ITALIAN OBSERVATIONS OF THE TOTAL SOLAR ECLIPSE (1905).—An interesting illustrated report of the organisation, equipment, and results of the Italian observations of the total eclipse of August last is given by Prof. Ricco in No. 7, vol. xxxv., of the *Memorie della Società degli Spettroscopisti Italiani.*

When first organised, the eclipse party included Prof. Tacchini, and, on his lamented death, the programme proposed had, therefore, to be somewhat modified.

Finally, it was decided that the expedition should make its observations at Alcalà de Chivert, the programme including spectroscopic and direct observations of the prominences, photography of the corona, photographic observations of the spectrum of the eclipsed sun with a slit spectroscope and a prismatic camera, and observations of the solar radiation, the ionisation of the atmosphere, and the polarisation of the coronal radiations.

Although the work was interfered with by clouds, some interesting and valuable observations were made, and are recorded in the article referred to above.

THE SPECTRA OF SUN-SPOTS AND RED STARS.—In a previous paper Profs. Hale and Adams considered the question of the similarity of the spectra of sun-spots and of fourth-type stars, and arrived at the conclusion that the coincidences met with in comparing the spectra suggested the existence of spots, similar to those on the sun, on such stars. Their evidence was confirmed by Sir Norman Lockyer, who further suggested that the temperature conditions of fourth-type stars, taking the absorbing atmospheres as a whole, are about the same as those obtaining in the restricted region of a spot nucleus in the sun's photosphere, both the stellar and the sun-spot atmospheres having a lower temperature than that indicated by the ordinary Fraunhoferic solar spectrum. In a paragraph added to the present paper, reprinted as a Contribution from the Solar Observatory, Mount Wilson (No. 8), Prof. Hale acknowledges the possibility of this suggestion affording the true explanation. Prof. Hale's conclusion was examined by Dr. W. M. Mitchell, who compared his Princeton observations of spot spectra with the star spectra, and was unable to confirm the coincidences of the lines.

In a paper now communicated to No. 5, vol. xxiii., of the *Astrophysical Journal*, Profs. Hale and Adams point out that in the spectra of fourth-type stars the spot lines may be obliterated by bright lines, and that their apparent absence may not, therefore, be accepted as final evidence until better photographs of the fourth-type spectra can be obtained. Such spectra will probably be obtained when the 5-feet reflecting telescope is erected at Mount Wilson and a suitable spectrograph adapted to it. Comparing the spot spectra with the spectrum of α Orionis, the same observers show that the lines of the elements vanadium, titanium, and manganese, which are strongly affected in spot spectra, are also especially strong in this third-type star.

THE BRITISH ASSOCIATION.

SECTION G.

ENGINEERING.

OPENING ADDRESS BY J. A. EWING, LL.D., F.R.S., M.INST.C.E., PRESIDENT OF THE SECTION.

I INTEND to devote this Address to considering in certain aspects the inner structure of metals and the manner in which they yield under strain. It will not be disputed that this is a primary concern of the engineer, who in all his problems of design is confronted by the limitations imposed on him by the strength and elasticity of the materials he employs. It is a leading aim with him to secure lightness and cheapness by giving to the parts such dimensions as are no larger than will secure safety, and hence it is of the first importance to know in each particular case how high a stress may be applied without risk of rupture or of permanent alteration in form. Again, the engineer recognises the merit, for structural purposes, of plasticity as well as strength, and in many of his operations he makes direct use of that property, as in the drawing of wires and tubes or the flanging of plates. He is concerned, too, with the hardening effect that occurs in such processes when work is expended on permanently deforming a metal in the cold state, and also with the restoration to the normal condition of comparative softness which can be brought about by annealing. Nor can he afford to be indifferent to the phenomena of "fatigue" in metals, which manifest themselves when a piece is subjected to repeated alternations or variations of stress—fatigue of strength and fatigue of elasticity, which, like physiological fatigue, admits under some conditions of rest-cure, inasmuch as it tends to disappear with the lapse of time. No apology need be made in selecting for a Presidential Address to Section G a subject that touches so many points of direct practical interest to engineers. It is a subject which has for me the additional attraction of lying in the borderland between engineering and physics—a borderland in which I have often strayed, and still love to stray, and I enter it to-day even at the risk of wandering into regions which, to engineers, may seem a little remote from home, regions where the landscape has, perhaps, a suspicious likeness to that of the country over which the learned men of Section A hold rule.

To engineers, quite as much as to physicists and chemists, we owe in recent years an immense extension of knowledge regarding the structure of metals. This has come about mainly by the intelligent use of the microscope. Take any piece of metal, in the state in which an engineer makes use of it, polish and lightly etch its surface, and examine it under the microscope, and you find that it is a congeries of a multitude of grains, every one of which may be proved to be a crystal. It is true that the boundaries of each grain have none of the characteristics of geometrical regularity which one is apt to look for in a crystal, but the grain is a true crystal for all that. Its boundaries have been determined by the accident of its growth in relation to the simultaneous growth of neighbouring grains—the grains have grown, crystal fashion, until they have met, and the surface of meeting, whatever shape it may happen to take, constitutes the boundary. But within each grain there is the true crystalline characteristic—a regular tactical formation of the little elements of which the crystal is built up. It is as if little fairy children had built the metal by piling brickbats in a nursery. Each child starts wherever it happens to be, placing its first brickbat at random, and then piling the others side by side with the first in geometrical regularity of orientation until the pile, or the branches it shoots out, meets the advancing pile of a neighbour; and so the structure goes on, until the whole space is entirely filled by a solid mass containing as many grains as there have been nuclei from which the growth began.

We now know that this process of crystal growth occurs not only in the solidification of a metal from the liquid state, but in many cases during cooling through a "critical" temperature when the metal is already solid. We know also that the process may in certain conditions go on slowly at very moderate temperatures. We know also that the process of annealing is essentially the raising of the metal to a temperature at which recrystallisation may take place, though the metal remains solid while this internal rearrangement of its particles goes on. Whether crystallisation occurs in solidifying from the liquid or during the cooling of an already solid piece it results in the formation of an aggregate of grains, each one of which is a true crystal. Their size may be large or small—in general, quick cooling means that crystallisation starts from many nuclei, and the resulting grains are consequently small; with very slow cooling you get a gross structure made up of grains of a much larger size.

For simplicity of statement I shall ask you in what follows to confine your attention to simple metals, omitting any reference to alloys. Alloys present many complexities, into which we need not at present enter. With simple metals every crystalline grain is made of the same substance: the elementary brickbats are all exactly alike, though there may be the widest variation from grain to grain as regards the form of the grain, and also as regards the direction in which the elementary brickbats are piled.

In any one grain they are piled with perfect regularity, all facing one way, like a regiment of perfectly similar soldiers formed up in rows, where each man is equidistant from his neighbours, before and behind, as well as to right and to left. Or perhaps I might compare them to the well-drilled flowers of an early Victorian wall-paper.

It was shown by Mr. Rosenhain and myself [1] that when a piece of metal is strained beyond its limit of elasticity, so that permanent set is produced, the yielding takes place by means of slips between one and another portion of each crystal grain. A part of each crystal slides over another part of the same crystal, as you might slide the cards in a pack. It is as if all the soldiers to one side of a given line were to take a step forward, those on the other side remaining as they were, or as if all the men in the front rows took a step to the left, while those in the rows behind kept their places. In other words, the plasticity which a metal possesses is due to the possibility of shear on certain planes in the crystal that are called "cleavage" or "gliding" planes. Plastic yielding is due to the occurrence of this shear; it may take place in three or more directions in a single grain, corresponding to the various possible planes of cleavage, and in each direction it may happen on few or many parallel planes, according to the extent of the strain to which the piece is subjected. Examine under the microscope the polished surface of a piece of metal which has been somewhat severely strained after polishing, and you find that the occurrence of this shear or slip is manifested on the polished surface by the appearance of little steps, which show themselves as lines or narrow bands when looked at from above. To these we gave the name of slip-bands. Just as the piece of metal is an aggregate of crystal grains, the change of shape which is imposed upon it in straining is an aggregate effect of the multitude of little slips which occur in the grains of which it is made up. Each grain, of course, alters its form in the process.

Speaking broadly, this distortion of the form of any one grain by means of slips leaves it still a crystal. If part of the group of brickbats moves forward, keeping parallel to themselves and to the others, the formation remains regular, except that a step is formed on the outermost rows; the orientation of the elements continues the same throughout. Considerations which I shall mention presently lead to some qualification of this statement. I now see reason to believe that in the process of slip there is a disturbance of the elementary portions or brickbats adjoining the plane of slip, which may alter their setting, and thereby introduce to a small extent some local departure from the perfectly homogeneous orientation which is the characteristic of the true crystal. In very severe straining there may even be a wide departure from true crystalline character. We shall recur to this later; but meanwhile it will suffice to say that substantially the slip which is involved in a plastic strain of moderate amount is a bodily translation, parallel to themselves, of part of the group of elementary brickbats or molecules which build up the grain. If a crystal the form of which has been altered, even largely, by such straining is cut and polished and etched it appears, under the microscope, to be to all intents and purposes as regular in the tactical grouping of its elements as any other crystal.

Further, in the process of straining we have, first, an elastic stage, extending through very small movements, in which there is no dissipation of energy and no permanent set. When this is exceeded, the slip occurs suddenly; the work done in straining is dissipated; if the straining force is removed a strain persists, forming a permanent "set"; if it continues to act it goes on (within certain limits) producing augmented strain. In general a large amount of strain may take place without the cohesion between the gliding surfaces being destroyed. Immediately after the strain has occurred there is marked fatigue, showing itself in a loss of perfect elasticity; but this will disappear with the lapse of time, and the piece will then be harder than at first. If, on the other hand, a process of alternate straining back and forth be many times repeated, the piece breaks.

[1] Ewing and Rosenhain, "The Crystalline Structure of Metals," Bakerian Lecture, *Phil. Trans. Roy. Soc.*, vol. cxciii. A, 1899.

These are now familiar facts. Can we attempt to explain them on the basis of a molecular theory which will at the same time offer a clue to the process of crystal-building as we find it in metals? I venture to make this Address the occasion of inviting attention to some more or less speculative considerations which may be held to go some little way towards furnishing the material for such an explanation.

At the Leeds Meeting of this Association, in 1890, it was my privilege to bring forward certain contributions to the molecular theory of magnetism, and to show a model which demonstrated that the rather complex phenomena of magnetisation were explainable on the very simple assumption that the magnetic molecules are constrained by no other forces than those which they mutually exert on one another in consequence of their polarities.[1] From this were found to result all the chief phenomena of permeability and magnetic hysteresis. Let us attempt to-day to apply considerations of a similar character to another group of physical facts, namely, those that are associated with the crystalline structure of metals and with the manner of their yielding under strain. Just as in dealing with magnetic phenomena, I take as starting-point the idea that the stability of the structure is due to mutual forces exerted on one another by its elementary parts or molecules, and that the clue to the phenomena is to be sought in the play of these mutual forces when displacement of the molecules occurs.

Iron and most of the useful metals crystallise in the cubic system; for simplicity we may limit what has to be said to them. Imagine a molecule possessing polarity equally in three directions, defined by rectangular axes. We need not for the present purpose inquire to what the polarity along the axes is due; it will suffice to assume that the molecule has six poles, three positive and three negative, and that these repel the like and attract the unlike poles of other molecules. We may make a model by using three magnetised rods fixed at right angles to one another at their middle points. I imagine, further, that the molecule has an envelope in the shape of a sphere, which touches the spherical envelopes of its neighbours, and assume that these spheres may turn on one another without friction.[2]

Think now of the process of crystal-building with a supply of such spherical molecules for brickbats. Starting with one molecule, let a second be brought up to it and allowed to take up its place under the action of the polar forces. It will have a position of stability when a positive pole in molecule A touches (or lies in juxtaposition to) a negative pole in molecule B, with the corresponding axes in line, and when the further condition is satisfied that the axes in molecule B the poles of which are not touched by A are stably situated with respect to the field of force exerted by the poles of A.

In other words, we have this formation:—

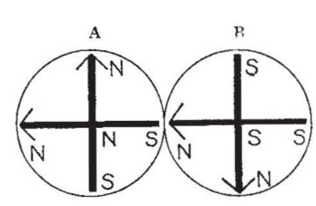

FIG. 1.

For convenience of representation in the diagram the poles are distinguished by the letters N. and S., but it must not be assumed that the polarities with which we are here concerned have anything to do with magnetism.

Suppose, now, that the crystal is built up by the arrival of other molecules, each of which in its turn assumes the position of maximum stability consistent with formation in

[1] "Contributions to the Molecular Theory of Induced Magnetism," *Proc. Roy. Soc.*, vol. xlviii., June 19, 1890, or *Phil. Mag.*, September, 1890.

[2] Or, let the envelope be a shell of any form, inside of which the axes of polarity are free to turn as a rigid system.

cubic or normal piling. The group in that case takes an arrangement which is essentially a repetition of this quartette :—

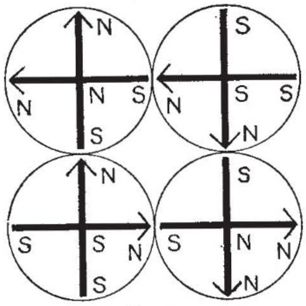

FIG. 2.

