

AFRICAN ENTOMOLOGICAL RESEARCH
COMMITTEE.

THE subjoined official announcement of the appointment of an African Entomological Research Committee will be received with much satisfaction in all quarters where the importance of a scientific basis for administrative and other official action is duly recognised. Among the advances of biological science in the last few years, none has been more remarkable than the discovery that the cause of many diseases, the nature and origin of which had hitherto escaped detection, was to be sought in the presence of parasitic micro-organisms of various kinds and qualities in the tissues of animals and plants. The part played by insects and ticks in the dissemination of these morbid parasites is now known to be of immense importance, and great efforts have already been made, not without success, to restrict the occurrence of malarial and other disorders by the systematic destruction of the insect-carriers of the organisms concerned. For this purpose it is essential to distinguish with accuracy between various closely allied species; and it is here that the work of the skilled entomologist proves its necessity. It was well remarked by Mr. A. E. Shipley, in his recent presidential address at Winnipeg to the Zoological Section of the British Association, that "a few years ago no knowledge could seem so useless to the practical man, no research more futile than that which sought to distinguish between one species of a gnat or tick and another; yet to-day they knew that that knowledge had rendered it possible to open up Africa and to cut the Panama Canal." This witness is true; and it would be difficult to point to a more complete demonstration of the fact that natural knowledge pursued for its own sake, without any direct view to future utility, will often lead to results of the most unexpected kind, and of the very highest practical importance. It is this that justifies the demand that both governments and such private individuals as have the means should do all in their power to encourage the study and pursuit of science as science, without waiting for such applications as may prove to be of commercial or political value.

When the benefits to be derived from the scientific treatment of a subject are so manifest as in the present case, even the most indifferent of public bodies can hardly afford to stand aloof; and it is to be hoped that the activity of the Colonial Office in this direction, begun under the auspices of Mr. Chamberlain, and culminating for the present in the recent action of Lord Crewe, may be taken as an indication that the Government of this country is becoming increasingly alive to the desirability of securing the cooperation of scientific authorities in administrative measures. But beyond this, the movement will deserve a still greater welcome if it helps to emphasise the importance of encouraging the pursuit of genuine science, even when no immediate prospect is offered of material results.

F. A. D.

In view of the intimate relation which is recognised as existing between certain insects and the propagation of diseases of both man and animals in tropical Africa, and of the similar relation between insects and economic plants, which is becoming more evident as settlement in the continent progresses, Lord Crewe has appointed a Scientific Committee, the object of which will be to further the study of economic entomology with special reference to Africa.

This body will be known as the African Entomological Research Committee, and Lord Cromer has consented to act as chairman. The other members of the committee are:—Lieut.-Colonel A. W. Alcock,

C.I.E., F.R.S., Mr. E. E. Austen, Dr. A. G. Bagshawe, Dr. J. Rose Bradford, F.R.S., Colonel Sir David Bruce, C.B., F.R.S., Dr. S. F. Harmer, F.R.S., Dr. R. Stewart MacDougall, Sir John Macfadyean, Sir Patrick Manson, K.C.M.G., F.R.S., Mr. R. Newstead, Prof. G. F. Nuttall, F.R.S., Prof. E. B. Poulton, F.R.S., Lieut.-Colonel D. Prain, C.I.E., F.R.S., Mr. H. J. Read, C.M.G., the Hon. N. C. Rothschild, Dr. D. Sharp, F.R.S., Dr. A. E. Shipley, F.R.S., Mr. S. Stockman, Mr. F. V. Theobald, and Mr. C. Warburton. Mr. A. C. C. Parkinson, of the Colonial Office, is acting as secretary to the committee, and Mr. Guy A. K. Marshall as scientific secretary.

Arrangements are being made to despatch a trained entomologist to the east side of tropical Africa and another to the west, for the purpose of stimulating official and other residents to collect and observe noxious insects, and of affording instruction in the use of scientific methods. By this means it is hoped to obtain throughout these territories an organised body of investigators who will communicate all their collections and observations to the committee. These collections will be classified by a number of British and in some instances Continental entomologists, and named specimens will be distributed to such institutions as may require them for purposes of instruction, both at home and in Africa. The committee will also keep in touch with the work which is being done in this branch of science in Egypt and the Sudan and in South Africa. The scientific results, including all observations and experiments made by the collectors, will be published from time to time in a journal or bulletin to be issued by the committee.

It is hoped that by such organised cooperation the knowledge of these matters will be materially increased, so as to render possible the application of effective remedial measures. Offers of cordial assistance have been received from such institutions as the British Museum (Natural History), the London and Liverpool Schools of Tropical Medicine, and the leading universities, in all of which valuable work has already been done in the same direction.

THE BRITISH ASSOCIATION AT WINNIPEG.

THE Winnipeg meeting of the British Association has been a complete success. Hundreds of citizens of Winnipeg, together with representatives of science in Canada, the United States, and Great Britain, filled the Walker Theatre when the presidential address was delivered by Sir Joseph Thomson on August 25. Mr. Francis Darwin, the retiring president, was unable to be present, but he sent a letter, which was read by Prof. Carey Foster, expressing the hope that the meeting would be worthy of its distinguished president. The addresses of presidents of sections were delivered on August 26 and 27, and we are able to print two of them this week, with a summary of the lecture delivered by Dr. A. E. H. Tutton on the former date. We have been forced, however, to omit parts of Prof. Armstrong's long address to the chemical section in order to find space this week for Dr. Smith Woodward's address to the section of geology.

The *Times* correspondent at Winnipeg reports that the proposal to create a separate section of agriculture, which at present is a subsection of botany, has been rejected. On Saturday, August 29, there were various excursions to places of interest. Nearly a hundred members visited Portage la Prairie, and were conveyed in motor-cars through the wheatfields, the trip covering a distance of thirty miles. The visitors were thus given the opportunity of seeing one of the

finest wheat areas in the province, and of studying modern methods of garnering the harvest. The botanical section visited Winnipeg Beach, while the members of the engineering section inspected St. Andrew's lock, now being built by the Dominion Government in order to furnish uninterrupted navigation, *via* Red River, between Winnipeg and the lake bearing the same name. In Winnipeg itself a reception was given by Lord Strathcona at his former residence at Silver Heights, and was attended by about fifteen hundred people.

The *Times* also reports that one of the tangible results of the meeting of the Association in Canada has been the purchase by Dr. Gray, the Warden of Bradfield College, Berks, and president of the section of educational science, of a ranch of 2000 acres near Calgary. A competent Canadian has been appointed superintendent, and it is Dr. Gray's intention to afford an opportunity to Bradfield boys, on the completion of their school course, to acquire practical knowledge of the farming and ranching conditions of Alberta.

Dr. T. G. Bonney, F.R.S., will be president of the Association for the meeting to be held at Sheffield next year from August 31 to September 7.

SECTION B.

CHEMISTRY.

OPENING ADDRESS (ABRIDGED) BY PROF. H. E. ARMSTRONG, PH.D., LL.D., F.R.S., PRESIDENT OF THE SECTION.

AFTER an interval only a year short of a quarter of a century, it is my privilege again to occupy the chair of this section, and that, too, under conditions of special significance. The British Association has never before sought to carry the banner of science so far west into British Dominions—never before was it so clear that the progress of humanity is linked with the progress of science by an indissoluble bond: science defined in a word being *knowledge*, not mere work nor mere lip knowledge, but systematised established knowledge, not assumed knowledge—although hypothesis often serves to guide inquiry and truth is arrived at only gradually and slowly by a series of rough approximations. Moreover, science is true knowledge of *every kind*—there is too often a tendency to give a narrow interpretation of the word. One reason probably why the term does not produce any proper effect upon the average British ear is that it is not an English word but a mere adaptation from the Latin—a language which apparently cannot be engrafted upon our Saxon tissues, although, perhaps, it may be that we have so little feeling for it because we have been allowed to learn so little else in our higher schools; monotony of diet ever favours diminutive growth. Germans, I always feel, enjoy a great advantage over us in possessing the popular word *Wissenschaft*—in calling science the *business of knowing*, the *business of gaining wisdom*, of *being wise*.

Naturally I am constrained on the present occasion to take stock of the position of our science, to draw a comparison between the condition of affairs chemical when we met in Aberdeen in 1885 and their present state. No like period of human history has been more fruitful of advance; at the same time, no period illustrates more clearly the difficulties that lie in the path of progress—because of the innate conservatism proper to human nature.

Before attempting to deal with any of the problems which concerned us at Aberdeen, I will first briefly pass the more salient features of advance in review. Few probably are aware how extraordinary is the command we now have of our subject. In 1885, in defending the tendency of chemists to devote themselves to the chemistry of carbon, I could speak of the great outcome of their labours as being the establishment of the doctrine of structure. Everything that has happened in the interval is in support of this contention. It is interesting that in a recent lecture¹ on the Physical Aspect of the Atomic Theory, the most prominent living exponent of physical theories has given a not unwelcome

¹ The Wilde Lecture, 1908. By Prof. Larmor, Sec.R.S., Manchester Literary and Philosophical Society Memoirs.

recognition of our right-mindedness in saying: "As time goes on it becomes increasingly difficult to resist the direct evidence for the simple view that, in many cases, chemical combination is not so much a fusion or intermingling of the combining atomic structures as rather an arrangement of them alongside one another under steady cohesive affinity, the properties of each being somewhat modified, though not essentially, by the attachment of the others; and that the space formulæ of chemistry have more than analogical significance." And again in the following passage, in which a far-reaching confession is made: "The aim of structural chemistry must go much deeper (than dynamical methods of treatment); and we have found it difficult, on the physical evidence, to gainsay the conclusion that the molecular architecture represented by stereo-chemical formulæ has a significance which passes beyond merely analogical representation and that our dynamical views must so far as possible be adapted to it." The remark made by Helmholtz in one of his letters, "that organic chemistry progresses steadily but in a manner which, from the physical standpoint, appears not to be quite rational," must be regarded as little more than a confession that he was out of his depth. When properly understood, nothing could be more rational and logical than the way in which our theory of structure has been gradually built up on an impregnable basis of fact, with the aid of the very simple conceptions of valency postulated by Frankland and Kekulé. Our security lies in the fact that the postulates of our theory have been tested in an almost infinite variety of cases and never found wanting; this is not to say they are applicable in all cases, but merely that whenever we are in a position to apply them we can do so without hesitation. Larmor refers to the habit of physicists of taking comfort in Helmholtz's remark; it will be well if instead they make themselves acquainted with our methods and with the results we have won, with a minimum of speculative effort, by the cultivation of an instinct or sense of feeling which experience shows to be an effective guide to action. Now that physical inquiry is largely chemical, now that physicists are regular excursionists into our territory, it is essential that our methods and our criteria should be understood by them. I make this remark advisedly, as it appears to me that of late years, while affecting almost to dictate a policy to us, physicists have taken less and less pains to make themselves acquainted with the subject-matter of chemistry, and especially with our methods of arriving at the root-conceptions of structure and of properties as conditioned by structure. It is a serious matter that chemistry should be so neglected by physicists and that the votaries of the two sciences should be brought so little into communion.

The central luminary of our system, let me insist, is the element carbon. The constancy of this element, the firmness of its affections and affinities, distinguishes it from all others. It is only when its attributes are understood that it is possible to frame any proper picture of the possibilities which lie before us, of the place of our science in the Cosmos. But, as Longfellow sings of the sea in his poem, "The Secret of the Sea," "Only those who brave its dangers comprehend its mystery"—only those who are truly conversant with the root-conceptions of organic chemistry are in a position to attempt the interpretation of the problems of our science as a whole or even to understand the framework upon which it is built up. And yet we continue to withhold the knowledge of the properties of carbon from students until a late period of their development; indeed, when I insisted recently that organic and inorganic chemistry should be taught as one subject to medical students,¹ I was told that it could not be; that the attempt had been made with disastrous consequences. I trust that ere long the futility of such an attitude will be generally realised.

It is remarkable how much our conceptions are now guided by geometrical considerations. The development by van 't Hoff of the Pasteur hypothesis of geometrical asymmetry has been attended with far-reaching consequences during the period under review, the completeness with which the fundamental properties of the carbon atom are symbolised by a regular tetrahedron being altogether astounding.

¹ "The Reform of the Medical Curriculum" (*Science Progress*, January and April, 1907).

Our present conception is that the carbon atom has tetrahedral properties in the sense that it has four affinities which operate practically in the direction of the four radii proceeding from the centre towards the four solid angles of a regular tetrahedron.

More than analogical significance—to use Larmor's expression—must be accorded to this symbol on account of its remarkable accordance with the facts generally, whether derived from the study of asymmetric optically active substances or from observation of the activity of ring structures of various degrees of complexity. Nothing is more surprising than the completeness with which the vast array of facts included in organic chemistry may be ordered by reference to the tetrahedral model. In the future, when our civilisation is gone the way of all civilisations and strangers dig on the sites of our ruined cities for signs of our life, they will find the tetrahedron and the benzene hexagon among the mystic symbols which they have difficulty in interpreting; if, like the ancient Egyptians, we made our tombs records of our wisdom, such symbols would long since have acquired sacred significance and the public would probably have learnt to regard them with awe and to respect them as totems. Chemists might at least wear them on aprons in imitation of the Freemasons; perhaps no two other symbols have so great a significance—they reach into life itself.

It would seem that carbon has properties which are altogether special, the influence which it exercises upon other elements in depriving them of their activity is so remarkable. In their recent discussion of the relation of crystalline form to structure, in which valency is represented as a function of the volume sphere of influence exercised by an element, Barlow and Pope arrive at the remarkable conclusion that carbon is probably the only element the atom of which has a volume sphere of influence four times that of the hydrogen atom; although it combines with four atoms of hydrogen, silicon apparently has only half the volume sphere of influence of carbon. This may, in a measure, account for the very great dissimilarity in behaviour of the two elements, which is most pronounced in their oxides, the single atom of carbon all but dominating two atoms of oxygen in carbon dioxide (which is consequently gaseous), whilst the atom of silicon in silicon dioxide in no way eclipses the two atoms with which it is associated but leaves both charged with residual affinity which enables them to form complex collocations of remarkable fixity in the fire. At bottom the differences between organic and inorganic nature are to be regarded as very largely the expression of this difference. Ropes of sand are proverbially treacherous; yet without sand, if silica had been a gaseous substance, our world might have worn a strangely different aspect.¹

The mineral world apparently owes its rigidity to the fact that the metals and certain other elements are so imperfectly capable of dominating oxygen that oxides generally polymerise with great readiness, giving rise to substances which do not even fuse easily. The organic, on the other hand, appears to be plastic by reason of the close approach to neutrality which is conditioned by association with carbon.

