SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. A. SENIER, PH.D., M.D., D.SC., PRESIDENT OF THE SECTION.

I AM sure it will be agreeable to the feelings of the members of this section that, before beginning my address, I should refer to the great losses we have sustained by death since our gathering last year at Portsmouth.

An active member and past-president has passed away in the death of Edward Divers, after a serious operation, undergone at his advanced age with characteristic fortitude. His devotion of his long life, in this country and Japan, to the advancement and diffusion of science is indelibly inscribed in its records. But Divers was more than an investigator and teacher; he was a beloved centre of our social life, and was particularly happy when he could bridge over the distance between the young beginner in research and the older experienced master. He understood and had the sympathy of both.

In Henry Forbes Julian, one of the victims of the awful disaster to the *Titanic*, we have lost a valued contributor to our proceedings; though he was best known as a geologist and metallurgical engineer. It was, however, by chemistry, under the inspiring influence of Sir Henry Roscoe, that his first enthusiasm for science was aroused. Forbes Julian was a leading technical adviser in mining undertakings, and his advice was much sought after, especially in South Africa, and even in Germany.

Another tragedy, from the shock of which we have not yet recovered, has deprived science of the young and promising inquirer, Humphrey Owen Jones. We know the dreadful details—he and his young wife—how they became sacrifices to the treacherous crags and snows of Mont Blanc.

And this, alas, is not all. On the very day of the fatal accident to Humphrey Jones, another young and promising chemist—John Wade—passed from us from the effects of a cycling accident. He was an inquirer of singular ability, and found time also to give us one of our deservedly most popular manuals of organic chemistry.

PART I.

The Nature and Method of Chemistry.

Perhaps there is no intellectual occupation which demands more of the faculty of imagination than the pursuit of chemistry, and perhaps also there is none which responds more generously to the yearnings of the inquirer. It is surely no commonplace occurrence that in experimental laboratories day by day the mysterious recesses of Nature are disclosed and facts previously quite unknown are brought to light. The late Sir Michael Foster, in his presidential address at the Dover meeting, said:—"Nature is ever making signs to us, she is ever whispering the beginnings of her secrets." The facts disclosed may have general importance, and necessitate at once changes in the general body of theory; and happily, also, they may at once find useful application in the hands of the technologist. Recent examples are the discoveries in radio-activity, which have found an important place as an aid to medical and surgical diagnosis and as a method of treatment, and have also led to the necessity of our revising one of the fundamental doctrines of the theory of chemistry—the indivisibility of atoms. But the facts disclosed may not be general or even seem important; they may appear isolated and to have no appreciable bearing on theory or practice—our journals are crowded with such—but he would be a bold man who would venture to predict that the future will not find use for them in both respects. To be the recipient of the confidences of Nature; to realise in all their virgin freshness new facts recognised as positive additions to knowledge, is certainly a great and wonderful privilege, one capable of inspiring enthusiasm as few other things can.

While the method of discovery in chemistry may be described, generally, as inductive, still all the modes of inference which have come down to us from Aristotle, analogical, inductive and deductive, are freely made use of. A hypothesis is framed which is then tested, directly or indirectly, by observation and experiment. All the skill, all the resource the inquirer can command, is brought into his service; his work must be accurate; and with unqualified devotion to truth he abides by the result, and the hypothesis is established, and becomes part of the theory of science, or is rejected or modified. In framing or modifying hypotheses imagination is indispensable. It may be that the power of imagination is necessarily limited by what is previously in experience—that imagination cannot transcend experience; but it does not follow, therefore, that it cannot construct hypotheses capable of leading research. I take it that what imagination actually does is—it rearranges experience and puts it into new relations, and with each successive discovery it gains in material for this process. In this respect the framing of a hypothesis is like experimenting, wherein the operator brings matter and energy already existing in Nature into new relations, new circumstances, with the object of getting new results. The stronger the imaginative power, the greater must be the chance of success. *The Times*, in a recent leading article on Science and Imagination, says:—"It has often been said that the great scientific discoverers . . . see a new truth before they prove it, and the process of proof is only a demonstration of the truth to others and a confirmation of it to their own reason." While never forgetting the essentially tentative nature of a hypothesis, still, until it has been tested and found wanting, there should be some confidence or faith in its truthfulness; for nothing but a belief in its eventual success can serve to sustain an inquirer's ardour when, as so often happens, he is met by difficulties well-nigh insuperable. In a well-known passage Faraday says:—"The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realised."