Along each row the polarity preserves the same direction, but the polarity of each row is opposite to that of each contiguous parallel row. This description applies equally to all three axes. The whole group (Fig. 3) consists of the quartettes of Fig. 2 piled alongside of and also on top of one another. In this way we arrive at what I take to be the simplest possible type of cubic crystal.

In this grouping each molecule has the alignment giving maximum stability, and it seems fair to assume that it will take that alignment when the crystal grain is formed under conditions of complete freedom, as in solidifying from the liquid state. As a rule, the actual process of crystal-building goes on dendritically; branches shoot out, and from them other branches proceed at right angles, leaving interstices to be filled in later. We have, therefore, to conceive of the molecules as piling themselves preferably in rows rather than in blocks, though ultimately the block form is arrived at. In this position of maximum stability each molecule has its six poles touching poles of contrary name.

Now comes a point of particular importance. Imagine two neighbouring molecules in the same block to be turned round, each through one right angle, in opposite senses. They will now each have five poles touching five poles of contrary name, but the sixth pole will touch a pole of the same name as itself. They are still stably situated, but much less stably than in the original configuration, and they will revert to that configuration if set swinging through an angle sufficient to exceed the limited range within which they are stable in the new position.

Similarly we may imagine a group of three, four, or more molecules, each to be turned through a right angle, thereby constituting a small group with more or less stability, but always with less than would be found if the normal configuration had been preserved. The little group in question may be made up of molecules in a row, or it

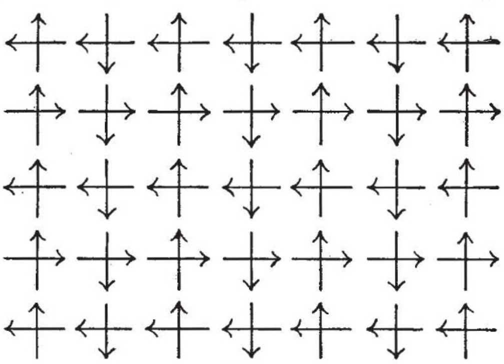

FIG. 3.

may be a quartette or block, or take such a form as a T or L. A sufficient disturbance tends to resolve it into agreement with the normal tactics of the molecules which build up the rest of the grain.

It is conjecturally possible that small groups of this kind, possessing little stability, may be formed during the process of crystallisation, so that here and there in the grain we may have a tiny patch of dissenters keeping one another in countenance, but out of complete harmony with their environment.

If this happens at all during crystallisation, it would seem less likely to happen in free crystallisation from a liquid state than in the more constrained process that occurs when a metal already in the solid state recrystallises at a temperature far below its melting-point. Though rare or absent in the first case, it might occur frequently in the second. There are differences in the appearance of crystal grains under the microscope in metal as cast and in metal as recrystallised in the solid state, of which this may be the explanation. It may also explain a difference pointed out by Rosenhain,[1] that the slip lines in cast metal are straight and regular, whereas in wrought iron and other metals which have recrystallised in the solid they rarely take a straight course across the crystal, but proceed in jagged, irregular steps. These may be due to the presence here and there of small planes of weakness, resulting from the existence of what I have called dissenting groups. Again, these groups, possessing, as they do, less stability than their normal neighbours, may be conjectured to differ from the normal parts of the grain in respect of electrolytic quality, and to be more readily attached by an etching reagent. Hence, perhaps, the conspicuous isolated geometrical pits that appear on etching a polished surface of wrought iron.

It will help in making clear these points, and others that are to follow, if we study the action of a model formed by grouping a number of polarised "molecules" in one plane, supporting them on fixed centres, about which they are free to turn. In the model before you the centres are uniformly spaced in rectangular rows, and the "molecules" are + shaped pieces of hardened steel, strongly magnetised along each of the crossed axes, each having, therefore, two north poles and two south poles. The third axis is omitted in the model, the movement to be studied with the help of the model being movement in one plane. On placing these "molecules" on their centres they readily take up the position already indicated in Fig. 3. Each one within the group has its four poles in close proximity to four poles of contrary name, and is, therefore, highly stable. If disturbed by being turned through a small angle, and let go, it swings back, transmitting a wave of vibration through the group, which is reflected from the edges, and is finally damped out in the model by pivot friction and air friction. We may assume some damping action (say by the induction of eddy-currents) in the actual solid, of which the model may be taken as a very crude representation.

By turning two molecules carefully round together, each through one right angle in opposite senses, we set up a dissenting pair, the equilibrium of which has feeble stability. A slight displacement, such as might be produced by the transmission of a vibrational wave, breaks them up, and they swing back to the normal configuration, giving out energy, which is taken up by the rest and is ultimately dissipated. By making the dissenting coterie consist of three or more we can give it additional strength.

An example is shown in Fig. 4, where the three molecules marked a, b, and c are turned round in this way.

Notice that the normal molecule d, adjoining a line of such dissenters, is in a peculiar position. His neighbours present to him three N. poles and one S. pole. He has the choice of conforming to the majority, or of throwing in his lot with the dissenters; and he has a third possible position of equilibrium (very feeble equilibrium) which is reached when his two S. poles are turned until the one neighbouring south pole faces just between them. I have laboured these points a little because they seem important when we come to speak of the effects of strain.

Consider now the straining action, which we may imitate in the model by sliding one part of the group past the other part. For this purpose the centres are cemented to two glass plates which can slide parallel to one of the axes.

[1] Rosenhain, "The Plastic Yielding of Iron and Steel," *Jour. Iron and Steel Institute*, No. 1 for 1904, p. 335.

At first, when the displacement by sliding is exceedingly small, the strain is a purely elastic one. The molecules adjacent to the plane of sliding pull one another round a little, but without breaking bonds, and if in this stage the strain is removed, by letting the plate slide back to its original position, there is no dissipation of energy. The work done in displacing the molecules is recovered in the return movement. We have here a representation of what happens between each pair of adjoining rows in the elastic straining of a metal. So far the action is within the limit of elasticity; it leaves no permanent effect: it is completely reversible.

But now let the process of straining be carried further. The opposing molecules try to preserve their rows intact, but a stage is reached when their resistance is overcome; the bonds are broken, and they swing back, unable to exert further opposition to the slip. The limit of elasticity has now been passed. Energy is dissipated; set has been produced; the action is now no longer reversible. The model shows well the general disturbance that is set up in molecules adjoining the plane of slip, which we may take to account for the work that is expended in a metal in producing plastic strain.

Moreover, when the slip on any plane stops and the molecules settle down again, the chances are much against their all taking up the normal orientation which they had before the disturbance. What I have called dissenting groups or unstable coteries are formed as a result of the disturbance. Here and there like poles are found in juxtaposition. Viewed as a whole, the molecular constitution of the metal in the region adjacent to the plane of slip

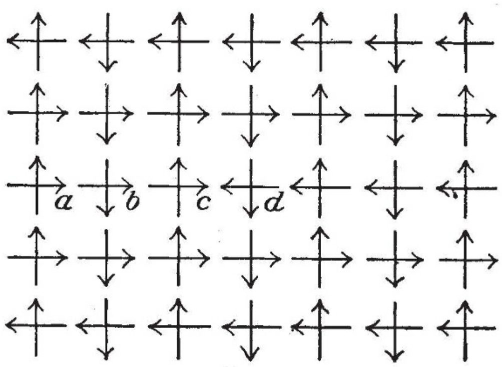

FIG. 4.

is now uncertain and patchy. It includes parts the stability of which is much less than normal. Individual molecules or small groups in it are very feebly stable; a touch would make them tumble into positions of greater stability.

Observe how all this agrees with what we know about the nature of plastic strain through experiments on iron or other metals. Its beginning is characteristically jerky. Once the critical force is reached, which is enough to start it, there is a big yield, which will not be stopped even by reducing the amount of the straining force.

Again, we know that there is a slow creeping action that continues after the straining force has done its main work. I ascribe this to the gradual breaking up of the more unstable groups which have been formed during the subsidence of disturbance in the earlier stage of the slip.

Further, we know that overstrained iron is very imperfectly elastic until it has had a long rest, or until it has been raised for a short time to a temperature such as that of boiling water.[1] This is to be expected when we recognise the presence of unstable individuals or groups resulting from the overstrain. When the elasticity of the overstrained piece is tested by removing and reapplying the load, some of these tumble into new positions, making inversible movements, which dissipate energy and produce hysteresis in the relation of the strain to the stress although the strain is quasi-elastic. At the ordinary temperature these unstable groups are gradually becoming resolved, no doubt

[1] J. Muir, "On the Recovery of Iron from Overstrain," *Phil. Trans.*, vol. cxciii. A, 1900.

through the action of the molecular movements that are associated with heat, and hence the slow progressive recovery of perfect, or nearly perfect, elasticity shown by the experiments of Muir. Let the temperature be raised and they disappear much more quickly; in warm surroundings the rest-cure for elastic fatigue does not need to be nearly so long.

Rosenhain[1] has recently shown that after the slip-bands on the surface of an overstrained specimen have been obliterated by polishing, traces of them will reappear on etching if only a short interval of time is allowed to lapse since the overstraining; but if time is given for complete recovery no traces are found. This is in remarkable agreement with the view now put forward, that the layers contiguous to the surface of slip contain for a time comparatively unstable groups. They are consequently different from the normal metal until the unstable groups are resolved, and the temporary difference manifests itself on etching, provided that is done while the difference still exists.

From the engineer's point of view a much more important matter than this fatigue of elasticity is the fatigue of strength that causes fracture when a straining action is very frequently repeated. Experiments which I made with Mr. Humfrey[2] showed that this action begins with nothing more or less than slight slip on surfaces where the strain is locally sufficient to exceed the limit of elasticity. An alternating stress, which makes the surfaces slip backwards and forwards many thousands, or it may be millions of times alternately, produces an effect which is seen on the polished surface as a development of the slip lines into actual cracks, and this soon leads to rupture.

We have, therefore, to look for an effect equivalent to an interruption of continuity across part or the whole of a surface of slip, an effect progressive in its character, becoming important after a few rubbings to and fro if the movement is violent, but only after very many rubbings if the movement is slight.

That there is a progressive action which spreads more or less into the substance of the grain on each side of the original surface of slip was clearly seen in the experiments referred to. It was found that a slip-band visible on the polished surface of the piece broadened out from a sharply defined line into a comparatively wide band with hazy edges, and this was traced to an actual heaping up of material on each side of the step which constituted the original line.

I think this suggests that under alternating stresses which cause repeated backward and forward slips, these do not occur strictly on the same surface in the successive repetitions, and hence the disturbance spreads to some extent laterally. It may be conjectured that slip on any surface leaves a more or less defective alignment of the molecular centres; that is to say, the rows on one side of the plane of slip cease to lie strictly in line with those on the other side. If this occurs over neighbouring surfaces, as a result of slips or a number of parallel planes very close together, the metal throughout the affected region loses its strictly crystalline character, and with it loses the cohesion which is due to strict alignment.

Mr. G. T. Beilby, in a very suggestive paper,[3] has advanced grounds for believing that portions of a metal may pass from a crystalline to an amorphous formation under the mechanical influence of severe strain, as in the hammering of gold leaf or the drawing of wire, and that this occurs in the polishing of a metallic surface, and also in the internal rubbing which takes place at a surface of slip within the grain. In both cases he suggests the formation of an altered layer. When a polished metal surface is etched, the altered layer is dissolved away, and the normal structure below it is revealed.

Without accepting all Mr. Beilby's conclusions, I think the idea of an altered and more or less amorphous layer is supported by the considerations I am now putting forward. We have assumed that in normal crystallisation the intermolecular forces lead to a normal piling, in which

[1] *Journ. Iron and Steel Institute*, 1906.
[2] Ewing and Humfrey, "The Fracture of Metals under Repeated Alternations of Stress," *Phil. Trans.*, vol. cc. A, 1902.
[3] Beilby, "The Hard and Soft States in Metals," *Phil. Mag.*, August, 1904.

each molecule touches six neighbours. But it may be conjectured that some of them may take up pyramidal piling (touching twelve others) under the compulsion of strong forces such forces, for example, as act on the superficial molecules of a surface that is being polished.

If this also occurs at a surface of slip, it gives us a clue to several known facts. It at least assists in explaining the familiar result that metal is hardened by straining in the sense of being made less plastic. Again, it accounts for the general increase of density which is found to take place in such an operation as wire drawing. Further, if a local increase of density occurs in the interior of a grain through piling of some molecules in the closer manner where repeated slips are going on, the concentration of material at one place requires it to be taken from another; in other words, the closer piling tends to produce a gap or crack in the neighbourhood where it occurs. This is consistent with what we know of the development of cracks through repeated alternations of strain.

Recourse to the model shows that with pyramidal piling the polar axes point in so random a manner that the aggregate may fairly be called amorphous. To illustrate this a group is shown with centres fixed at the corners of equilateral triangles.

It is obvious that any pyramidal piling at a surface of slip tends to bar further slip at that particular surface. Hence not only the augmented hardness due to strain, but the tendency in repeated alternations to lateral spreading of the region on which slip occurs. The hardness due to straining is, of course, removed when we raise the metal to such a temperature that complete recrystallisation occurs, normal piling being then restored in the new grains.