Nothing is more striking than the remarkable diversity

¹ The solid model of silica which Barlow and Pope have constructed has very remarkable attributes, in that the oxygen atoms appear to be uniformly related and in intercommunication throughout its mass so that a mass of silica, whatever its size, may almost be regarded as a single molecular complex. A similar view may be taken of the plastic metals such as those of the platinum group, gold, silver and copper. Whether when rendered brittle by association with small amounts of impurity these are resolved into simpler molecular complexes or whether the molecules merely become separated by substances which promote discontinuity and brittleness, it is impossible to say at present. The cause of hardness in mineral materials is, however, a question of no slight interest and importance. The property is strikingly exemplified in the diamond. It is difficult to understand the intense hardness of this material, on the assumption that the diamond is composed of paraffinoid carbon—that is to say, carbon with all its affinities satisfied. At present we appear to have no clue to the manner in which affinity acts in promoting the formation of such solids. But it is obvious that all solids are possessed of some degree of "surface affinity," as they not only grow when placed in solutions but determine the separation of solid from a solution at a degree of saturation which is often considerably below that at which the solution is actually saturated with the substance; and such surface affinity, moreover, is selective, as the determinative effect is exercised only upon the substance itself or substances isomorphous with it—although exception must be made in favour of water, which all surfaces appear to attract. Sir James Dewar's observations on the condensation of gases by charcoal at low temperatures afford most striking illustrations of surface affinity.

of properties manifest both in the materials which at present we are content to call elements and in the compounds formed by their interaction; the range of variation met with in the case of the compounds of carbon with hydrogen and oxygen alone is almost infinite. We are almost compelled to attribute this diversity more to differences in the complexity and structure of the molecules than to differences in their material composition. The chemist, of necessity, must be a dreamer, knowing as he does that things are not as they seem to be. But this is not sufficiently remembered; indeed, students are systematically trained up in an atmosphere of pretence. The beginner is allowed to regard *elementary oxygen*, for example, as a colourless gas, which is generally harmless until things are presented to it in a more or less heated condition, whereat it takes umbrage and burns them up. He would regard elementary carbon as a soft black substance, which if smeared on the face of the white man makes him look like a nigger, were it not that he also learns that at times it is the hardest and whitest substance known; of organic chemistry, which alone can give him honest ideas of carbon, he is not allowed to hear, as I have said. The sting of awakening conscience is salved by the introduction of a long Greek word when he is told that the two substances, soot and diamond, are *allotropic* forms of the element carbon; nevertheless, he regards them both as elementary carbon. Gradually, perhaps, he awakens to a sense of the wrong that he has suffered at the hands of his teachers, as he realises that from no one substance can he gather what the properties of an element are, that after all the elementary substance is but an ideal—in other words, a mere concept. If appreciative, he then learns to think of the blandness of water, the sweetness of sugar, the sourness of vinegar, the causticity of soda, indeed every distinctive property of every known oxygen compound as more or less a property of, more or less conditioned by, the element oxygen; he is brought back, in fact, to the position from which Lavoisier started, as he realises that the oxygen gas which he inhales is not elementary oxygen; he can then perhaps appreciate the wonderful acumen which this greatest of chemical philosophers displayed when he wrote: "Nous avons donné à la base de la portion respirable de l'air le nom d'oxygène en le dérivant de deux mots grecs *ὀξύς*, acide, *γεννῶμαι*, j'engendre, parce qu'en effet une des propriétés les plus générales de cette base est de former des acides en se combinant avec la plupart des substances. Nous appellerons donc gaz oxygène la réunion de cette base avec le calorique." We have allowed a century to pass without recognising the wonderfully accurate powers of prevision displayed by Lavoisier; what is worse, we have been so far led astray that instead of regarding oxygen as the characteristic and attractive elements in acids, hydrogen has been allowed to usurp the position: the extent to which the cult of the hydrogen ion now dominates the text-books is well known; in days to come, when the history of our times is written, it will be referred to as a remarkable example of chemical shortsightedness.

Names are needed for the elements which would serve to distinguish the ideal elementary substances from the forms in which they are known to us. No more appropriate name than oxygen could possibly be selected for the fundamental material; if the *gen* terminal could be applied to elementary materials generally, it would be an advantage; it would not be easy, however, if this were done, to devise an appropriate separate name applicable to the active constituent of air.¹

¹ In naming the inert gas in air, which he ultimately termed *azotic gas*, having proposed the name *azote* for the element, Lavoisier had in view as alternatives the terms *alcaligen* and *nitrogen*. As there was no proof that the element was a constituent of alkalies other than ammonia, he rejected the former name on the ground that it might convey too broad an impression; in course of time the latter is become the popular name, except in France, where motives of piety have prevailed; but the French practice has been justified by the universal use of the term *azo* in connection with many nitrogen derivatives.

Had Lavoisier realised that the alkalies and basic oxides generally owe their basicity to oxygen as much as acids and acidic oxides generally owe their acidity to oxygen—the one being oxygen tempered by metal, the other oxygen tempered by non-metal—as the number of basic oxides far outweighs the number of acidic oxides, he might well have chosen the name *alcaligen* rather than oxygen. The choice he made was a particularly happy one and striking evidence of his genius and sense of euphony—for oxygen is *par excellence* the acid-forming element and is most truly called *sour-stuff*, the stuff of which sour things are made—for whatever the properties of the

In 1885 I closed my address with a reference to the structure of the elements which implied that their behaviour was that of compound substances; the feeling that this is the case has long been general among chemists. Our present attitude towards this problem is a curious one and not altogether satisfactory—it is impossible to deny that we have somewhat lost sense of proportion, even if our methods have not savoured of the unscientific. The discovery of radium appears to have upset our balance—we have been carried away by the altogether mysterious and unprecedented behaviour of this weird and wondrous substance. But may we not ask: Is radium an element? Has it not been too generally, too hastily assumed that it is? Little as we know of it, does not its behaviour straightway outclass it as an element? Surely it does! Is not the established fact that an emanation proceeds from it, which in turn decomposes and gives rise to helium, a proof of its compound nature? Again, is the evidence of such a character as to justify us in asserting that uranium is the parent of radium? If it be such, must not uranium also disappear from the list of elements; must it not indeed be removed on the ground that it gives rise to uranium without any reference to its supposed relationship to radium?

The answers given to such questions must depend on our definition of an element. At present we seem to be without one.

The conception that the breakdown of radium is spontaneous and apart from all external impulse or control is also one which should be received with caution. There is reason to suppose that in all ordinary cases in which compounds undergo decomposition spontaneously, the decomposition is conditioned by an impurity; the effect, moreover, is usually cumulative. This is true of highly explosive substances, such as chloride of nitrogen and gun-cotton, for example. It might be supposed that something similar would happen in the case of radium—but apparently such is not the case; it is assumed that occasionally a molecule explodes spontaneously, not only without being incited thereto, but also without in any way affecting its neighbours.

The alternative explanation that radium in some way acts as a receiver, transforming energy from some external source to which ordinary substances fail to respond and being thereby stimulated to decompose, is at present out of favour, although perhaps more in accordance with its peculiar behaviour.¹

The liberation of helium as a product of radio-active change is in itself a significant fact, in view of the possibility that helium may be an element of intense activity. Nothing in connection with the problem is more surprising, however, than the apparent production, in course of time, of a whole series of degradation products which differ initial oxide of a series, as the proportion of oxygen is increased, the acidic qualities are invariably strengthened.

The choice of a terminal connoting the elementary radicles which would be applicable generally and also acceptable is very difficult. If usage do not forbid change, probably our ears will decline to allow us to be systematic. The terminal *gen* is not applicable to many present names. In the interest of euphony, exception may be taken to the adoption of *ion* as a final syllable. In English ears most of the words with this ending have an ugly sound if pronounced so as to make it significant; moreover, our object is to secure a term which is applicable to the elementary material, whatever its state; the term *ion* is suggestive of a particular state—a state of chemical activity; and at present there is no agreement as to the nature of an ion. The terms atom, radicle (simple and compound), ion and molecule now all have their separate meaning and value and are indispensable.

The only terminal which seems in any way likely to be generally satisfactory in use is the terminal *yl*, which is already applied to organic radicles; its use might well be extended to radicles generally.

¹ I may here put on record the opinion Lord Kelvin expressed on this question in a letter to me dated September 13, 1906:—

"Ever since, nearly four years ago, we heard of the hundred calories per hour given out by radium, I have had on my mind the question of some possible mechanism such as that which you suggest by which energy from surrounding matter (far or near) could automatically come into radium to supply the energy of the heat which it gives out. The more I think of the question the less I see of that possibility. At present I can see nothing else than that the energy given out is taken from a previously existing store of potential energy of repulsive force between separable constituents of radium.

"The disintegration of the radium atom" is wantonly nonsensical. It is nonsense very misleading and mystifying to the general public, because, if what is at present called radium can be broken into parts, it is not an atom.

"Energy of an atom" implies a thorough misunderstanding of the meaning of the word energy, which is capacity for doing work.

"I admire most sincerely and highly the energy of the workers in Radio-activity and the splendid experimental results which they have already got by resourceful and inventive experimental skill and laborious devotion. I feel sure that as things are going on we shall rapidly learn more and more of the real truth about radium."

greatly in stability—such behaviour is entirely without precedent and not at all becoming in elements.

No such remarkable and inspiring problem has ever before been offered for solution. We can only wonder at the results and admire the genius which some have displayed in interpreting them, Rutherford in particular. Yet outsiders may well hold judgment in suspense for the present: whilst it is permitted to workers to make use of hypothesis in every possible way in extending inquiry, the public are in no wise called upon to accept such hypothesis as fact.

But apart from the suggestion that elements may give rise to others spontaneously, we have been entertained of late with stories of elements being converted into others under the influence of the energy let loose by the breakdown of radium. There is reason, however, to suppose that the powers of radium may have been greatly overpainted; energy of almost any degree of intensity in the form of high-tension electricity is now at our disposal, and the effect which radium produces on living tissues, glass, &c., is of the same character as that effected by the Röntgen ray discharge, the only difference being that the effect is produced somewhat more rapidly; it is not to be imagined, therefore, that the discovery of radium has put any very novel intensity of power into our hands.

I pass to the consideration of the classification of the elements. The recognition of certain properties, the association of certain ideals with the several elements, is a necessary step in classifying the elements in accordance with Mendelejeff's great generalisation—or rather it may be said to be both involved in and an outcome of Mendelejeff's conception.

Until recently our difficulty was to understand the relationship of the metallic and the non-metallic elements; now we are confronted with another problem—that of the existence of inert "paraffinoid" elements. It is commonly assumed that these are monatomic, but the evidence on which this assumption is based is absolutely unconvincing, and would be generally admitted to be so were we in the habit of looking before we leapt to conclusions. Assuming that the elements are compounds, the formation of inert compounds does not appear to be out of place, in view of the existence of practically inert hydrocarbons. But, on the other hand, in view of the properties of nitrogen, which is one of the most active of substances in the monatomic state, although an inert gas in the diatomic condition, it may well be that the inertness of helium and the other members of the argon group is also simulated. Sir James Dewar's observations have shown that helium and charcoal have no inconsiderable affinity at the boiling point of the former, which is within five degrees of the absolute zero, the molecular heat of absorption (apart from that due to liquefaction) of helium at that temperature being apparently as high as about sixty calories. The proof he has also given that helium alone does not convey an electric discharge is also of significance since the passage of a discharge through it under ordinary conditions is an indication that it can be included with other substances in a conducting system. Such evidence as there is therefore points to the elements under discussion being different from the others only in the degree of stability of their molecules.

Of late years the difficulty of classifying the elements has been increased rather than diminished, not merely because of the discovery of the inert gases but also on account of the apparent impossibility of ordering the position of an element such as tellurium in accordance with its atomic weight. There appears to be little room left for doubt that the value cannot be far removed from that of iodine; it should be considerably lower. It may be pointed out that the accepted value of selenium is closer to that of bromine than would be expected if a relationship were maintained corresponding to that between chlorine and sulphur. It would seem that Mendelejeff's original conception of the elements as a simple series in which the properties are periodic functions of the atomic weights must be abandoned in favour of some more comprehensive scheme. From the chemist's point of view, it is impossible to abandon the guiding principle underlying the arrangement in family groups, which dates back to Dumas; perhaps insufficient attention has been paid in the past to the maintenance of this principle.

Taking into account this principle, it is impossible to arrange a long series of elements such as the rare earths continuously in order of atomic weight, as they would be brought into every family in the table by such a procedure: the difficulty has been got over by Brauner, who has proposed to arrange a large number of the rare earths in a single vertical series under barium. Biltz has made a similar proposal.

The principle had been advocated by me previously in an article written for the "Encyclopædia Britannica."¹

In the arrangement I have proposed, it is not only assumed that there may be as many as sixteen vertical series of elements of which the elements from hydrogen to oxygen are initial terms, some series being at present unrepresented, it is also suggested that groups of elements occur in perhaps four of these series, numbers 4, 8, 12 and 16, the largest being that of the so-called rare earths in series 8.

The principle which is assumed to be in operation is that which is so clearly manifest in the case of hydrocarbons: successive vertical series of elements correspond to successive isologous series of homologous hydrocarbons. In the case of the hydrocarbons, the passage from one isologous series to another often takes place from a term several places removed from the origin of the series—for example, from benzene, C_6H_6 , which may be regarded as primarily a derivative of hexane to naphthalene, $C_{10}H_8$, which is not an immediate derivative of benzene but of butylbenzene. It is conceivable that at the genesis of the elements a process was at work corresponding to that by which a hydrocarbon such as naphthalene is derived from benzene, and by which the former then serves in turn as the point of departure for more complex hydrocarbons of other series. There is no reason, from this point of view, why progression should not take place along a particular line and that terms should exist in a series through which this line passes but below it—for example, that antimony and iodine may bear a direct linear relationship, but that tellurium, instead of being the element in the progression series in the oxygen group, is a homologue of greater weight. The same view may be taken of selenium. In this way, it would be possible to maintain selenium and tellurium in the oxygen-sulphur series, from which they cannot well be separated, whilst retaining Mendelejeff's conception of a genetic relationship along the series. The only departure involved is in assuming that instead of forming a single linear series ascending regularly in spiral progression—a series which can, as it were, be strung on a single spirally wound cord—the elements closely simulate a series of homologous isologous hydrocarbons. From this point of view, it is easy also to understand that some vertical series are unrepresented.