But a hypothesis to be useful, to be admitted as a candidate for rank as a scientific theory, must be capable of immediate, or at least of possible, verification. Many years ago, in the old Berlin laboratory in the Georgenstrasse, when our imaginations were wont, as sometimes happened, to soar too far above the working benches, our great leader used to say:—"I will listen readily to any suggested hypothesis, but on one condition—that you show me a method by which it can be tested." As a rule, I confess we had to return to the workaday world, to our bench experiments. No one felt the importance of the careful and correct employment of hypotheses more than Liebig. In his Faraday lecture Hofmann says of Liebig:—"If he finds his speculation to be in contradiction with recognised facts, he endeavours to set these facts aside by new experiments, and failing to do so he drops the speculation." Again, he gives an illustration of how on one occasion, not being able to divest himself of a hypothesis, he missed the discovery of the element bromine. While at Kreuznach he made an investigation of the mother-liquor of the well-known salt, and obtained a considerable quantity of a heavy red liquid which he believed to be a chloride of iodine. He found the properties to be different in many respects from chloride of iodine; still, he was able to satisfy all his doubts, and he put the liquid aside. Some months later he received Balard's paper announcing the discovery of bromine, which he recognised at once as the red liquid which he had previously prepared and studied. Thus, though imagination is indispensable to a chemist, and though I think chemists should be, and let us hope are, poets, or at least possess the poetic temperament, still, little can be achieved without a thorough laboratory training; and he who discovers an improved experimental method or a new differentiating reaction is as surely contributing to the advancement of science as he who creates in his imagination the most beautiful and promising hypothesis.

It may never be possible to trace in civilisation's early records the exact period and place of the origin and beginnings of our science, but the historical student has been led, it appears to me, by a sure instinct to search for this in such lands of imaginative story as ancient Egypt and Arabia. For is there anything more fittingly comparable with the marvellous experiences of a chemical laboratory than the wonderful and fascinating stories that have come down to us in "The Arabian Nights"? Those monuments of poetic building of which Burton, in the introduction to his great translation, says that in times of official exile in less-favoured lands, in the wilds of Africa and America, he was lifted in imagination by the jinn out of his dull surroundings, and was borne off by him to his beloved Arabia, where under diaphanous skies he would see again "the evening star hanging like a golden lamp from the pure front of the western firmament; the after-glow transfiguring and transforming as by magic the gazelle-brown and tawny-clay tints and the homely and rugged features of the scene into a fairyland lit with a light which never shines on other soils for seas. Then would appear," &c. I cannot help thinking that the study of such books as this, the habit of exercising the imagination by reconstructing the scenes of beauty and enchantment which they describe, might do much to strengthen and sharpen the imaginative faculty, and greatly increase its efficiency as an indispensable tool in the hands of the pioneer who seeks to extend the boundaries of knowledge. *The Times,* in the leading article already quoted, says that, as with a Shakespeare, "it is the same with imaginative discoverers in science. . . . But the faculty is not merely a fairy gift that can be exercised without pains. As the sense of right is trained by right action, so the sense of truth is trained by right thinking and by all the labour which it involves. That is as true of the artist as of the man of science; and one of the greatest achievements of science has been to prove this fact and so to justify the imagination and distinguish it from fancy."

Again, let it not be forgotten that chemistry in its highest sense—that is, in its most general and useful sense—is purely a world of the imagination, is purely conceptual. And in addition to this, moreover, it is based, like all science, on the underlying assumption of the uniformity of Nature, an assumption incapable of proof. If we think of the science as a body of abstract general theory, and exclude for the moment from our purview its innumerable practical applications, and also all special individual facts not yet known to be related to general theory, then what remains are the more or less general facts or laws. These it is which give the power of prediction in newly arising cases of a similar character; the power of foresight by which the claim of chemistry to its position as a science is justified. Chemistry, as such, is a complicated ideal structure of the imagination, a gigantic fairy palace, and, be it noted, can only continue to exist so long as there are minds capable of reproducing it. Think of all the speculation—and speculation too of the highest utility when translated into concrete applications—about the internal structure of molecules. I venture to say that the most magnificent creations of the world's greatest architects are not more elaborate or more beautiful or more fairylike than the chemist's conception of intramolecular structure and the magical transformations of which molecules are capable; and yet no one has had direct sensuous experience of any molecule or atom, or possibly ever will. It is well from time to time to recall these truths and realise where we are. But although the conceptual nature of science is unquestionable, it certainly contains truth in some form as tested by deductive concrete realisation, by correctness of prediction, and during the last century or two has undoubtedly given to man a mastery over Nature never before dreamt of.

A Brief Historical Retrospect.

The foundations of chemistry, as we now know it, were laid under the influence, the guidance, of three great theories: first, the theory of the alchemists of the transmutation of metals by means of the philosopher's stone; second, the theory of phlogiston, connected so much with the names of Becher and Stahl, which held sway for some two centuries; third, the theory of combustion, the quantitative period of chemistry, inaugurated by the great Scottish chemist Black by his introduction of the balance. How this led to a veritable renascence of chemistry in the hands of Lavoisier and the other giants of that stirring period—the close of the eighteenth and commencement of the nineteenth centuries—is well known. Looking back at the warfare which was waged about these older theories, for and against them, one realises now that there were elements of truth on both sides; for have we not in the work of Sir William Ramsay and others the revival of transmutation, and does not the essential truth of phlogiston survive in the modern doctrine of heat? In one of Dr. Johnson's letters to Boswell there is a curious reference to transmutation. He says that a learned Russian had at last succeeded, but, fearing the consequences to society, he had died without revealing the secret.