Taking a previously unstrained piece, it is clear that the facility with which slip will occur at any particular surface of slip in any particular grain depends not only on the nature of the metal and on the orientation of the surface in question to the direction of the stress, but also on the amount of support the grain receives from its neighbours in resisting slip there. In other words, for a given orientation of surface the resistance to slip may be said to consist of two parts; one is inherent in the surface itself, and the other is derived from the position of the grain with reference to other grains.

To make this point clear, think of a grain (under stress) in which there is a gliding surface oriented in the most favourable direction for slipping. Slip on this surface can take place only when its yielding compels the neighbours (which are also under stress) to yield with it, and the surfaces in these on which slip is compelled to occur are, on the whole, less favourably situated. Hence the original grain cannot yield until the stress is considerably in excess of that which would suffice to make it yield if it stood alone, or had neighbours equally favourably inclined.

Apply this consideration to the case of steel, where there are two classes of grains: the ferrite, which is simply iron, and the pearlite, which is a harder structure. Slip on any ferrite grain is resisted partly by the strength of the surface itself, and partly by the impossibility of its yielding without forcing slip to take place on neighbouring (harder) grains. Now suppose the structure is a very gross one, such as Mr. Stead has shown may be found in steel that is seriously overheated. On the large grains of ferrite in overheated steel the resistance to slip will be but little greater than it would be in iron, and, consequently, under an alternating stress fatigue of strength, leading to rupture, may be produced by a very moderate amount of load. Mr. Stead[1] has shown how the effects of overheating can be removed by the simple expedient of raising the steel to a temperature sufficient to cause recrystallisation—a homœopathic remedy that transforms the gross structure of the overheated metal into an ordinarily fine structure, where no ferrite grain can yield without compelling the yielding of many pearlite grains. Hence we find, as Rogers[2] has demonstrated by experiment, that steel cured by reheating from the grossness of structure previously produced by overheating, has an immensely increased power to resist the deteriorating effects of often repeated stress.

I trust you will not feel I have abused the license of the Chair in presenting contributions to molecular theory that are for the most part in the nature of speculative suggestions, thrown out in the hope that they may some time lead to fuller and more definite knowledge. Remote as they may seem to be from the concerns of the workaday engineer, they relate to the matter which it is his business to handle, and to the *rationale* of properties, without which that matter would be useless to serve him. We have attempted to penetrate into its very heart and substance in order the better to comprehend the qualities and functions on which the practical work of engineering relies. The man whose daily business leads him through familiar tracks in a forest does well to stray from time to time into the shady depths that lie on either hand. The eyes of his imagination will be opened. He will at least learn his own limitations, and, if he is fortunate, he may gain some clearing on a hilltop which commands a wider view than he has ever had before.

1 See especially a paper by J. E. Stead and A. W. Richards on "The Restoration of Dangerously Crystalline Steel by Heat Treatment," *Journ. of the Iron and Steel Inst.*, No. 2, 1903.

2 F. Rogers, "Heat Treatment and Fatigue of Steel," *Journ. Iron. and Steel Inst.*, No. 1, 1905.

SECTION I.

PHYSIOLOGY.

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"THE investigators who are now working with such earnestness in all parts of the world for the advance of physiology have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life and of the self-acting machinery by which they are regulated for the general good of the organism."[1]

In this admirable and concise manner the late Sir John Burdon-Sanderson described the aims and methods of physiology. The words were spoken in 1881, when the British Association last met in this historic city. At that time the subjects of Anatomy and Physiology formed a subsection of the Section of Biology, and it was presided over by this distinguished man, whose recent death has deprived not only physiology but natural science of one of its most honoured leaders. His continuous work, extending over a period of fifty years, was remarkable from many points of view, but in none more than the extent of its scope. Sanitary science, hygiene, practical medicine, botany, pathology, and physiology have all been illuminated and extended by his researches. His claim for being included among the great names in English science does not rest merely upon his acknowledged eminence as an original and exact investigator, but also upon the influence which, for four decades, he exerted upon other workers in medical science, endowing their investigations with purpose and materially helping to give English physiology and pathology their proper scientific status. Many circumstances contributed to make this influence widely felt; among these were the peculiar charm of his manner, his striking and commanding personality, the genuine enthusiasm with which he followed the work of others, the devotion with which he advocated the use of experimental methods, his scientific achievements, and his extensive knowledge. All these qualities of mind and character marked him as one of those great masters who inspire the work and mould the thought of a generation. It is in tribute to his memory that, as one of his pupils and his successor in the Oxford Chair of Physiology, I utilise this occasion for recalling such fruitful features of his scientific conceptions as are expressed in the felicitous phrase which I have quoted.

Probably the most important of the many services which Burdon-Sanderson rendered to English medical science was that of helping to direct physiological and pathological inquiry towards its proper goal. It will be admitted by all who knew him intimately that among his most characteristic scientific qualifications were the insight with which he realised the essence of a physiological problem,

1 Address to the Subsection of Anatomy and Physiology, by J. Burdon-Sanderson, *British Association Report*, York, 1881.

and the tenacity with which he kept this essential aspect in view. The faculty which enables the mind to review the varied aspects of complex phenomena and to determine which of these are mere incidents, or external trappings, and which constitute the core of the subject, is one which every scientific worker must possess in a higher or lower degree; it may, indeed, be confidently asserted that scientific training is successful only in so far as it develops a nice and just discrimination of this character. Many attain this capacity after several years of labour and effort; but in the case of rare and gifted individuals its possession comes so early as to seem almost an intuitive endowment. In 1849, during his student days at Edinburgh, Burdon-Sanderson showed by the character of his earliest scientific work that he viewed the proper aim of physiological inquiry as essentially the study of processes. At the present time it may appear superfluous to dwell upon the importance of this standpoint, but fifty-seven years ago this aspect of the subject was rarely, in this country, a stimulating influence in physiological work, whilst, as regards pathology, the point of view taken by Burdon-Sanderson was, even in 1860, probably unique.

The obvious fact that living processes occur in connection with certain definite structural forms transferred attention from the end to one of the means, and thus education and research in physiology and pathology were almost entirely confined to the elucidation of that structural framework in which the essential processes were now displayed and now concealed. Improved methods of microscopic technique revealed the complexity of this structure, and minute anatomy absorbed the interest of the few physiologists and pathologists who prosecuted researches in this country. Even when attention was directed to the living processes, it was with an unconscious anatomical bias, and detailed descriptions of structural framework were advanced as affording a sufficient scientific explanation of the character of the subtle processes which played within the structure. Yet upon the Continent the great physiologists of that time had long realised that physiological study must ascertain the characters of these processes, and that research conducted along experimental lines could alone advance scientific physiology as distinct from scientific anatomy. In 1852 Burdon-Sanderson went from Edinburgh to Paris to study the methods used in physics and chemistry. Whilst there he came under the inspiring influence of one of these great Continental physiologists, Claude Bernard, and his views as to the proper end of physiological inquiry received from this master ample confirmation. The sentence which I have quoted from the York address sets forth with scientific precision his enlarged conception of living phenomena, for whilst it asserts that the characteristics of processes form the true aim of all physiological investigation, it defines the particular processes which should be investigated as chemical and physical, and it particularises two further aspects of these, the machinery for their coordination described as self-acting, that is automatic, and the *raison d'être* of their occurrence, which is said to be the welfare of the whole organism. All these various aspects are strikingly exemplified in the progress of physiology in this country and in the researches now being carried on both at home and abroad; their consideration may thus be not inappropriate in a general address such as it is my privilege to deliver to-day.

At the outset it is desirable to refer to certain wide issues which are involved in the statement that the business of the physiologist is "to acquire an exact knowledge of the chemical and physical processes of animal life." The limitation of physiology to ascertainable characters of a chemical and physical type does not commend itself to certain physiologists, physicists and chemists, who have revived under the term "neo-vitalism" the vitalistic conceptions of older writers. They deny that physiological phenomena can ever be adequately described in terms of physics and chemistry, even if these terms are in the future greatly enlarged in consequence of scientific progress. It is undoubted that there are many aspects of living phenomena which in the existing state of our knowledge defy exact expression in accordance with chemical and physical conceptions; but the issues raised have a deeper significance than the mere assertion of present ignorance, for those who adopt "neo-vitalism" are prepared to state not only that certain physiological phenomena are, from the chemical and physical point of view, inexplicable to-day, but that from the nature of things they must for ever remain so. This attitude implies that it is a hopeless business for the physiologist to try by the use of more appropriate methods to remove existing discrepancies between living and non-living phenomena, and this is accentuated by the use of a peculiar nomenclature which, in attributing certain phenomena to vital directive forces, leaves them cloaked with a barren and, from the investigator's point of view, a forbidding qualification.

It is of course possible in describing phenomena to employ a new and special terminology, but since many aspects of the phenomena of living processes can be described in accordance with physical and chemical conceptions, the creation of a vitalistic nomenclature duplicates our terminology. A double terminology is always embarrassing, but it becomes obstructive when it is of such diversity that description in the one can never in any circumstances bear any scientific relation to that in the other. In this connection it is somewhat significant that the one kind, namely vitalistic, is abandoned as soon as the observed phenomena to which it referred have been found to be capable of expression in terms of the other. The reason for this abandonment raises questions of principle, which appear to me to render it impossible for a scientific physiologist seriously to employ vitalistic nomenclature in describing physiological phenomena. Science is not the mere catalogue of a number of observed phenomena; such a miscellaneous encyclopædia may constitute what many people would describe as knowledge; but science is more than this. It is the intellectual arrangement of recognised phenomena in a certain orderly array, and the recognition of any phenomenon is only the first step towards the achievement of this end. The potent element in science is an intellectual one essentially connected with mental grouping along one particular line, that which tends to satisfy our craving for causative explanation. Hence it involves the intellectual recognition of widespread characteristics, so general in their distribution that they are termed fundamental. The most fundamental of such characteristics are those which possess the widest intellectual sphere, and in natural science these are the broad conceptions of matter and motion which form the essential basis of both chemistry and physics. If this grouping is, in regard to any phenomenon, at present impracticable, then this subject-matter cannot be justly regarded as forming a part of natural science, though it might be considered as natural knowledge, and in so far as this is the case in physiology it appears to me to be a confession of present scientific ignorance. If, however, it is boldly asserted that the nature of any phenomenon is such that it can never by any possibility be brought into accord with the broad conceptions which I have indicated, then I fail to understand how it can claim to bear any relation to natural science, since, *ex hypothesi*, it can never take its proper place in the causative chain which man forges as a limited but intelligible explanation of the world in which he lives. Only in so far as physiological phenomena are capable of this particular intellectual treatment and take part in this intellectual construction can we hope to obtain, however dimly, a knowledge of permanent backgrounds among the shifting scenes of the living stage, and thus, by gradually introducing order amidst seeming confusion, claim that gift of prevision which has long been enjoyed by other branches of natural science.