In discussing the chief attributes of the elements none is so difficult to deal with as that of valency, using the term in the broadest possible sense, not merely as indicative of the number of units of affinity but as including the, at present, all but incomprehensible problems of residual affinity and elementary character. I discussed the subject somewhat fully in my former address, dwelling especially on the properties of negative elements and their power of acting as linking agents; this view has met with ample confirmation in the interval, and will, I believe, be found to be of wide application in the future. I have already referred to the manner in which it is exemplified by silica.

The greatest advance in the discussion of the problems of valency in recent years is that made by Barlow and Pope, as their method of treatment is one which applies to solid substances—the correlation of structure with crystalline form which it effects promises to be of far-reaching importance.

Apart from hydrogen, carbon is the one element of certain character, always acting as a tetrad—its affinities may be only incompletely satisfied but they are always exercised, it may be supposed, even in ethenoid and similar compounds; carbon monoxide apparently is the only exception to this rule, its relative inactivity being one of the most puzzling enigmas of our science, especially as the oxide becomes one of the most active of known substances when only two atoms of hydrogen are added to it. Most other elements (non-metallic) seem to vary in valency, the valency beyond a certain minimum being dependent on the nature

of the association. Of late years, attention has been directed in particular to the quadrivalency of oxygen in many of its compounds.

The quadrivalency of sulphur in substances such as trimethylsulphonium iodide, Me_3SI , having been proved to demonstration by the production of optically active compounds of this type (Pope and Peachey), it can no longer be supposed that in such cases we are dealing with compounds in which the negative constituents of the parent molecules are conjoined, e.g. $MeI:SM_2$. And yet we must contemplate the existence of such compounds as possible—in the case of nitrogen, for example, as ammonia must be supposed to form the compound $NH_3:OH_2$ in preference to the hydroxide NH_4OH , the latter being only a very minor constituent, the former the major component of the aqueous solution of the gas; hydrogen chloride, on the other hand, appears only to afford one product with ammonia, viz. NH_4Cl . The existence of such differences affords clear proof in the case of the non-metallic elements other than carbon that valency is not merely a variable but also a reciprocal or dependent function.

There is no reason to suppose that hydrogen ever acts otherwise than as a simple monad; and the behaviour of the alkalis and alkaline earths in salts would seem to justify the conclusion that they have no tendency to vary in valency, were it not for the existence of well-defined non-volatile hydrides of these metals which are clearly substances of some degree of molecular complexity. Such compounds are illustrations of the difficulties which surround the subject. It has long been clear that the exhibition of the higher valency by an element is a process of a different order from that manifest when it exerts only its lower proper valency measured in terms of positive radicles such as H or C_nH_{2n+1} radicles. What that difference is we are not able at present to decide—carbon (together with silicon) differs from almost all other elements, especially in combining with hydrogen and analogous radicles to the extent of its maximum valency.

The proposition I made in 1888 (*Phil Mag.*, Series V., 25, 21) that the valency lines should, in some cases, be represented as passing through the atom, so that each is capable of acting in two directions, is the only consistent mode of expressing varying valency which has been devised, the only one, moreover, by which attention is directed to the great difference.

In many cases probably there has been a tendency to exaggerate the valency value—in the case of chlorine, for example, in assuming that it functions as a heptad in the perchlorates. In this and many other instances, it suffices to assume that the chlorine and oxygen atoms are united in a closed ring, the chlorine functioning as a triad. Some such explanation will doubtless be given of the structure of the metallic ammonias and similar compounds. The co-ordination values introduced by Werner serve only to establish certain empirical relationships and are useful for the purposes of classification. The perhaps more rational plan of dealing with such compounds suggested by Abegg has a similar value.

It is to the advantage of the hypothesis formulated by Barlow and Pope that the elements are represented as of constant valency in so far as their relative volume spheres of influence are concerned—the compound in which the higher valency is manifest being derived from that of lower valency by the opening out of the close packed arrangement and the insertion of certain new elements; but the fact that in such cases the volume is altered not in one direction alone in the crystalline structure but proportionately in all directions would seem to show that the volume sphere of atomic influence does actually change; the change is one, however, which affects all the atoms in the complex proportionately.

At present, unfortunately, our methods of treating the problems of valency are such that we cannot in any way give expression to the energy side of the phenomena.

Of late there has been talk of electrons in this connection, but what is said is little more than superficial paraphrase, in the advanced scientific slang of the day, of the ideas which have long been current. When, following Odling, we represent valency by dashes written after the elementary symbol, we give clear expression by means of a simple convention to certain ideas that are well understood by all

¹ *Cf.* Roy. Soc. Proc., 1902, vol. lxx., pp. 86-94.

among us who are versed in the facts; to speak of electrons and use dots instead of dashes may serve to mislead the unwary, who hang on the lips of authority, into a belief that we have arrived at an explanation of the phenomena, but those who know that we have reached only the let-it-be-granted stage and who feel that the electron is possibly but a figment of the imagination¹ will remain satisfied with a symbolic system which has served us so long and so well as a means of giving simple expression to facts which we do not pretend to explain. Not a few of us who listened to the discussion of the nature of the atom at Leicester could not but feel that the physicists knew nothing of its structure and were wildly waving hands in the air in the endeavour to grasp at an interpretation which would permit of mathematical interpretation being given to the facts. Until the credentials of the electron are placed on a higher plane of practical politics, until they are placed on a practical plane, we may well rest content with our present condition and admit frankly that our knowledge is insufficient to enable us even to venture on an explanation of valency.

In 1885 and again in 1888, I ventured to call in question the interpretation of valency which Helmholtz had given in the Faraday lecture in 1881. On the present occasion, I would insist still more emphatically on the insufficiency of the atomic charge hypothesis; especially that it affords no satisfactory explanation of variable valency and of those fine shades of difference which are manifest, especially in the case of nitrogen, when the radicle attached to the dominant element is varied. In 1885 I discussed this question with reference to the nature of electrolytes and questioned the conclusion Helmholtz arrived at that electrolytes belong to the class of typical compounds the constituents of which are united by atomic affinities, not to the class of molecular aggregates. The opinion I then ventured to give was as follows:—

“The current belief among physicists would appear to be that primarily the dissolved electrolyte—the acid or the salt—is decomposed almost exclusively. We are commonly told that sulphuric acid is added to water to *make it conduct*, but the chemist desires to know why the solution becomes conducting. It may be that in all cases the ‘typical compound’ is the actual electrolyte—*i.e.* the body decomposed by the electric current—but the action only takes place when the typical compounds are conjoined and form the molecular aggregate, for it is an undoubted fact that HCl and H₂SO₄ dissolve in water, forming ‘hydrates.’ This production of an ‘electrolytical system’ from dielectrics is, I venture to think, the important question for chemists to consider. I do not believe that we shall be able to state the exact conditions under which chemical change will take place until a satisfactory solution has been found.”

The position is not very different now. Although the propagation of the ionic dissociation cult has assumed the form of a fine art, we are still as far as ever from agreement as to the nature of chemical change; the speculation has not helped us in the least to clarify our ideas; at most we learn that interactions are between ions, and even these, as a rule, are supposed to remain apart until they enter into the solid state. Throughout all these years I have never varied my opinion that the dissociation hypothesis is incompatible with the facts. On more than one occasion I have stated definite reasons which induce me to deny its usefulness,² and these arguments have never been met; in fact, there has been little but a conspiracy of silence on the part of the upholders of the creed.

A large amount of work bearing on the subject has been done, chiefly by H. Brereton Baker. Strangely enough, no proper notice of his results has been taken outside England, and even there the importance of the observations has not been sufficiently appreciated. Perhaps the most remarkable

¹ In my opinion the experimental evidence is in no way satisfactory. It appears to me to be desirable that in studying the phenomena of electric discharge in gases and especially in vapours of complex substances, the horrible pitfalls should be taken into account with which the field of work is studded; unless every precaution to secure purity—precautions such as Baker and Dewar have taught us to use—be taken at every step, the conclusions based on all such observations must be open to grave doubt.

² Compare Chem. Soc. Trans. 1895, 1122; Royal Soc. Proc. 1886, xl., 268; 1892, lxx., 99; 1903, lxxii., 258; 1904, lxxiii., 537; 1906, lxxviii., 204; 1907, lxxix., 586; 1908, lxxx., 80; Science Progress, April, 1908.

feature in the situation is that Baker himself scarcely seems to be alive to the meaning of the evidence which he has supplied; the attitude which he has displayed in his recent Wilde lecture can only be described as halting. Baker has shown, in case after case, that the occurrence of change is dependent on the presence of moisture, his greatest feat perhaps being the observation that it is possible not only to prepare nitrous anhydride in the solid and liquid states but to volatilise it unchanged if only water be excluded.

I venture to think there is only one point of view from which the problem of chemical change can be approached, that, namely, which we owe to Faraday—to which hitherto justice has in no way been done—on which I dwelt persistently in my previous address: that the forces termed chemical affinity and electricity are one and the same. In every case of chemical change there is a coincident electrical change, an electric flux; on the other hand, every case of electrical change is accompanied by chemical change, some alteration in molecular configuration is effected; the force of chemical affinity is in some way disturbed by a momentary displacement of the molecules when a current passes through a conductor. Such being the case, the conditions determinative of chemical change can only be those which permit of an electric flux. Two substances in apposition do not give rise to a current; at least three are required to determine a slope of potential. Chemical change can only take place if one of the three be an electrolyte. In all cases apparently the chemical change supervenes upon the electrical, the electrolyte being resolved into its ions, one of which at least combines coincidentally with the adjacent electrode. Apparently these considerations are applicable to changes generally. And it should be added that, according to this view, the catalyst actually determines the occurrence of change.

The only other criterion which it is necessary to apply in order to decide whether change be possible in any given case is to consider if the change contemplated be one involving development of energy. It is important to remember also that a change which could not otherwise take place becomes possible when a suitable depolariser is introduced into the circuit.

The evidence that similar considerations apply to the gaseous and the liquid states cannot well be gainsaid. Before framing a theory of chemical change it is therefore necessary to formulate a definition of an electrolyte. It is doubtful if any single substance be an electrolyte; the conductivity of fused salts may well be and probably is conditioned by some admixture. Aqueous solutions of alkalies, acids and salts without exception are electrolytes. *Everything points to the fact that in such solutions the solvent and solute act reciprocally; the contention that the solute alone is active cannot be justified.* As water is altogether peculiar in its activity as a solvent and is a solvent which gives rise to conducting solutions, an explanation of its efficiency must be sought in its own special and peculiar properties.

Since 1886 this conclusion has been impressed upon me with indisputable force, and I have frequently ascribed the effect produced by the one constituent upon the other in a solution to the residual affinity of the negative elements in the two compounds which act reciprocally. It was only recently, however, that I saw my way to postulate a complete theory which would serve to account for the properties of solutions and generally that I realised how the reciprocal effect might be produced.

I would substitute for the misleading conception that liquids are comparable in their behaviour with gases the idea that the liquid state is one in which the residual affinity of the negative elements in particular always comes into play and causes the formation of molecular aggregates of various degrees of complexity; moreover, that the alteration in the properties of any given solvent by the dissolution in it of another substance is largely, and, in some cases, mainly due to a disturbance of the equilibrium natural to the solvent by an alteration in the proportion in which the several aggregates are present. The alteration in some particular property produced in a given mass of the solvent may, from this point of view, be taken as the measure of the activity of a substance, just as the alteration in the pressure of a particular volume is taken as the measure of the alteration produced in a gas. In the case

of non-electrolytes, if only a small amount of the solvent be withdrawn by combination with the solute, the alterations may be regarded as almost entirely due to the "mechanical" interference of the substance introduced, opportunity being given for the simpler, more attractive molecules of the solvent to exist in greater proportion because of the diminution of the chance of reuniting which is conditioned by the presence of practically inert molecules of another kind; if a more or less considerable amount of the solvent become associated with the solute the conditions become more complex, but similar considerations apply. From such a point of view a liquid is rendered more active by the addition of any soluble substance. Its vapour pressure is therefore diminished; the internal "osmotic" stresses are raised; its freezing-point is lowered.

Although it is generally admitted that water is not a uniform substance but a mixture of units of different degrees of molecular complexity, the degree of complexity and the variety of forms is probably underestimated and little or no attention has been paid to the extent to which alterations produced by dissolving substances in it may be the outcome and expression of changes in the water itself. The attempt to extend the "laws" which are applicable to the gaseous state to liquids has led us away from the truth by narrowing our conceptions. If the contention be justifiable that the alterations attending dissolution are very largely alterations in the character of the water, attention has been directed of late far too exclusively to the dissolved substance.

To give emphasis to the view, I have advocated¹ the restriction of the name *water* to the liquid mixture and have proposed that the simple molecule represented by the symbol OH_2 be termed *Hydrone*. The generalised expression



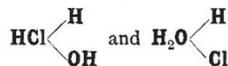
may be considered to be representative of the state of equilibrium in water—that is to say, of the character of the change which it undergoes when the conditions are varied either physically or by dissolving substances in it—in the sense that it pictures the resolution of the more complex into simpler forms and *vice versa*, without taking into account the variety of molecular forms (x, x^1, x^2, \dots) which are present.

It is probable that the agreement between "theory" and practice on which reliance has been placed, particularly in interpreting osmotic phenomena, is more often than not only apparent and fictitious, and but the outcome of counterbalancing effects which have been left out of account. We are too prone to believe in constants; we need to remember that, except perhaps in the case of the perfectly gaseous state, *constants are dependent variables*. To take an example, it is assumed that glucose and cane sugar produce like osmotic effects when used in equivalent proportions; indeed, it has been the fashion of late years to treat non-electrolytes as harmless neutrals: in point of fact they differ as much in behaviour as do electrolytes, and such a conclusion must be viewed with the gravest suspicion. Recently Dr. Eyre and I have been able to show that three substances so similar as methylic, ethylic and propylic alcohols produce effects in precipitating salts from solution which are markedly different, propylic alcohol being the most effective although the least soluble. It is clear that the precipitant does not act mainly by itself combining with and withdrawing water in direct competition with the salt; but that it promotes the *dissociation of water* by the mechanical interposition of its molecules; in fact, that the "dehydrating" powers of the *water* are enhanced owing to the increase in the proportion of simple molecules in the liquid conditioned by the presence of the solute.