After the discovery of oxygen and the beginnings of quantitative chemistry, the science was ready for Dalton's great discoveries respecting combination by weight; the corresponding discoveries by Gay-Lussac on combination of gases by volume, and, through the latter, for Avogadro's famous hypothesis. Dalton had indeed, by reviving an old Greek suggestion, proposed to explain his discoveries by his atomic theory, but neither this nor our molecular theory, though the latter was inherent in the laws of gaseous combination of Gay-Lussac and in Avogadro's hypothesis, was finally put upon its present basis until Cannizzaro took up the subject half a century later. Meanwhile Dulong and Petit had completed their studies of atomic heat, and Mitscherlich had pointed out the relation between isomorphism and molecular structure. When it is considered how little is known of solid or liquid structure, and that our present knowledge of molecules is only of gaseous molecules, it is fortunate that these methods of study of solids are available. The same may be said of the results of the work of Kopp and his successors on molecular volumes. Of other aids to fixing our conception of molecules and atoms I need only refer to the periodic law, the studies of the properties of dilute solutions, of electrolytic dissociation, and of surface tension of liquids.

Liebig, in his first inquiry, begun before he went to Gay-Lussac in Paris, proved that silver fulminate and silver cyanate, though distinct substances, had exactly the same composition; thus was opened that great chapter in the history of chemistry which Berzelius named isomerism. Perhaps nothing in chemistry has given rise in recent years to more intellectual and practical activity than isomerism. Wöhler's classical synthesis of urea, by the metastasis of ammonium cyanate, added another instance of isomerism, and Berzelius soon afterwards announced the isomerism of tartaric and racemic acids. Wöhler's synthesis of urea, followed, as it was, by numerous other laboratory syntheses, showed that substances which occur in living organisms are not different form those which may be prepared artificially, and the old distinction between inorganic and organic chemistry disappeared—there is, of course, only one chemistry The words, it is true, have survived, but only for reasons of practical convenience.

Arter isomerism the next great step forward in the study of intra-molecular structure was the discovery of groups partially individualised which are capable of remaining intact through many reactions. Gay-Lussac had previously noticed the cyanogen group as tommon to cyanides; but it was the celebrated paper by Wöhler and Liebig on the radical of benzoic acid which finally established the existence of compound radicals or groups such as benzoyl, and obtained for the theory of compound radicals the position in chemistry it now holds. Bunsen followed somewhat later with the discovery of cacodyl, and now such groups are almost innumerable. In many respects, by the experimental skill which it shows, the clearness of its logical method, and the beauty of its form and diction, this memoir is a model of what a scientific communication should be. I will read the opening paragraph, using Hofmann's translation:—"When a chemist is fortunate enough to encounter, in the darksome field of organic nature, a bright point affording him guidance to the true path by following which he may hope to explore the unknown region, he has good reason to congratulate himself, even though he may be conscious of being still far from the desired goal." Of this memoir Berzelius, in a letter quoted by Hofmann (Faraday lecture), says:—"The facts put forward by you give rise to such considerations that they may well be regarded as the dawn of a new day in vegetal (organic) chemistry."

The history of the advance of chemistry since the days of the Giessen laboratory is bewildering in its extent. This has been largely due to the Giessen laboratory itself, which sent trained investigators, each carrying with him some touch of its master's magic, into all civilised lands. I cannot attempt to even catalogue the results here. One thing may be said, that chemistry is not worked out, as some have thought; but rather the opportunities of discovery seem greater and more promising than at any previous period.

PART II.

Sub-atoms, Atoms, Molecules, Molecular Aggregates; Valency.

Whether in the light of recent researches it may become necessary to give up that portion of Dalton's theory of atoms in which he regards them as undecomposable and indivisible; or whether we may consider them, as Prout suggested a hundred years ago, as different aggregates of sub-atoms of a uniform kind of matter; or whether they must be regarded as complexes built in the manner supposed by the electron hypothesis; also what should be our attitude towards the related problem of transmutation—all this I pass over, the more willingly that these subjects were discussed so recently by so high an authority as Sir William Ramsay in his address to the Association last year at Portsmouth.

I assume that we are fairly satisfied with our present atoms and their respective weights, and this no matter how the atoms are constructed, and that we shall be satisfied with them so long as they disport themselves in chemical changes as indivisible entities. And further, I assume that we are satisfied with our molecules and their respective weights, as determined by the application of Avogadro's hypothesis. Whether the molecular weight is obtained by direct determination of gaseous density or by taking advantage of the properties of dilute solutions, in either case the molecular weight which results is the weight of a supposed gaseous molecule, for the latter method depends for its justification on the former. All our molecular weights are weights of molecules in the gaseous state or are supposed to be; they are not necessarily applicable to liquids, and much less to solids: solids and liquids may well consist of far more complex particles.