Neo-vitalism, like its parent vitalism, is fostered by the imperfect and prejudiced view which man is prone to take in regard to his own material existence. This existence is, for him, the most momentous of all problems, and it is therefore not surprising that he should assume that in physiology, pathology, and, to a lesser degree, in biology, events are dealt with of a peculiarly mystic character, since many of these events form the basis of his sensory experience and occur in a material which he regards with a special proprietary interest. He is reluctant to believe that those phenomena which constitute the material part of his existence can be intellectually regarded as processes of a physicochemical type, differing only in complexity from those exhibited in the non-living world, and impelled by this reluctance he fabricates for them, out of his own conceit, a special and exclusive realm. The logical pressure of physical and chemical conceptions forbids the postulation, by either the public or the neo-vitalist, of such an incongruous entity as a vital chemical element capable of blending with the familiar chemical elements recognised in the material world; yet the physiological processes of life are in popular estimation still held to be due to peculiar forces blending with those of the material world, but so essentially different that they can only be described as "vital." The neo-vitalistic school of men of science, without adopting this popular view in its entirety, retains the same term for such physiological characteristics of cell processes as, with our present limited knowledge and with our present inadequate methods of investigation, seem to be in disagreement with present chemical and physical conceptions. This disagreement is accentuated by the assumption of directive vital forces, and since these cannot be ranged alongside those of chemistry and physics, transcendental phenomena may be always expected to occur the orderly array of which as part of natural science is not merely a futile but on *a priori* grounds an absolutely impossible task. In order to justify this description as representing the views of some neo-vitalists, I will quote a few sentences from the presidential address delivered in 1898 by Prof. Japp in the Chemical Section of this Association. This address dealt with the formation of the optically active substances found in vegetable and animal tissues or their extracts. It asserts that "the absolute origin of compounds of one-sided symmetry to be found in the living world is a mystery as profound as the origin of life itself." In regard to this it may be remarked that the absolute origin of anything, living or non-living, is a mystery which science does not attempt to solve, relative not absolute causation being the object of scientific grouping, hence this assertion does not necessarily imply any fundamental distinction between the two classes of phenomena. But there is more than appears upon the surface, for the whole argument leads up to the sweeping statement that "no fortuitous concourse of atoms, even with all eternity for them to clash and combine in, could compass this feat of the formation of the first optically active organic compound." It is thus inferred that because the manner of such formation cannot be accounted for in the present condition of scientific knowledge, its scientific causation is from the nature of things unknowable. However, although unknowable in the strictly scientific sense, the intellectual craving for causative explanation of some sort urges Prof. Japp to say, "I see no escape from the conclusion that at the moment when life arose a directive force came into play." There is here introduced a grandiose term for life which is viewed as involving directive forces; the term, however, adds nothing to our physiological knowledge, is not in itself explanatory, and not only offers no new method of physiological investigation, but brands as useless all the methods derived from physics and chemistry, past, present, and future. In a recent work Prof. Moore has attempted to set forth a conception which shall be vitalistic in essence, and yet not so completely out of touch with the principles of natural science.[1] He regards living cells as transformers of energy and thus leaves them absolutely dependent upon its receipt; the transformed mode which is achieved by the cells is, however, one which cannot be interpreted in terms of the familiar modes presented in the non-living world. He terms the transformed mode "biotic energy," and the distinction between this and "vital directive force" appears to be its absolute dependence upon the other modes for its appearance. It thus does not run counter to the law of the conservation of energy, and warrants, in the opinion of some, the confident expectation that it will be found capable of precise scientific expression. I confess that I am unable to share this confidence. The introduction of the conception entails the same double terminology to which I have referred, and I feel convinced that the assumption, in the case of any given physiological phenomena, of biotic energy as a causative explanation, would be immediately abandoned if the phenomena were subsequently found to be explicable on physical and chemical conceptions. Biotic energy appears to me as only an intellectual compromise, an abortive attempt to clothe the naked form of vitalism in a decent scientific dress; but, although partially clothed, it offers, like neo-vitalism, no new method for physiological investigation, and must, in consequence, remain barren, never contributing towards physiological achievement. To what extent its adoption may be an intellectual solace is a question which does not fall within the scope of physiology. Certain physiological phenomena are especially brought forward as necessitating the assumption of vitalistic or biotic conceptions; among these are the phenomena of nervous activities, the formation and activities of enzymes, and the passage of substances through living membranes. The question of the nervous activities will be dealt with later; but as regards the diffusion of gases or substances in solution through cellular membranes a few general considerations may be advanced now.[1] The passage of substances into and through non-living membranes is modified in regard to both the velocity and the selective character of the passage by a large number of factors, among which are nature of substance, pressure, osmotic index, temperature, and the structural, electrolytic, and chemical characters of the membrane. Tissue membranes, whether animal or vegetable, possess a complicated particulate structure, and it is obvious that experiments must be carried out extensively on dead tissue membranes in order to determine how far the general particulate arrangement may modify the rate and character of the passage. In this respect our present information is not sufficiently extensive to warrant any definite general statement, and such experimental evidence as exists opens up difficult problems in molecular physics which still await solution; moreover, the presence of electrolytes, by assisting adsorption, appears to modify the apparent rate and character of the total passage, and further experiments are necessary on this point. But in the living membrane, especially when it is composed of cellular units, the whole question is additionally complicated by the great probability that the cells are the seat of chemical processes the nature of which is imperfectly known; such processes constitute the metabolism of the cells. It would, therefore, be somewhat surprising if the phenomena of the passage of substances through such cellular membranes were in strict accord with the passage of similar substances through non-living membranes which have not the same particulate framework and are not the possible seat of similar chemical processes. The statement, therefore, that any discrepancy between the two classes of phenomena necessitates the assumption of a peculiar vital directive force disregards the circumstance that between the conditions in the one case and those in the other lies a large and little explored field; moreover, such a statement implies, without any warrant, that any physico-chemical explanation must necessarily be insufficient in the case of the living membrane, although it is realised that there may be active chemical processes of the operations of which we have at present little exact knowledge.

What possible justification is there, therefore, for branding as hopeless all further physical and chemical investigation of certain aspects of the phenomena by attributing these to vital directive forces? The gaps and imperfections of the palæontological record were triumphantly vaunted by the opponents of evolution; and now that the work of successive years has convincingly contributed towards the filling up of these gaps not only has this objection collapsed, but the hypothesis of special creations which it supported has been involved in its fall. There are indications that the discrepancies in diffusion phenomena through widely different structures may be knit by the results of experiment on intermediate modifications. It may be many

[1] See article by B. Moore in "Recent Advances in Physiology and Biochemistry." Edited by L. Hill, F.R.S. (London: Arnold, 1906.)

[1] The conception of Ostwald as to the action of catalytic substances is extremely suggestive in connection with the activities of enzymes, both intracellular and extracellular. It is possible that the changes brought about by enzymes may, with the growth of our knowledge in physical chemistry, be shown to be of the same order as those which slowly occur in the absence of enzymes, and that the enzyme itself by facilitating adsorption phenomena may merely act by accelerating the velocity of the special change. See Leathes, "Problems in Animal Metabolism" (London: Murray, 1906).

years before these are completed, but the introduction of vitalism or biotic energy as a fictitious causative explanation is so opposed to the spirit and the progress of science that we may safely predict the complete abandonment of this position at a comparatively early date.

I venture now to define my own position in regard to this matter. I assert that, although the complexity of living tissues makes our present knowledge extremely limited, it is essentially unscientific to say that any physiological phenomenon is caused by vital force or is an argument in favour of "vitalism," and that, if this phraseology is offered as a sufficient description of the phenomenon, its further scientific study is prejudiced because the only terminology which admits of scientific exactitude is excluded. I assert, further, that if the term "vitalism" connotes no more in physiology than the term "living," its employment does not in any way enlarge our intellectual view of the subject-matter of physiology, and can only be considered either as meaningless tautology or as an expression of faith; but if the term has some additional, occult, and mystic significance, then its employment is detrimental to the progress of physiology, exerting as obstructive an influence upon the growth of our science as the conception of special creation exerted upon the progress of biology.

Vitalism is not the only "ism" which, perhaps unwittingly, obstructs physiological progress; it is, however, far more worthy of respect than others which I do not propose to particularise, for it is a twig of that lusty tree which, in philosophy, still claims the largest share of men's belief. The vitalist, leaving the more solid ground of physics and chemistry, enters the realm of metaphysics and there attaches himself to that distinguished circle of idealists whose pedigree extends back to Plato. If, as may be asserted with great confidence, idealism in philosophy will endure as long as thought exists, then it might be expected that vitalism in physiology will never entirely cease. The history of physiology, however, reveals the fluctuating extent of its influence. Potent a century or more ago, vitalism nearly disappeared between 1850 and 1870 under the pressure of the application of physical and chemical methods to physiology; it revived again towards the century's close, the ripple of a wide-spreading wave of idealistic philosophy. Materialism and idealism have been described by Huxley as appearing in the history of philosophy like "the shades of Scandinavian heroes eternally slaying one another and eternally coming to life again." As a physiologist, I do not venture to touch however lightly upon this metaphysical duel, since I frankly admit my own incapacity to do so and the particular applicability to my own powers of the words of Gibbon that "it is much easier to ascertain the appetites of a quadruped than the speculations of a philosopher." It is therefore without any intention of casting any suspicion of doubt upon the confidence felt as to the persistence of idealism in philosophy that I suggest that neo-vitalism in physiology bears upon its surface the signs of its own decay. One such sign is the circumstance that even its most ardent exponents refuse to follow the lead of this *ignis fatuus*, but assiduously investigate living processes by the most improved chemical and physical methods; another is that when any so-called vitalistic aspect of some physiological phenomenon is rendered explicable on physical and chemical lines, the vitalist abandons in this instance his peculiar standpoint. Neo-vitalism has of late thus lost its corrosive character; it now spreads as a thin but tenacious film over physiological conceptions and is in this way mildly obstructive, but its obstructive viscosity is continually yielding to the accumulating mass of the more precise knowledge which it endeavours to obscure. Research along physical and chemical lines into physiological processes is its uncompromising opponent, so that there is every reason for believing with Huxley that the weight and increasing number of those who refuse to be the prey of verbal mystifications have begun to tell.

The recent history of physiological progress shows that investigations confined to the study of physical and chemical processes have been the one fruitful source of physiological knowledge. It would be impossible to give even a brief survey of the chief results which have, during the last twenty years, been thus obtained. Out of the enormous wealth of material I select one of great importance and promise. It is that of the constitution of the nitrogenous compound familiarly known as proteid, which from its close association with protoplasm, the physical basis of life, has a fundamental significance and has therefore attracted the attention of many competent investigators. Important researches have been made on this subject by physiological chemists, notably Hofmeister and Kossel, and at the present time the subject is also being studied by one of the ablest organic chemists of the day, Emil Fischer, whose previous work on carbohydrates is so illuminating.[1] In the splendid chemical laboratory at Berlin, with its unparalleled equipment, a succession of researches have been carried out dealing not only with the constitution of the simpler proteid derivatives, but also with the important and difficult problem of the synthetic grouping of these derivatives into more complex compounds. The success which has so far attended these investigations is so pronounced as to encourage the hope that the future may reveal the chemical constitution of proteid itself and thus bring us perceptibly nearer to its possible synthetic formation. We congratulate ourselves that this problem has at last attracted the earnest attention of organic chemists.

I now invite your attention to those further aspects indicated in the opening sentence of this address, which imply the presence of automatic mechanisms by which the various processes of the body organs are regulated and coordinated for the welfare of the whole organism.

Many such automatic mechanisms are now known. Some of these are of an obvious chemical type, the mechanism being the production in minute quantity of chemical substances which are conveyed to remote organs by the circulating blood. In this way adrenalin, a substance elaborated by the medullary portion of the suprarenal organs, augments the activities of the muscles, particularly those of the arterioles. From his recent researches, Langley[2] is disposed to believe that many chemical compounds which augment or diminish the activity of muscles and glands do not act by altering the differentiated tissue, but play upon a hypothetical receptive substance which lies at the junction of the tissue with its entering nerves. This middleman, so situated as to lie in the interstices of the neuro-muscular junction, bears a relation to the muscle or gland-cell somewhat analogous to that which the fulminating cap bears to the cartridge, and it is quite conceivable that it is maintained in an appropriate condition of instability or explosiveness by the direct action of chemical substances conveyed to it in minute amounts by the blood.

It is remarkable how many of these strictly chemical automatic mechanisms have been discovered in the last few years, thus substantiating the views of Brown-Séquard. The automatic character of the mechanism which determines the secretion of the pancreatic fluid was revealed by the experiments of Bayliss and Starling, which showed that definite chemical compounds are formed in the lining cells of the small intestine, and that treatment with weak acid, such as occurs in the acid chyme, liberates a substance which, absorbed into the blood, has the special function of stimulating the pancreatic cells.[3] A similar automatic mechanism has been found by Edkins to exist in the stomach, for although the flow of gastric juice is initiated by nervous channels, the subsequent peptic secretion is largely augmented through the presence in the blood of chemical substances elaborated and absorbed in the pyloric portion of the stomach wall.[4] Marshall and Jolly have recently shown that substances elaborated in the maternal ovaries, and particularly in the corpus luteum,[5] determine, when introduced into the circulating blood, the changes necessary for the proper attachment of the embryo to the uterine wall and thus the further development of the embryo during the first stages of pregnancy. The researches of Starling and Miss Lane-Claypon indicate

[1] E. Fischer, *Berichte Deutsch. Gesellschaft*, xxxviii. 1905. (See also "La synthèse des matières protéiques," par L. C. Maillard. *Revue Générale des Sciences.* Févr. 1906. Paris.)
[2] J. N. Langley, *Journ. of Physiol.*, xxxiii. 1905, p. 374, and Croonian Lecture. Roy. Soc., 1906.
[3] Bayliss and Starling, *Journ. of Physiol.*, xxviii. 1902, p. 325.
[4] Edkins, "On the Chemical Mechanism of Gastric Secretion," *Proc. Roy Soc.*, B. lxxvi., 1905, p. 376.
[5] Marshall and Jolly, *Phil. Trans. Roy. Soc.* London, B, 1905, p. 198.

that chemical substances formed during pregnancy in the tissues of the fœtus will, if introduced into the maternal blood, directly evoke the appropriate activities of the remote mammary glands.[1]

These are only a few instances of a class of mechanisms, strictly chemical in character, by which the activities of remote and dissimilar organs are automatically coordinated; a further class of such mechanisms, although involving a chemical substance conveyed by the blood, carries out the actual regulation by means of the central nervous system. An example of this class is afforded by the researches of Haldane and Priestley upon the carbonic-acid gas in the pulmonary air. These show that the alveolar pressure of carbonic acid in the lung spaces remains constant even when the atmospheric pressure is considerately altered in amount. The constancy is due to the circumstance that the respiratory nerve centres are exquisitely sensitive to a rise in this carbonic-acid pressure. Any such rise slightly augments the carbonic-acid tension of the pulmonary blood, which, on being conveyed to the nerve centres, arouses their greater activity, and the increased efficiency of the respiratory ventilation, thus produced, rapidly reduces the amount of the very agent which is its exciting cause.[2] The researches of Hill and Greenwood, with air pressures up to seven atmospheres, bear out the conclusion that by this automatic mechanism the air in the lung alveoli has a practically constant pressure of carbonic acid in any given individual.[3]

The introduction, in this example, of the respiratory centres and nerves raises the question whether the nervous system, which is in a very special sense the channel for the regulation and coordination of the various activities of the body, may not itself be conceived to be a supreme example of an automatic physico-chemical mechanism, the transference from one part to another taking place, not through the flow of blood containing chemical substances, but through a more subtle physico-chemical flow along the highly differentiated nervous strands of which this system consists. The nervous system is not popularly regarded in this light; on the contrary it is considered to be the special seat of vital directive forces, and it is held, even by some scientific men, that the nervous energy which it manifests is so transcendental in its essence that it can never be brought into line with those modes of energy prevailing in chemistry and physics. There is, moreover, a widespread belief, founded upon conscious volitional power, that nervous energy can be spontaneously created, and that even if its manifestations are bound up with the integrity of certain definite nervous structures, these structures only form the material residence of genii, temporarily in possession, endowed with the powers of hypothetical homunculi at the bidding of which the manifestations either take place or cease.[4]

The complexity of nervous structure and the apparently uncertain character of nervous activities furnished the older writers with plausible reasons for assuming the existence of animal spirits, but the extensive researches of half a century progressively suggest that nervous phenomena may be regarded as the sum of particular physico-chemical processes localised in an intricate differentiated structure, the threads of which are being unravelled by neurological technique. This chapter of physiology still bristles with difficult problems and obscure points, yet the unmistakable trend of the immense advances which have been made in recent years is towards the assumption that nervous processes do not in their essence differ from processes occurring elsewhere in both the living and non-living worlds.