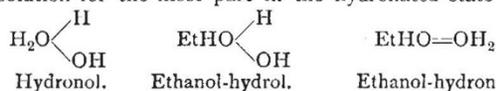
The same effect is obvious when the reduction of the electric conductivity of a salt, such as potassium chloride, by equivalent quantities of the three alcohols is considered. This amounts to about 6 per cent. in the case of methylic, 12 in that of ethylic, and 17 in that of propylic alcohol; the reduction effected by glucose, however, amounts to about 27, and that effected by cane sugar to no less than 42 per cent. In these two latter cases the amount of water actually withdrawn from the solution by the sugar is probably considerable, and the "mechanical effect" of the

solute is therefore exercised in a more concentrated solution—more concentrated, that is to say, than those in which the alcohols act. If, therefore, solutions of glucose and cane sugar of equivalent strength produce like osmotic effects, it is because unperceived compensating factors are at work in the solutions which in algebraic sum have the same aggregate influence.

To explain the effect produced by substances which give rise to conducting solutions when dissolved in water (acids, alkalis, and salts), it is necessary to consider the special nature of the changes which may be supposed to attend dissolution in such cases. Why, it may be asked, is an aqueous solution of hydrogen chloride a conductor, whilst that of alcohol is a non-conductor? I believe the answer to be that it is because, in the former case alone, the two components of the solution are *reciprocally distributed*; that it is because two correlative systems—



are produced which interact under the influence of the electric stress.¹ In the case of alcohol no such interchange takes place. It may be that the alcohol is hydrolyated to some slight extent, but the hydrol must be less basic than hydronol; probably, like ammonia, alcohol exists in solution for the most part in the hydronated state:—



Much more must be learnt of the properties of solutions before definite decisions can be arrived at with regard to such delicate and refined issues.

I would apply the interpretation here given of the nature of conducting solutions generally to the explanation of all cases of chemical change: in other words, I assume that in all cases correlative systems are present which are formed by the reciprocal distribution of the interacting substances. From this point of view the solvent is no mere medium but an active participant in the series of interchanges of which, as a rule, only the final product is noticeable.

The solution thus offered of the complex problem discussed very fully in my Address in 1885, which has ever since occupied my thoughts, will, I trust, be found to be helpful, although by no means complete in all its details.

In effect, the doctrine makes no demand which the chemist should not be able to grant forthwith, as it is generally supposed that hydrols are easily formed—to give an example, in the case of the conversion of chloral, CCl_3COH , into chloraldehydrol (chloral-hydrate), $\text{CCl}_3\text{CH}(\text{OH})_2$. The novelty of the conception lies in supposing that the occurrence of electrolysis involves the interaction of the hydrol and its correlative and the explanation which it affords of the difference between electrolytes and non-electrolytes.

It is essentially an association theory, although it involves the dissociation of the interacting substances but never the production of separated ions. In the case of aqueous solutions the amount of the distributed substances may be taken as the measure of the activity—of the degree of ionisation, so-called. A wrong view prevails that the so-called molecular conductivities are measures of activity; they are in reality only measures of the relative activities under corresponding conditions of the substances to which they refer. The molecular conductivity of an acid is at a maximum in its weakest solutions: presumably it is then present to the maximum extent in its simplest state and in the active hydrolyated state; but as a hydrolytic agent its activity is at a maximum near to the opposite end of the scale. In other words, the hydrolytic activities of a series of acids are in the order of their molecular conductivities in solutions of comparable strength, but molecular-hydrolytic and molecular-electrolytic activity run in opposite directions; the maximum electrolytic conductivity of an acid solution, which is manifest at a particular

¹ I would repeat the plea I put forward in 1885 that the use of the term hydrochloric acid as applied to hydrogen chloride is undesirable if not unjustifiable; the solution of the gas may be said to contain *chlorhydric acid*, $\text{HCl}(\text{OH}_2)_x$. From my point of view, oxygen is a constituent of all acids.

¹ Roy. Soc. Proc. 1908, xxxi., 80; *Science Progress*, January, 1909.

degree of concentration—presumably at the point at which the two forms of the distributed materials most nearly balance—is also in no way identical with maximum molecular hydrolytic activity. On these assumptions not a few of the deductions based on the ionic dissociation hypothesis are clearly fallacious.

It has been asserted that the association hypothesis does not admit of quantitative treatment, and that therefore it is at a disadvantage; but if the quantitative meaning given to various results in accordance with the tenets of the dissociation hypothesis be more often than not one which is inadmissible, little is gained by applying the speculation quantitatively. As already remarked, the only cases in which chemical and electrolytic activity can be compared by the methods proposed are those in which the comparison is made between solutions of comparable or equivalent strength—that is to say, between compounds arranged in vertical series in the order of their activity.¹ Electrolytes are comparable in most, if not in all, their properties when the comparison is made in this way; but order of activity is one thing, actual activity another. It is in this sense, and this sense only, that we may agree with Arrhenius in his statement, "L'activité électrolytique se confond avec l'activité chimique."

The ionic dissociation hypothesis is a beautiful mare's-nest, which fails apparently to fit the facts whenever it is examined. "And the moral of that is," to quote the words of the classical Duchess so well known to children, "we must not use the words *ion* and *ionisation* in any speculative sense but confine their application to cases such as were contemplated by Faraday when he introduced the term *ion*; the conception of activity, whether electrolytic or chemical, should alone be attached to such words; no idea of actual, separate, individual existence should enter into our minds in using them: the ion is to be thought of merely as the potentially active, transferable radicle in a compound, not as a separated particle enjoying independent existence." It is so easy to speak of dissociation when it is desired to give expression to the idea; the first thing the scientific speaker or writer should guard against is ambiguity.

The subject of gaseous interchanges must not be left out of account, although it is impossible to do justice to it. Mendelejeff's contention that gaseous interchanges are usually bimolecular has been defended by Dixon and Larmor of late. But the facts must be faced. The almost inconceivable frequency of the molecular impacts must not be forgotten. The extraordinary attractive power of the hydrone molecule is also to be remembered—this would tend to promote the formation of aggregates with which the necessary third substance would every now and then form a bimolecular system—which, however, would in reality be at least trimolecular. The proportion of hydrone molecules in a dried gas has probably been under-estimated—the density of hydrone being very low (9)—as no dehydrating agent can be supposed to remove all such molecules or even nearly all; the hydrated substance must have a certain pressure of dissociation. Sir James Dewar's appears to be the only method which is in any way deserving of the epithet absolute; the results he has obtained with helium in a radiometer are strongly in favour of my view. Lastly, the gradual growth in velocity of the explosive wave up to the point of detonation as the compression becomes greater is clear indication that reduction in volume and increase of opportunity for the formation of systems of the proper degree of complexity is a matter of great consequence. Even the behaviour of cordite is significant, particularly the projection of unburnt rodlets of the material from the gun: apparently it is not decomposed by shock intramolecularly but is decomposed by heat into gases, which interact explosively.

Having dealt with the subjects of chemical change and the nature of solutions, however inadequately, I must now

¹ Solutions of acids and alkalis have maximum conducting power at certain relatively high degrees of concentration. Hydrolytic activity also increases steadily in the case of acids as the solution becomes more concentrated; whether it attains to a maximum and whether this coincides with the conductivity maximum is uncertain at present; it is very difficult to decide this point experimentally, as the rate of change is so rapid in strong solution; moreover, the action takes another course in strong solutions, as compounds are formed by the interaction of the hydrolyte and hydrolist, so that two changes are superposed which cannot well be followed separately.

endeavour to justify my opening reference to the importance of the organic side of our science.

The province of organic chemistry is so vast that it may appear to be difficult to distinguish the main lines of advance from the by-paths which intersect the field of inquiry in every direction. In reality this is not the case; certain salient features stand out which must attract attention if once attention be directed to them. The efforts of the chemist to elucidate structure and to correlate structure with function have been extraordinarily successful. In the first place, as already remarked, the student of the subject now has his attention concentrated on the tetrahedron as the symbol of the *functional activity* of carbon; however numerous the compounds, he knows that certain simple rules can be laid down as applicable to all. It is established beyond question that carbon atoms have a remarkable tendency to form ring systems. The affinities of the atom seem to act almost rigidly in certain directions, which appear to be those of lines drawn from the middle point of a regular tetrahedron to its angular points. Rings containing either five or six atoms of carbon are therefore those which are most readily formed and of maximum stability; carbon atoms may and do unite in pairs, threes and fours, but compounds of this order are far less permanent than those containing either five or six atoms, as the affinities appear to meet in such a manner that they do not satisfy each other and consequently the compounds enter somewhat readily into combination with other substances. When the number of carbon atoms exceeds six, not only is there less tendency to form a ring system but the stability of the system is slight; when the number is considerable, stability is attained by the formation of complex systems, consisting of several rings conjoined (camphor, naphthalene, anthracene, &c.).

The behaviour of carbon compounds generally, in so far as this may be regarded as dependent on the condition of the carbon, is extraordinarily simple, and may be summed up in the statement that it is either paraffinoid, ethenoid or benzenoid.¹ Paraffinoid carbon is incapable of combining with other substances and but slightly attractive, so that the hydrogen atoms are by no means easily displaced from paraffinoid compounds²; ethenoid carbon combines readily with various substances, forming compounds of paraffinoid type; in the benzenoid state, carbon appears to combine somewhat readily with a variety of substances, but the products enjoy only an ephemeral existence and usually escape notice, as they at once break down, giving rise to benzenoid substitution derivatives, so that in this last form carbon simulates the paraffinoid state but is more active.

In earlier years our attention was concentrated on benzene and the benzenoid compounds; much was done to elucidate the structure of these hydrocarbons and of their derivatives; meanwhile these latter have proved to be of extraordinary significance technically, notably as dye-stuffs, but also on account of their medicinal value, as perfumes and in photography.

The structure of benzene has been the subject of much discussion during the period under consideration. I trust I shall not be accused of parental bias if I urge that the centric³ formula is the best expression of the *functional activity* of the hydrocarbon benzene and its immediate derivatives; the attempts which have been made of late years to resuscitate the Kekulé oscillation hypothesis in one form or another appear to me to be devoid of practical significance. Any formula which represents benzene as an ethenoid must be regarded as contrary to fact. But in considering the properties of benzenoid compounds generally, it is necessary to make use of the Kekulé conception as well as the centric expression. The model of benzene devised by Barlow and Pope subserves a somewhat different and complementary purpose, being primarily of importance

¹ A fourth condition requiring recognition is that of the carbon in acetylene; at present the acetylene compounds are so few in number, however, that this form may be left out of account.

² The displacement of the hydrogen associated with carbon is in all probability a secondary phenomenon; it is likely that this is true generally and that hydrogen is never merely removed or attracted away but always has its place taken by a radicle which becomes temporarily attached to the multivalent atom with which the hydrogen is associated.

³ I have discussed this matter somewhat fully in a recent essay, with reference to the nature of amorphous carbon, in connection with the remarkable work of Sir James Dewar on the absorption of gases by charcoal at low temperatures (*Journal of the Royal Institution*).

on the geometric side in discussing the relation of form to structure.¹

The discovery of trimethylene by Freund and the subsequent introduction of synthetical methods of preparing polymethylenes by W. H. Perkin, jun., mark the onset of a new era, opening out as they did the possibility of understanding the structure of camphor and the terpenes and other constituents of the volatile oils from plants.

Chemist after chemist had attempted in vain to solve the riddle presented by camphor. Suddenly, in a moment of inspiration, a satisfactory solution of the problem was offered by Bredt. The acceptance of the bridged ring, the special feature of the Bredt formula of camphor, marks the introduction of a new moment into organic chemistry.

The recognition of similar rings in several hydrocarbons of the terpene class, mainly in consequence of the masterly work of von Baeyer, has contributed in no slight degree to an understanding of these compounds; nevertheless, much remains to be learnt and there are many and serious difficulties to be overcome before we shall be in a position to appreciate the genetic relationship of all the substances included in the group. When the account of the work is written it will form one of the most striking and fascinating chapters in the history of our science.

Among the many names of those who have contributed to its development the first to be mentioned is that of Wallach, to whose unwearied efforts, continued during a long series of years, so much is owing. The synthetic work carried out with brilliant success in recent years by W. H. Perkin may also be referred to as of extraordinary promise but of well-nigh inconceivable difficulty.

Before leaving this chapter, reference should be made to the almost protean character of camphor, as disclosed by the work of inquirers such as Kipping, Pope, Forster, Lapworth and Lowry; no other substance has lent itself to use in quite so many directions and with such fruitful results. Special mention may be made of the demonstration which Pope has given, with the aid of the camphor-sulphonic acids, that nitrogen, sulphur, selenium and tin give rise to optically active substances in all respects analogous to those furnished by carbon. The success with which Kipping's arduous labours have been crowned is also very noteworthy, taking into account the many difficulties he has overcome in preparing optically active silicon compounds. The extension of the Pasteur-van 't Hoff theory of asymmetry inferentially to all elements which are at least quadrivalent, now accomplished, is of superlative importance.

Lowry's refined observations on the conditions which determine the interconversion of isodynamic forms of some of the camphor derivatives may also be cited as of special value as a contribution to the study of metamerism and the conditions which determine chemical change generally.