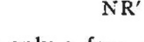
Gradually the central problem of chemistry has become more and more the study of internal structure of molecules—of gaseous molecules. The enormous number and variety of the compounds of carbon, with which so many workers have enriched the science during the last hundred years, and the special adaptability of these compounds to the experimental study of molecular structure, have led investigators to make use of them rather than of the so-called inorganic compounds: thus out of inquiries into the intra-molecular structure of these compounds arose and were developed the theories of types of Gerhardt, Williamson, and Kekulé. These are now, however, looked upon more as aspects of the general problem. More fruitful has been the study of the compound radicals or individualised groups of Wöhler and Liebig. But gradually these molecular structures have been regarded, in agreement with the views of Dumas, as complete wholes; like fairy temples, which from different points of view show different parts in relief, accentuating, it may be, this or that column or frieze or pediment. Kekulé's brilliant and suggestive theory of chain compounds and ring compounds did more than any other theory to guide and stimulate research in chemistry in recent times. Like Gay-Lussac's theory of gaseous combination, though built in the first place only upon a few facts, this theory has proved true of the thousands of others with which we have since become acquainted; there seems indeed to be a need of a new psychology to account for such truly marvellous foresight as is here exhibited. The atoms forming these varied structures were, however, regarded as being arranged in a plane, until the great discoveries of Pasteur made it necessary for chemists to extend their conceptions and to frame hypotheses of three dimensions. Thus have arisen in the hands of Le Bel and van't Hoff and others our modern theories of stereo-chemistry. When isomerism occurs in an element Berzelius names it allotropy. It seems to me that now, when molecules of the elements do not differ essentially from molecules of compounds, there is no longer any distinctive meaning in the term, and that it might well be abandoned. I would like also to make another suggestion respecting nomenclature: that when we distinguish ring compounds as *cyclic* we might appropriately adopt the word *hormathic* (from the Greek word for a chain or a row) for chain compounds.

But in order to understand the linking of atoms in these molecular edifices some combining value had to be assigned to the different atoms. This idea of valency of the atoms was, no doubt, implied in Gerhardt's theory of types; but it did not gain much attention until later, when Frankland and Kolbé formulated an empirical theory of variable valency. Kekulé thought that atoms could not vary in their valency; but the alternative formulæ which he put forward to explain cases of difficulty would appear to be, rather, an attempted explanation of variable valency. It might be more correct to say that Kekulé's formulæ constitute an anticipation of Werner's theory of auxiliary valencies, the theory which seems to find most favour at the present day. Fixed valency can scarcely now be defended, in view of the existence of such compounds, for example, as the two fluorides and the two chlorides of phosphorus; the two oxides of carbon, ammonia and ammonium chloride; and, for example, the two series of compounds respectively of iron, mercury, and copper. Variable valency of atoms is, empirically at least, an establish fact.

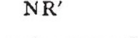
By the latest conceptions of variable atomic valency and its extension almost without limit—so that, for example, oxygen may be regarded as quadrivalent and even sexivalent—no doubt the existence of numerous compounds which previously presented difficulties can be explained. There are, however, others long known to chemists, such as double salts and the combination of water with salts, formerly called "molecular compounds," definite and individual, in which these views do not assist us. These compounds do not exist as gases, and unless they admit of experimental study by the methods of dilute solution, even their gaseous molecular weights cannot be ascertained.

It is noteworthy that in most of the instances recently investigated where variable valency has been assumed the compounds studied have been easily decomposable solids or liquids, and for one reason or another their gaseous molecular weights could not be determined. Many of these compounds, indeed, only exist at low temperatures. As instances of work of this kind I may mention Collie and Tickle on quadrivalent oxygen in dimethylpyrone derivatives; Gomberg on triphenylmethyl; Landolf on acetone dihydrofluoride; Thiele and Peter on methyl-iodo-dichloride; and similar studies by Kehrmann, Willstätter and Iglauer, Bülow and Sicherer, Baeyer and Villiger, Archibald and McIntosh, Chattaway, Pfeiffer and Truskier, and others.

Another most interesting class of solids which are capable of existing in two isomeric forms distinguished from each other by such physical properties as density or colour are the Schiff's basis or anils. Some of these were studied by Hantzsch, who proposed to explain their existence by the Hantzsch-Werner stereo hypothesis:—



$$\begin{array}{r} HO.C_6H_4.CH \\ \| \\ NR' \end{array}$$



$$\begin{array}{l} HC.C_6H_4OH \\ \| \\ NR' \end{array}$$

But since only a few, and these not very satisfactory, compounds show this isomerism, which do not contain the hydroxyl group, other suggestions have been put forward to account for the isomerism, by Anselmino and by Manchot.