As regards structure it is generally assumed by neurologists that the whole system is a fabric of interwoven elements termed neurons, each with a nucleated nerve cell and offshoots, one of which may be extended as a nerve fibré, whilst no nerve fibre exists which is not the offshoot of one such cell. This neuron theory is based upon developmental history and upon the suggestive fact that each nerve cell forms an independent trophic centre for its own distributed processes. It is undoubted that, like the atomic theory in chemistry, the neuron theory has proved of enormous service, enabling neurologists to disentangle the woven strands of nerve-cell processes even in such an intricate woof as that of the central nervous mass. There are, however, difficulties associated with its full acceptance in physiology, as indeed there are said to be in connection with the full acceptance of the atomic theory in chemistry; but dismissing these for the moment, I pass on to consider the presumable character of such a conception of nervous activities as would be demanded on the supposition that the nervous system is, as regards all essentials, an automatic physicochemical mechanism.

In the nerve fibres, which are undoubtedly the offshoots of nerve cells, the only demonstrable changes during the actual passage of nervous impulses are of an electrical type. These resemble the effects which would occur if there were redistributions of such electrolytes as are known to exist within and around the differentiated fibrillated core or axon of each nerve fibre. All the better-known aspects of nerve-fibre activities are in accordance with such an electrolytic conception. The exquisite sensibility of nerve to physical and chemical changes of a sudden character would be associated with the fluctuating and variable character of electrolytic distribution, this instability being characteristic of particular electrolytes in colloidal solutions; hence physical and chemical alterations primarily affecting the nerve envelope will, by modifying the electrolytic distribution, produce physico-chemical change in the internal axon itself. Such changes, when once produced at any point in the differentiated fibrillar continuum of the nerve fibre, must in accordance with the conception first propounded by Hermann be propagated or transmitted along this continuum. The redistribution of electrolytes at the seat of the external impression being itself a source of electromotive effects, electrical currents demonstrably flow from this point into the contiguous parts of the fibrillar continuum. Such flow of current must reproduce in this neighbouring continuum that electrolytic redistribution which is the fundamental aspect of nerve-fibre activity. Thus, by this comparatively simple automatic mechanism, the physico-chemical electrolytic change is successively assumed by the various portions which compose the length of the differentiated axon, and the new or active phase is propagated along a nerve fibre as infallibly as a flame speeds along a fuse when one end is ignited; in this way the conception explains how a so-called nervous impulse is brought into being. Further, the brief duration of the activity of the nerve, its rapid development and slower decline, and the circumstance that a second external change cannot arouse a second activity if it occurs very shortly after an effective predecessor, all have their counterpart on the electrolytic side, and we have convincing evidence that the electrolytic redistribution during activity cannot be again produced until the electrolytic condition has more or less returned to its original resting poise: the real peculiarity of the living tissue is its persistent tendency to re-establish the electrolytic concentration of this resting poise.[1] Finally experiments show more and more convincingly that the capacity of the nerve to respond to external changes, as well as the magnitude and duration of the aroused activities, are particularly susceptible to modification by all those agents which are most potent in affecting electrolytic aggregates, such as temperature, electrolysis, and impregnation with various electrolytes.

These electrical indications of nerve-fibre activities are fundamentally the same whether the fibres occur in peripheral nerve trunks or in the bundles which course through the central masses; and thus, if the whole system consisted of nothing but the united strands of differentiated nerve fibres, nervous phenomena would be merely the expression of the development, along appropriately distributed tracts, of similar electrolytic changes primarily started by some external physical or chemical alteration. But additional complications are introduced by the existence of nerve-fibre endings and by the interposition of the nerve

[1] Starling and Lane-Claypon.

[2] Haldane and Priestley, "The Regulation of Lung Ventilation." *Journ. of Physiol.*, xxxii. 1905.

[3] Hill and Greenwood, "The Influence of Increased Barometric Pressure on Man." *Proc. Roy. Soc.*, vol. lxxvii B, 1906, p. 442.

[4] Lodge, "Life and Matter" (London: Williams and Norgate, 1906). "Matter is the vehicle of mind, but it is dominated and transcended by it" (p. 123). "Contemplate a brain-cell, whence originates a certain nerve-process whereby energy is liberated with some resultant effect" (p. 168). "It is intelligence which directs; it is physical energy which is directed and controlled and produces the result in time and space" (p. 169).

[1] Gotch and Burch, *Journ. of Physiol.*, vol. xxiv. 1899, p. 410.

cells. According to the neuron theory the fibres of different nerve cells end more or less blindly, and, at any rate in vertebrates, do not demonstrably unite at their termini within the central mass; hence gaps exist at the junction unbridged by the differentiated structural continuum. But since the nervous impulse can pass from one set to the other, a physiological continuum undoubtedly exists; it is necessary, therefore; to assume either that the electrolytic change in one neuron can by mere contiguity in space arouse a similar change in a neighbouring neuron process, or that a differentiated connection actually exists, but of such structural delicacy that it cannot be microscopically demonstrated. Recently several physiologists have stated their belief in such continuity; one of these, E. Pfluger, bases his view upon the admitted intracellular nature of peripheral nerve endings in muscles, glands, epithelial cells, and electrical organs. Arguing from analogy, he infers that the central nerve endings of one neuron probably pierce and enter the cell processes of another neuron.[1] Such a connection can be actually seen, as a pericellular plexus, in the ganglia of crustacea, and has been occasionally described as observed in higher animals. Whether the central termini of neuron processes are in reality joined by extremely fine fibrillar filaments or whether they end blindly in mere juxtaposition, it is undoubted that the functional synapsis presents peculiar features. The chief peculiarities of synaptic activities as distinct from the activities of the nerve fibres are the following:—Marked retardation in the maximum rate of propagation; irreciprocity of conduction, which is favoured in the natural or homodromous direction, whilst in the unnatural or heterodromous direction it is obstructed or completely blocked; susceptibility to fatigue; special susceptibility to stimulation and impairment by definite chemical substances, by strychnine, absinthe, anæsthetics, &c.; the presence of a resistance which diminishes rapidly when subjected to the assault of a series of entering or centripetal nervous impulses even when each member of the series is alone quite powerless to force a passage. All these peculiarities are more or less demonstrable in all nerve endings, peripheral as well as central, and are presumably, therefore, related to the character of the propagation which occurs in the finely-divided non-medullated twigs or "arborisations" into which the nerve fibres break up in such endings, and possibly to some further "receptive" substance lying beyond the endings. The retarded propagation, showing itself by an apparent delay, occurs in the motor nerve endings of muscles and in the multitudinous nerve endings of electrical organs, as well as in the central nervous system. Garten's researches on non-medullated nerves suggest that it may be connected with such slowed development of the electrolytic redistribution and of its accompanying electromotive alterations as is demonstrable in these structures.[2] Irreciprocity of conduction occurs where nerve endings are continued into muscle substance, since the activity process passes from nerve to muscle, but not the reverse way. In 1896 Engelmann succeeded by means of a double muscle-bath in so modifying one end of a muscle fibre that the wave of contraction, whilst it travelled freely along the muscle fibre from the unmodified to the modified portion, would not do so the reverse way.[3] The particular modification which produced this abnormal result is an interesting one; it is the development of an abnormally sluggish type of mobility, the whole activity of the modified region being greatly prolonged by means of veratria. This suggests that difference in the duration of the active process on the two sides of a central nervous synapsis would, if present, be one factor in producing the well-known central irreciprocity. The susceptibility to fatigue may be associated with this augmented difficulty of propagation, and it undoubtedly occurs to a marked extent in muscular nerve endings; for, according to the investigations of Joteyko, it may be more pronounced in this peripheral ending than it is even in the spinal cord.[4] Even the so-called summation phenomena—that is, the ease with which a succession of centripetal impulses can force a passage as opposed to the difficulty with which a single such impulse does so—is not peculiar to the central mass, but is observed more or less in peripheral nerve endings; for instance, those of electrical organs. Finally, the results obtained by Wedenski suggest that anæsthetics have a particular affinity for nerve endings, including the peripheral ones in the muscles; and although the causation is at present imperfectly known, it does not seem improbable that they may act upon some such specific substance as that which is conceived of by Langley under the term "receptive."[1]

All the phenomena hitherto described are thus not necessarily aspects of the activity of that particular mass which constitutes the body of the nerve cell, but of nerve endings with their fine arborisations. As regards direct electrical evidence of electrolytic changes in these finer branches, it so happens that Nature has provided some nerve endings on such a magnificent scale that this evidence is readily obtained. In the electrical organs of fishes the essential structure consists of a pile of numerous discs each invaded by nerve endings, and the electric shock of the fish is the sum of all the electrical changes in this pile when an efferent nervous impulse reaches each of its component discs. Its potency is due to the number of these components, but in each single component it is of the same order as the electromotive change in a nerve, and its character is such as might be produced by electrolytic redistribution occurring simultaneously in the immense number of nerve endings which are present in each disc of the electrical organ. Although displaying the peculiarities of apparent delay, &c., just referred to, the general character of the shock of the organ is such as to warrant the belief that electrolytic conceptions of nerve-fibre activity can be extended to the activities of nerve endings.

There remains that special part of the whole neuron which is the effective source both of its development and of its maintenance, the nerve cell. Continuity with a nerve cell is essential for the integrity of both the structure and the function of a nerve fibre, but it is undoubted that, in its turn, the nerve cell is also dependent upon the existence of its processes in an unimpaired state. Thus the cell suffers a change which comes on slowly but with great certainty if any part of the neuron has been mutilated, or if the cell has been shorn of some of its offshoots. That it forms a special part of the conducting path is indicated by the occurrence of intracellular and nuclear alterations when a prolonged series of impulses travel towards it, and a further more remarkable point is that it also appears to change if the entering nervous impulses with their electrolytic concomitants are no longer able to reach it. This suggests that nerve cells, far from being spontaneous actors, are in a very real sense dependents; they form only one possible conducting portion of the whole differentiated tract, and atrophy when this tract is broken or is from any circumstance not utilised. That the cell is primarily trophic and only incidentally a conductor is suggested by Bethe's experiments upon crustacea. Owing to pericellular connections the actual nerve cell may be removed in these animals without severing the whole conducting tract, for a portion lies around but outside the cell; and since, even after such removal, the usual reflex movements of the supplied antennæ are resumed, the cell cannot in this instance be regarded as essential for the discharge of the motor impulses which evoke the antennæ movements.[2]

In higher animals such removal of the cell body has been imperfectly carried out by Steinach in the dorsal spinal ganglia, but in the central mass it is impossible to perform a crucial experiment of this kind so as to determine whether or no the substance of nerve cells can create nervous impulses. There are two particular features of reflex movements which may be cited as indicating that a motor nerve cell has at its call a store of nervous energy which it can spontaneously discharge. The first of these is the well-known fact that the character of reflex movements is such as to indicate the rhythmical discharge of groups of centrifugal nerve impulses the periodicity of

1 E. Pfluger, "Ueber den elementaren Bau des Nervensystems," *Archiv f. die Ges. Physiol.* cxii., 1906.
2 Garten, "Beiträge zur Physiologie der marklosen Nerven." Jena, 1903.
3 Engelmann, "Versuche über irreciproke Reizleitung in Muskelfasern," *Archiv f. die Ges. Physiol.*, lxii. 1896, p. 400.
4 Joteyko, "Travaux de l'Institut Solvay," Bruxelles, iii. 2, 1900.

1 Wedenski, "Erregung, Hemmung und Narkose," *Archiv f. die Ges. Physiol.* c., 1903.
2 Bethe, *Allgemeine Anat. u. Physiol. des Nervensystems*, 1903, p. 99.

which bears no relation to that of the centripetal ones. But it must be remembered that even in nerve fibres it is possible for a succession of stimuli to evoke a different succession of electrolytic changes and of nerve impulses, provided that some of the successive stimuli fall within the period of inexcitability which occurs during the establishment of each new electrolytic poise.[1] We have, therefore, only to assume, as is very probable, that in the central portion of the nervous path this poise is prolonged in its development, and numbers of centripetal impulses must necessarily fail; hence the emergent ones will have a special periodicity indicative of the duration of the swing of the electrolytic rearrangement which occurs when the synapses *plus* the cells are traversed by the entering impulse.