Not the least interesting feature of camphor is the light thrown by its behaviour on the influence which oxygen exercises as an attractive element and on the part which

¹ The time is now approaching when it will be possible to extend the study of benzenoid compounds beyond the formal and superficial stage; hitherto we have been content to develop the methods of preparing such substances and to determine their number and their distinctive properties. Everything has to be learnt as to the exact character of the changes which attend their formation from the parent substance benzene and as to the exact nature of their inter-relationship. The impression produced by benzene, in my mind, is that of an eminently plastic system capable of responding to every slight change that may be impressed upon it. Nothing is more remarkable than the difference between benzene and its homologues, so obvious in the extraordinary increase in activity which attends the introduction of hydrocarbon radicles in place of one or more hydrogen atoms. But such plasticity is not characteristic of benzene only: if the properties of benzenesulphonic acid be contrasted with those of the various substituted benzenesulphonic acids, it is clear that every variation meets with some response from the sulphonic group; what is still more remarkable, if the hydrogen in the hydroxylic group in the phenolsulphonic acids be displaced by other radicles, not only does the oxygen atom to which the radicle is attached seem to respond to the change but the benzenoid system and the still more distant sulphonic system are also affected. It is well known that the physical constants are all variables in the case of benzenoid compounds. Perhaps the most remarkable confirmation of the view here advanced, however, is that afforded by the conclusion arrived at by Barlow and Pope that in the case of benzene derivatives, although the spheres of influence of the carbon and hydrogen atoms are relatively the same as in the parent compound, the spatial arrangement of the component spheres of atomic influence remaining practically unchanged, nevertheless the actual volumes of the spheres of influence of both carbon and hydrogen alter proportionally to the alteration in molecular volume. Thus they maintain that in the case of the conversion of benzene (molecular volume 77.4) into tetrabromobenzene (molecular volume 130.2), the volumes of the spheres of influence of both carbon and hydrogen expand in the ratio of 77.4 : 130.2. Such a conclusion is very noteworthy.

spatial configuration may play in determining directions of change. It is clear that, whatever the agent, the attack is always delivered from the oxygen centre and that the direction in which the attack becomes effective depends on the position which the agent can take up relatively to the various sections of the molecule.¹

It must be confessed that our efforts to penetrate behind the veil in the case of the higher carbohydrates—starch and cellulose in particular—have not been rewarded with success.

Moreover, though much has been done of late years to unravel the nature of the vegeto-alkaloids, substances such as quinine are still only partially deciphered and not one of the more complex alkaloids has been produced synthetically. In view of the fact that quinine is still the one effective and practically safe anti-malarial medicine, the disclosure of its constitution is much to be desired. The isolation of adrenaline from the suprarenal capsule and the discovery that this alkaloid—which is an extraordinarily active substance physiologically—plays a most important part in controlling vital processes is of supreme interest. Other glands—the pituitary gland, for example—appear to contain peculiar active substances, which are of particular consequence in regulating animal functions. The discovery of such substances affords clear proof that life is largely dependent on what may be termed chemical control.

In addition to indigo, the simpler yellow and red natural colouring-matters have now been thoroughly examined, but this class of substance still affords abundant opportunity to investigators. Kostanecki's comprehensive studies of the xanthone group may be referred to as of particular value.

Attention may be directed here to the investigation of brazilin and hæmatoxylin by W. H. Perkin and his various co-workers, not merely as being full of interest and importance as a contribution to our knowledge of the relation between colour and structure and as a brilliant example of technical skill but because of the illustration it affords of the extreme intricacy of such inquiries and of the vast amount of labour they entail. The general public probably has not the slightest conception of the difficulties which attend such research work and of its costliness.

As an investigator of vegetable colouring-matters, no one has been more assiduous or has displayed greater skill of late years than A. G. Perkin. His recent refined investigation of the colour-yielding constituents of the indigo plant is of exceptional value at the present time, although it is to be feared that it comes too late to save the situation in India. The work of the brothers Perkin, it may be pointed out, is of exceptional interest on the human side as well as from the scientific standpoint, as their enthusiasm and wonderful manipulative skill afford a striking and noteworthy example of hereditary genius.

Two substances of commanding interest which have long resisted attack—the red colouring-matter of the blood and leaf-green—are at last going the way of all things chemical, as the secret of their nature is being wrung from them. In Willstätter's skilful hands chlorophyll is proving to be by no means the fugitive material it was supposed to be; the complexity of the problem it offers, however, seems to be far beyond anything that could have been anticipated; so much greater will be the interest attaching to the final solution. The discovery that green chlorophyll is a magnesium salt is of special importance, as the first clear indication of the manner in which magnesium salts are of service to plants.

Apart from the special interest which attaches to the investigation of vegetable colouring-matters on account of their being coloured substances, such inquiries are of value as furnishing material for the discussion of the metabolic activity of plants.²

¹ Cf. Chem. Soc. Trans.

² But a note of sadness pervades the story. The effect of learning to understand Nature always appears to be that we at once brush her aside when we have wrested from her the secrets which she has so long preserved inviolate. No sooner did we learn the nature of the madder colouring-matters than we proceeded to prepare them artificially—thus putting an end to the cultivation of a valuable crop. Indigo is meeting with a like fate, a catastrophe which might well have been avoided had scientific assistance been called in at the proper time. Not content with making natural colouring-matters, we set to work to outrival the rainbow in our laboratories and the feminine world is decked with every variety of colour in consequence, although unfortunately our blends too often lack the beauty of those of truly natural origin, which rarely, if ever, offend the eye. We congratulate ourselves on our cleverness in thus imitating Nature but no idea of thrift

Even colloids are being brought into line. Studded as they are with active centres (oxygen or nitrogen atoms), they seem to be able to attract and retain hydrone molecules at their surfaces in ways which give them their peculiar glue-like attributes: as a consequence living tissue appears to be little short of animated water.

To the present generation of students, the organo-metallic compounds must have appeared to belong to the past; the discovery of methides of platinum and gold by Pope will not only serve to re-awaken interest in this group of compounds but is of primary importance as a contribution to our knowledge of the valency of these elements; the stability of the platinum derivatives is altogether astonishing.

The discovery announced in June last, at the International Congress of Chemistry, by Mond, of compounds of carbonic oxide with ruthenium and uranium is a striking and most welcome extension of his previous labours, which had placed us in possession of carbonyls of nickel, iron and cobalt. The metallic carbonyls possess altogether remarkable properties: at present, these defy explanation; nickel carbonyl in particular seems to be an exception to all rules. The complex iron carbonyls made known by Dewar and Jones also have most fascinating attributes, the variety of colours they display being specially interesting. The marked individuality of the members of the iron group as exemplified in their carbonyl derivatives is in striking contrast with the tendency they display to behave as related elements; the deeper problems of valency are clearly exposed for consideration in such peculiarities.

The discoveries of the special activity of magnesium as a synthetic agent and of the superior value of nickel as a catalyst in fixing hydrogen are other illustrations of the individuality of metallic elements. We are greatly indebted to the French chemists for the invaluable preparative methods they have based on the use of these two agents.

Although satisfactory progress has been made in almost every direction, many of the nitrogen compounds are still not properly understood. It is clear that we are as yet in no way seized with understanding of the attributes of this element as we are of those of oxygen and carbon, particularly in the case of mixed carbon-nitrogen compounds: we can make nothing of the physical data such substances afford. Nitrogen, in fact, is an extraordinary element, far more remarkable than any other; its "temper" appears to vary more than that of any other element according to the character of its associates—nothing could be more remarkable, for example, than the change in properties from ammonia, NH_3 , through hydrazine, NH_2NH_2 , to azoimide, N_3H . No other element can be so poisonous, so immediately fatal to life. We lack a model symbolic of its functions—which means that we are unable to fathom its vagaries and reduce them to simple order.

The oximes and the diazo-compounds in particular have given rise to much dispute. Stereo-chemical formulæ have been assigned to these, but probably they have little relation with the truth; although they have been of service by supplying symbols which can be offered up at examinations, by confining attention they have served to sterilise inquiry. No better illustration could be given of the truth of the remark made by my friend the Professor that man is an idolater by nature, a fact that chemists should always bear in mind.

The compounds in question are difficult substances to handle, far too prone to undergo change without invitation—it is to be feared that many of the conclusions which have been arrived at are based on incomplete if not unsatisfactory evidence.¹ When I think of the state of our

possesses us: moreover, our attempts to imitate if not to undo her work are never direct but are always made with her aid, with Nature's product—coal; we are no longer content to ride on horseback but must rush through space and instead of watching the birds fly seek to emulate them but always with the aid of fuel won by Nature from the soil and air in days long past. Too much is being done in every direction to waste natural resources, too little to conserve them, too little to employ man in his proper place—as tiller of the soil. Here lies the chemist's opportunity. At no very distant date, perhaps, when petrol is exhausted, toll will be taken from the sun in the form of starch or sugar and this will be converted into alcohol.

¹ Since this was written, Thiele's discovery of "Azomethane," MeN:NMe , has been announced. This is described as being, in the solid state, a distinctly coloured, very pale yellow substance. There can be little if any doubt, therefore, that, as Robertson and I have argued, the colourless so-called syn- and anti-diazo-salts cannot possibly be compounds of the $-\text{N}=\text{N}-$ or diazene type; such compounds would all be at least yellow in colour.

knowledge, I am reminded of the father of diazo-chemistry, Peter Griess, and of his marvellous experimental gifts; there is great need of such a man to re-investigate the whole subject.

If we inquire as to the general effect of the increase of knowledge of organic compounds, it is clear that the lessons which emerge from all modern inquiries are such as to justify Larmor's remark that our conceptions of structure must be granted more than analogical significance. Everything tends to show that function and structure are most closely connected—odour, taste, colour, physiological effect, are specific rather than general properties, each conditioned in its special variety by some special structure; we are approaching very closely to a time when it should be possible to discuss such properties with considerable confidence.

Still, it must not be forgotten that the problems they offer are all valency problems, and that the nature of valency eludes us entirely at present.

The greatest advance which chemists may pride themselves upon having made during the past decade or two remains to be considered. In 1885, I spoke as follows:—"The attention paid to the study of carbon compounds may be more than justified both by reference to the results obtained and to the nature of the work before us; the inorganic kingdom refuses any longer to yield up her secrets—new elements—except after severe compulsion; the organic kingdom, both animal and vegetable, stands ever ready before us. Little wonder, then, if problems directly bearing upon life prove the more attractive to the living. The physiologist complains that probably, 95 per cent. of the solid matters of living structures are pure unknowns to us, and that the fundamental chemical changes which occur during life are entirely enshrouded in mystery. It is in order that this may no longer be the case that the study of carbon compounds is being so vigorously prosecuted. Our weapons—the knowledge of synthetical processes and of chemical function—are now rapidly being sharpened, but we are yet far from ready for the attack."

My forecast has been more than justified; indeed, the advance to be recorded is nothing short of marvellous: the great problems of vital chemistry appear now no longer to be unattainable to our intelligence—their cryptic character seems to have disappeared almost suddenly. Many have contributed in greater or less degree, but none in such measure as Emil Fischer, whose work both in the sugar group and in connection with the albuminoids must for ever rank as monumental.

It is difficult to appreciate the extent to which the practical genius of this chemist has carried us—difficult alike for those who understand the subject and those who do not; the significance of his labours is only apparent when the bearing of his results on the interpretation of vital phenomena is fully considered. In 1885 we were disputing as to the structure of substances such as glucose and galactose; now we not only are satisfied that they belong to the group of aldhexoses (aldoses) derived from normal hexane, but, taking into account the monumental discoveries of Pasteur, to which precision has been given by van 't Hoff's great generalisation, we are in a position to assign fully resolved structural formulæ not only to the natural products but to the nine other isomeric aldhexoses which Fischer has prepared artificially.

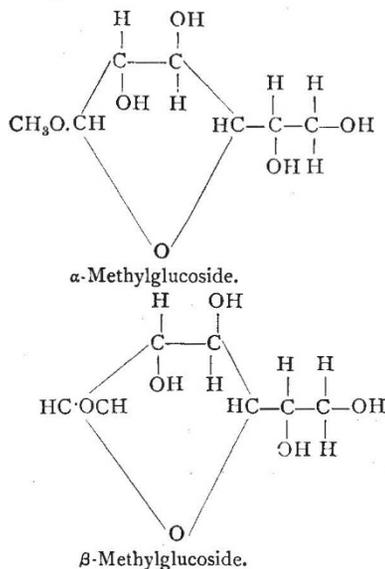
It is a striking fact that only three of the sixteen possible aldhexoses and but a single ketohexose (fructose), of which many are possible, are met with naturally. Nature is clearly most sparing, most economical, in her use of materials. And not only is this true of the hexoses, as very few of the possible lower and higher homologous carbohydrates occur in vegetable or animal materials and the condensed carbohydrates (cane sugar, starch, &c.) are all formed apparently from the hexoses and pentoses which occur naturally. The albuminoids, the alkaloids, the terpenes are also optically active substances; in other words, only a limited number of the possible forms are present. There is reason to suppose that the compounds of natural occurrence stand in close genetic connection and belong with few exceptions to the same series of enantiomorphs; in no other way is it possible to account for the occurrence of one only of the pair of enantiomorphous isomerides and for the relatively small number

of compounds. Moreover, not only the sugars and most of the other products of the disintegration of the albuminoids but also the amino-acids, in like manner, are derivatives of compounds containing at most six atoms of carbon; the fats alone are of a considerably higher degree of complexity but they are probably collocations of the simpler units.

The terpenes and essential oils are mostly C_{10} derivatives; the alkaloids have complex formulæ but the units of which they are composed are simple; as all of them are optically active, it is clear that only some of the possible enantiomorphous combinations are present.

We are bound, therefore, to assume that a large proportion of the changes which occur in living organisms—which constitute vital metabolism—are directed changes. What is the nature of the directive power? We are already able to go far in explaining this, although our knowledge is mainly of analytical changes, the nature of synthetic changes being, at present, only inferentially disclosed to us.

It has long been known that under natural conditions many complex compounds such as starch, cane sugar, and other similar substances are broken down hydrolytically, not by the unassisted action of water but by the co-operation of enzymes; the effect produced by these enzymes is precisely similar to that of acids, except that all acids produce the effect, acting only with different degrees of readiness, whilst enzymes are strictly selective, a given enzyme acting only, as a rule, either on a single substance or on a series of substances similar in structure. Indisputable evidence has been obtained that the enzymes which act on the carbohydrates are intimately related in structure to the compounds which they attack, fitting them—to use the apt simile introduced by E. Fischer—much as a key fits into a good lock: the slightest alteration in the structure of the carbohydrate is sufficient to throw the enzyme out of action. The closeness of the association is well illustrated by the case of the two methylglucosides, which differ merely in the manner displayed by the following formulæ:—



The relative positions of the single hydrogen atom and of the hydroxyl group attached to the carbon atom are merely interchanged, but this is sufficient to render the one (the α) proof against the action of emulsin, the enzyme of the almond; the other (the β) proof against that of maltase, the enzyme present in yeast.

