training in one or other of its many forms its proper place in the school curriculum.

The author does well to plead for considerable liberty in the teaching of these subjects. Freedom on the part of the teacher to use his own initiative and judgment in determining the exercises to be given to his pupils is essential, for unless he himself is interested in the work in which his pupils are engaged, his instruction will prove of little value. The author is on equally safe ground when he says, "It is not only teachers that call out for liberty; local education authorities are beginning actively to resent the evil of a central bureaucracy drawn tighter and tighter."

Although the number of text-books and essays on manual training already published is very large, we believe Mr. Legge's book will be found to be a valuable addition to the works which teachers and administrators may usefully consult.

THE AUSTRALIAN MEETING OF THE BRITISH ASSOCIATION.

INAUGURAL ADDRESS BY PROF. WILLIAM BATESON, M.A., F.R.S., PRESIDENT.

PART I.—MELBOURNE.

THE outstanding feature of this meeting must be the fact that we are here—in Australia. It is the function of a president to tell the association of advances in science, to speak of the universal rather than of the particular or the temporary. There will be other opportunities of expressing the thoughts which this event must excite in the dullest heart, but it is right that my first words should take account of those achievements of organisation and those acts of national generosity by which it has come to pass that we are assembled in this country. Let us, too, on this occasion, remember that all the effort, and all the goodwill, that binds Australia to Britain would have been powerless to bring about such a result had it not been for those advances in science which have given man a control of the forces of nature. For we are here by virtue of the feats of genius of individual men of science, giant-variations from the common level of our species; and since I am going soon to speak of the significance of individual variation, I cannot introduce that subject better than by calling to remembrance the line of pioneers in chemistry, in physics, and in engineering, by the working of whose rare —or, if you will, abnormal—intellects a meeting of the British Association on this side of the globe has been made physically possible.

I have next to refer to the loss within the year of Sir David Gill, a former president of this association, himself one of the outstanding great. His greatness lay in the power of making big foundations. He built up the Cape Observatory; he organised international geodesy; he conceived and carried through the plans for the photography of the whole sky, a work in which Australia is bearing a conspicuous part. Astronomical observation is now organised on an international scale, and of this great scheme Gill was the heart and soul. His labours have ensured a base from which others will proceed to discovery otherwise impossible. His name will be long remembered with veneration and gratitude.

As the subject of the addresses which I am to deliver here and in Sydney I take *Heredity*. I shall attempt to give the essence of the discoveries made by Mendelian or analytical methods of study, and I shall ask you to contemplate the deductions which these physiological facts suggest in application both to evolutionary theory at large and to the special case of the natural history of human society.

Recognition of the significance of heredity is modern. The term itself in its scientific sense is no older than Herbert Spencer. Animals and plants are formed as pieces of living material split from the body of the parent organisms. Their powers and faculties are fixed in their physiological origin. They are the consequence of a genetic process, and yet it is only lately that this genetic process has become the subject of systematic research and experiment. The curiosity of naturalists has of course always been attracted to such problems; but that accurate knowledge of genetics is of paramount importance in any attempt to understand the nature of living things has only been realised quite lately even by naturalists, and with casual exceptions the laity still know nothing of the matter. Historians debate the past of the human species, and statesmen order its present or profess to guide its future as if the animal man, the unit of their calculations, with his vast diversity of powers, were a homogeneous material, which can be multiplied like shot.

The reason for this neglect lies in ignorance and misunderstanding of the nature of variation; for not until the fact of congenital diversity is grasped, with all that it imports, does knowledge of the system of hereditary transmission stand out as a primary necessity in the construction of any theory of evolution, or any scheme of human polity.

The first full perception of the significance of variation we owe to Darwin. The present generation of evolutionists realises perhaps more fully than did the scientific world in the last century that the theory of evolution had occupied the thoughts of many and found acceptance with not a few before ever the "Origin" appeared. We have come also to the conviction that the principle of natural selection cannot have been the chief factor in delimiting the species of animals and plants, such as we now with fuller knowledge see them actually to be. We are even more sceptical as to the validity of that appeal to changes in the conditions of life as direct causes of modification, upon which latterly at all events Darwin laid much emphasis. But that he was the first to provide a body of fact demonstrating the variability of living things, whatever be its causation, can never be questioned.

There are some older collections of evidence, chiefly the work of the French school, especially of Godron[1] —and I would mention also the almost forgotten essay of Wollaston[2]—these, however, are only fragments in comparison. Darwin regarded variability as a property inherent in living things, and eventually we must consider whether this conception is well founded; but postponing that inquiry for the present, we may declare that with him began a general recognition of variation as a phenomenon widely occurring in nature.

If a population consists of members which are not alike but differentiated, how will their characteristics be distributed among their offspring? This is the problem which the modern student of heredity sets out to investigate. Formerly it was hoped that by the simple inspection of embryological processes the modes of heredity might be ascertained, the actual mechanism by which the offspring is formed from the body of the parent. In that endeavour a noble pile of evidence has been accumulated. All that can be made visible by existing methods has been seen, but we come little

1 "De l'Espèce et des Races dans les Êtres Organisés," 1859.
2 "On the Variation of Species," 1856.

if at all nearer to the central mystery. We see nothing that we can analyse further—nothing that can be translated into terms less inscrutable than the physiological events themselves. Not only does embryology give no direct aid, but the failure of cytology is, so far as I can judge, equally complete. The chromosomes of nearly related creatures may be utterly different both in number, size, and form. Only one piece of evidence encourages the old hope that a connection might be traceable between the visible characteristics of the body and those of the chromosomes. I refer, of course, to the accessory chromosome, which in many animals distinguishes the spermatozoon about to form a female in fertilisation. Even it, however, cannot be claimed as the cause of sexual differentiation, for it may be paired in forms closely allied to those in which it is unpaired or accessory. The distinction may be present or wanting, like any other secondary sexual character. Indeed, so long as no one can show consistent distinctions between the cytological characters of somatic tissues in the same individual we can scarcely expect to perceive such distinctions between the chromosomes of the various types.

For these methods of attack we now substitute another, less ambitious, perhaps, because less comprehensive, but not less direct. If we cannot see how a fowl by its egg and its sperm gives rise to a chicken or how a sweet pea from its ovule and its pollen grain produces another sweet pea, we at least can watch the system by which the differences between the various kinds of fowls or between the various kinds of sweet peas are distributed among the offspring. By thus breaking the main problem up into its parts we give ourselves fresh chances. This analytical study we call Mendelian because Mendel was the first to apply it. To be sure, he did not approach the problem by any such line of reasoning as I have sketched. His object was to determine the genetic definiteness of species; but though in his writings he makes no mention of inheritance, it is clear that he had the extension in view. By cross-breeding he combined the characters of varieties in mongrel individuals and set himself to see how these characters would be distributed among the individuals of subsequent generations. Until he began this analysis nothing but the vaguest answers to such a question had been attempted. The existence of any orderly system of descent was never even suspected. In their manifold complexity human characteristics seemed to follow no obvious system, and the fact was taken as a fair sample of the working of heredity.

Misconception was especially brought in by describing descent in terms of "blood." The common speech uses expressions such as consanguinity, pure-blooded, half-blood, and the like, which call up a misleading picture to the mind. Blood is in some respects a fluid, and thus it is supposed that this fluid can be both quantitatively and qualitatively diluted with other bloods, just as treacle can be diluted with water. Blood in primitive physiology being the peculiar vehicle of life, at once its essence and its corporeal abode, these ideas of dilution and compounding of characters in the commingling of bloods inevitably suggest that the ingredients of the mixture once combined are inseparable, that they can be brought together in any relative amounts, and in short that in heredity we are concerned mainly with a quantitative problem. Truer notions of genetic physiology are given by the Hebrew expression "seed." If we speak of a man as "of the blood-royal" we think at once of plebeian dilution, and we wonder how much of the royal fluid is likely to be "in his veins"; but if we say he is "of the seed of Abraham" we feel something of the permanence and indestructibility of that germ which can be divided and scattered among all nations, but remains recognisable in type and characteristics after 4000 years.

I knew a breeder who had a chest containing bottles of coloured liquids by which he used to illustrate the relationships of his dogs, pouring from one to another and titrating them quantitatively to illustrate their pedigrees. Galton was beset by the same kind of mistake when he promulgated his "Law of Ancestral Heredity." With modern research all this has been cleared away. The allotment of characteristics among offspring is not accomplished by the exudation of drops of a tincture representing the sum of the characteristics of the parent organism, but by a process of *cell-division*, in which numbers of these characters, or rather the elements upon which they depend, are sorted out among the resulting germ-cells in an orderly fashion. What these elements, or *factors* as we call them, are we do not know. That they are in some way directly transmitted by the material of the ovum and of the spermatozoon is obvious, but it seems to me unlikely that they are in any simple or literal sense material particles. I suspect rather that their properties depend on some phenomenon of arrangement. However that may be, analytical breeding proves that it is according to the distribution of these genetic factors, to use a non-committal term, that the characters of the offspring are decided. The first business of experimental genetics is to determine their number and interactions, and then to make an analysis of the various types of life.

Now the ordinary genealogical trees, such as those which the studbooks provide in the case of the domestic animals, or the Heralds' College provides in the case of man, tell nothing of all this. Such methods of depicting descent cannot even show the one thing they are devised to show—purity of "blood." For at last we know the physiological meaning of that expression. An organism is pure-bred when it has been formed by the union in fertilisation of two germ-cells which are alike in the factors they bear; and since the factors for the several characteristics are independent of each other, this question of purity must be separately considered for each of them. A man, for example, may be pure-bred in respect of his musical ability and cross-bred in respect of the colour of his eyes or the shape of his mouth. Though we know nothing of the essential nature of these factors, we know a good deal of their powers. They may confer height, colour, shape, instincts, powers both of mind and body; indeed, so many of the attributes which animals and plants possess that we feel justified in the expectation that with continued analysis they will be proved to be responsible for most if not all of the differences by which the varying individuals of any species are distinguished from each other. I will not assert that the greater differences which characterise distinct species are due generally to such independent factors, but that is the conclusion to which the available evidence points. All this is now so well understood, and has been so often demonstrated and expounded, that details of evidence are now superfluous.