The second feature which more particularly suggests spontaneous cellular activity is the well-known fact that reflex centrifugal discharges may continue after the obvious centripetal ones have ceased. This is preeminently the case when the central mass is rendered extremely unstable by certain chemical compounds, such as strychnine, &c. There are, however, suggestive indications in connection with such persistent discharges. The more completely all the centripetal paths are blocked by severance and other means, the less perceptible is such persistent discharge, and since nervous impulses are continually streaming into the central mass from all parts, even from those in apparent repose, it would seem that could we completely isolate nerve cells, their discharge would probably altogether cease. In this connection a suggestive experiment was carried out some years ago upon the spinal cord of the mammal.[2] A portion was isolated *in situ* by two cross-sections, and a part of this isolated cord was split longitudinally into a ventral half containing the motor or centrifugal nerve cells and a dorsal half containing the breaking up of the centripetal nerves; each half was then examined for those electrolytic changes which indicate the presence of nervous impulses. It was found that, even in the strychnised animal, no electrical effects could be detected in the ventral half of the cord or its issuing roots, although such effects were marked in the whole cord, and occurred in the dorsal half which contained the centripetal nerve fibres.

This experiment indicates that even in the hyper-excitable condition produced by strychnine the spinal motor nerve cells did not discharge centrifugal impulses when cut off from their centripetal connections. It is corroborated by the results obtained by Baglioni in the frog and small mammal,[3] and, taken in connection with those previously mentioned, it affords considerable foundation for asserting that the chief *rôle* of the nerve cell is trophic, and that, as regards issuing nerve impulses, it only forms a modified part of the conducting path. The more we investigate the physiology of the nervous system, the stronger becomes our belief that for centrifugal discharges to occur centripetal impulses must be primarily started either in the peripheral sensory surfaces by changes of a physical or chemical type occurring in the external world, or at some point in the nerve continuum by local chemical or physical changes within the body, especially those due to the chemical condition of the blood. Having been thus started they course along definite structural paths, and the only direct indications of this passage consist of such phenomena as would be produced by the redistribution of concentrated groups of electrolytes—a purely physico-chemical process.

This conception places the propagation of the nervous excitatory state as the sole determining factor of nerve activities, central or peripheral. It derives additional support from the circumstance that it is in harmony with that aspect of these activities which is comprised under the term, inhibition. Any effective regulating system must be able to bring into play both incentive and restraint—the whip and the reins. The possession by the central nervous mechanism of inhibitory powers is remarkable both for its extent and its delicacy. It appears more and more probable that this is achieved by the propagation of nervous impulses of the ordinary type. Thus, recent researches by Sherrington show that the propagated impulses from a given central mass may, although normally inhibitory to the centrifugal discharge of another mass, become directly incentive if the second controlling centre has its excitability abnormally augmented by strychnine, tetanus toxin, &c.[1] As regards their fundamental characters it thus appears that both augmenting and inhibiting impulses belong to the same category. Moreover, such theories of central inhibition as embrace all the phenomena involve as their essential basis the cutting-off of the potent centripetal supply to the inhibited centre. In the interference theory this cutting-off is assumed to be caused by the arrival of other nerve impulses which, breaking into the path of normal centripetal flow, obstruct and run counter to this potent stream. In the ingenious drainage theory, propounded by McDougall, the cutting-off is an indirect one, it being assumed that the new stream enters other side-channels, and thereby opens up a short circuit through which the potent ones drain away without reaching the centrifugal centre. Even Langley's conception of receptive substances played upon by impulses must be associated with a check in the efficiency of the continuous centripetal supply.

From the foregoing it appears that the physiologist has definite grounds for believing that, as far as present knowledge goes, both the production and cessation of central nervous discharges are the expression of propagated changes, and that these changes reveal themselves as physico-chemical alterations of an electrolytic character. The nervous process, which rightly seems to us so recondite, does not, in the light of this conception, owe its physiological mystery to a new form of energy, but to the circumstance that a mode of energy displayed in the non-living world occurs in colloidal electrolytic structures of great chemical complexity. There is a natural prejudice against the adoption of this view, but such prejudice should surely be mitigated by the consideration that this full admission of physiology into the realm of natural science, by forcing a more comprehensive recognition of the harmony of Nature, is invested with intellectual grandeur.

With such questions as the essential meaning of consciousness and the interpretation of the various aspects of mind revealed by introspective methods, the physiologist, as such, has no direct concern. For his purpose states of consciousness are regarded merely as signs that certain nervous structures are in a state of physiological activity; and he thus limits the scope of physiology to the objective world. This limitation of physiology does not prohibit a treatment of the subjective world along lines calculated to display that intellectual causative array which characterises science; it merely indicates that this particular application of scientific method is not physiology, but that something else, still more profound, which is now termed psychophysics.

But if objective phenomena form the subject-matter of the physiologist, then "the legitimate materialism of science" must constitute his working hypothesis; and his "well-defined purpose" must be to adapt and apply the methods of physics and chemistry for the analysis of such phenomena as he can detect in all physiological tissues, including the nervous system. The trend of such a strictly physiological analysis is towards a conception in which the highest animal appears as an automaton composed of differentiated structures exquisitely sensitive to the play of physical and chemical surroundings.[2] The various parts of the animal body are linked by circulating fluids and by one special structure, the nervous system; in this linking of parts the physiologist detects the working of automatic chemical mechanisms of great delicacy which, once developed, are retained and perfected in proportion as they efficiently regulate the various bodily activities and coordinate them for the welfare of the whole organism. The

1 Gotch and Burch, *Journ. of Physiol.*, vol. xxiv. 1899, p. 410; Boycott, *Journ. of Physiol.*, vol. xxiv. 1899, p. 144; Buchanan, *Journ. of Physiol.*, vol. xxvii 1901, p. 98, &c.

2 Gotch and Horsley, *Phil. Trans.*, vol. clxxxii. pp. 267-526. (London, 1891.)

3 Baglioni, *Archiv f. die Ges. Physiol.*, 1900, Supplement, pp. 198-242. (Leipzig.)

1 Sherrington, *Proc. Roy. Soc.*, vol. lxxvi. B, pp. 269-297. (London, 1905.)

2 See Huxley, "On the Hypothesis that Animals are Automata," Evening Address, Brit. Assoc, Belfast, 1874. Re-published in "Collected Essays," vol. i. (Macmillan, 1904.)

plastic nature of nervous tissue renders it, in accordance with the principles of natural selection, particularly favourable for progressive change in this direction, and thus developments may occur which reach their highest physiological expression in the brain of man.

In conclusion, attention may be drawn to the peculiar instability of living processes and structures. The living units show that significant mutability which the physiologist describes as metabolism. This mutability appears to be encouraged or discouraged by the extent to which it fulfils a purpose, and this purpose in a living organism is the dominating law of its own development. The fulfilment of this purpose by means of physical and chemical change is such a general characteristic of living processes that a physiologist may with some confidence suggest that this fulfilment is the distinctive mark of a living thing.

SECTION K.

BOTANY.

Opening Address by Prof. F. W. Oliver, M.A., D.Sc., F.R.S., President of the Section.

The Seed, a Chapter in Evolution.

As the subject of the first portion of my Address I propose to consider the place of the seed in the evolutionary history of plants. The seed-character is the distinctive mark of three great groups of plants—the Pteridosperms, Gymnosperms (including Cordaiteæ), and Angiosperms. Nor will it be seriously questioned that the possession of this organ has given supremacy to seed-bearing plants over groups not thus characterised in a majority of the types of environment where vegetation is able to exist. Exceptions, of course, there are, though few of them are wholly immune from the invasion of the Spermophyte. The sort of habitat, for instance, in which Zostera flourishes—sometimes to the exclusion of other forms—is held more as a result of vegetative aggressiveness than in virtue of any special power conferred by the seed-habit.

Our stock of knowledge of those plants which had attained to the seed-bearing condition in a bygone age has undergone some extension during the last few years; the seed, too, has shed its glamour over other branches of morphological inquiry, so that no serious apology is necessary for its selection as the subject of this morning's discourse.

It is generally conceded that the primitive vegetation arose in the waters, and that with the parting of the waters and the emerging of land and continents this primitive stock of plants was sufficiently plastic to take advantage of the new conditions, throwing up successive hordes which effected a footing on the land, and in time peopled the whole earth with forms adapted to the varying habitats and climates as they differentiated.

Of the character of these primæval aquatic types no direct information has been vouchsafed. It is a matter of inference that they possessed much in common with the green Algæ of to-day, which, living in a biologically stable medium, are commonly regarded as their nearest representatives. Be that as it may, the complexity of the life-history of existing Algæ and the frequent presence of neutral generations seem significant of the capacity of their progenitors to originate forms with sporophytes adapted to terrestrial conditions.

In our Liverworts and Mosses on the one hand and the Ferns and their allies on the other, two divergent evolutionary lines are represented, both fitted to existence upon land surfaces, but handicapped by the retention of a non-terrestrial method of effecting the sexual process. In the Bryophytes the physiological continuity and dependence of the sporophyte upon the gametophyte is preserved throughout, and it never rises above the status of an elaborate spore-capsule; whilst the gametophyte, though often reaching a complex vegetative differentiation, offering many analogies with the sporophytes of higher plants, is condemned to pigmy dimensions through the incubus of the inherited aquatic mechanism of fertilisation.

Though remote from the series that have culminated in seed-plants, the Bryophytes are a group offering many an instructive parallel with the main series of plants; certainly these forms have remained too long a thing apart. Haberlandt and Goebel have shown us—to name no others—how happy is the hunting-ground which the Bryophytes provide. Further work is still required, directed more especially to certain important points in the life-history.

With the regular vascular cryptogams the relations between the stages are of course different. Here we find large complex sporophytes holding the ground, but hampered by the ever-recurring necessity of dependence upon outside water for the performance of the reproductive process.

The land problem was solved on ingenious lines. The differentiation of gametophytes which accompanied heterospory rendered possible the retention of the larger spore and female prothallus. Thus retained aloft, the drawback of the double existence is overcome and the advantages of the elaborated sporophyte more fully realised. The water conditions are brought directly under the plant's control through the device of the pollen-chamber, and the way paved for the ideal seed with siphonogamy.

All the elements of the seed were present before, but combined compactly in this new way we recognise what is virtually a fresh stage intercalated in the life-history. Further elaboration came bit by bit as the possibilities were successively realised. With the evolution of the seed, the plant rose at a bound to a higher plane, and this structure in its perfected form has become the very centre of the plant's existence.

The case of Cycas and Ginkgo with motile sperms affords an extreme demonstration of the inertia of heredity, the persistence in living seed-plants of the original aquatic flagellate type.

Obsolete as they are and faced with extinction, these survivors from the middle epoch of the world's history still hold their ground in a few scattered localities. In this connection we shall listen with interest to Prof. Pearson's account of the Encephalartos-scrub of South Africa which is to occupy us during the course of the present sitting of the Section.

How the sperms became replaced ultimately by the passive cells of the pollen-tube we have no knowledge.

If the conjecture be well founded that the change came late rather than early, then the conservatism of the spermophytic line in this respect stands in marked contrast to the adaptability that is so characteristic of another phylum of aërial plants. The ready evolution of siphonogamy in the form of fertilising tubes, so common in the Fungi, perhaps finds its explanation in the close filiation of this group with primitive and plastic forms. The fertilising tube may reasonably be regarded as a special case of a general susceptibility to chemiotactic stimuli which distinguished the whole hyphal complex of the group from very early times. In the case of the spermophyte, on the other hand, the motile spermatozoid seems to have persisted through a long and complicated ancestral history, so that its elimination may have been less easy of achievement.

The seed, once evolved, became the centre of a host of accessory organs, constituting what we know collectively as the fruit and flower. By these it has been robbed, as we shall see, of many of its pristine functions, and at the same time has undergone marked structural reduction. In the highly elaborated Angiosperm more especially we find an almost stereotyped uniformity in seed-structure contrasting with an infinite diversity in the outward floral husk.

In attempting a sketch of the origin of the seed one has to admit at the outset that recent discoveries bring us no nearer to its prototype than we were a decade ago. For the seeds of the Pteridosperms are advanced structures recalling quite vividly the type long familiar in living Cycads. It would be overstating the case to say they have nothing primitive about them, but there is a long chapter in evolution to be deciphered before we can connect, say, the seed of Lyginodendron with the sporangium of any Fern at present known to us.

The great interest of the recent correlation of seeds with Coal Measure plants lies less in the structure of these correlated seeds than in the very extensive series of plant-remains which we have thus come to recognise as belonging to the earlier Spermophytes.

For the position of these plants had remained in suspense.

The elaborate anatomical investigation which their vegetative organs had received at the hands of Williamson, Scott, Solms-Laubach, and others showed them to occupy a transitional position between the Ferns and Cycads. In certain respects they showed an advance in the cycadian direction, whilst in others they were wholly fern-like. Their fructifications were unknown, and their nature remained an open question. It was for this group, or series of transitional groups, that Potonié proposed the appropriate name of Cycadofilices.