In my own laboratory, associated with Mr. F. G. Shepheard and also with Miss Rosalind Clarke, I have made a study of various Schiff's bases for the purpose of investigating the remarkable property which some of these bases exhibit of *phototropy*. By phototropy is meant the capability of reversible change of colour in solids depending upon the presence or absence of light. Incidentally, too, I wished to study another physical property which many Schiff's bases possess, in common with other substances, of reversible change of colour with raising or lowering of temperature. This property we have called *thermotropy*, and many old instances will be remembered of substances of simpler constitution which exhibit it: thus, when subjected to the temperature of solid carbon dioxide, ordinary sulphur becomes colourless, red oxide of mercury becomes yellow, vermilion becomes scarlet, and on return to the ordinary temperature the original colours reappear.

As has been pointed out in a recent communication by Biilman, it is most important in these discussions that we should be perfectly clear in the use of terms. I take it for granted that *isomerism* is a general term for compounds differing in some respect but having the same composition. If the molecules (gaseous) have the same weights they are *metamerides*; if of different weights they are *polymerides*. When solids crystallise in more than one form they are *polymorphous*. Now it does not seem reasonable to suppose that reversible colour changes such as those exhibited by phototropes or thermotropes involve such violent intra-molecular changes as the breaking and reconnecting of atomic linkages. For example, take the three bases, salicylidene-*m*-toluidine, which in the dark or immediately it is exposed to light is yellow, but on continued exposure to light quickly becomes orange, and changes back again to its original colour in the dark; salicylidene-*m*-aminophenol, which at ordinary temperatures is orange, but is much paler at the temperature of solid carbon dioxide, on raising the temperature to nearly the melting-point (128·9°) becomes orange red, and these changes take place in the reverse order, again on cooling; salicylidene-*p*-aminobenzoic acid, studied by ourselves and by Manchot and Furlong independently, shows a wider range of thermotropic change between bright yellow and blood-red, and is also phototropic. To explain such changes as these and the others of a similar nature previously referred to, I think some less drastic hypothesis should be sought than intra-molecular breaking, and consequent metastasis or polymerisation. Though doubtless the hypothesis of Hantzsch and Werner could be invoked, or the modified hypotheses of Manchot or Anselmino, I think there should be some simpler explanation. Someone suggests polymorphism. Now polymorphism means that a change of crystalline form takes place which might doubtless connote change of colour. If one watches phototropic crystals changing colour under the influence of light from yellow to red, and notices that after remaining in the dark the same crystals have changed back to the original colour, and, remember, that these changes can be repeated with the same crystals apparently without limit, it will not be considered likely that this phenomenon depends on a reversible change of crystalline form. In a communication to the Chemical Society some three years ago Mr. Shepheard and I put forward the following suggestion:—"Evidence is accumulating of reversible isomeric reactions, like those described in this paper, which are indicated by physical differences, such as changes of colour. It is possible that these may be explained by hypotheses, similar to that of Hantzsch and Werner, assuming intra-molecular rearrangement; but in the case of phototropy and thermotropy it should not be forgotten that the substances exhibiting these phenomena are solids. No one will doubt, however, that these differences of colour depend on isomeric change of some kind, but in the case of solids we know practically nothing of their molecules, not even of their relative molecular weights. The molecules of solids are probably far more complex than those of liquids or gases; indeed, they may be rather complex groups or aggregates of ordinary gaseous molecules, which would give rise to far more numerous possibilities of isomerism. It appears to us that phototropic and thermotropic reactions are more probably due to isomeric changes affecting the aggregation of molecules in solids than to intra-molecular change of molecules derived from a study of gases."

It seems to me that just as atoms may be structures built of sub-atoms of some kind, and just as molecules of gases are built of atoms variously linked together, it is reasonable to conceive that molecules might combine to form aggregates, particularly when constituting solids; that as the sub-atoms may be conceived to have a combining valency—and the atoms are already accredited with this property, and in addition, as is supposed with Thiele's partial or Werner's auxiliary valencies—molecules may have valencies also whereby to combine into molecular aggregates. It may be presumed that such aggregates are more complicated in structure, and thus may give rise to greater variety of isomerides, and be more readily transmutable than gaseous molecules. If such aggregates of gaseous molecules exist they might explain not only the easily changed isomerides recently studied, but also the large class of "molecular compounds" of the older chemists. I imagine someone saying that in suggesting this hypothesis—which by the way is not new, for it is mentioned in Ostwald's "Outlines"—I am violating the canon to which I have myself subscribed, as a condition of a scientific hypothesis, that it should be verifiable. Perhaps we carry our critical faculty sometimes too far. It is most highly scientific to doubt, but doubt which is merely destructive has little value; rather, with Descartes, it should lead on to construction, for he who builds even imperfectly is better than he who simply destroys. And I do not doubt that some way will be found to study solids and obtain data that will lead to the determination of their molecular aggregate weights. The study of molecular volumes of solid solutions; the remarkable results obtained by Pope and Barlow; Tutton's work on crystallography, and much besides, induce the hope that some day solids, like gases, will find their Avogadro.

PART III.

Pursuit of Chemistry Justified by its Useful Applicability.