But for the benefit of those who are unfamiliar with such work let me briefly epitomise its main features and consequences. Since genetic factors are definite things, either present in or absent from any germ-cell, the individual may be either "pure-bred" for any particular factor, or its absence, if he is constituted by the union of two germ-cells both possessing or both destitute of that factor. If the individual is thus pure, all his germ-cells will in that respect be identical, for they are simply bits of the similar germ-cells which united in fertilisation to produce the parent

organism. We thus reach the essential principle, that an organism cannot pass on to offspring a factor which it did not itself receive in fertilisation. Parents, therefore, which are both destitute of a given factor can only produce offspring equally destitute of it; and, on the contrary, parents both pure-bred for the presence of a factor produce offspring equally pure-bred for its presence. Whereas the germ-cells of the pure-bred are all alike, those of the cross-bred, which results from the union of dissimilar germ-cells, are mixed in character. Each positive factor segregates from its negative opposite, so that some germ-cells carry the factor and some do not. Once the factors have been identified by their effects, the average composition of the several kinds of families formed from the various matings can be predicted.

Only those who have themselves witnessed the fixed operations of these simple rules can feel their full significance. We come to look behind the simulacrum of the individual body, and we endeavour to disintegrate its features into the genetic elements by whose union the body was formed. Set out in cold general phrases, such discoveries may seem remote from ordinary life. Become familiar with them, and you will find your outlook on the world has changed. Watch the effects of segregation among the living things with which you have to do—plants, fowls, dogs, horses, that mixed concourse of humanity we call the English race, your friends' children, your own children, yourself—and however firmly imagination be restrained to the bounds of the known and the proved, you will feel something of that range of insight into nature which Mendelism has begun to give. The question is often asked whether there are not also in operation systems of descent quite other than those contemplated by the Mendelian rules. I myself have expected such discoveries, but hitherto none have been plainly demonstrated. It is true we are often puzzled by the failure of a parental type to reappear in its completeness after a cross—the merino sheep or the fantail pigeon, for example. These exceptions may still be plausibly ascribed to the interference of a multitude of factors, a suggestion not easy to disprove; though it seems to me equally likely that segregation has been in reality imperfect. Of the descent of quantitative characters we still know practically nothing. These and hosts of difficult cases remain almost untouched. In particular the discovery of E. Baur, and the evidence of Winkler in regard to his "graft hybrids," both showing that the subepidermal layer of a plant—the layer from which the germ-cells are derived—may bear exclusively the characters of a part only of the soma, give hints of curious complications, and suggest that in plants at least the interrelations between soma and gamete may be far less simple than we have supposed. Nevertheless, speaking generally, we see nothing to indicate that qualitative characters descend, whether in plants or animals, according to systems which are incapable of factorial representation.

The body of evidence accumulated by this method of analysis is now very large, and is still growing fast by the labours of many workers. Progress is also beginning along many novel and curious lines. The details are too technical for inclusion here. Suffice it to say that not only have we proof that segregation affects a vast range of characteristics, but in the course of our analysis phenomena of most unexpected kinds have been encountered. Some of these things twenty years ago must have seemed inconceivable. For example, the two sets of sex organs, male and female, of the same plant may not be carrying the same characteristics; in some animals characteristics, quite independent of sex, may be distributed solely or predominantly to one sex; in certain species the male may be breeding true to its own type, while the female is permanently mongrel, throwing off eggs of a distinct variety in addition to those of its own type; characteristics, essentially independent, may be associated in special combinations which are largely retained in the next generation, so that among the grandchildren there is numerical preponderance of those combinations which existed in the grandparents—a discovery which introduces us to a new phenomenon of polarity in the organism.

We are accustomed to the fact that the fertilised egg has a polarity, a front and hind end, for example; but we have now to recognise that it, or the primitive germinal cells formed from it, may have another polarity shown in the groupings of the parental elements. I am entirely sceptical as to the occurrence of segregation solely in the maturation of the germ-cells,[3] preferring at present to regard it as a special case of that patch-work condition we see in so many plants. These mosaics may break up, emitting bud-sports at various cell-divisions, and I suspect that the great regularity seen in the F_2 ratios of the cereals, for example, is a consequence of very late segregation, whereas the excessive irregularity found in other cases may be taken to indicate that segregation can happen at earlier stages of differentiation.

The paradoxical descent of colour-blindness and other sex-limited conditions—formerly regarded as an inscrutable caprice of nature—has been represented with approximate correctness, and we already know something as to the way, or, perhaps, I should say ways, in which the determination of sex is accomplished in some of the forms of life—though, I hasten to add, we have no inkling as to any method by which that determination may be influenced or directed. It is obvious that such discoveries have bearings on most of the problems, whether theoretical or practical, in which animals and plants are concerned. Permanence or change of type, perfection of type, purity or mixture of race, "racial development," the succession of forms, from being vague phrases expressing matters of degree, are now seen to be capable of acquiring physiological meanings, already to some extent assigned with precision. For the naturalist—and it is to him that I am especially addressing myself to-day—these things are chiefly significant as relating to the history of organic beings—the theory of evolution, to use our modern name. They have, as I shall endeavour to show in my second address to be given in Sydney, an immediate reference to the conduct of human society.

I suppose that everyone is familiar in outline with the theory of the origin of species which Darwin promulgated. Through the last fifty years this theme of the Natural Selection of favoured races has been developed and expounded in writings innumerable. Favoured races certainly can replace others. The argument is sound, but we are doubtful of its value. For us that debate stands adjourned. We go to Darwin for his incomparable collection of facts. We would fain emulate his scholarship, his width and his power of exposition, but to us he speaks no more with philosophical authority. We read his scheme of evolution as we would those of Lucretius or of Lamarck, delighting in their simplicity and their courage. The practical and experimental study of variation and heredity has not merely opened a new field; it has given a new point of view and new standards of criticism. Naturalists may still be found

[3] The fact that in certain plants the male and female organs respectively carry distinct factors may be quoted as almost decisively negativing the suggestion that segregation is confined to the reduction division.

expounding teleological systems[4] which would have delighted Dr. Pangloss himself, but at the present time few are misled. The student of genetics knows that the time for the development of theory is not yet. He would rather stick to the seed-pan and the incubator.

In face of what we now know of the distribution of variability in nature, the scope claimed for natural selection in determining the fixity of species must be greatly reduced. The doctrine of the survival of the fittest is undeniable so long as it is applied to the organism as a whole, but to attempt by this principle to find value in all definiteness of parts and functions, and in the name of science to see fitness everywhere, is mere eighteenth-century optimism. Yet it was in application to the parts, to the details of specific difference, to the spots on the peacock's tail, to the colouring of an orchid flower, and hosts of such examples, that the potency of natural selection was urged with the strongest emphasis. Shorn of these pretensions the doctrine of the survival of favoured races is a truism, helping scarcely at all to account for the diversity of species. Tolerance plays almost as considerable a part. By these admissions almost the last shred of that teleological fustian with which Victorian philosophy loved to clothe the theory of evolution is destroyed. Those who would proclaim that whatever is is right will be wise henceforth to base this faith frankly on the impregnable rock of superstition and to abstain from direct appeals to natural fact.

My predecessor said last year that in physics the age is one of rapid progress and profound scepticism. In at least as high a degree this is true of biology, and as a chief characteristic of modern evolutionary thought we must confess also to a deep but irksome humility in presence of great vital problems. Every theory of evolution must be such as to accord with the facts of physics and chemistry, a primary necessity to which our predecessors paid small heed. For them the unknown was a rich mine of possibilities on which they could freely draw. For us it is rather an impenetrable mountain out of which the truth can be chipped in rare and isolated fragments. Of the physics and chemistry of life we know next to nothing. Somehow the characters of living things are bound up in properties of colloids, and are largely determined by the chemical powers of enzymes, but the study of these classes of matter have only just begun. Living things are found by a simple experiment to have powers undreamt of, and who knows what may be behind?

Naturally we turn aside from generalities. It is no time to discuss the origin of the Mollusca or of Dicotyledons, while we are not even sure how it came to pass that *Primula obconica* has in twenty-five years produced its abundant new forms almost under our eyes. Knowledge of heredity has so reacted on our conceptions of variation that very competent men are even denying that variation in the old sense is a genuine occurrence at all. Variation is postulated as the basis of all evolutionary change. Do we then as a matter of fact find in the world about us variations occurring of such a kind as to warrant faith in a contemporary progressive evolution? Till lately most of us would have said "yes" without misgiving. We should have pointed, as Darwin did, to the immense range of diversity seen in many wild species, so commonly that the difficulty is to define the types themselves. Still more conclusive seemed the profusion of forms in the various domesticated animals and plants, most of them incapable of existing even for a generation in the wild state, and therefore fixed unquestionably by human selection. These, at least, for certain, are new forms, often distinct enough to pass for species, which have arisen by variation. But when analysis is applied to this mass of variation the matter wears a different aspect. Closely examined, what is the "variability" of wild species? What is the natural fact which is denoted by the statement that a given species exhibits much variation? Generally one of two things: either that the individuals collected in one locality differ among themselves; or perhaps more often that samples from separate localities differ from each other. As direct evidence of variation it is clearly to the first of these phenomena that we must have recourse—the heterogeneity of a population breeding together in one area. This heterogeneity may be in any degree, ranging from slight differences that systematists would disregard, to a complex variability such as we find in some moths, where there is an abundance of varieties so distinct that many would be classified as specific forms but for the fact that all are freely breeding together. Naturalists formerly supposed that any of these varieties might be bred from any of the others. Just as the reader of novels is prepared to find that any kind of parents might have any kind of children in the course of the story, so was the evolutionist ready to believe that any pair of moths might produce any of the varieties included in the species. Genetic analysis has disposed of all these mistakes. We have no longer the smallest doubt that in all these examples the varieties stand in a regular descending order, and that they are simply terms in a series of combinations of factors separately transmitted, of which each may be present or absent.