We know now that the Lyginodendreæ and Medulloseæ bore seeds attached to their fronds. The seeds have been found attached in some cases to reduced fronds consisting of a branching rachis, in others to fronds of the normal filicinean type. Indeed, so far as habit is concerned, these plants may rightly be described as seed-bearing Ferns.

As such, indeed, most people will be content to regard them—as forms, that is, having close filicinean relationship in which the reproductive method has been profoundly modified, the internal anatomy to a less extent, and the habit hardly at all. Had these Pteridosperms come to light during the lifetime of Hofmeister that master of morphology must have pounced upon them as furnishing an important link in his chain. These fossils and the spermatozoa which the Japanese botanists discovered in the seeds of Cycas and Ginkgo, indeed, afford the most convincing direct evidence of the soundness of the Hofmeisterian scheme that it is possible to conceive. Nor is that all. For by confirming the indications first revealed by the earlier investigation of the vegetative anatomy, the Pteridosperms have afforded us a striking object-lesson of the value of the anatomical method—of the significance of purely anatomical characters too long ignored by the systematist.

Not so long ago, when new examples of these Pteridosperms were turning up on every hand, some pessimists were inclined to wonder whether, after all, any groups of real Ferns existed in the Palæozoic rocks. Such sporangia as were known might well be the pollen-sacs of seed-bearing plants. All doubts on this score are happily set at rest by the detection of germinating Fern-spores in contemporary beds. Nor can I think of any more fitting tail-piece to the investigations which lead the way to the Pteridosperms than the discovery, by the same investigator, of the antidote to these rather disturbing views. However, it is needless to dwell further on these matters now, in view of Dr. Scott's address to-morrow upon the Present State of Palæozoic Botany.

But to return to the history of the seed. In the absence of direct evidence, one can only conjecture that some old generalised type of sporangium formed its prototype, something substantial, on the lines of a Botryopteris or Zygopteris, perhaps. The heterospory that was the precursor of the seed-like condition must have been a transient phase, or else it is lost in the pre-Carboniferous obscurity. Be that as it may, the passage from the dehiscent to the indehiscent monosporal megasporangium finds its analogy in every group of plants. Where there is extreme numerical reduction of the contained structures—be they spores or seeds—a multitude of cases in the Fungi, in the Algæ, and the angiospermic flowering plants show that dehiscence tends to become obsolete. The failure to dehisce does not appear to be directly correlated with any mechanical difficulty in ejaculation. It is more probably one of those obscure cases of interdependence of phenomena in which the vegetable kingdom abounds. A special investigation directed to the elucidation of this point might be expected to yield interesting results.

We now come to the consideration of a most characteristic organ of the seed—the pollen-chamber. This cavity arises at the apex of the megasporangium, above the big megaspore, and is found in all the Palæozoic seeds, with the sole exception, so far as I am aware, of the "seed-like" structures in Lepidocarpon and Miadesmia. The utility of the pollen-chamber is manifest, but its antecedents are quite unknown. Upon such a structure as this may have depended the success of the seed-method at a critical stage in its evolution. In the viviparous Selaginellas, described some years ago in America, the archegonium on the prothallus of the retained megaspore is fertilised by sperms liberated from microspores which become caught in the lips of the open megasporangial wall. This analogy suggests to us that the pollen-chamber cavity may be a relic or modification of the original place of dehiscence. If this conjecture be true, we have here what was once an exit-pore converted to the purposes of ingress, just as we find, in so many Thallophytes, tubes and beaks, once, as it is supposed, the orifices of zoospore discharge, now serving for the reception of male gametes.

A great feature in the early seed types was the complexity of the integument, and this still holds good in recent Cycads and some other Gymnosperms. Protective envelopes are so commonly associated with reproductive organs, and the nutritive conditions are so favourable to their production, that a naked nucellus strikes one as anomalous. If future research confirm the supposition that the ferns which stand in possible relation to early seed-plants were ex-indusiate, like the Marattiaceæ, recent and fossil, then no doubt the seed-coat is a new formation, having no true homology with, but merely homoplastic resemblance to, ordinary Fern-indusia. The only case of a naked nucellus that recalls itself is the rather mysterious instance of Lepidocarpon in which Dr. Scott reports the not infrequent occurrence of non-integumented megasporangia with the prothallus fully developed.

The robust nature of the seed envelope, which was often drupaceous, is in complete harmony with the whole character of the seed if you regard the habit at its inception as a xerophilous adaptation. And such no doubt it was, an improved method whereby the plant became independent of chance water at a very critical stage in the life-history. Some of the peculiarities of fossil seed-coats, especially the ribbing of the Lagenostomas and several other genera, may be attributed to a multiple origin of this structure, at any rate in some cases. The remarkable circlet of tentacles which surrounds the summit of *Lagenostoma physoides* (best known by Williamson's earlier name *Physostoma elegans*) suggests that a number of foliar lobes have been incorporated in the seed, whilst the presence of perimicropylar ridges and the septate canopy in allied forms may be taken as only a less evident indication of the same thing.

The relation between the integument and sporangial body of recent Gymnosperm seeds is found to be an inconstant character, and the same is true of the fossils. In general character the relationship recalls that which obtains between the ovary and receptacle of an Angiosperm. The Lagenostomas resemble Cycas and Pinus in having the integument free at the apex only, whilst Taxus, Phyllocladus, and Araucaria are in agreement with the Trigonocarpons and other seeds, which are generally attributed to Medulloseæ, in having an integument which rises freely from the chalaza. It is interesting to note that the fossil seeds of the latter group show an additional complexity in the wall of the nucellus. For in them a series of tracheal strands or even a mantle of tracheides is found running up from the chalaza to the pollen-chamber. It is evident that nothing was spared in these older seeds to ensure adequate access of water to the pollen-chamber where the sperms must have been liberated.

In due time the protective sheath, or testa, appropriated other functions supplementary to that of protection. Of these the most important must have been the reception of the pollen. A very striking feature in all the Lagenostomas is the way in which the tip of the nucellus (where the orifice of the pollen-chamber is situated) projects beyond the integument. In these seeds the microspores must have had direct access to the pollen-chamber without first descending a micropylar canal.

In the Medullosean seeds also the nucellus is distinguished by a long beak, as Dr. Scott and Mr. Maslen have shown recently for Trigonocarpon, and, as we know, in Stephanospermum, and many other cases. So far as we know, this beak does not extend to the surface, though it engages with the micropylar canal, and is continued some distance up.

Though it can hardly be supposed that the long beak has been inherited from the ancestral sporangium, its presence may be none the less significant of what took place when the seed method was initiated. The direct pollination in Lagenostoma may well be a survival from the old days when no proper micropyle existed. But when the micropyle closed in, the conservative nucellus would for a while endeavour to maintain direct communication with the exterior. The beak-like appendage on this view would be a new formation evolved *pari passu* with the integument.

A peculiar and distinctive, though negative, feature common to the whole range of Palæozoic seeds that have become known to us is the lack of an embryo. Occasionally small-sized seeds are met with, as in *Lagenostoma Lomaxi*, and now and then immature-looking stages, of which the best example is Renault's Cordaitean ovule, so often figured in the books. But apart from such rarities the petrifactions agree in being at a stage which, in the light of recent Cycads, is to be interpreted as corresponding to the time of fertilisation. The pollen-chamber is charged with pollen-grains, whilst in good examples the megaspore is filled with a prothallus which frequently shows indications of archegonia at its upper extremity. All these specimens will be dismissed by some as abortive, and any conclusions drawn from the negative character as invalid. Without ignoring this contingency another view is, of course, possible. The normal fall of the seed may have followed pollination at a short interval, much as is reported for Cycas and Ginkgo to-day. The "resting period" in these seeds would then perhaps coincide with the maturation of the sperms, whilst the subsequent embryonic history might have been carried through without a pause. This view gains support from the filicinean relationship, for of course the fertilised egg of a Fern continues its development without interruption. If the modification of the pteridophytic life-history that culminated in these early seeds were directed, as seems probable, to ensuring a greater certainty in bringing the gametes together under conditions favourable to their union, it would follow that the other great advantage arising from the seed-habit was of later acquisition. In other words, the ordinary seed with resting embryo was evolved by stages. There is a great lacuna in our knowledge of the early adjustment of the embryo to intraseminal existence. Whilst evidence of Palæozoic seeds with resting embryos is altogether wanting, we are confronted in the Mesozoic rocks with the Bennettiteæ, all of which possess a well-marked dicotyledonous embryo practically filling the seed-cavity. It is mere conjecture to suggest that this change has been wrought in response to some climatic stimulus, though the marked xerophilous facies of many of the Mesozoic Cycadophyta seems quite consistent with such a view. Be that as it may, one cannot fail to recognise that the resting seed with an embryo marks a great advance on the Pteridosperm, an advance hardly less important to the welfare of the plant than was the earlier type of seed on the extended life-history of the filicinean prototype.

This stage of the seed-history would be of exceptional interest if we could hope to recover any morsels of direct evidence. As yet we remain in the dark as to the morphological nature of the embryonic organs, how far we are dealing with new structures produced from a protocorm, as Prof. Bayley Balfour has suggested;[1] how far they represent the old filicinean organs adjusted to intraseminal life. What chance there may be of the solution of this difficult problem by the application of other methods may emerge perhaps from the discussion on the phylogenetic value of early seedling characters which is to be opened next Tuesday morning by my colleagues Mr. Tansley and Miss Thomas.

Reference has already been made to the view that the seed, as we find it in the majority of spermophytes with its resting embryo, shows definite adaptation to seasonal periodicity. It would be interesting to learn how far the seeds of plants long accustomed to uniform conditions, such as the rainy tropical forest, behave in this respect. The point does not appear to have been very fully investigated. Indeed, there is a rich field for both observational and experimental work upon obscure seed-problems awaiting any one who can devote continuous attention to the subject. Is there any solid foundation for the supposed "physiological dimorphism" among seeds according to which, as one reads in the older books, the earlier ripening seeds are adapted to an immediate germination, whilst the later ones are reserved for the following spring? It may be that we have here but one more illustration of the operation of temperature as the limiting factor, but in any case the matter wants clearing up. An experimental investigation of the relations of "albuminous" and "exalbuminous" seeds would probably repay the trouble involved. Does any condition or set of conditions under the control of the operator exert an influence in this connection?

The mention of the early germination of seeds brings to mind the most striking instance of all—that of the tropical Mangrove, in which, as is so well known, the seed germinates on the tree, so that the young plant is extruded, and in some instances falls, from the parent free of its envelopes.

Our interest in this type of vegetation has been revived through the researches of Mr. H. B. Guppy incorporated in his recent contribution on "Plant-dispersal in the Pacific." This volume, perhaps the most important contribution to the biology of tropical plants that has appeared since the death of the lamented Schimper, is distinguished alike for its wealth of new observations and its engaging freshness of treatment. There is one suggestion of Mr. Guppy's concerning the vivipary of Mangroves which may occupy our attention for a few moments.

As a result of his studies in the Pacific and elsewhere Mr. Guppy has arrived at the conclusion that the Mangrove type of vegetation is a very ancient one, dating back to the times when climate was more uniform and moist than we know it to-day. The viviparous habit he conjectures to have been once very general, whilst to-day this primitive condition is making its last stand along the tropical shores. Traces of vivipary still occur among inland plants, such as Crinum, whilst in other cases it reappears intermittently under conditions not fully ascertained. Mr. Guppy supposes the ordinary fruiting way of plants with caducous fruits or seeds, that germinate after an interval, to have arisen by a modification of the continuous viviparous method in the sense that the seed has come to fall earlier and earlier until the stage now characteristic of practically all Spermophytes has been reached.

Piecing the data together, this seems to be the position: The earliest known seeds appear to have remained on the plant just long enough to receive their pollen; but in time, it is reasonable to suppose, the advantage of remaining longer was realised, and the fall of the seed was postponed until fertilisation was followed by the occupation of the seed-cavity by an embryo. Here in seclusion the embryo could remain until germination was convenient. Starting at the other end, our modern seed, according to Mr. Guppy, has been evolved by the gradual retention of the viviparous embryo; or, to put it in another way, the detachment of the seed has been hastened so that it falls long before germination is due.

Well, these theories fail to meet in the middle, as they should if they are to present us with an epitome of the whole seed-history. Perhaps there were troublous times in that middle epoch, so that the continuity has become obscure! Or possibly another view may be admissible of the relation of vivipary to normal seed-production. Most botanists, I take it, have been inclined to regard vivipary as the *dernier cri* in seed-history, the ultimate stage in the way of possible reproductive advance in seed-bearing methods that the higher plants have yet attained. The Mangrove process might even be conceived as the starting-point, under certain contingencies, of a whole new race of plants with life-histories complicated by fresh alternations—homologous alternations—far beyond any of which we have knowledge to-day!

Schimper and others who have given attention to the subject found no reason for regarding vivipary as other than an adaptation to special circumstances, an extreme condition that had arisen independently in several cycles of affinity. Before the contrary can be accepted a good deal of positive evidence will be needed, drawn from the non-Mangrove representatives of groups in which vivipary occurs, to show that the relationship is other than has been generally supposed. Moreover, if the viviparous habit were formerly of wide occurrence some traces of it might reasonably be expected in the fossil record. So far as can

[1] Presidential Address, Section K, Glasgow, 1901, p. 9.

be ascertained, such have not been forthcoming, nor can I hear of any record of recent Mangroves being preserved in this way. Seeds and embryos appear to be so uniform on the whole that it is difficult to understand how they could have passed through a viviparous phase in the later stages of their evolution.