The enzyme may be pictured as attaching itself to a surface of the molecule, and at the same time as associated with hydrone in such a manner that this is brought to bear at the junction which undergoes disruption. The action of acids, although similar, is simpler in that the attachment is not to the molecule as a whole but only at, or near to, the junction which is resolved.

NO. 2079, VOL. 81]

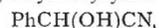
In the case of the albuminoids, the action is probably more local in character, in so far as the resolution of their polypeptide section is concerned, the same enzyme being able to effect the resolution of a considerable number of compounds.

All the peptolytes have in common the junction $C.CO.N$; the peptoclasts by which such substances are gradually resolved probably fit this group alone; but other enzymes are of more complex organisation, akin to that of the sacroclasts—such as arginase, for example. In principle, however, the enzymes are to be regarded as all acting alike, each as fitting some particular asymmetric centre if not the whole of the molecule which undergoes hydrolytic disruption under its influence, the asymmetric centre being that at which the cleavage is effected.

In synthetic changes the operation is reversed. It may be supposed that the separation of hydrone is determined by the circumstance that water can be formed by the interaction of this hydrone as it separates with that which is attached to the hydrolated enzyme; the formation of water, in fact, plays a great part in such changes.

The action of oxydases may be regarded from a similar point of view. The early observations of Adrian Brown on the oxidising activity of *Bacterium Xylinum*, coupled with the later work of Bertrand, afford clear proof that these enzymes are possessed of selective powers. It is conceivable that such enzymes become attached to a molecule at some one centre, and that they then deliver their attack at some more or less distant point by presenting the oxygen with which they are loosely associated at this point. It is easy, on such an assumption, to understand how ethenoid linkages may be developed in various positions in the molecule of a fatty acid.

Rosenthaler's recent observations on the formation of optically active phenyl hydroxycyano methane,



from hydrogen cyanide and benzaldehyde on shaking the solution with emulsin are, however, among the most significant yet made. I have myself confirmed his statements. The ease with which the change takes place—the manner in which the change is accelerated by the enzyme—is altogether remarkable.

Although there can be little doubt, in the case of plants and animals, that the synthetic processes do not occur spontaneously and directly between the interagents, but are for the most part at some stage or other directed or controlled, it cannot well be supposed that every asymmetric compound is the direct outcome of a controlled process; nor is it necessary to assume that such is the case. Not a few asymmetric compounds are probably but secondary products formed by the breakdown of compounds which are the products of directed synthesis.

Ehrlich's observations on the formation of the amylalcohols from the isomeric aminocaproic acids (leucines) may be referred to in this connection. Taking into account the manner in which the vegetable organism is provided with conservative powers and its tendency to retain nitrogen, in view of the peculiar structure of the members of the terpene group—especially the presence of the isopropyl group and of methyl in association with the ring in such hydrocarbons—it is highly probable that the terpenes are derived from amino-acids. A molecule of leucine, a molecule of alanine and a molecule of formaldehyde obviously provide the materials for the production of methylisopropylidihydrobenzene; it is not difficult to picture the series of changes which would lead to the formation of the hydrocarbon from such a conjunction.

The general impression produced by facts such as have been referred to is that directive influences are the paramount influences at work in building up living tissues. These come into operation, it is to be supposed, at a very early stage in the case of the plant. The initial step probably involves the electrolysis of water under the influence of solar energy and the reduction by the hydrogen thus liberated of the carbon dioxide, which is eventually converted into formaldehyde, either directly or, it may be, through the intermediation of oxalic and formic acids. The part which chlorophyll plays in this process can only be surmised: it is not improbable that reduced chlorophyll is the active reducing-agent: that chlorophyll itself is active

in conditioning the resolution of water under the influence of solar energy into reduced chlorophyll and oxygen or, more probably, a labile peroxide, from which oxygen is independently split off at a subsequent stage, it may be under the influence of a so-called catalase.

Whatever the process by which the plant acquires its initial store of carbonaceous material, the formaldehyde is apparently at once made use of and, in part at least, converted into starch. The view may be taken that glucose is the primary product of condensation—that the formaldehydrol molecules become ranged against a glucose template in series of sixes, which are soldered by enzymic influence into a single molecule by the interaction of contiguous hydrogen and hydroxyl radicles along the chain.

The glucose is thereafter carried a stage higher and converted into maltose or it may be that a maltose template is effective from the beginning and that the biose is the immediate product of condensation; the conversion of maltose into starch must take place in some similar manner. The recent observation that cellobiose is a β -glucoside enables us to realise that the formation of cellulose differs from that of starch in that the glucose molecule, instead of being converted into the β -glucoside maltose, becomes changed into the correlated β -glucoside, a membrane being thus secured which can resist the diastatic enzymes by which starch is attacked.

The formation of the albuminoid substances may be regarded from a similar point of view. At present, however, there is no satisfactory evidence to show at what stage nitrogen is introduced into the molecule. As the plant takes up nitrogen in the form of nitrate, not as ammonia, it is probable that the nitrate is reduced to hydroxylamine and that this, rather than ammonia, is the active synthetic agent. Formaldehyde and hydroxylamine would yield formaldoxime, which would easily pass into methylamine on reduction; the interaction of formaldoxime and formaldehydrol might give rise to a higher aldoxime which would be easily convertible into amino-acetic acid (glycine). Higher glycines might be formed from glycine by syntheses similar to those Erlenmeyer has effected; but to account for the formation of asymmetric amino-acids it is necessary to assume that the action is controlled at this stage and that the glycine is formed against a template perhaps under the influence of an enzyme.

Another conceivable mode of formation is by the fermentative degradation of glucosamine.

Until we know more of the order in which the amino-acid radicles are united in the various albuminoids and of the character of the associations other than those which are characteristic of polypeptides, we can consider the formation of albuminoids only from a very general point of view; but taking into account the very different proportions in which amino-acids and other cleavage products are formed on hydrolysing substances of different origin, it is clear that the several sections of the molecule must be differently ordered in the different proteins; again, therefore, it is necessary to assume that the formation of such substances is directed. We may picture molecule after molecule as being "brought into line" against a template and the junctions which are required to bind the whole series together as being made through the agency of the enzymic dehydrating influence before referred to.

Attention has been directed to the relatively simple way in which the hydrocarbons are constructed, that even the paraffins are not to be visualised as so many ducks strung upon a ramrod, Münchhausen fashion, but as forming curls, owing to the natural set of the affinities. This probably is true of complex substances such as the proteins.

Protoplasm, in fact, may be pictured as made up of large numbers of curls, like a judge's wig—all in intercommunication through some centre, connected here and there perhaps also by lateral bonds of union. If such a point of view be accepted, it is possible to account for the occurrence in some sections of the complex series of interchanges which involve work being done upon the substances brought into interaction, the necessary energy being drawn from some other part of the complex where the interchanges involve a development of energy.

The conclusions thus arrived at may be utilised in discussing the problem of heredity. The inheritance of

parental qualities, the need to assume continuity of the germ plasma and the comparative unimportance from the standpoint of heredity of somatic qualities, as well as the non-inheritance of mere environmental effects (acquired characters), are all necessary consequences of the view I have advanced.

The general similarity of structure throughout organised creation may well be conditioned primarily by properties inherent in the materials of which all living things are composed—of carbon, of oxygen, of nitrogen, of hydrogen, of phosphorus, of sulphur. At some early period, however, the possibilities became limited and directed processes became the order of the day. From that time onward the chemistry prevailing in organic nature became a far simpler chemistry than that of the laboratory; the possibilities were diminished, the certainties of a definite line of action were increased. How this came about it is impossible to say; mere accident may have led to it. Thus we may assume that some relatively simple asymmetric substance was produced by the fortuitous occurrence of a change under conditions such as obtain in our laboratories and that consequently the enantiomorphous isomeric forms of equal opposite activity were produced in equal amount. We may suppose that a pool containing such material having been dried up dust of molecular fineness was dispersed; such dust falling into other similar pools near the crystallisation point may well have conditioned the separation of only one of the two isomeric forms present in the liquid. A separation having been once effected in this manner, assuming the substance to be one which could influence its own formation, one form rather than the other might have been produced. An active substance thus generated and selected out might then become the origin of a series of asymmetric syntheses. How the complicated series of changes which constitute life may have arisen we cannot even guess at present; but when we contemplate the inherent simplicity of chemical change and bear in mind that life seems but to depend on the simultaneous occurrence of a series of changes of a somewhat diverse order, it does not appear to be beyond the bounds of possibility to arrive at a broad understanding of the method of life. Nor are we likely to be misled into thinking that we can so arrange the conditions as to control and reproduce it; the series of lucky accidents which seem to be required for arrangements of such complexity to be entered upon is so infinitely great.

It is impossible to rate chemistry at too high a value in Canada. The maintenance of the fertility of your fields, the proper utilisation of your vast mineral wealth, the purity of your food supplies¹ will depend mainly on the watchful care and skill of chemists; but the educational value of the subject may also be set very high. If properly taught in your schools, it will afford a means superior to all others, I believe, of training faculties which in these days should be developed in every responsible citizen. No other subject lends itself so effectively as a means of developing the experimental attitude of mind—the attitude of working with a clearly conceived purpose to a desired end, which is so necessary to success in these days; and if care be taken to inculcate habits of neatness and precision and of absolute truthfulness, if care be taken to teach what constitutes evidence, the moral value of such work is incalculable. But to be effective it must be done under proper conditions, systematically; the time devoted to the work must be adequate; I would even advocate that the subject be allowed to come before conventional geography and history and other unpractical subjects, assuming that the training is given in a practical way and with practical objects in view, not in the form of mere lessons learnt by rote; if taught in the form of mere didactic lessons it is as worthless as any other subject as mental discipline. Let me add that I would confine the teaching to a narrow range of problems but make it very thorough with reference to these.

¹ I should like to take this opportunity of saying that it is impossible to over-rate the public value of the great work which Dr. Wiley has undertaken in the United States in endeavouring to secure the supply of food free from deleterious ingredients. At home we certainly need some one to preach a similar crusade and to free us from doctored infants' foods and the innumerable host of medicines by which even our fair fields are disfigured.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY ARTHUR SMITH WOODWARD, LL.D.,
F.R.S., V.P.Z.S., Sec. G.S., KEEPER OF GEOLOGY IN
THE BRITISH MUSEUM, PRESIDENT OF THE SECTION.

THE circumstances of the present meeting very clearly determine the subject of a general address to be expected from a student of extinct animals. The remarkable discoveries of fossil backboneed animals made on the North American continent during the last fifty years suggest an estimate of the results achieved by the modern systematic methods of research; while the centenary celebration of the birth of Darwin makes it appropriate to consider the extent to which we may begin deducing the laws of organic evolution from the life of past ages as we now know it. Such an address must, of course, be primarily biological in character, and treat of some matters which are not ordinarily discussed by Section C. The subject, however, can only be appreciated fully by those who have some practical acquaintance with the limitations under which geologists pursue their researches, and especially by those who are accustomed to geological modes of thought.

There has been an unfortunate tendency during recent years for the majority of geologists to relinquish the study of fossils in absolute despair. More ample material for examination and more exact methods of research have altered many erroneous names which were originally used; while the admission to scientific publications of too many mere literary exercises on the so-called "law of priority" has now made it necessary to learn not one, but several names for some of the genera and species which are commonly met with. Even worse, the tentative arrangement of fossils in "genetic series" has led to the invention of a multitude of terms which often serve to give a semblance of scientific exactitude to the purest guesswork, and sometimes degenerate into a jargon which is naturally repellent to an educated mind. Nevertheless, I still hope to show that, with all these difficulties, there is so much of fundamental interest in the new work that it is worth while to make an effort to appreciate it. Geology and palæontology in the past have furnished some of the grandest possible contributions to our knowledge of the world of life; they have revealed hidden meanings which no study of the existing world could even suggest; and they have started lines of inquiry which the student of living animals and plants alone would scarcely have suspected to be profitable. The latest researches are the logical continuation of this pioneer work on a more extensive scale, and with greater precision; and I am convinced that they will continue to be as important a factor in the progress of post-Darwinian biology as were the older studies of fossils in the philosophy of Cuvier, Brongniart, and Owen.

In this connection it is necessary to combat the mistaken popular belief that the main object of studying fossils is to discover the "missing links" in the chain of life. We are told that the idea of organic evolution is not worthy of serious consideration until these links, precise in character, are forthcoming in all directions. Moreover, the critics who express this opinion are not satisfied to consider the simplest cases, such as are afforded by some of the lower grades of "shell-fish" which live together in immense numbers and have limited powers of locomotion. They demand long series of exact links between the most complex skeletal frames of the backboneed animals, which have extreme powers of locomotion, are continually wandering, and are rarely preserved as complete individuals when they are buried in rock. They even expect continual discoveries of links among the rarest of all fossils, those of the higher apes and man. The geologist, on the other hand, knowing well that he must remain satisfied with a knowledge of a few scattered episodes in the history of life which are always revealed by the merest accident, marvels that the discovery of "missing links" is so constant a feature of his work. He is convinced that, if circumstances were more favourable, he would be able to satisfy the demand of the most exacting critic. He has found enough continuous series among the mollusca, for example, and so many suggestions of equally gradual series among the

higher animals, that he does not hesitate to believe without further evidence in a process of descent with modification. The mere reader of books is often misled by the vagaries of nomenclature to suppose that the intervals between the links are greater than in reality; but for the actual student it is an everyday experience to find that fossils of slightly different ages which he once thought distinct are linked together by a series of forms in which it is difficult to discover the feeblest lines of demarcation. He is therefore justified in proceeding on the assumption that in all cases the life of one geological period has passed by a natural process of descent into that of the next succeeding period; and, avoiding genealogical guesswork which proves to be more and more futile, he strives to obtain a broad view of the series of changes which have occurred, to distinguish between those which denote progress and those which lead to stagnation or extinction. When the general features of organic evolution are determined in this manner, it will be much easier than it is at present to decide where missing links in any particular case are most likely to be found.