In the pursuit of all this abstract theory, and still more so in the bewildering multitude of undigested individual facts, there is danger that important and fundamental, even moral, considerations may be lost sight of. For example, take the fundamental question: Why should we pursue chemistry? No doubt it is considered by its votaries, those who seek in our laboratories to advance the science, that they are entitled to have provided for them, and will be rewarded by the provision of, the ordinary means of livelihood; but these, it will scarcely be denied, could generally be far better assured by other pursuits. It is suggested that intellectual discipline is a reason; but, I ask, for what purpose? Will anyone pretend that intellectual discipline without utilitarian object, without the possibility of using it for the betterment of society, is a worthy pursuit? I think not. But, in any case, none of us have devoted ourselves to chemistry merely for the sharpening of our wits. Again, someone suggests that chemistry and learning generally should be pursued for their own sake. In a recent most interesting and inspiring academic address[1] Prof. Sir Walter Raleigh commends "those who seek nothing from knowledge but the pleasure of understanding." If such a statement bears its most obvious meaning then, I venture to think that, in common with intellectual discipline without the intention of applying to a useful object the intellect so trained, such a reason is selfish, inadequate, and unworthy, and does not justify the pursuit of anything. No; research in chemistry apart from the possibility of applying it to the advantage of humanity cannot be defended. The mastery of the seemingly unlimited resources of Nature which chemistry achieves more and more and its use to alleviate the misery and add to the happiness of mankind are the only worthy and effective defence. And that this is the underlying ideal, in point of fact, that leads the chemist onward, not necessarily that he is always conscious of it, but always when he reflects, I think cannot be doubted. But, of course, no narrow idea of utility must be aimed at. Practically any chemical inquiry may lead to results of material advantage. Certainly nothing could be more mischievous than to make a narrow immediate utility the test. It would be easy to illustrate all this from the records of science, but instances in point are so well known that it is unnecessary.

On the other hand, it should not be forgotten that in making use of the manifold advantages derived from the growth of science, humanity, on its part, owes a great debt to scientific inquirers, and ought to feel it a sacred duty to do in return all in its power by support and encouragement to further scientific research. As Sir Walter Raleigh, in the address already referred to, says:—"It is so easy to use the resources of civilisation that we fall into the habit of regarding them as if they were ours by right. They are not ours by right; they come to us by free gift from the thinkers."

Some Concrete Applications of the Science.

That this advantage to civilisation has been, and is, the result of the pursuit and consequent advance of chemistry is happily a truth that is well known. There is scarcely an industry or a profession that has not been materially influenced or even created by the discoveries of chemistry, and therefore the welfare of nations is most intimately concerned in promoting its advancement. Now, it is common knowledge that no country has appreciated this to the same degree as Germany. It will, therefore, be worth our while to consider a moment the inauguration in Berlin, a year ago, of an entirely new institution, the Kaiser Wilhelm Institut, for the promotion and organisation of chemical research. This research is to be effected throughout the German Empire, in the universities, the technical high schools, or in works, and it is supported mainly, at least at first, by subscriptions of the chemical manufacturers. An address of very great importance was delivered at its opening by Prof. Emil Fischer, than whom, perhaps, no one living has added more to the progress of chemistry. A translation of this address appeared in NATURE, and, with additions, has since been published in a convenient book form.[2] In this address an authoritative account is given of the main contributions of chemistry to the national welfare, which even to those familiar with the subject must be astonishing in their importance, variety, and universality. It includes the applications of the science to problems of nourishment, to agriculture, and food supply; to engineering, metallurgy, cements; to clothing, artificial silk, or to colouring—dyes; to indiarubber production, both natural and artificial; to perfumery—artificial violet and other artificial floral perfumes, even that of the rose; to synthetic camphor; to drugs and synthetic materia medica, including the recent arsenic and selenium organic compounds which promise so much in the treatment of cancer and other fatal diseases; to radio-activity, to therapeutics, to the destruction of pathogenic microbes; to methods of sewage disposal; to the preparation of efficient ex-

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[1] "The Meaning of a University." (Clarendon Press, 1911.)

[2] "Chemical Research in its Bearings on National Welfare." (London, 1912.)

plosives; and to many other useful objects. In connection with the manufacture of explosives the public should know that the ability to wage war is becoming more and more dependent on the work of chemists. When the supply of mineral nitrates is exhausted, or even before that event, the requisite nitrogen compounds will have to be provided in some other way, and almost certainly they will be obtained synthetically from the atmospheric gases which even now are becoming a commercial source.

The Time-spirit and Science.