The appearance of contemporary variability proves to be an illusion. Variation from step to step in the series must occur either by the addition or by the loss of a factor. Now, of the origin of new forms *by loss* there seems to me to be fairly clear evidence, but of the *contemporary acquisition* of any new factor I see no satisfactory proof, though I admit there are rare examples which may be so interpreted. We are left with a picture of variation utterly different from that which we saw at first. Variation now stands out as a definite physiological event. We have done with the notion that Darwin came latterly to favour, that large differences can arise by accumulation of small differences. Such small differences are often mere ephemeral effects of conditions of life, and as such are not transmissible; but even small differences, when truly genetic, are factorial like the larger ones, and there is not the slightest reason for supposing that they are capable of summation. As to the origin or source of these positive separable factors, we are without any indication or surmise. By their effects we know them to be definite, as definite, say, as the organisms which produce diseases; but how they arise and how they come to take part in the composition of the living creature so that when present they are treated in cell-division as constituents of the germs, we cannot conjecture.

It was a commonplace of evolutionary theory that at least the domestic animals have been developed from a few wild types. Their origin was supposed to present no difficulty. The various races of fowl, for instance, all came from *Gallus bankiva*, the Indian

[4] I take the following from the abstract of a recent Croonian Lecture "On the Origin of Mammals" delivered to the Royal Society:—"In Upper Triassic times the larger Cynodonts preyed upon the large Anomodont, Kannemeyeria, and carried on their existence so long as these Anomodonts survived, but died out with them about the end of the Trias or in Rhætic times. The small Cynodonts, having neither small Anomodonts nor small Cotylosaurs to feed on, were forced to hunt the very active long-limbed Thecodonts. The greatly increased activity brought about that series of changes which formed the mammals—the flexible skin with hair, the four-chambered heart and warm blood, the loose jaw with teeth for mastication, an increased development of tactile sensation and a great increase of cerebrum. Not improbably the attacks of the newly-evolved Cynodont or mammalian type brought about a corresponding evolution in the Pseudosuchian Thecodonts which ultimately resulted in the formation of Dinosaurs and Birds." Broom, R., Proc. Roy. Soc., B., 87, p. 88.

jungle-fowl. So we are taught; but try to reconstruct the steps in their evolution and you realise your hopeless ignorance. To be sure there are breeds, such as black-red game and brown leghorns, which have the colours of the jungle-fowl, though they differ in shape and other respects. As we know so little as yet of the genetics of shape, let us assume that those transitions could be got over. Suppose, further, as is probable, that the absence of the maternal instinct in the leghorn is due to loss of one factor which the jungle-fowl possesses. So far we are on fairly safe ground. But how about white leghorns? Their origin may seem easy to imagine, since white varieties have often arisen in well-authenticated cases. But the white of white leghorns is not, as white in nature often is, due to the loss of the colour-elements, but to the action of something which inhibits their expression. Whence did that something come? The same question may be asked respecting the heavy breeds, such as Malays or Indian game. Each of these is a separate introduction from the East. To suppose that these, with their peculiar combs and close feathering, could have been developed from pre-existing European breeds is very difficult. On the other hand, there is no wild species now living any more like them. We may, of course, postulate that there was once such a species, now lost. That is quite conceivable, though the suggestion is purely speculative. I might thus go through the list of domesticated animals and plants of ancient origin and again and again we should be driven to this suggestion, that many of their distinctive characters must have been derived from some wild original now lost. Indeed, to this unsatisfying conclusion almost every careful writer on such subjects is now reduced. If we turn to modern evidence the case looks even worse. The new breeds of domestic animals made in recent times are the carefully selected products of recombination of pre-existing breeds. Most of the new varieties of cultivated plants are the outcome of deliberate crossing. There is generally no doubt in the matter. We have pretty full histories of these crosses in gladiolus, orchids, cineraria, begonia, calceolaria, pelargonium, etc. A very few certainly arise from a single origin. The sweet pea is the clearest case, and there are others which I should name with hesitation. The cyclamen is one of them, but we know that efforts to cross cyclamens were made early in the cultural history of the plant, and they may very well have been successful. Several plants for which single origins are alleged, such as the Chinese primrose, the dahlia, and tobacco, came to us in an already domesticated state, and their origins remain altogether mysterious. Formerly single origins were generally presumed, but at the present time numbers of the chief products of domestication, dogs, horses, cattle, sheep, poultry, wheat, oats, rice, plums, cherries, have in turn been accepted as "polyphyletic" or, in other words, derived from several distinct forms. The reason that has led to these judgments is that the distinctions between the chief varieties can be traced as far back as the evidence reaches, and that these distinctions are so great, so far transcending anything that we actually know variation capable of effecting, that it seems pleasanter to postpone the difficulty, relegating the critical differentiation to some misty antiquity into which we shall not be asked to penetrate. For it need scarcely be said that this is mere procrastination. If the origin of a form under domestication is hard to imagine, it becomes no easier to conceive of such enormous deviations from type coming to pass in the wild state. Examine any two thoroughly distinct species which meet each other in their distribution, as, for instance, *Lychnis diurna* and *vespertina* do. In areas of overlap are many intermediate forms. These used to be taken to be transitional steps, and the specific distinctness of *vespertina* and *diurna* was on that account questioned. Once it is known that these supposed intergrades are merely mongrels between the two species the transition from one to the other is practically beyond our powers of imagination to conceive. If both these can survive, why has their common parent perished? Why when they cross do they not reconstruct it instead of producing partially sterile hybrids? I take this example to show how entirely the facts were formerly misinterpreted.

When once the idea of a true-breeding—or, as we say, homozygous—type is grasped, the problem of variation becomes an insistent oppression. What can make such a type vary? We know, of course, one way by which novelty can be introduced—by crossing. Cross two well-marked varieties—for instance, of Chinese Primula—each breeding true, and in the second generation by mere recombination of the various factors which the two parental types severally introduced, there will be a profusion of forms, utterly unlike each other, distinct also from the original parents. Many of these can be bred true, and if found wild would certainly be described as good species. Confronted by the difficulty I have put before you, and contemplating such amazing polymorphism in the second generation from a cross in Antirrhinum, Lotsy has lately with great courage suggested to us that all variation may be due to such crossing. I do not disguise my sympathy with this effort. After the blind complacency of conventional evolutionists it is refreshing to meet so frank an acknowledgment of the hardness of the problem. Lotsy's utterance will at least do something to expose the artificiality of systematic zoology and botany. Whatever might or might not be revealed by experimental breeding, it is certain that without such tests we are merely guessing when we profess to distinguish specific limits and to declare that this is a species and that a variety. The only definable unit in classification is the homozygous form which breeds true. When we presume to say that such and such differences are trivial and such others valid, we are commonly embarking on a course for which there is no physiological warrant. Who could have foreseen that the apple and the pear—so like each other that their botanical differences are evasive—could not be crossed together, though species of Antirrhinum so totally unlike each other as *majus* and *molle* can be hybridised, as Baur has shown, without a sign of impaired fertility? Jordan was perfectly right. The true-breeding forms which he distinguished in such multitudes are real entities, though the great systematists, dispensing with such laborious analysis, have pooled them into arbitrary Linnean species, for the convenience of collectors and for the simplification of catalogues. Such pragmatical considerations may mean much in the museum, but with them the student of the physiology of variation has nothing to do. These "little species," finely cut, true-breeding, and innumerable mongrels between them, are what he finds when he examines any so-called variable type. On analysis the semblance of variability disappears, and the illusion is shown to be due to segregation and recombination of series of factors on pre-determined lines. As soon as the "little species" are separated out they are found to be fixed. In face of such a result we may well ask with Lotsy, is there such a thing as spontaneous variation anywhere? His answer is that there is not.

Abandoning the attempt to show that positive factors can be added to the original stock, we have

further to confess that we cannot often actually prove variation by loss of factor to be a real phenomenon. Lotsy doubts whether even this phenomenon occurs. The sole source of variation, in his view, is crossing. But here I think he is on unsafe ground. When a well-established variety like "Crimson King" Primula, bred by Messrs. Sutton in thousands of individuals, gives off, as it did a few years since, a salmon-coloured variety, "Coral King," we might claim this as a genuine example of variation by loss. The new variety is a simple recessive. It differs from "Crimson King" only in one respect, the loss of a single colour-factor, and, of course, bred true from its origin. To account for the appearance of such a new form by any process of crossing is exceedingly difficult. From the nature of the case there can have been no cross since "Crimson King" was established, and hence the salmon must have been concealed as a recessive from the first origin of that variety, even when it was represented by very few individuals, probably only by a single one. Surely, if any of these had been heterozygous for salmon this recessive could hardly have failed to appear during the process of self-fertilisation by which the stock would be multiplied, even though that selfing may not have been strictly carried out. Examples like this seem to me practically conclusive.[5] They can be challenged, but not, I think, successfully. Then again in regard to those variations in number and division of parts which we call meristic, the reference of these to original cross-breeding is surely barred by the circumstances in which they often occur. There remain also the rare examples mentioned already in which a single wild origin may with much confidence be assumed. In spite of repeated trials, no one has yet succeeded in crossing the Sweet Pea with any other leguminous species. We know that early in its cultivated history it produced at least two marked varieties which I can only conceive of as spontaneously arising, though, no doubt, the profusion of forms we now have was made by the crossing of those original varieties. I mention the Sweet Pea thus prominently for another reason, that it introduces us to another though subsidiary form of variation, which may be described as a *fractionation* of factors. Some of my Mendelian colleagues have spoken of genetic factors as permanent and indestructible. Relative permanence in a sense they have, for they commonly come out unchanged after segregation. But I am satisfied that they may occasionally undergo a quantitative disintegration, with the consequence that varieties are produced intermediate between the integral varieties from which they were derived. These disintegrated conditions I have spoken of as subtraction—or reduction—stages. For example, the Picotee Sweet Pea, with its purple edges, can surely be nothing but a condition produced by the factor which ordinarily makes the fully purple flower, quantitatively diminished. The pied animal, such as the Dutch rabbit, must similarly be regarded as the result of partial defect of the chromogen from which the pigment is formed, or conceivably of the factor which effects its oxidation. On such lines I think we may with great confidence interpret all those intergrading forms which breed true and are not produced by factorial interference.