Among these general features which have been made clear by the latest systematic researches, I wish especially to emphasise the interest and significance of the persistent progress of life to a higher plane, which we observe during the successive geological periods. For I think palæontologists are now generally agreed that there is some principle underlying this progress much more fundamental than chance-variation or response to environment however much these phenomena may have contributed to certain minor adaptations. Consider the case of the backboneed animals, for instance, which I happen to have had special opportunities of studying.

We are not likely ever to discover the actual ancestors of animals on the backboneed plan, because they do not seem to have acquired any hard skeleton until the latter part of the Silurian period, when fossils prove them to have been typical and fully developed, though low in the backboneed scale. The ingenious researches and reasoning of Dr. W. H. Gaskell, however, have suggested the possibility that these animals originated from some early relatives of the scorpions and crustaceans. It is therefore of great interest to observe that the Eurypterids and their allies, which occupy this zoological position, were most abundant during the Silurian period, were represented by species of the largest size immediately afterwards at the beginning of the Devonian, and then gradually dwindled into insignificance. In other words, there was a great outburst of Eurypterid life just at the time when backboneed animals arose; and if some of the former were actually transformed into the latter, the phenomenon took place when their powers both of variation and of multiplication were at their maximum.

Fishes were already well established and distributed over perhaps the greater part of the northern hemisphere at the beginning of Devonian times; and then there began suddenly a remarkable impulse towards the production of lung-breathers, which is noticeable not only in Europe and North America, but also probably so far away as Australia. In the middle and latter part of the Devonian period, most of the true fishes had paddles, making them crawlers as much as swimmers; many of them differed from typical fishes, while agreeing with lung-breathers, in having the basis of the upper jaw fused with the skull, not suspended; and some of them exhibited both these features. Their few survivors at the present day (the *Crossopterygians* and *Dipnoans*) have also an air-bladder, which might readily become a lung. The characteristic *Amia* of the Devonian period, therefore, made a nearer approach to the land animals than any group of fishes of later date; and it is noteworthy that in the Lower Carboniferous of Scotland—perhaps even in the Upper Devonian of North America, if footprints can be trusted—amphibians first appeared. In Upper Carboniferous times they became firmly established, and between that period and the Trias they seem to have spread all over the world; their remains having been found, indeed, in Europe, Spitsbergen, India, South Africa, North and South America, and Australia.

The *Stegocephala* or *Labyrinthodonts*, as these primitive amphibians are termed, were therefore a vigorous race; but the marsh-dwelling habits of the majority did not allow of much variation from the salamander-pattern. Only in Upper Carboniferous and Lower Permian times did some

of their smaller representatives (the Microsauria) become lizard-like, or even snake-like in form and habit; and then there suddenly arose the true reptiles. Still, these reptiles did not immediately replace the Stegocephala in the economy of Nature; they remained quite secondary in importance at least until the Upper Permian, in most parts even until the dawn of the Triassic period. Then they began their flourishing career.

At this time the reptiles rapidly diverged in two directions. Some of them were almost exactly like the little *Sphenodon*, which still survives in some islands off New Zealand, only retaining more traces of their marsh-dwelling ancestors. The majority (the Anomodonts or Theromorphs) very quickly became so closely similar to the mammals that they can only be interpreted as indicating an intense struggle towards the attainment of the higher warm-blooded grade; and there is not much doubt that true mammals actually arose about the end of the Triassic period. Here, again, however, the new race did not immediately replace the old, or exterminate it by unequal competition. Reptiles held their own on all lands throughout the Jurassic and Cretaceous periods, and it was not until the Tertiary that mammals began to predominate.

As to the beginning of the birds, it can only be said that towards the end of the Triassic period there arose a race of small Dinosaurs of the lightest possible build, exhibiting many features suggestive of the avian skeleton; so it is probable that this higher group also originated from an intensely restless early community of reptiles, in which all the variations were more or less in the right direction for advancement.

In short, it is evident that the progress of the backboneed land animals during the successive periods of geological time has not been uniform and gradual, but has proceeded in a rhythmic manner. There have been alternations of restless episodes which meant real advance, with periods of comparative stability, during which the predominant animals merely varied in response to their surroundings, or degenerated, or gradually grew to a large size. There was no transition, for instance, between the reptiles of the Cretaceous period and the mammals which immediately took their place in the succeeding Eocene period: those mammals, as we have seen, had actually originated long ages before, and had remained practically dormant in some region which we have not yet discovered, waiting to burst forth in due time. During this retirement of the higher race the reptiles themselves had enjoyed an extraordinary development and adaptation to every possible mode of life in nearly all parts of the globe. We do not understand the phenomenon—we cannot explain it; but it is as noticeable in the geological history of fishes as in that of the land animals just considered. It seems to have been first clearly observed by the distinguished American naturalist, the late Prof. Edward D. Cope, who termed the sudden fundamental advances "expression points" and saw in them a manifestation of some inscrutable inherent "bathmic force."

Perhaps the most striking feature to be noticed in each of these "expression points" is the definite establishment of some important structural character which had been imperfect or variable before, thus affording new and multiplied possibilities of adaptation to different modes of life. In the first lung-breathers (*Stegocephala*), for example, the indefinite paddle of the mud fishes became the definite five-toed limb; while the incomplete backbone reached completeness. Still, these animals must have been confined almost entirely to marshes, and they seem to have been all carnivorous. In the next grade, that of the reptiles, it became possible to leave the marshes; and some of them were soon adapted not only for life on hard ground or in forests, but even for flight in the air. Several also assumed a shape of body and limbs enabling them to live in the open sea. Nearly all were carnivorous at first, and most of them remained so to the end; but many of the Dinosaurs eventually became practically hoofed animals, with a sharp beak for cropping herbage, and with powerful grinding teeth. In none of these animals, however, were the toes reduced to less than three in number, and in none of them were the basal toe-bones fused together as they are in cattle and deer. It is also noteworthy that the brain in all of them remained very small and simple. In the final grade of backboneed life, that of the mammals, each of the

adaptive modifications just mentioned began to arise again in a more nearly perfected manner, and now survival depended not so much on an effective body as on a developing brain. The mammals began as little carnivorous or mixed-feeding animals with a small brain and five toes, and during the Tertiary period they gradually differentiated into the several familiar groups as we now know them, eventually culminating in man.

The demonstration by fossils that many animals of the same general shape and habit have originated two or three times, at two or three successive periods, from two or three continually higher grades of life, is very interesting. To have proved, for example, that flying reptiles did not pass into birds or bats, that hoofed Dinosaurs did not change into hoofed mammals, and that Ichthyosaurs did not become porpoises; and to have shown that all these later animals were mere mimics of their predecessors, originating independently from a higher yet generalised stock, is a remarkable achievement. Still more significant, however, is the discovery that towards the end of their career through geological time totally different races of animals repeatedly exhibit certain peculiar features, which can only be described as infallible marks of old age.

The growth to a relatively large size is one of these marks, as we observe in the giant *Pterodactyls* of the Cretaceous period, the colossal Dinosaurs of the Upper Jurassic and Cretaceous, and the large mammals of the Pleistocene and the present day. It is not, of course, all the members of a race that increase in size; some remain small until the end, and they generally survive long after the others are extinct; but it is nevertheless a common rule that the prosperous and typical representatives are successively larger and larger, as we see them in the familiar cases of the horses and elephants of the northern hemisphere, and the hoofed animals and armadillos of South America.

Another frequent mark of old age in races was first discussed and clearly pointed out by the late Prof. C. E. Beecher, of Yale. It is the tendency in all animals with skeletons to produce a superfluity of dead matter, which accumulates in the form of spines or bosses as soon as the race they represent has reached its prime and begins to be on the down-grade. Among familiar instances may be mentioned the curiously spiny *Graptolites* at the end of the Silurian period, the horned *Pariasaurians* at the beginning of the Trias, the armour-plated and horned Dinosaurs at the end of the Cretaceous, and the cattle or deer of modern Tertiary times. The latter case—that of the deer—is specially interesting, because fossils reveal practically all the stages in the gradual development of the horns or antlers, from the hornless condition of the Oligocene species, through the simply forked small antlers of the Miocene species, to the largest and most complex of all antlers seen in *Cervus sedgwicki* from the Upper Pliocene and the Irish deer (*C. giganteus*) of still later times. The growth of these excrescences, both in relative size and complication, was continual and persistent until the climax was reached and the extreme forms died out. At the same time, although the palaeontologist must regard this as a natural and normal phenomenon not directly correlated with the habits of the race of animals in which it occurs, and although he does not agree with the oft-repeated statement that deer may have "perfected" their antlers through the survival of those individuals which could fight most effectively, there may nevertheless be some truth in the idea that the growths originally began where the head was subject to irritating impacts, and that they so happened to become of utility. Fossils merely prove that such skeletal outgrowths appear over and over again in the prime and approaching old age of races; they can suggest no reasons for the particular positions and shapes these outgrowths assume in each species of animal.

It appears, indeed, that when some part of an animal (whether an excrescence or a normal structure) began to grow relatively large in successive generations during geological time, it often acquired some mysterious impetus by which it continued to increase long after it had reached the serviceable limit. The unwieldy antlers of the extinct *Sedgwick's deer* and Irish deer just mentioned, for example, must have been impediments rather than useful weapons. The excessive enlargement of the upper canine teeth in the so-called sabre-toothed tigers (*Machærodus* and its allies)

must also eventually have hindered rather than aided the capture and eating of prey. The curious gradual elongation of the face in the Oligocene and Miocene Mastodons, which has lately been described by Dr. Andrews, can only be regarded as another illustration of the same phenomenon. In successive generations of these animals the limbs seem to have grown continually longer, while the neck remained short, so that the head necessarily became more and more elongated to crop the vegetation on the ground. A limit of mechanical inefficiency was eventually reached, and then there survived only those members of the group in which the attenuated mandible became shortened up, leaving the modified face to act as a "proboscis." The elephants thus arose as a kind of after-thought from a group of quadrupeds that were rapidly approaching their doom.

The end of real progress in a developing race of back-boned animals is also often marked by the loss of the teeth. A regular and complete set of teeth is always present at the commencement, but it frequently begins to lack successors in animals which have reached the limit of their evolution, and then it soon disappears. Tortoises, for instance, have been toothless since the Triassic period, when they had assumed all their essential features; and birds have been toothless since the end of Cretaceous times. The monotreme mammals of Australasia, which are really a survival from the Jurassic period, are also toothless. Some of the latest Ichthyosaurs and Pterodactyls were almost or quite toothless; and I have seen a jaw of an Upper Cretaceous carnivorous Dinosaur (*Genyodectes*) from Patagonia so completely destitute of successional teeth that it seems likely some of these land reptiles nearly arrived at the same condition.

Among fishes there is often observable still another sign of racial old age—namely, their degeneration into eel-shaped forms. The Dipnoan fishes afford a striking illustration, beginning with the normally shaped *Dipterus* in the Middle Devonian, and ending in the long-bodied *Lepidosiren* and *Protopterus* of the present day. The Palæozoic Acanthodian sharks, as they are traced upwards from their beginning in the Lower Devonian to their end in the Permian, also acquire a remarkable elongation of the body and a fringe-like extension of the fins. Among higher fishes, too, there are numerous instances of the same phenomenon, but in most of these the ancestors still remain undiscovered, and it would thus be tedious to discuss them.

Finally, in connection with these obvious symptoms of old age in races, it is interesting to refer to a few strange cases of the rapid disappearance of whole orders of animals, which had a practically world-wide distribution at the time when the end came. Local extinction, or the disappearance of a group of restricted geographical range, may be explained by accidents of many kinds; but contemporaneous universal extinction of widely spread groups, which are apparently not affected by any new competitors, is not so easily understood. The Dinosaurs, for instance, are known to have lived in nearly all lands until the close of the Cretaceous period; and, except perhaps in Patagonia, they were always accompanied until the end by a typically Mesozoic fauna. Their remains are abundant in the Wealden formation of Western Europe, the deposit of a river which must have drained a great continent at the beginning of the Cretaceous period; they have also been found in a corresponding formation which covers a large area in the State of Bahia, in Brazil. They occur in great numbers in the freshwater Upper Cretaceous Laramie deposits of Western North America, and also in a similar formation of equally late date in Transylvania, South-east Europe. In only two of these regions (South-east England and West North America) have any traces of mammals been found, and they are extremely rare fragments of animals as small as rats; so there is no reason to suppose that the Dinosaurs suffered in the least from any struggle with warm-blooded competitors. Even in Patagonia, where the associated mammal-remains belong to slightly larger and more modern animals, these fossils are also rare, and there is nothing to suggest competition. The race of Dinosaurs seems, therefore, to have died a natural death. The same may be said of the marine reptiles of the orders Ichthyosauria, Plesiosauria, and Mosasauria. They had a practically world-wide distribution in the seas of the Cretaceous period, and the Mosasauria especially must have been extremely abundant and flourishing. Nevertheless, at

the end of Cretaceous times they disappeared everywhere, and there was absolutely nothing to take their place until the latter part of the Eocene period, when whales and porpoises began to play exactly the same part. So far as we know, the higher race never even came in contact with the lower race; the marine mammals found the seas vacant, except for a few turtles and for one curious Rhynchocephalian reptile (*Champsosaurus*), which did not long survive. Another illustration of the same phenomenon is probably afforded by the primitive Carnivora (the so-called Sparassodonta), which were numerous in South America in the Lower Tertiary periods. They were animals with a brain as small as that of the thylacines and dasyures which now live in Tasmania. They appear to have died out completely before they were replaced by the cats, sabre-toothed tigers, and dogs, which came down south from North America over the newly emerged isthmus of Panama at the close of the Pliocene period. At least, the remains of these old carnivores and their immigrant successors have never yet been found associated in any geological formation.