But students of history know that there are certain periods that for some unexplained reason are specially fruitful in certain departments of intellectual or artistic development. Prof. Sir Walter Raleigh, for instance, a high authority on this subject, says:—"The human body, so far as we know, has not been improved within the period recorded by history; nor has the human mind, so far as we can judge, gained anything in strength or grace." Further, regarding literature:—"The question is not by how much we can excel our fathers, but whether with toil and pains we may make ourselves worthy to be ranked with them." Again:—"In the beautiful art which models the human figure in stone or some other enduring material, who can hope to match the Greeks? In the art of building who can look at the crowded confusion of any great modern city, with all its fussy and meaningless wealth of decoration, like a pastrycook's nightmare, and not marvel at the simplicity, the gravity, the dignity and the fitness of the ancient classic buildings? How can the seasoned wisdom of life be better or more searchingly expressed than in the words of Virgil or Horace, not to speak of more ancient teachers?" Thus all things are not progressing. The time-spirit now, and for some two centuries past, seems to have chosen to take under its particular guardianship the physical and natural sciences, their cultivation and applications, rather than philosophy or architecture or sculpture, or painting or literature. We shall do well to recognise this, and not waste our resources in striving to fight against it.

Present Indiscriminate Elementary Teaching and Neglect of Research.

Large sums of money are expended in this country on the diffusion of some knowledge of chemistry among all classes of scholars and students; in fact, scarcely anyone escapes from a smattering, largely undigested if not indigestible, either forced on them by regulations or by allurements of bribes in the form of prizes, scholarships, or academic laurels. And if this is not good for scholars and students, it is worse for masters or professors. Our professors work "whole time" at this "stall-feeding" process, and if they happen to be strong men mentally and physically they may be able when weary with work to devote any overtime to—what I submit is far the more important matter for the State—the advancement of science by research. But this pursuit requires, for its successful prosecution, for resource and initiative to be at their best, that all the faculties should be in readiness in their fullest strength, freedom, and adaptability. How many, alas! are not strong men, and in their praiseworthy endeavours, notwithstanding, to contribute something to the achievements of their time succumb as martyrs to their devotion. The truth of this statement, I fear, is too well known to many of us here. In Germany this strain of elementary teaching is more recent, and is only now being felt. Prof. Emil Fischer in his address (*loc. cit.*) says of it: "During the last ten years a scheme of practical education of the masses has developed." "But this very education of the masses tends mentally to exhaust the teacher, and to a great extent, certainly to a higher degree than is desirable or indeed compatible with the creative power of the investigator, there prevails in modern educational laboratories a condition of overstrained activity." And again, "In the harassing cares of the day the teacher too readily loses that peace of mind and broad view of scientific matters necessary for tackling the larger problems of research." Laboratories, he says, are wanted "which should permit of research in absolute tranquillity, unencumbered by the duties of teaching." I have given these quotations from Prof. Fischer's address as indicating the matured judgment of a highly competent authority, communicated in the presence of the German Emperor on an historic occasion. His words are words of great weight, and no country which regards its future welfare can afford to ignore them.

Sir Walter Raleigh (*loc. cit.*) says that every university is bound to help the poor . . . but that does not mean that a university is doing good if it helps those who have no special bent for learned pursuits to acquire with heavy labour and much assistance—just so much as may enable them to pass muster; on the contrary, it is doing harm. I would like to invite the attention of all who are seriously interested in the country's welfare to reconsider the present policy in the teaching of chemistry· and this applies also to other sciences. For the advancement of civilisation, for the increased welfare of the race by the technical applications of our science, it is not the indiscriminate teaching of the masses and the multiplication of examinations that is wanted, but the training of the few, of capable investigators. I do not propose necessarily that we should interfere with, or much less abandon, much of our present elementary teaching, and I know that elementary, largely technical, training in chemistry is needed for medicine and engineering; but I do propose that our first endeavour should be to secure under present conditions in the present college or works laboratories, or in laboratories to be specially provided, that capable men, of whom we have many, should be able to devote themselves to research without the worry of teaching and examining or of providing the ways and means of livelihood. There is, happily, reason to believe that this vital need is to some extent becoming known; for there have been several recent instances where a particular investigator has been afforded the means, financially, of prosecuting his particular researches in tranquillity. The diversion of endowments to such purposes, instead of their going to the foundation of additional school or undergraduate scholarships, cannot be too highly commended.

We may learn a lesson which bears on this from that remarkably prolific period of our science, the close of the eighteenth and the beginning of the nineteenth centuries. It was then no easy matter to pass the precincts of a chemical laboratory; only the fittest survived the ordeal. At the beginning of the nineteenth century the traditions of Berthollet and Lavoisier in Paris were kept alive by Gay-Lussac; in England those of Cavendish and Priestley by Davy; and Berzelius in Sweden worthily maintained the older school of Bergmann and Scheele. By a happy fate the interest of Alexander v. Humboldt was the means of both Liebig and Dumas being admitted to the intimacy of Gay-Lussac; and in Sweden Wöhler was fortunate to gain the confidence of Berzelius; and in London, Faraday that of Davy. The achievements of these men—Liebig, Dumas, Wöhler, and Faraday—are part of the history of science. To me it contains a lesson, in point, of great importance. The opportunity offered them was beset with difficulties. No bribes such as scholars or students expect to-day were offered them; they knew no examinations, and their available apparatus and laboratory equipment were of the smallest and crudest description; but they were eager students with whom the master was in sympathy, and it is common knowledge that they completed the foundations of our science. Now I ask, considering the thousands of students whom we teach and examine to-day, are we doing as well in the interest of the country as our predecessors a century ago? Who can confidently answer in the affirmative? No; whatever else is done, the country needs the provision of men whose untrammelled energy should be devoted to original chemical research. Even as intellectual discipline the value of research is of the highest importance. In his address to the British Association at Winnipeg, Prof. Sir J. J. Thomson bears testimony to this. He says: "I have had considerable experience with students beginning research in experimental physics, and I have always been struck by the quite remarkable improvement in judgment, independence of thought, and maturity produced by a year's research. Research develops qualities that are opt to atrophy when the student is preparing for examinations, and, quite apart from the addition of new knowledge to our store, is of the greatest importance as a means of education."