It is to be inferred that these fractional degradations are the consequence of irregularities in segregation. We constantly see irregularities in the ordinary meristic processes, and in the distribution of somatic differentiation. We are familiar with half segments, with imperfect twinning, with leaves partially petaloid, with petals partially sepaloid. All these are evidences of departures from the normal regularity in the rhythms of repetition, or in those waves of differentiation by which the qualities are sorted out among the parts of the body. Similarly, when in segregation the qualities are sorted out among the germ-cells in certain critical cell-divisions, we cannot expect these differentiating divisions to be exempt from the imperfections and irregularities which are found in all the grosser divisions that we can observe. If I am right, we shall find evidence of these irregularities in the association of unconformable numbers with the appearance of the novelties which I have called fractional. In passing let us note how the history of the sweet pea belies those ideas of a continuous evolution with which we had formerly to contend. The big varieties came first. The little ones have arisen later, as I suggest by fractionation. Presented with a collection of modern sweet peas how prettily would the devotees of continuity have arranged them in a graduated series, showing how every intergrade could be found, passing from the full colour of the wild Sicilian species in one direction to white, in the other to the deep purple of "Black Prince," though happily we know these two to be among the earliest to have appeared.

Having in view these and other considerations which might be developed, I feel no reasonable doubt that though we may have to forgo a claim to variations by addition of factors, yet variation both by loss of factors and by fractionation of factors is a genuine phenomenon of contemporary nature. If, then, we have to dispense, as seems likely, with any addition from without we must begin seriously to consider whether the course of evolution can at all reasonably be represented as an unpacking of an original complex which contained within itself the whole range of diversity which living things present. I do not suggest that we should come to a judgment as to what is or is not probable in these respects. As I have said already, this is no time for devising theories of evolution, and I propound none. But as we have got to recognise that there has been an evolution, that somehow or other the forms of life have arisen from fewer forms, we may as well see whether we are limited to the old view that evolutionary progress is from the simple to the complex, and whether after all it is conceivable that the process was the other way about. When the facts of genetic discovery become familiarly known to biologists, and cease to be the preoccupation of a few, as they still are, many and long discussions must inevitably arise on the question, and I offer these remarks to prepare the ground. I ask you simply to open your minds to this possibility. It involves a certain effort. We have to reverse our habitual modes of thought. At first it may seem rank absurdity to suppose that the primordial form or forms of protoplasm could have contained complexity enough to produce the divers types of life. But it is easier to imagine that these powers could have been conveyed by extrinsic additions? Of what nature could these additions be? Additions of material cannot surely be in question. We are told that salts of iron in the soil may turn a pink hydrangea blue. The iron cannot be passed on to the next generation. How can the iron multiply itself? The power to assimilate the iron is all that can be transmitted. A disease-producing organism like the pebrine of silkworms can in a very few cases be passed on through the germ-cells. Such an organism can multiply and can produce its characteristic effects in the next generation. But it does

[5] The numerous and most interesting "mutations" recorded by Prof. T. H. Morgan and his colleagues in the fly, Drosophila, may also be cited as unexceptionable cases.

not become part of the invaded host, and we cannot conceive it taking part in the geometrically ordered processes of segregation. These illustrations may seem too gross; but what refinement will meet the requirements of the problem, that the thing introduced must be, as the living organism itself is, capable of multiplication and of subordinating itself in a definite system of segregation? That which is conferred in variation must rather itself be a change, not of material, but of arrangement, or of motion. The invocation of additions extrinsic to the organism does not seriously help us to imagine how the power to change can be conferred, and if it proves that hope in that direction must be abandoned, I think we lose very little. By the re-arrangement of a very moderate number of things we soon reach a number of possibilities practically infinite.

That primordial life may have been of small dimensions need not disturb us. Quantity is of no account in these considerations. Shakespeare once existed as a speck of protoplasm not so big as a small pin's head. To this nothing was added that would not equally well have served to build up a baboon or a rat. Let us consider how far we can get by the process of removal of what we call "epistatic" factors, in other words those that control, mask, or suppress underlying powers and faculties. I have spoken of the vast range of colours exhibited by modern sweet peas. There is no question that these have been derived from the one wild bi-colour form by a process of successive removals. When the vast range of form, size, and flavour to be found among the cultivated apples is considered it seems difficult to suppose that all this variety is hidden in the wild crab-apple. I cannot positively assert that this is so, but I think all familiar with Mendelian analysis would agree with me that it is probable, and that the wild crab contains presumably inhibiting elements which the cultivated kinds have lost. The legend that the seedlings of cultivated apples become crabs is often repeated. After many inquiries among the raisers of apple seedlings I have never found an authentic case—once only even an alleged case, and this on inquiry proved to be unfounded. I have confidence that the artistic gifts of mankind will prove to be due not to something added to the make-up of an ordinary man, but to the absence of factors which in the normal person inhibit the development of these gifts. They are almost beyond doubt to be looked upon as *releases* of powers normally suppressed. The instrument is there, but it is "stopped down." The scents of flowers or fruits, the finely repeated divisions that give its quality to the wool of the merino, or in an analogous case the multiplicity of quills to the tail of the fantail pigeon, are in all probability other examples of such releases. You may ask what guides us in the discrimination of the positive factors and how we can satisfy ourselves that the appearance of a quality is due to loss. It must be conceded that in these determinations we have as yet recourse only to the effects of dominance. When the tall pea is crossed with the dwarf, since the offspring is tall we say that the tall parent passed a factor into the cross-bred which makes it tall. The pure tall parent had two doses of this factor; the dwarf had none; and since the cross-bred is tall we say that one dose of the dominant tallness is enough to give the full height. The reasoning seems unanswerable. But the commoner result of crossing is the production of a form intermediate between the two pure parental types. In such examples we see clearly enough that the full parental characteristics can only appear when they are homozygous—formed from similar germ-cells, and that one dose is insufficient to produce either effect fully. When this is so we can never be sure which side is positive and which negative. Since, then, when dominance is incomplete we find ourselves in this difficulty, we perceive that the amount of the effect is our only criterion in distinguishing the positive from the negative, and when we return even to the example of the tall and dwarf peas the matter is not so certain as it seemed. Professor Cockerell lately found among thousands of yellow sunflowers one which was partly red. By breeding he raised from this a form wholly red. Evidently the yellow and the wholly red are the pure forms, and the partially red is the heterozygote.

We may then say that the yellow is *YY* with two doses of a positive factor which inhibits the development of pigment; the red is *yy*, with no dose of the inhibitor; and the partially red are *Yy*, with only one dose of it. But we might be tempted to think the red was a positive characteristic, and invert the expressions, representing the red as *RR*, the partly red as *Rr*, and the yellow as *rr*. According as we adopt the one or the other system of expression we shall interpret the evolutionary change as one of loss or as one of addition. May we not interpret the other apparent new dominants in the same way? The white dominant in the fowl or in the Chinese primula can inhibit colour. But may it not be that the original coloured fowl or primula had two doses of a factor which inhibited this inhibitor? The Pepper moth, *Amphidasys betularia*, produced in England about 1840 a black variety, then a novelty, now common in certain areas, which behaves as a full dominant. The pure blacks are no blacker than the cross-bred. Though at first sight it seems that the black *must* have been something added, we can without absurdity suggest that the normal is the term in which two doses of inhibitor are present, and that in the absence of one of them the black appears.

In spite of seeming perversity, therefore, we have to admit that there is no evolutionary change which in the present state of our knowledge we can positively declare to be not due to loss. When this has been conceded it is natural to ask whether the removal of inhibiting factors may not be invoked in alleviation of the necessity which has driven students of the domestic breeds to refer their diversities to multiple origins. Something, no doubt, is to be hoped for in that direction, but not until much better and more extensive knowledge of what variation by loss may effect in the living body can we have any real assurance that this difficulty has been obviated. We should be greatly helped by some indication as to whether the origin of life has been single or multiple. Modern opinion is, perhaps, inclining to the multiple theory, but we have no real evidence. Indeed, the problem still stands outside the range of scientific investigation, and when we hear the spontaneous formation of formaldehyde mentioned as a possible first step in the origin of life, we think of Harry Lauder in the character of a Glasgow schoolboy pulling out his treasures from his pocket—"That's a wassher—for makkin' motor cars"!

As the evidence stands at present all that can be safely added in amplication of the evolutionary creed may be summed up in the statement that variation occurs as a definite event often producing a sensibly discontinuous result; that the succession of varieties comes to pass by the elevation and establishment of sporadic groups of individuals owing their origin to such isolated events; and that the change which we see as a nascent variation is often, perhaps always, one of loss. Modern research lends not the smallest encouragement or sanction to the view that gradual evolution occurs by the transformation of masses of individuals, though that fancy has fixed itself on

popular imagination. The isolated events to which variation is due are evidently changes in the germinal tissues, probably in the manner in which they divide. It is likely that the occurrence of these variations is wholly irregular, and as to their causation we are absolutely without surmise or even plausible speculation. Distinct types once arisen, no doubt a profusion of the forms called species have been derived from them by simple crossing and subsequent recombination. New species may be now in course of creation by this means, but the limits of the process are obviously narrow. On the other hand, we see no changes in progress around us in the contemporary world which we can imagine likely to culminate in the evolution of forms distinct in the larger sense. By intercrossing dogs, jackals, and wolves new forms of these types can be made, some of which may be species, but I see no reason to think that from such material a fox could be bred in indefinite time, or that dogs could be bred from foxes.

Whether science will hereafter discover that certain groups can by peculiarities in their genetic physiology be declared to have a prerogative quality justifying their recognition as species in the old sense, and that the differences of others are of such a subordinate degree that they may in contrast be termed varieties, further genetic research alone can show. I myself anticipate that such a discovery will be made, but I cannot defend the opinion with positive conviction.

Somewhat reluctantly, and rather from a sense of duty, I have devoted most of this address to the evolutionary aspects of genetic research. We cannot keep these things out of our heads, though somtimes we wish we could. The outcome, as you will have seen, is negative, destroying much that till lately passed for gospel. Destruction may be useful, but it is a low kind of work. We are just about where Boyle was in the seventeenth century. We can dispose of alchemy, but we cannot make more than a quasi-chemistry. We are awaiting our Priestley and our Mendeléeff. In truth it is not these wider aspects of genetics that are at present our chief concern. They will come in their time. The great advances of science are made like those of evolution, not by imperceptible mass-improvement, but by the sporadic birth of penetrative genius. The journeymen follow after him, widening and clearing up, as we are doing along the track that Mendel found.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. F. T. TROUTON, M.A., SC.D., F.R.S., PRESIDENT OF THE SECTION.