These various considerations lead me to think that there is also deep significance in the tendency towards fixity in the number and regularity (or symmetry) in the arrangement of their multiple parts, which we frequently observe in groups of animals as we trace them from their origin to their prime. It is well known that in certain of the highest and latest types of bony fishes the vertebræ and fin-rays are reduced to a fixed and practically invariable number for each family or genus, whereas there is no such fixity in the lower and earlier groups. In the earliest known Pycnodont fishes from the Lower Lias (*Mesodon*) the grinding teeth form an irregular cluster, while in most of the higher and later genera they are arranged in definite regular rows in a symmetrical manner. Many of the lower backboned animals have teeth with several cusps, and in some genera the number of teeth seems to be constant; but in the geological history of the successive classes the tooth-cusps never became fixed individual entities, readily traceable throughout whole groups, until the highest or mammalian grade had been attained. Moreover, it is only in the same latest grade or class that the teeth themselves can be treated as definite units, always the same in number (forty-four), except when modified by degeneration or special adaptation. In the earlier and lower land animals the number of vertebræ in the neck depends on the extent of this part, whereas in the mammal it is almost invariably seven, whatever the total length may be. Curiously constant, too, in the modern even-toed hoofed mammals is the number of nineteen vertebræ between the neck and the sacrum.

I am therefore still inclined to believe that the comparison of vital processes with certain purely physical phenomena is not altogether fanciful. Changes towards advancement and fixity which are so determinate in direction, and changes towards extinction which are so continually repeated, seem to denote some inherent property in living things, which is as definite as that of crystallisation in inorganic substances. The regular course of these changes is merely hindered and modified by a succession of checks from the environment and Natural Selection. Each separate chain of life, indeed, bears a striking resemblance to a crystal of some inorganic substance which has been disturbed by impurities during its growth, and has thus been fashioned with unequal faces, or even turned partly into a mere concretion. In the case of a crystal the inherent forces act solely on molecules of the crystalline substance itself, collecting them and striving, even in a disturbing environment, to arrange them in a fixed geometrical shape. In the case of a chain of life (or organic phylum) we may regard each successive animal as a temporary excrescence of colloid substance round the equally colloid germ-plasm which persists continuously from generation to generation. The inherent forces of this germ-plasm, therefore, act upon a consecutive series of excrescences (or animal bodies), struggling not for geometrically arranged boundaries, but towards various other symmetries, and a fixity in number of multiple parts. When the extreme has been reached, activities cease, and sooner or later the race is dead.

Such are some of the most important general results to which the study of fossils has led during recent years; and they are conclusions which every new discovery appears to make more certain. When we turn to details, however, it

must be admitted that modern systematic researches are continually complicating rather than simplifying the problems we have to solve. Prof. Charles Depéret has lately written with scant respect of some of the pioneers who were content with generalities, and based their conclusions on the geological succession of certain anatomical structures rather than on a successive series of individuals and species obtained from the different layers of one geological section; but even now I do not think we can do much better than our predecessors in unravelling real genealogies. At least Prof. Depéret's genealogical table of the Lower Tertiary pig-like Anthracotheriidae, which he publishes as an illustration of "évolution réelle," seems to me to be no more exact than several tables of other groups by previous authors which he criticises. His materials are all fragmentary, chiefly jaws and portions of skulls; they were obtained from several isolated lake-deposits, of which the relative age cannot be determined by observing the geological superposition; and they represent a group which is known to have lived over a large part of Europe, Asia, Northern Africa, and North America. There is therefore no certainty that the genera and species enumerated by Prof. Depéret actually originated one from the other in the region where he happened to find them; he has demonstrated the general trend of certain changes in the Anthracotheriidae during geological time, but really nothing more.

Even when a group of animals seems to have been confined to one comparatively small region, where the series is not complicated by migration to and from other parts of the world, modern research still emphasises the difficulty of tracing real lines of descent. The primitive horned hoofed animals of the family Titanotheriidae, for example, are only known from part of North America, and they seem to have originated and remained there until the end. As their fossil skeletons are abundant and well preserved, it ought to be easy to discover the exact connections of the several genera and species. Prof. Osborn has now proved, however, that the Titanotheres must have evolved in at least four distinct lines, adapted "for different local habitat, different modes of feeding, fighting, locomotion, &c., which took origin, in part at least, in the Middle or Upper Eocene." They exhibit "four distinct types in the shape and position of the horns, correlated with the structure of the nasals and frontals, and indicative of different modes of combat among the males." The ramifications of the group are indeed so numerous that the possibility of following chains of ancestors begins to appear nearly hopeless.

Among early reptiles the same difficulties are continually multiplied by the progress of discovery. About twenty years ago it began to appear likely that we should soon find the terrestrial ancestors of the Ichthyosauria in the Trias; and somewhat later a specimen from California raised hopes of obtaining them by systematic explorations in that region. During more recent years Prof. J. C. Merriam and his colleagues have actually made these explorations, and the result is that we now know from the Californian Trias a multitude of reptiles, which need more explanation than the Ichthyosauria themselves. Prof. Merriam has found some of the links predicted between Ichthyosaurs and primitive land reptiles, but he has by no means reached the beginning of the marine group; and while making these discoveries he has added greatly to the complication of the problem which he set out to solve.

Serious difficulties have also become apparent during recent years in determining exactly the origin of the mammals. For a long time after the discovery of the Anomodont or Theromorph reptiles in the Permian-Trias of South Africa, it seemed more and more probable that the mammals arose in that region. Even yet new reptiles from the Karoo formation are continually being described as making an astonishingly near approach to mammals; and, so far as the skeleton is concerned, the links between the two grades are now very numerous among South African fossils. Since these reptiles first attracted attention, however, they have gradually been found in the Permian and Trias of a large part of the world. Remains of them were first met with in India, then in North America, and next in Scotland, while during the last few years Prof. W. Amalitzky has disinterred so many nearly complete skeletons in the north of Russia that we are likely soon to learn more about them from this European country than from

the South African area itself. Quite lately I have received numerous bones from a red marl in Rio Grande do Sul, Southern Brazil, which show that not merely Anomodonts, but also other characteristic Triassic land reptiles were likewise abundant in that region. We are therefore now embarrassed by the richness of the sources whence we may obtain the ancestors of mammals. Whereas some years ago it appeared sufficient to search South Africa for the solution of the problem, we are now uncertain in which direction to turn. We are still perhaps inclined to favour the South African source; but this is only because we know nothing of the Jurassic land animals of that part of the world, and we cherish a lingering hope that they may eventually prove to have included the early mammals for which we have so long sought in vain.

The mystery of the origin of the marine mammals of the order Sirenia and Cetacea appears to have been diminished by the discoveries of the Geological Survey of Egypt, Dr. Andrews and Dr. Fraas in the Eocene and Oligocene deposits of the Mokattam Hills and the Fayum. It is now clear that the Sirenians are closely related to the small primitive ancestors of the elephants; while, so far as the skull and dentition are concerned, we know nearly all the links between the early toothed whales (or Zeuglodonts) and the primitive ancestors of the Carnivora (or Creodonts). The most primitive form of Sirenian skull hitherto discovered, however, is not from Egypt, but from the other side of the world, Jamaica; and exactly the same Zeuglodonts, even with an associated sea-snake, occur so far away from Egypt as Alabama, U.S.A. The problem of the precise origin of these marine mammals is therefore not so simple as it would have appeared to be had we known only the Egyptian fossils. The progress of discovery, while revealing many most important generalities, has made it impossible to vouch for the accuracy of the details in any "genealogical tree."

Another difficulty resulting from the latest systematic researches is suggested by the extinct hoofed mammals of South America. The llamas, deer, and peccaries existing in South America at the present time are all immigrants from the northern continent; but during the greater part of the Tertiary period there lived in that country a large number of indigenous hoofed mammals, which originated quite independently of those in other regions. They seem to have begun in early Eocene times much in the same manner as those of the northern hemisphere; but as they became gradually adapted for life on hard ground, they formed groups which are very different from those with which we are familiar in our part of the world. Some of them (Protheriidae) were one-toed mimics of the horses, but without the advanced type of brain, the deepened grinding teeth, the mobile neck, or the really effective wrist and ankle. Others (Toxodontidae) made some approach towards rhinoceroses in shape and habit, even with a trace of a horn on the nose. Until their independent origin was demonstrated, these curious animals could not be understood; and it is probable that there are innumerable similar cases of parallel development of groups, by which in our ignorance we are often misled.

It would be easy to multiply instances, but I think I have now said enough to show that every advance in the study of fossils reveals more problems than it solves. During the last two decades the progress in our knowledge of the extinct backboneed animals has been truly astonishing, thanks especially to the great explorations in North America, Patagonia, Egypt, Madagascar, and South Africa. Whole groups have been traced a long way towards their origin; but with them have been found a number of previously unknown groups which complicate all questions of evolution to an almost bewildering extent. Animals formerly known only by fragments are now represented by nearly complete skeletons, and several which appeared to have a restricted geographical range have now been found over a much wider area; but while this progress has been made, numerous questions have arisen as to the changing connections of certain lands and seas which previously seemed to have been almost settled. The outlook both of zoology and of geology has, therefore, been immensely widened, but the only real contribution to philosophy has been one of generalities. Some of the broad principles to which I have referred are now so clearly established that we can often predict what will be the main

result of any given exploration, should it be successful in recovering skeletons. We are no longer bold enough to restore an entirely unknown extinct animal from a single bone or tooth, like the trustful Cuvierian school; but there are many kinds of bones and teeth of which we can determine the approximate geological age and probable associates, even if we have no exact knowledge of the animals to which they belong. A subject which began by providing material for wonder-books has thus been reduced to a science sufficiently precise to be of fundamental importance both to zoology and to geology; and its exactitude must necessarily increase with greater and greater rapidity as our systematic researches are more clearly guided by the experience we have already gained.

NOTES.

THE special report agreed upon by the Select Committee on the Daylight Saving Bill contains the following conclusion:—"Having regard to the great diversity of opinion existing upon the proposals of the Bill and to the grave doubts which have been expressed as to whether the objects of the measure can be attained by legislation without giving rise, in cases involving important interests, to serious inconvenience, your committee recommend that the Bill be not further proceeded with." We are glad the committee has arrived at this conclusion, which embodies the views expressed in these columns on several occasions. Most people are in favour of the principle of making the best and fullest use of daylight hours, but the compulsory alteration of the system of time-reckoning for several months of the year is quite a different matter. As we have pointed out, in engineering, building, agricultural and other industries in which it is difficult to carry on work by artificial light, the hours of labour are already adapted to the daylight hours in different seasons. Here we have the voluntary adoption of the principle of daylight saving, and we are in complete sympathy with any movement to encourage the extension of the custom to other industrial or commercial circles where earlier hours of commencing work during certain months are practicable or desirable. This can be accomplished, however, without legislation, and the committee has acted wisely in recommending that the Bill, which would make a seasonal change of time compulsory, be not carried further.

A BILL to promote the economic development of the United Kingdom and the improvement of the roads therein was introduced in the House of Commons on August 26 by the Chancellor of the Exchequer. In the explanatory memorandum of this Bill it is stated that Part i. enables the Treasury to make free grants and loans for the purpose of aiding and developing forestry, agriculture, and rural industries, the reclamation and drainage of land, the improvement of rural transport (other than roads), the construction and improvement of harbours and canals, and the development and improvement of fisheries, and for any other purpose calculated to promote the economic development of the United Kingdom. A grant or loan must be made either to or through a Government department, and all applications for grants or loans have to be referred to an advisory committee, and the recommendations of the committee considered before the grant or loan is made; but the responsibility of making the grant or loan will rest with the Treasury, who will not be bound by the recommendations of the committee. All grants and loans will be made out of a separate fund, which will be fed by (1) sums annually voted by Parliament; (2) a sum of 2,500,000*l.* charged on the Consolidated Fund and payable in five annual instalments of 500,000*l.* each in 1911, 1912, 1913, 1914, and 1915; (3) sums received by way of interest on

and repayment of loans and the profits made as the result of a grant or loan in cases where the repayment of such profits is made a condition of the grant or loan. Power is given to the Board of Agriculture and Fisheries and the Department of Agriculture and Technical Instruction for Ireland to acquire land (compulsorily if necessary) for any purpose for which a grant is made to them. As the *Times* points out, this part of the Bill will permit the expenditure of money on scientific research and experimental work of a kind likely to be beneficial to agriculture. Part ii. of the Bill constitutes a Road Board for the purpose of improving the facilities for motor traffic. In addition to the power of acquiring land for the purposes of new roads proposed to be constructed by the Road Board, the Board is given power to acquire land in rural districts on either side of any such proposed road to the extent of 220 yards in depth.

DR. ALEXANDER RUSSELL has been appointed principal of Faraday House in succession to the late Mr. H. E. Harrison.

DR. T. H. BRYCE, lecturer on anatomy at Queen Margaret College, Glasgow, has been appointed Regius professor of anatomy in the University of Glasgow in succession to Prof. J. Cleland.

WE regret to see the announcement of the death, at sixty-seven years of age, of Prof. E. C. Hansen, head of the physiological department of the Carlsberg Laboratory, Copenhagen, for studies of chemistry and plant physiology, with particular reference to fermentation.

THE death is announced, at sixty-four years of age, of Dr. Radcliffe Crocker, distinguished particularly by his work on diseases of the skin. Dr. Crocker was the first president of the dermatological section of the Royal Society of Medicine, and made many valuable contributions to the literature of dermatology, among them being his "Treatise on Diseases of the Skin" and "Atlas of Diseases of the Skin."

THE twentieth annual general meeting of the Institution of Mining Engineers will be held at Newcastle-upon-Tyne on September 15. The following are among the papers to be read or discussed:—fire-damp caps and the detection of fire-damp in mines by means of safety-lamps, E. B. Whalley and W. M. Tweedie; equipment for the study of flame-caps and for miscellaneous experiments on safety-lamps, Prof. G. R. Thompson; electricity in coal mines, R. Nelson.

THE Budapest correspondent of the *Times* reports that the sixteenth International Congress of Medicine was officially opened there on August 29 by the Archduke Joseph in the name of the King of Hungary and Emperor of Austria. More than 4300 members have enrolled their names in the list, and they include a large number of eminent authorities on medicine from all parts of the world. We hope to give, in a future issue, an account of subjects of wide scientific interest and importance brought before the congress.

THE next International Congress of Mining, Metallurgy, Applied Mechanics, and Practical Geology will be held at Düsseldorf during the last week of June, 1910, under the auspices of the Rhenish-Westphalian Mining Industry. An influential committee of organisation has been formed which is charged with the making of the arrangements for the reading and discussion of papers, visits to places of technical interest, and social entertainments. Further information can be obtained in due course on application to the secretary of the Iron and Steel Institute or to the committee of organisation, Jacobistrasse, 3-5, Düsseldorf.