And the object and ideal are wrong also in our system of technical training. We aim too much at giving elementary instruction to artisans, which, though important in itself, can never take the place of the higher education of leaders or managers of industrial works. This is different in Germany, where, although the training of artisans is by no means neglected, the chief energy is directed to the training and teaching of the smaller class of managers. There is, too, in Germany a far more intimate relation between academic and industrial work, and the leaders in each often interchange posts. In one respect we have an advantage over Germany; it is important that this should be understood. The higher technical instruction across the Rhine has not been undertaken by the universities, but is carried out in separate institutions. With us the universities have gradually undertaken, in addition to the older technical subjects, theology, medicine, and law, the various branches of engineering and agriculture, and even commerce. This, it is to be hoped, will be extended so that the highly trained technologist may have the advantage of the undoubted humanising influence of the university.

Conclusion.

I have not attempted in this address any complete survey of chemistry, either its growth in the past or its present condition, but I have endeavoured to give some account of the sort of thing chemistry is—of its method—and to maintain three theses: (1) That the logical method by which chemistry advances is not a simple one, and requires as one essential element the use of a highly developed imagination. To render this more efficient I have advocated special training. (2) Without violating, I hope, the canons of the proper use of hypothesis, I have proposed, in order to account for certain isomeric and other phenomena, the conception of solid molecular aggregates, although I am not able at present to indicate precise methods for its further investigation. These molecular aggregates are supposed to be formed by the combination of gaseous molecules just as the latter are formed by the combination of atoms. (3) As a matter of vital interest to the continued well-being of this country I have insisted strongly that our educational resources devoted to chemistry should be directed, in the first place and chiefly, to the highest possible training of promising students in the prosecution of research, and that the giving to the many of elementary instruction should be at least a secondary consideration.

Now I do not wish to dictate how this last proposition could be best carried into effect. I think we should distinguish three classes of chemists, or technical chemists, whose domains would more or less overlap. Occasionally there will be a man, like the late Sir William Perkin, who would combine all three. The three classes are: first, the pure chemist, devoted to scientific discovery only; second, the technical chemist, who prepares the discoveries of the pure chemist for the technologist, and has to determine such questions as economical production and, for example, the conversion of colours into dyes; third, the technologist or works manager. These three classes should be in close relation to one another. By such a scheme we should probably overcome by education one of our most serious present difficulties—the ignorance of owners of works of the value of science.

It is a matter deserving most earnest consideration whether, under the propitious influence of our own time-spirit, it would be possible to organise research and develop it without interfering with its essential freedom and initiative, and this in each of the three classes I have mentioned, either by means of some of our existing institutions, or by the inauguration here of such an organisation as the Kaiser Wilhelm Institut in Berlin.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY B. N. PEACH, LL.D., F.R.S.,
PRESIDENT OF THE SECTION.

The Relation between the Cambrian Faunas of Scotland and North America.

Introduction.

EVER since the announcement made by Salter in 1859 that the biological affinities of the fossils found in the Durness Limestone are more closely linked with American than with European forms, the relation between the older palæozoic faunas of Scotland and North America has been a subject of special interest to geologists. The subsequent discovery of the *Olenellus* fauna in the north-west Highlands furnished striking confirmation of Salter's opinion. This intimate relationship raises questions of prime importance bearing upon the sequence and distribution of life in Cambrian time in North America and north-west Europe, on the probable migration of forms from one life-province to another, and on the palæogeographical conditions which doubtless affected these migrations.

On this occasion, when the British Association revisits the border of the Scottish Highlands, it seems appropriate to refer to some of these problems. With this object in view I shall try to recapitulate briefly the leading features of the life-history of Cambrian time in Scotland and North America, to indicate the relation which these life-provinces bear to each other, and, from these data, to draw some inferences regarding the probable distribution of land and sea which then obtained in those regions.

The two great rock groups in Scotland that are universally admitted to be older than Cambrian time are the Lewisian Gneiss and the Torridon Sandstone. The Lewisian Gneiss, as mapped by the Geological