WE have lost since the last meeting of the Section several distinguished members who have in the past added so much to the usefulness of our discussions. These include Sir Robert Ball, who was one of our oldest attendants, and was president of the section at the Manchester meeting in 1886; Prof. Poynting, who was president of the section at Dover in 1899; and Sir David Gill, who was president of the Association at Leicester in 1907.

It seems appropriate at this meeting in the City of Melbourne to mention one who passed away from his scientific labours somewhat previous to the last meeting. I allude to W. Sutherland of this city, whose writings have thrown so much light on molecular physics and whose scientific perspicacity was only equalled by his modesty.

This meeting of the British Association will be a memorable one as being indicative, as it were, of the scientific coming of age of Australia. Not that the maturity of Australian science was unknown to those best able to judge, indeed the fact could not but be known abroad, for in England alone there are many workers in science hailing from Australia and New Zealand, who have enhanced science with their investigations and who hold many important scientific posts in that country. In short, one finds it best nowadays to ask of any young investigator if he comes from the Antipodes.

This speaks well for the universities and their staffs, who have so successfully set the example of scientific investigation to their pupils.

Radio-activity and kindred phenomena seem to have attracted them most of late years, and it would perhaps have been appropriate to have shortly reviewed in this address our knowledge in these subjects, to which the sons of Australasia have so largely contributed.

Twenty-five years ago FitzGerald and others were speculating on the possibility of unlocking and utilising the internal energy of the atom. Then came the epoch-making discovery of Becquerel, to be followed by the brilliant work of Rutherford and others showing us that no key was required to unlock this energy, the door lay open.

We have still facing us the analogous case of a hitherto untapped source of energy arising from our motion through the ether. All attempts, it is true, to realise this have failed, but nevertheless he would be a brave prophet who would deny the possibility of tapping this energy despite the ingenious theories of relativity which have been put forward to explain matters away. There is no doubt but that up to the present nothing hopeful has been accomplished towards reaching this energy and there are grave difficulties in the way; but "Relativity" is, as it were, merely trying to remove the lion in the path by laying down the general proposition that the existence of lions is an impossibility. The readiness with which the fundamental hypotheses of "Relativity" were accepted by many is characteristic of present-day physics, or perhaps more correctly speaking is an exaggerated example of it.

Such an acceptance as this could hardly be thought of as taking place half-a-century ago when a purely dynamical basis was expected for the full explanation of all phenomena, and when facts were only held to be completely understood if amenable to such treatment; while, if not so, they were put temporarily into a kind of suspense account waiting the time when the phenomenon would succumb to treatment based on dynamics.

Many things, perhaps not the least among them radio-activity, have conspired to change all this and to produce an attitude of mind prepared to be content with a much less rigid basis than would have been required by the natural philosophers of a past generation. These were the sturdy Protestants of science, to use an analogy, while we of the present day are much more catholic in our scientific beliefs, and in fact it would seem that nowadays to be used to anything is synonymous with understanding it.

Leaving, however, these interesting questions, I will confine my remarks to a rather neglected corner of physics, namely to the phenomena of absorption and adsorption of solutions. The term adsorption was introduced to distinguish between absorption which takes place throughout the mass of the absorbing material and those cases in which it takes place only over its surface. If, for instance, glass, powdered so as to provide a large surface, is introduced into a solution of a salt in water, we have in general some of the salt leaving the body of the solution and adhering in one form or other to the surface of the

glass. It is to this the term adsorption has been applied. Physicists have now begun to take up the question seriously, but it was to biologists and especially physiological chemists that most of our knowledge of the subject in the past was due, the phenomenon being particularly attractive to them, seeing that so many of the processes they are interested in take place across surfaces.

As far as investigations already made go, the laws of adsorption appear to be very complicated, and no doubt many of the conflicting experimental results which have been obtained are in part due to this, workers under somewhat different conditions obtaining apparently contradictory effects.

On the whole, however, it may be said that the amount adsorbed increases with the strength of solution according to a simple power law, and diminishes with rise of temperature; but there are many exceptions to these simple rules. For instance, in the case of certain sulphates and nitrates the amount adsorbed by the surface of, say, precipitated silica, only increases up to a certain critical point as the strength of the solution is increased. Then further increase in the strength of the solution causes the surface to give up some of the salt it has already adsorbed or the amount adsorbed is actually less now than that adsorbed from weaker solutions. Beyond this stage for still greater concentrations of the solutions the amount adsorbed goes on increasing as before the critical point was reached.

There is some reason for thinking that there are two modes in which the salt is taken up or adsorbed by the solid surface. The first of them results from a simple strengthening of the solution in the surface layers; the second, which takes place with rather stronger concentrations, is a deposition in what is apparently analogous to the solid form. It would seem that the first reaches out from the solid surface to about 10^{-8} cm.—which is the order of the range of attraction of the particles of the solid substance.

The cause of the diminution in the adsorption layer at a certain critical value of the concentration is difficult to understand. Something analogous has been observed by Lord Rayleigh in the thickness of layers of oil floating on the surface of water. As oil is supplied the thickness goes on increasing up to a certain point, beyond this, on further addition of oil, the layer thins itself at some places and becomes much thicker at others, intermediate thicknesses to these being apparently unstable and unable to exist. As helping towards an explanation of the diminution in the adsorption layer, we may suppose that as the strength of the solution is increased from zero, the adsorption is at first merely an increased density of the solution in the surface layer. For some reason, after this has reached a certain limit, further addition of salt to the solution renders this mode of composition of the surface layers unstable, and there is a breaking up of the arrangement of the layer with a diminution in its amount. We may now suppose the second mode of deposition to begin to show its effect with a recovery in the amount of the surface layers and a further building up of the adsorption deposits.

On account of passing through this point of instability the process is irreversible, so that the application of thermo-dynamics to the phenomenon of adsorption is necessarily greatly restricted in its usefulness.

A possible cause of the instability in the adsorption layer which occurs at the critical point may be looked for in the alternations in the sign of the mutual forces between attracting particles of the kind suggested by Lord Kelvin and others. Within a certain distance apart—the molecular range—the particles of matter mutually attract one another, while at very close distances they obviously must repel, for two particles refuse to occupy the same space. At some intermediate distances the force must pass through zero value. It has for various reasons been thought that, in addition, the force has zero value at a second distance lying between the first zero and the molecular range, with accompanying alternations in the sign of the force. Thus, starting from zero distance apart of the particles, the sign of the force is negative or repulsive; then, as the distance apart is supposed to increase, the force of repulsion diminishes, and after passing through zero value becomes positive or attractive; next, as the distance is increased, the force diminishes again, and after passing through a second zero becomes negative for a second time; finally, the force on passing through a third zero becomes positive, and is then in the stage dealt with in capillary and other questions.

As an instance, of where these alternations of sign seem to be manifest, may be mentioned the case of certain crystals when split along cleavage planes. The split often runs along further than the position of the splitting instrument or inserted wedge seems to warrant. This would occur if the particles on either side of the cleavage plane were situated at the distance apart where the force between them was in the first attractive condition, for then on increasing the distance between the particles by means of the wedge the force changes sign and becomes repulsive, thus helping the splitting to be propagated further out.

Assuming that a repulsive force can supervene between the particles in the adsorption layer, through the particles becoming so crowded in places as to reduce their mutual distances to the stage when repulsion sets in, we might expect that an instability would be set up.

As already stated, a rise in temperature reduces in general the amount adsorbed, but below the critical point the nitrates and sulphates are exceptional, for rise in temperature here increases the amount adsorbed from a given solution. This obviously necessitates that the isothermals cross one another at the critical point in an adsorption-concentration diagram. This may perhaps account for some observers finding that adsorption did not change with temperature. We have another exception to the simple laws of adsorption in the case of the alkali chlorides; this exception occurs under certain conditions of temperature and strength of solution. The normal condensation into the surface layer is reversed and the salt is repelled into the general solution instead of being attracted by the surface. In other words, it is the turn of the other constituent of the solution, namely, the water, to be adsorbed.

It is a very well known experiment in adsorption to run a solution such as that of permanganate of potash through a filter of sand, or, better, one of precipitated silica, so as to provide a very large surface. The first of the solution to come through the filter has practically lost all its salt owing to having been adsorbed by the surface of the sand.

I was interested in finding a few months ago that Defoe, the author of "Robinson Crusoe," in one of his other books, depicts a party of African travellers as being saved from thirst in a place where the water was charged with alkali by filtering the water through bags of sand. Whether this is a practical thing or not is doubtful, or even if it has ever been tried; for it is only the first part of the liquid to come through the filter which is purified, and very soon the surface has taken up all the salt it can adsorb, and after that, of course, the solution comes through intact. It is

interesting, however, to know that so long ago as Defoe's time the phenomenon of adsorption from salt solutions had been observed. It is not so well known that in the case of some salts under the circumstances mentioned above, the first of the solution to come through the sand filter is stronger instead of weaker. This, as already mentioned, is because water, or at least a weaker solution, forms the adsorption layer.

Most of the alkali chlorides as the temperature is raised show this anomalous adsorption, provided the strength of the solution is below a certain critical value differing for each temperature. For strengths of solution above these values the normal phenomenon takes place.

No investigations seem to have been made on the effect of pressure on adsorption. These data are much to be desired.

The investigation of adsorption and absorption should throw light on osmosis, as in the first place the phenomenon occurs across a surface necessarily covered with an adsorption layer, and in the second place, as we shall see, the final condition is an equilibrium between the absorption of water by the solution and that by the membrane.

The study of the conditions of absorption of water throughout the mass of the colloidal substance of which osmotic membranes are made is of much interest. Little work has been done on the subject as yet, but what little has been done is very promising.

It is convenient to call the material of which a semi-permeable membrane is made the semi-permeable medium. The ideal semi-permeable medium will not absorb any salt from the solution but only water, but such perfection is probably seldom to be met with. If a semi-permeable medium such as parchment paper be immersed in a solution, say, of sugar, less water is taken up or absorbed than is the case when the immersion is in pure water. The diminution in the amount absorbed is found to increase with the strength of the solution. It is at the same time found that the absorption or release of water by the semi-permeable medium according as the solution is made weaker or stronger is accompanied by a swelling or shrinkage greater than can be accounted for by the water taken up or rejected.

