afford to have constructed and which, all the same, gave him less accurate results on the composition of water than Cavendish achieved with much less elaborate apparatus. There is a modern ring in Lavoisier's words : " It is an inevitable effect of the stage of perfection which chemistry now begins to approach, of requiring costly and complicated instruments and apparatus ". He also lacked the flexibility of mind of Priestley, saying, for example in respect of an opinion which he had been forced to give up : " It will be appreciated how much it has cost me to give up my first ideas ; it is only after many years of reflexion and after a long sequence of experiments and observations . . . that I have decided to do so ".

I have introduced this digression with no intention of belittling such a man as Lavoisier. The historian of science can, however, find many things which can have a significance in his own time.

Let us now return to our main subject. It has often been said that Lavoisier introduced the quantitative method into chemistry, but everyone here will know that this is not true. The quantitative method had been used by Van Helmont with success, by Hales without success, and by Black with great success in a limited field. Lavoisier's quantitative method succeeded because it had many more materials to work upon than were available previously. The new gases discovered by Priestley made all the difference, and these were discovered by purely chemical methods, no use being made of any methods derived from astronomy, or physics, or mathematics. With these discoveries, Lavoisier was able, by the use of methods derived from physics and mathematics, to transform chemistry, and he was the first great physical chemist.

It was Davy, no slavish follower of Lavoisier and undoubtedly the most imposing and significant figure in chemistry in the opening years of the nineteenth century, who said that Lavoisier's chemical system lacked completeness even for its time. It took no account of such things as the laws of combining proportions, a subject which had received attention by Homberg, Bergman, Cavendish, Kirwan and Richter before Lavoisier's book was published. The major work in this field had been carried out by Jeremias Benjamin Richter, who was to continue it until 1807, the same year in which Dalton's atomic theory was announced to the chemical world. Richter was a very good practical chemist, but his main contribution was in theory. He first realised what we now call the law of reciprocal proportions or equivalents in its fuller extent, and he gave tables of chemical equivalents. He failed to impress the contemporary chemists for two reasons. In the first place, a mathematician as well as a chemist, he believed that the numbers representing combining proportions should be subject to mathematical laws and form series. We know that the numbers do not, in fact, follow any such mathematical regularities. Richter felt authorised to alter some of the numbers in order to make them fit into the series, and chemists very naturally refused to follow him. In the second place, Richter failed to notice that his numerous tables could be greatly simplified by adopting a single element as the basis of the series of equivalents, and as soon as this was done by Fischer in 1802, Richter's work became appreciated through the description of it given in Berthollet's *Statique Chimique* in 1803. By the time this book reached England, Dalton had given his first table of atomic weights at a meeting of the Manchester Literary and Philosophical Society, and his atomic theory was not due to any influence by Richter. Dalton was also a mathematician. He seems to have arrived at the chemical atomic theory through a combination of Newton's theory that gaseous pressure is due to the repulsion of the particles of gases with the theory that these particles are surrounded by envelopes of caloric of various sizes. Both these assumptions are erroneous, and once Dalton had appreciated the possibility of atoms differing in weight he proceeded on the lines of experiments on chemical combining proportions, as had been done by Richter.

Perhaps the most striking example of the relation of physical to chemical methods is furnished by the development of the law of mass action. Throughout the eighteenth century the idea that chemical changes were due to the action of forces between particles was held. Geoffroy in 1718 arranged substances in a table of affinities, but as (so I have seen somewhere) the Paris Academy did not favour Newton's name "attraction", he used the name "rapport", which with the derivation *ratio*, λόγος, might lead us to the Stoic idea of force. Generally, however, it was believed that affinity should be explained as due to modified gravitational attraction, although Newton himself had not favoured this idea. A substance AB, say a salt of an acid A and a base B, is decomposed by another substance C, say another acid, to form BC with liberation of A because the attraction of B and C is greater than that of A and B. Bergman in 1775 and Berthollet in 1801 both started with the same idea of affinity as universal attraction, but reached diametrically opposed results. Bergman supposed that the decomposition would be complete, affinity acting as what was called an absolute force, whilst Berthollet thought it should be incomplete, the result of an equilibrium of forces, modified by such forces as the elasticity of gases or the cohesion of solids. That the same theory can lead to two incompatible results shows that it has nothing to do with the matter. After a long series of chemical investigations it became clear that gravitational attraction cannot help in the study of chemical changes. The law of mass action is a law in the formulation of which the idea of force does not enter. It is a chemical law in the sense that it is a quantitative statement of chemical phenomena. Many chemists may feel that is better left as it is, and can derive satisfaction from a generalisation of such wide applicability.

Chemists should always be interested in the attempts of mathematicians and physicists to explain the fundamental laws of chemistry which have been arrived at by methods peculiar to the science, but if they are wise they will continue to use these laws, and feel satisfaction in them, even though they cannot yet be reduced to terms of concepts which seem rather foreign to the content of chemistry.

At various times we find the physical sciences dominated by particular aspects of belief, and chemistry has necessarily been influenced by these. In the period of alchemy, the notions of primary matter and substantial forms were predominant. In the seventeenth century atomism began to penetrate the science, in the eighteenth century it was hoped that the idea of universal attraction could be extended to include chemical phenomena, in the nineteenth century energy, and in the twentieth the quantum theory. All these are still insufficient to cover the whole science, which goes its way to new discoveries made by its own methods.

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