The amount of water absorbed by a semi-permeable medium from a solution is found by experiment to depend upon the hydrostatic pressure. If the pressure be increased the amount of water absorbed by the semi-permeable medium is increased. It is always thus possible by the application of pressure to force the semi-permeable medium to take up from a given solution as much water as it takes up from pure water at atmospheric pressure.

It is not possible for a mass of such a medium to be simultaneously in contact and in equilibrium with both pure water and with a solution all at one and the same pressure, seeing that the part of the medium in contact with the pure water would hold more water than that part in contact with the solution and consequently diffusion would take place through the mass of the medium.

If, however, the medium be arranged so as to separate the solution and the water and provided the medium is capable of standing the necessary strain, it is possible to increase the pressure of the solution without increasing the pressure of the water on the other side. Thus the part of the medium which is in contact with the solution is at a higher pressure than that part in contact with the pure solvent; consequently the medium can be in equilibrium with both the solution and the solvent, for if the pressures are rightly adjusted the moisture throughout the medium is everywhere the same.

The ordinary arrangement for showing osmotic pressure is a case such as we are considering, and equilibrium throughout the membrane is only obtained when the necessary difference in pressure exists between the two sides of the membrane.

This condition would eventually be reached no matter how thick the membrane was. It is sometimes helpful to think of the membrane as being very thick. It precludes any temptation to view molecules as shooting across from one liquid to the other through some kind of peep-holes in the membrane.

The advantage in a thin membrane in practice is simply that the necessary moisture is rapidly applied to the active surface, thus enabling the pressure on the side of the solution to rise quickly, but it has no effect on the ultimate equilibrium.

As far as that goes, the semi-permeable membrane or saturated medium might be infinitely thick, or, in other words, there need be no receptacle or place for holding the pure solvent outside the membrane at all. In fact, the function of the receptacle containing the pure solvent is only to keep the medium moist, and is no more or no less important than the vessel of water supplied to the gauze of the wet-bulb thermometer. It is merely to keep up the supply of water to the medium.

The real field where the phenomenon of osmosis takes place is the surface of separation between the saturated semi-permeable medium and the solution. Imagine a large mass of colloidal substance saturated with water and having a cavity containing a solution. The pressure will now tend to rise in the cavity until it reaches the osmotic pressure—that is, until there is established an equilibrium of surface transfer of molecules from the solution into the medium and back from the medium into the solution.

No doubt, the phenomenon as thus described occurs often in nature. It is just possible that the high-pressure liquid cavities, which mineralogists find in certain rock crystals, have been formed in some such manner in the midst of a mass of semi-permeable medium; the pure solvent in this case being carbon dioxide and the medium colloidal silica, which has since changed into quartz crystal.

In considering equilibrium between a saturated semi-permeable medium and a solution there seems to me to be a point which should be carefully considered before being neglected in any complete theory. That is, the adsorption layer over the surface of the semi-permeable medium. We have seen that solutions are profoundly modified in the surface layers adjoining certain solids, through concentration or otherwise of the salts in the surface layer, so that the actual equilibrium of surface transfer of water molecules is not between the unmodified solution and the semi-permeable medium, but between the altered solution in the absorption layer and the saturated medium. Actual determinations of the adsorption by colloids are much wanted, so as to be able to be quite sure of what this correction amounts to or even if it exists. It may turn out to be zero. If there is adsorption, however, it may possibly help to account for part of the unexpectedly high values of the osmotic pressure observed at high concentrations of the solution, the equilibrium being, as we have seen, between the saturated medium and a solution of greater concentration than the bulk of the liquid, namely, that of the adsorption layer. In addition, when above the critical adsorption point, there may be a deposit in the solid state. This may produce a kind of polarised equilibrium of surface transfer in which the molecules which discharge from the saturated medium remain unaltered in amount, but those which move back from the adsorption layer are reduced owing to this de-

posit, thus necessitating an increase in pressure for equilibrium. If either or both of these effects really exist, it would seem to require that the pressure should be higher for equilibrium of the molecular surface transfer than if there were no adsorption layer and the unaltered solution were to touch the medium, but at the same time it should be remembered that there is a second surface where equilibrium must also exist—that is, the surface of separation of the adsorption layer and the solution itself. It is just possible that the two together cancel each other's action.

Quantitative determinations of absorption by solid media from solution are hard to carry out, but with a liquid medium it is not so difficult. Ether constitutes an excellent semi-permeable medium for use with sugar solution, because it takes up or dissolves only a small quantity of water and no sugar. A series of experiments using these for medium and solution has shown (1) that the absorption of water from a solution diminishes with the strength of the solution; and (2) that the absorption of water for any given strength of solution increases with the pressure. This increase with pressure is somewhat more rapid than if it were in proportion to the pressure. On the other hand, from pure water ether absorbs in excess of normal almost in proportion to the pressure. Certainly this is so up to 100 atmospheres. This would go to confirm the suggestion already made that the departure from proportionality in the osmotic pressure is attributable to absorption.

By applying pressure ether can be thus made to take up the same quantity of water from any given solution as it takes up from pure water at atmospheric pressure. It is found by experiment that this pressure is the osmotic pressure proper to the solution in question.

Decidedly the most interesting fact connected with the whole question of osmotic pressure, the behaviour of vapour pressures from solution, and the equilibrium of molecular transfer of solutions with colloids, is that discovered by van 't Hoff, that the hydrostatic pressure in question is equal to what would be produced by a gas having the same number of particles as those of the introduced salt. Take the case of a mass of colloid or semi-permeable medium placed in a vessel of water; the colloid when in equilibrium at atmospheric pressure holds what we will call the normal moisture. By increasing the pressure this moisture can be increased to any desired amount. Now, on introducing salt the moisture in the colloid can be reduced at will. The question is, what quantity of salt must be introduced just to bring back the amount of the moisture in the colloid to normal? Here we get a great insight into the internal mechanism of the liquid state. The quantity of salt required turns out to be, approximately at least, that amount which if in the gaseous state would produce the pressure. So that normality can be either directly restored by removing the pressure or indirectly by introducing salt in quantity which just takes up the applied pressure. That this is so naturally suggested that the salt, although compelled to remain within the confines of the liquid, nevertheless produces the same molecular bombardment as it would were it in the gaseous state, though, of course, the free path must be viewed as enormously restricted compared with that in the gaseous state.

Many have felt a difficulty in accepting this view of a molecular bombardment occurring in the liquid state, but of recent years much light has been thrown on the subject of molecular movements in liquids, especially by Perrin's work, so that much of the basis of this difficulty may be fairly considered as now removed.

Quite analogous to the reduction from the normal of the moisture held by a semi-permeable medium brought about by the addition of salt to the water, is the reduction in the vapour pressure arising from the presence of a salt in the water. The vapour pressure is likewise increased by the application of hydrostatic pressure, which may be effected by means of an inert gas. In both cases the hydrostatic pressure which must be applied to bring back to normality is equal to that which the added salt would exert if it were in the state of vapour or, in other words, the osmotic pressure.

The two cases are really very similar. In both there is equal molecular transfer backwards and forwards across the bounding surface. In the one a transfer from that solution to the semi-permeable medium and back from it into the solution. In the other a transfer from the solution into the superambient vapour and back from it into the solution.

The processes are very similar, namely, equal molecular transfer to and fro across the respective surfaces of separation.

Thus we may in the case of osmotic equilibrium attribute the phenomenon with Callendar to evaporation, but not evaporation in its restricted sense, from a free surface of liquid, but as we have seen from a saturated colloidal surface into the solution. This process might perhaps be better referred to as molecular emigration, the term migration being already a familiar one in connection with liquid phenomena.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. WILLIAM J. POPE, M.A., LL.D., F.R.S., PRESIDENT OF THE SECTION.

THE British Association has been firmly established as one of the institutions of our Empire for more than half a century past. The powerful hold which it has acquired probably arises from the welcome which every worker in science extends to an occasional cessation of his ordinary routine—a respite during which the details of the specific inquiry in hand may be temporarily cast aside, and replaced by leisurely discussion with colleagues on the broader issues of scientific progress.

The investigator, continually occupied with his own problems and faced with an ever-increasing mass of technical literature, ordinarily finds little time for reflection upon the real meaning of his work; he secures, in general, far too few opportunities of considering in a philosophical sort of way the past, present, and future of his own particular branch of scientific activity. It is not difficult to form a fairly accurate survey of the position to which chemistry had attained a generation ago, perhaps even a few years ago; probably no intellect at present existing could pronounce judgment upon the present position of our science in terms which would commend themselves to the historian of the twenty-first century. Doubtless even one equipped with a complete knowledge of all that has been achieved, standing on the very frontier of scientific advance and peering into the surrounding darkness, would be quite incompetent to make any adequate forecast of the conquests which will be made by chemical and physical science during the next fifty years. At the same time, chemical history tells us that progress is the result in large measure of imperfect attempts to appreciate the present and to forecast the future. I therefore propose to

lay before you a sketch of the present position of certain branches of chemical knowledge and to discuss the directions in which progress is to be sought; none of us dare cherish the conviction that his views on such matters are correct, but everyone desirous of contributing towards the development of his science must attempt an appreciation of this kind. The importance to the worker and to the subject of free ventilation and discussion of the point of view taken by the individual can scarcely be over-estimated.

The two sciences of chemistry and physics were at one time included as parts of the larger subject entitled natural philosophy, but in the early part of the nineteenth century they drew apart. Under the stimulus of Dalton's atomic theory, chemistry developed into a study of the interior of the molecule, and, as a result of the complication of the observed phenomena, progressed from stage to stage as a closely reasoned mass of observed facts and logical conclusions. Physics, less entangled in its infancy with numbers of experimental data which apparently did not admit of quantitative correlation, was developed largely as a branch of applied mathematics, such achievements of the formal physics of the last century as the mathematical theory of light and the kinetic theory of gases are monuments to the powers of the human intellect.

The path of chemistry, as an application of pure logical argument to the interpretation of complex masses of observations, thus gradually diverged from that taken by physics as the mathematical treatment of less involved experimental data, although both subjects derived their impetus to development from the speculations of genius.

It is interesting to note, however, that during recent years the two sciences, which were so sharply distinguished twenty years ago as to lead to mutual misunderstandings, are now converging. Many purely chemical questions have received such full quantitative study that the results are susceptible to attack by the methods of the mathematical physicist; on the other hand, the intense complication perceived during the fuller examination of many physical prolems, has led to their interpretation by the logical argument of the chemist because the traditional mathematical mode of attack of the phycisist has proved powerless to deal with the intricacies exhibited by the observed facts.

The progress of chemistry during the last century has been mainly the result of the coordination of observed facts in accordance with a series of hypotheses each closely related in point of time to the one preceding it. The atomic theory, as it was enunciated by Dalton in 1803, was a great impetus to chemical investigation, but proved insufficient to embrace all the known facts; it was supplemented in 1813 by Avogadro's theorem—that equal volumes of gases contain the same number of molecules at the same temperature and pressure. These two important theoretical developments led to the association of a definite physical meaning with the idea of molecular *composition*, but ultimately proved insufficient for the interpretation of the ever-increasing mass of chemical knowledge collected under their stimulus. A further great impetus followed the introduction by Frankland and Kekulé, in 1852 onwards, of the idea of valency and the mode of building up constitutional formulæ; the conception of molecular *constitution* thus arose as a refinement on the Daltonian notion of molecular composition. In course of time the theoretical scheme once more proved insufficient to accommodate the accumulated facts, until, in 1874, van 't Hoff and Le Bel demonstrated the all-important part which molecular *configuration* plays in the interpretation of certain classes of phenomena known to the organic chemist.

During the early days of chemical science—those of Dalton's time and perhaps also those of Frankland and Kekulé—we can believe that chemical theory may have lacked the physical reality which it now seems to us to present; the attitude of our predecessors towards the theoretical interpretation of their observations was rather that described by Plato: "as when men in a dark cavern judge of external objects by the shadows which they cast into the cavern." In the writings of the most clear-sighted of our forerunners we can detect an underlying suspicion of a possibility that, at some time or other, the theory by means of which chemical observations are held together may undergo an entire reconstruction; a very few years ago Ostwald made a determined attempt to treat our science without the aid of the molecular hypothesis, and indeed suggested the desirability of giving the Daltonian atomic theory decent burial.

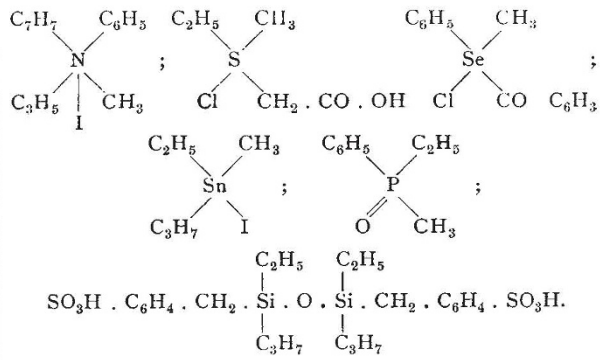
The last ten years or so has seen a change in this attitude. The development of organic chemistry has revealed so complete a correspondence between the indications of the conception of molecular constitution and configuration and the observed facts, and recent work on the existence of the molecule, largely in connection with colloids, with radioactivity, and with crystal structure, is so free from ambiguity, that persistence of doubt seems unreasonable. Probably most chemists are prepared to regard the present doctrine of chemical constitution and configuration as proven; although they may turn a dim vision towards the next great development, they have few misgivings as to the stability of the position which has already been attained.

Let us consider how far the study of organic chemistry has hitherto led us; we may pass over the gigantic achievements of those who in past generations determined constitution and performed syntheses, thus making the subject one of the most perfect examples of scientific classification which exist, and turn to the question of molecular configuration. In 1815 Biot observed that certain liquid organic substances deflect the plane of polarisation of a transmitted ray of light either to the right or to the left; half a century later Pasteur and Paternò pointed the obvious conclusion, namely, that the right- or left-handed deviation thus exerted must be due to a corresponding right- or left-handedness in the configuration of the chemical molecule. A scheme representing such right- or left-handedness, or enantiomorphism, was first enunciated by van 't Hoff and Le Bel upon the basis of the previously established doctrine of chemical constitution; briefly stated, the idea suggesed was that the methane molecule, CH_4, was not to be regarded as extended in a plane in the manner represented by the Frankland-Kekulé constitutional formula, but as built up symmetrically in three-dimensional space. The carbon atom of the methane molecule thus occupies the centre of a regular tetrahedron, of which the apices are replaced by the four hydrogen atoms. A methane derivative, in which one carbon is separately attached to four different univalent atoms or radicles of the type CXYZW, should thus exist in two enantiomorphous configurations, one exhibiting right- and the other left-handedness. The inventors of this daringly mechanistic interpretation of the far less concrete constitutional formulæ were able to interpret immediately a large number of known facts, previously incomprehensible, by means of their extension of the Frankland-Kekulé view of constitution. They showed that every substance then known, which in the liquid state exhibited so-called optical activity, could be

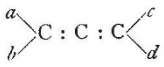
regarded as a derivative of methane in which the methane carbon atom was attached to four different univalent atoms or groups of atoms; a methane carbon atom so associated is termed an asymmetric carbon atom. It is of interest to note that the van 't Hoff-Le Bel deduction resulted from the discussion of the behaviour of organic substances of some molecular complexity; the optically active substances then known were mostly the products of animal or vegetable life, and among them none occurs which contains less than three carbon atoms in the molecule. Lactic acid, $CH_3.CH(OH).CO.OH$, is practically the most simple optically active substance of natural occurrence; it contains twelve atoms in the molecule, and it has only recently been found possible to associate optical activity with a much more simply constituted substance, namely, chloriodomethanesulphonic acid, $CHClI.SO_3H$, the molecule of which contains less than 5 per cent. of carbon and only nine atoms, four more than the minimum number, five, which theoretically can give rise to optical activity.[1]

The working out of the practical consequences of the doctrine of the tetrahedral configuration of the methane carbon atom by von Baeyer, Emil Fischer, and Wislicenus is now a matter of history; the acquisition of masses of experimental data, broad in principle and minute in detail, placed the van 't Hoff-Le Bel hypothesis beyond dispute. The rapid growth of organic chemistry as a classified subject contrasted strongly with that of inorganic chemistry, in which the collection of a great variety of detailed knowledge incapable of far-reaching logical correlation formed the most striking feature; in fact, the extension of the conclusion, proven in the case of carbon compounds, that the Frankland-Kekulé constitutional formulæ must be translated into terms of three-dimensional space, to compounds of elements other than carbon, did not immediately follow the application of the theory to this element. Twenty years ago, indeed, the idea prevailed that carbon compounds differed radically from those of other elements, and we were not prepared to transfer theoretical conclusions from the organic to the inorganic side of our subject. In 1891, however, Le Bel stated that he had found optical activity associated wth asymmetry of a quinquevalent nitrogen atom; although the experimental work upon which this conclusion was founded is now known to be incorrect,[2] the conception thus put forward was important, as suggesting that the notion of space-configuration could not be restricted logically to methane derivatives. When it was proved in 1899 that benzylphenylallylmethylammonium iodide could exist in a right- and left-handed configuration, it became necessary to admit that the spacial arrangement of the parts of a chemical molecule, previously restricted to methane derivatives, must be extended to ammonium salts.[3]

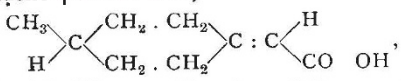
The demonstration that optical activity or enantiomorphism, of molecular configuration is associated not only with the presence of an asymmetric quadrivalent carbon atom, but also with that of a nitrogen atom attached to five different radicles, was the result of an improvement of technique in connection with the study of optical activity; previously the resolution into optically active components of a potentially optically active basic substance had been attempted with the aid of naturally occurring optically active weak acids of the general type of *d*-tartaric acid. The application of the strong *d*- and *l*-bromocamphorsulphonic acids and the *d*- and *l*-camphorsulphonic acids to such purposes rendered possible the isolation of the optically active substances containing no asymmetric atom other than one of quinquevalent nitrogen. The resolution of asymmetric quaternary ammonium salts of the kind indicated was rapidly followed by the preparation of optically active substances in which the enantiomorphism is associated with the presence of an asymmetric sulphur, selenium, tin, phosphorus, or silicon atom; compounds of the following constitutions were thus obtained in optically active modifications:—



In all this work, and amongst all the varied classes of optically active compounds prepared, it was in every instance possible to indicate one particular quadrivalent or quinquevalent atom in the molecule which is separately attached to four or five different atoms or radicles; the enantiomorphism of molecular configuration may be detected, in fact, by the observation that such an asymmetric atom is present. It must, however, be insisted that the observed optical activity is the result of the enantiomorphism of the molecular configuration; the asymmetry of a particular atom is not to be regarded as the cause of the optical activity, but merely as a convenient geometrical sign of molecular enantiomorphism. In 1874 van 't Hoff realised that molecular enantiomorphism and optical activity might conceivably exist without the presence of an asymmetric carbon atom, and suggested that compounds of the type



should be of this kind. Previously this particular case had escaped realisation experimentally, but an example fulfilling similar conditions was described in 1909; in this year the *d*- and *l*-isomerides of 1-methyl-*cyclo*hexylidene-4-acetic acid,

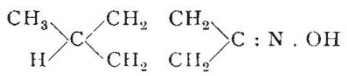


were obtained.[4] The consideration of the constitution of these substances shows no carbon atom which is attached to four different groups, but a study of the solid model representing the molecular configuration built up in accordance with the van 't Hoff-Wislicenus conclusions reveals the enantiomorphism.

It is of some importance to note that the configurations assigned to such optically active substances as have been mentioned above, on the basis of the experimental evidence, are of as symmetrical a character as the conditions permit; the Kekulé formula for methane, CH_4, in which all five atoms lie in the same plane, is not of so highly symmetrical a character as the van 't Hoff-Le Bel configuration in which the four hydrogen atoms are situate at the apices of a

[1] Pope and Read, Trans. Chem. Soc., 1914, 105, 811.
[2] *Ibid.*, 1912, 101, 519.
[3] Pope and Peachey, Trans. Chem. Soc., 1899, 75, 1127.
[4] Perkin, Pope, and Wallach, Trans. Chem. Soc., 1909, 95, 1789; Perkin and Pope, Trans. Chem. Soc., 1911, 99, 1510.

regular tetrahedron described about the carbon atom as centre. Some influence seems to be operative which tends to distribute the component radicles in an unsymmetrical molecule in as symmetrical a manner as possible; recent work indicates, however, that this is not always true. During the past few years Mills and Bain[5] have shown that the synthetic substance of the constitution



can be resolved into optically active modifications. The conclusion is thus forced upon us that the trivalent nitrogen atom in such compounds is not environed in the most symmetrical manner possible by the surrounding components of the molecule; the experimental verification which the conclusions of Hantzsch and Werner, concerning the isomerism of the oximes, thus derive, constitutes the first really direct evidence justifying their acceptance.

Quite recently, and by the application largely of the optically active powerful sulphonic acids derived from camphor, Werner has made another great advance in connection with the subject of optical activity. He has obtained a number of complex compounds of chromium, cobalt, iron, and rhodium in optically active modifications.

The foregoing brief statement probably suffices to indicate the progress which has been made during the last twenty years in demonstrating that the atoms or radicles associated in the chemical molecule do not lie in one plane, but are disposed about certain constituent atoms in three-dimensional space; careful study of the present stage of progress shows that we must attribute to molecular configuration, as determined by modern chemical methods, a very real significance. It can no longer be supposed to possess the purely diagrammatic character which attached to the Frankland-Kekulé constitutional formulæ; it seems to be proved that the men who developed the doctrine of valency were not merely pursuing an empirical mode of classification, capable of various modes of physical interpretation, but were devising the main scheme of a correct mechanical model of the chemical universe.

The development of a branch of science such as that now under discussion is, to a considerable extent, an artistic pursuit; it calls for the exercise of manipulative skill, of a knowledge of materials, and of originality of conception, which probably originate in intuition and empiricism, but must be applied with scientific acumen and logical judgment. For reasons of this kind many gaps occur in our present knowledge of the subject; although so many important conclusions find an unshakable foundation on facts relating to optical activity, we have as yet no clear idea as to why substances of enantiomorphous molecular configuration exhibit optical activity. Great masses of quantitative data referring to optical activity have been accumulated; something has been done towards their correlation by Armstrong, Frankland, Pickard, Lowry, and others, but we still await from the mathematical physicist a theory of optical activity comparable in quantitative completeness to the electro-magnetic theory of light. Until we get such a theory it seems unlikely that much further progress will be made in interpreting quantitative determinations or rotation constants.

That aspect of stereochemistry which has just been so briefly reviewed represents a situation which has been attained during the natural development of organic chemistry by methods which have now become traditional; progress has been made by the application of strictly logical methods of interpretation to masses of experimental data, and each new conclusion has been checked and verified by the accumulation of fresh contributions in the laboratory. The sureness of the methods adopted could not fail to lead to the intrusion of stereochemistry into adjacent fields of scientific activity; bio-chemistry, the study of the chemical processes occurring in living organisms, is already largely dominated by stereochemistry, and the certainty with which stereochemistry has inspired us as to the reality of the molecular constitution of matter is exerting a powerful influence in other branches of natural science. Quite possibly, however, the acquaintance which every chemist possesses of the great progress already made upon one particular set of lines is to some extent an obstacle to his appreciation of new directions in which further great stereochemical advances may be anticipated.

A little reflection will show that the study of the relation between the crystalline form and chemical constitution or configuration of substances in general may confidently be expected to lead to important extensions of our knowledge of the manner in which the atoms are arranged in molecular complexes. The earlier crystallographic work of the nineteenth century led to the conclusion that each substance affects some particular crystalline form, that the regular external crystalline shape is an expression of the internal structure of the crystal, and that a determination of the simpler properties—geometrical, optical, and the like—of a crystalline material constitutes a mode of completely characterising the substance. Later work during the last century demonstrated that the properties of crystalline substances are in entire harmony with a simple assumption as to the manner in which the units or particles of the material are arranged; the assumption is that the arrangement is a geometrically "homogeneous" one, namely, an arrangement in which similar units are uniformly repeated throughout the structure, corresponding points presenting everywhere a similar environment. The assumption of homogeneity of structure imposes a definite limitation upon the kinds of arrangement which are possible in crystals; it leads to the inquiry as to how many types of homogeneous arrangement of points in space are possible, and to the identification of these types with the known classes of crystal symmetry. The final conclusion has been attained that there are 230 geometrically homogeneous modes of distributing units, or points representing material particles, throughout space; these, the so-called 230 homogeneous "point-systems," fall into the thirty-two types of symmetry exhibited by crystalline solids. The solution of the purely geometrical problem here involved was commenced by Frankenheim in 1830, and finally completed by Barlow in 1894; it brings us face to face with the much larger stereochemical problem—that of determining what the units are which become homogeneously arranged in the crystal, why they become so arranged, and in what way a connection can be established between chemical constitution and crystal structure.

Since the conception of homogeneity of structure alone is clearly insufficient for the interpretation of the more advanced problem some further assumption must be made as a foundation for any really comprehensive attempt to collate the quantities of isolated facts bearing upon the subject. Of the many assumptions which have been made in this connection only one, which may now be stated, has as yet proved fruitful in the sense that it serves to correlate large numbers of known experimental facts, and that it

[5] Trans. Chem. Soc., 1910, 97, 1866.

indicates the way to the discovery of fresh facts. The assumption is that each atom in a crystalline structure acts as a centre of operation of two opposing forces: (*a*) a repellent force, attributable to the kinetic energy of the atom, and (*b*) an attractive force, both forces, like gravity, being governed by some inverse distance law. Such an assumption forms an essential part of the classical work of Clerk Maxwell and van der Waals on the kinetic theory of gases and liquids. Its application to solid crystalline substances, where it must be applied in conjunction with the principle of structural homogeneity, was made by Barlow and myself in 1906.

The operation of the assumption just stated is readily visualised by considering the simplest possible case, that, namely, of a crystalline element each molecule of which consists of but one atom and in which all the atoms are similar. Consideration of this kind of case shows that the set of identically similar centres of attracting and opposing forces will be in equilibrium when one particular simple condition is fulfilled; the condition is that, with a given density of packing of the centres, the distance separating nearest centres is a maximum. Two homogeneous arrangements of points fulfil this condition, and these exhibit the symmetry of the cubic and the hexagonal crystalline systems.

Since the nature of the two arrangements of points is not easily realised by mere inspection, the systems must be presented in some alternative form for the purpose of more clearly demonstrating their properties; this is done conveniently by imagining each point in either arrangement to swell as a sphere until contact is made with the neighbouring points. The two arrangements then become those shown in Figs. 1 and 2, and are distinguished as the cubic and the hexagonal closest-packed assemblages of equal spheres; they differ from all other homogeneous arrangements in presenting maximum closeness of packing of the component spheres. The equilibrium condition previously remarked—that, with a given density of distribution of the force centres in space, the distance separating nearest centres is a maximum—is revealed in the assemblages of spheres as the condition that the spheres are arranged with the maximum closeness of packing.

A further step is yet necessary. Each point in the arrangements considered is regarded as the mean centre of an atom of the crystalline element, but the assumption originally made states nothing about the magnitude of the atom itself; it is therefore convenient to regard the whole of the available space as filled by the atoms, without interstices. This is conveniently done by imagining tangent planes drawn at each contact of sphere with sphere, so partitioning the available space into plane-sided polyhedra, each of which may be described as the domain of one component atom. The twelve-sided polyhedra thus derived from the cubic and the hexagonal assemblages represent the solid areas throughout which each atom exercises a predominant influence in establishing the equilibrium arrangement.

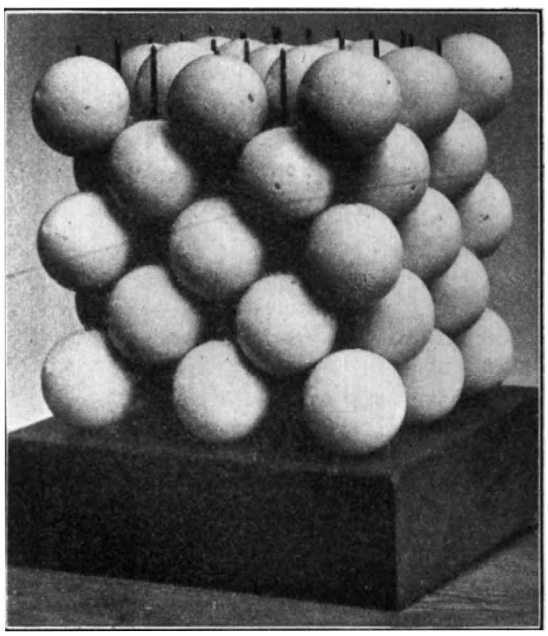


FIG. 1.

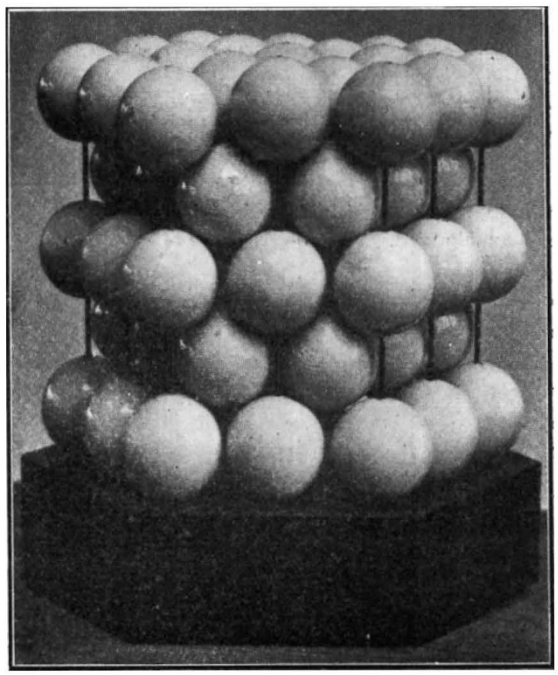


FIG. 2.

(*To be continued.*)

NOTES.

A REUTER telegram states that the New Zealand meeting of the British Association has been cancelled, and that the members will return home after visiting Brisbane and Melbourne.

WE deeply regret to have to record the death, at Torquay, on August 14, of Mr. A. J. Jukes-Browne, F.R.S.

THE death is reported, in his sixty-fourth year, of Dr. Franklin W. Hooper, director since 1889 of the Brooklyn Institute of Arts and Sciences. He had previously been professor of natural science at Adelphi College, Brooklyn. He was the organiser of