The viviparous Mangroves, on the other hand, are full of diversity in detail, and these differences would surely have left a permanent mark had the course pursued been in conformity with Mr. Guppy's very interesting suggestion. That there is a rich field awaiting detailed investigation in connection with the fascinating subjects opened up by Mr. Guppy will be admitted by most naturalists.

In glancing back at the early seed-structures one is struck with the complexity of their organisation as compared with the relative simplicity of modern seeds. The pollen-chamber, the large elaborate integument, and the complicated vascular arrangements, so characteristic of the Pteridosperm seed, have for the most part passed away, giving place to much simpler structures. Occasional exceptions no doubt occur; the seeds of Palms have remarkable integuments, whilst those of Magnolia, some Aroids, Sapotaceæ, &c., show an unusual development of vascular tissue. Most astonishing of all perhaps is the integumental tracheal sheath which closely invests the nucellus of Cassytha.[1] Though evidence of their precise function be lacking, the fact that many of these structures belong to the tropical forest makes closer knowledge desirable. For in these localities the conditions must have long been relatively stable; thus increasing the chance that the structures referred to still perform their pristine functions. These and other cases like them need elucidation, but to the broad statement that the seeds of recent Spermophytes are organised on simple lines there can be no question. This reduction in complexity may be accounted for on two grounds. In the first place fertilisation by motile sperms has been replaced by fertilisation by pollen-tubes. Instead of sperms being discharged into an internal water-chamber upon which the archegonia abutted, the male cells are carried through soft tissues to the egg in a plastic tube.

In other spheres the like befalls. If primitive man had occasion to journey from Baker Street to Waterloo, he penetrated the forest and then swam the river; to-day his descendants are projected from the one to the other with accuracy and despatch in a subterranean passage.

Just at what stage the improvisation of the pollen-chamber gave place to the newer method we have no knowledge. Perhaps some information on this point may emerge from Dr. Wieland's exhaustive researches into the extensive Yale collections of American Cycadeoideas. For the Bennettiteæ already show a simplification of the seed in certain respects; though, owing to the late stages of development usually found in European examples, this point could be cleared up.

The other cause that must have played a prominent part in the simplification of the seed was the association with it of other structures which relieved it of a part of the original load of duties that fell to its lot. The dense heads of Bennettites show us this, and the same may be said of most Coniferous strobili. But the Angiospermic ovary provides the best example of a special organ inclosing the seed or ovule, affording it protection during the immature stages and also collecting the pollen. The steps by which this came about remain hidden, and any discussion of the matter is of course premature. The carpels may have been derived from reduced sporophylls or from portions of sporophylls that were more closely associated with the seeds. The cupule of Lyginodendron is an organ rather suggestive in this connection. One is tempted to compare it with a rudimentary ovary, playing the serviceable part of a moist air-chamber for the seed during the earlier stages of its development.

However, the origin of the fruit and of the flower, with all its manifold organs, must be left to the future: they form no part of our theme. Some day a happy discovery will yield a clue, and the reproach that we are in entire ignorance of the affinities of the dominant phylum will be removed.

[1] M. Mirande, "Le dévelop. et l'anat. d. Cassythacées," *Ann. d. Sc. Nat.*, 9e sér. bot., tom. ii., 1905.

The history of the seed, as I read it from the imperfect and fragmentary data that are available, has been a series of advances spread over long geological periods. The possibilities of the seed-habit were realised only bit by bit, and the high efficiency of the modern seed depends in large degree upon the close association of other structures which cooperate in its functions. No doubt the first step, the retention of the megaspore, was the most important of all; though, that this might be effective, some contrivance for the capture of the pollen-grains must have accompanied it. Later steps in the process of seed-evolution would include the adjustment of an intraseminal embryonic stage, and in time the substitution of the pollen-tube for the liberation of sperms.

Now assuming, as I think we are entitled to assume, that seeds have come into existence along some such lines as those thus crudely blocked out, there is a great difficulty in conceiving the process other than discontinuous. Every one of the stages emphasised involves the conception of something more abrupt than mere gradual variation. And there is, of course, the old difficulty confronting us as to how the organ or mechanism came to be preserved at its inception. All these difficulties vanish when it is recognised that effective variation is of the discontinuous order, and that the successive changes involved may be considerable enough to be designated jumps. Happily such views, based upon experimental results, have been formulated by De Vries in his Mutation Theory. That theory is so well known to botanists in this country that any exposition here is quite superfluous. The least thing that can be said in its support is that it is perfectly tenable. But we may go much further than that. Apart from the Theory of Natural Selection, no modern hypothesis of evolution has been so helpful or so likely to stimulate further work. The results of continued investigations in this field, now so actively pursued, will be awaited by all biologists with a keen and sympathetic expectancy. Not the least of the advantages that follow in the wake of the Mutation Theory is the shortening of the time required for the evolutionary process. As the physicist imposes a time limit to the period during which life has been possible on the earth, a working theory that reconciles the demands of the biologist with the physical limitations is decidedly reassuring. In this connection it is very interesting to note that Monsieur Grand'Eury, one of the most active and distinguished workers in the field of palæobotany, should have found data supporting the view of mutation.[1] In tracing the passage of fossil plants through great thicknesses of rock he has been impressed on the one hand with the high degree of permanence of certain forms, and on the other with the suddenness, when the moment came, with which one species passes into another.

The collection of data of this kind from our own Coal Measures appears to me a very pressing necessity in view of the rapidity with which the coalfields are being exhausted. Indeed, the present is an unique opportunity which can never recur, and the chance of systematically utilising it is slipping away. Whatever view one may hold as to the expediency of making exhaustive collections of the recent flora, there can be no two opinions of our manifest duty to "make hay while the sun shines" in the matter of the coal fossils. Regarded as systematically arranged collections showing how the plants occur in definite localities, the contents of most of our museums, as I am assured by competent authorities, are practically worthless. That innumerable specimens of the greatest value are preserved in museums may be readily conceded; but my point is that these collections have been made without system, and that details of precise locality and horizon are frequently wanting. All this has to be done over again, and I believe local societies working in touch with a central organisation could do a memorable service which would earn them the gratitude of future generations and at the same time provide a fresh outlet to their energies.

To us the coal industry, with its vast resources, is a convenient mechanism for making fossil plants accessible. The colliery proprietor may be relied on to afford all reasonable facilities for the acquisition of select examples

[1] Grand'Eury, *Comptes rendus*, tom. cxlii p. 25.

from these superabundant and embarrassing waste products. Should he incline to go further and contribute towards the modest funds necessary to carry out the undertaking worthily, he would increase the debt which science owes to industry. The thousandth part of the revenue arising from the export tax on coal would amply suffice for the purpose. Indeed, I can think of no more appropriate way of celebrating the abolition of that burdensome impost.

If I have dwelt to-day on the seed to the exclusion of other features, it is because I am convinced of its supreme importance. The evolution of the seed must have been one of the most pregnant new departures ever inaugurated by plants. The revelations of the last few years afford us, it is true, but the merest glimpse of the first stage reached, the rise of the Pteridosperms. The conquest of the world must have been slow then as it is now. The great forests of Lepidodendrons and Calamites were not reduced to mere Lycopodiums and Equisetums all at once. In this prolonged struggle, even if the Lycopods never produced a race to share the spoils, as some suppose, there is the evidence of Lepidocarpon that their reproductive methods underwent a certain if ineffectual modification in the same direction as their eventual supplanters. Probably the seed plants asserted themselves wherever physical changes overwhelmed old habitats. The rise and fall of the land, so great a feature in Carboniferous times, would favour the younger group. For as new ground became available for colonisation there would be opportunity of competing on at least equal terms with the effete types that cumbered the forest land. Nor should we forget that the seeds were well equipped with dispersal-mechanisms almost as varied as they are to-day.

A somewhat similar struggle is now in progress between the Angiosperms and Gymnosperms, but so slowly that we hardly notice it. A future age may have to be content to know its Gymnosperms from dwarf forms like those which the Japanese are so fond of producing in their pot-cultivations! But perhaps all calculations will be upset by the more effective intervention of the human race. On present indications the vegetation of the future should consist of cultivated crops and the weeds that accompany them; that is, unless the Chemist comes to our aid and solves the problem on other lines.

Botany in England.

I now turn to other matters. The period of twenty-five years that has elapsed since the British Association last met in this City all but includes the rise of modern botany in this country. During the middle decades of last century our botanists were preoccupied with arranging and describing the countless collections of new plants that poured in from every quarter of an expanding empire. The methods inculcated by Linnæus and the other great taxonomists of the eighteenth century had taken deep root with us and choked out all other influences. Schleiden's "Principles of Botany," which marked a great awakening elsewhere, failed to arouse us. The great results of Von Mohl, Hofmeister, Nägeli, and so many other notable workers, which practically transformed botany, were at first without visible effect.

It was not that we were lacking in men capable of appreciating the newer work. Henfrey, Dr. Lankester (the father of our President), not to mention others, were continually bringing these results before societies, writing about them in the journals, and translating books. But the thing never caught on—it would have been surprising if it had. You may write and talk to your contemporaries to your heart's content, and leave no lasting impression. The schools were not ready. No movement of the sort could take root without the means of enlisting the sympathies of the rising generation. It was only in the 'seventies that effective steps were taken to place botany on the higher platform; and the service rendered in this connection by Thiselton-Dyer and Vines is within the knowledge of us all. Like the former in London, so the latter at Cambridge aroused great enthusiasm by his admirable courses of lectures. Great service, too, was rendered by the Clarendon Press, which diffused excellent translations of the best Continental text-books—a policy which it still pursues with unabated vigour, though the need of them is, I hope, less urgent now than formerly. Already at the time of the last meeting in York (1881) a select band of Englishmen were at work upon original investigations of the modern kind. The individuals who formed this little group of pioneers in their turn influenced their pupils, and so the movement spread and grew. It would be premature to enter fully into this phase of the movement, so I will pass on with the remark that modern botany was singularly fortunate in its early exponents.

Whenever the history of botany in England comes to be written, one very important event will have to be chronicled. This is the foundation of the Jodrell Laboratory at Kew, which dates from the year 1876. Hidden away in a corner of the Gardens this unpretentious appendage of the Kew establishment has played a leading part in the work of the last twenty-five years. Here you were free to pursue your investigations with the whole resources of the Gardens at your command. I suppose there is hardly a botanist in the country who has not, at some time or other, availed himself of these facilities, and who does not cherish the happiest memories of the time he may have spent there. Certainly Jodrell displayed rare sagacity in his benefactions, which included, in addition to the laboratory that bears his name, the endowments of the Chairs of Animal Physiology and Zoology at University College, London.

Sir William Thiselton-Dyer, who has so recently retired from the Directorship of Kew, had every means of knowing that his happy inspiration of founding a laboratory at Kew was a most fertile one. It would not be surprising if the future were to show that of the many changes inaugurated during his period of service this departure should prove by far the most fruitful.

Another incident belonging to the early days ought not to be overlooked: I refer to the notable concourse of Continental and American botanists at the Manchester meeting of the British Association in 1887. The genuine interest which they evinced in our budding efforts and the friendly encouragement extended to us on that occasion certainly left an abiding impression and cheered us on our way.

We are not forgetful of our obligations. We regard them in the light of a sort of funded debt on which it is at once a pleasure and a duty to pay interest. The dividends, I believe, are steadily increasing—a happy result which I am confident will be maintained.

But I should be lacking in my duty did I permit the impression to remain that botany is anything but a sturdy and natural growth among us. The awakening, no doubt, came late, and at first we were influenced from without in the subject-matter of our investigations. But many lines of work have gradually opened out, whilst fruitful new departures and important advances have not been wanting. We still lean a little heavily on the morphological side, and our most urgent need lies in the direction of physiology. As chemists and physicists realise more fully the possibilities of the "botanical hinterland," one may expect the conventional frontier to become obliterated. As Mr. F. F. Blackman has pointed out in a recent interesting contribution,[1] the chemist's point of view has undergone a change with the growth of the science of physical chemistry, and is now much more in line with that of the biologist than was formerly the case. This natural passage from the problems of the one to those of the other should be the means of attracting into our body recruits possessing the necessary chemical equipment to attack physiological problems.

As the position gains strength on the physiological side, it will become possible to render more effective service to agriculture and other branches of economic botany.

This is of importance for a variety of reasons. Among others it will bring public support and recognition which will be all for good, and it will provide an outlet for our students. It will also afford unrivalled opportunities for experiments on the large scale. Even should economic conditions, which compel us to import every vegetable product, continue to prevail in this country, this will not be so in the Colonies. As time goes on, one may reasonably expect an increasing demand for trained botanists, ready to turn their hands to a great variety of economic problems.

1 "Incipient Vitality," *New Phytologist*, vol. v. p. 22.

From this rough sketch we see that the prevailing school of botany has arisen very independently of that which preceded it. The discontinuity between them you might almost call abrupt. All through the middle parts of the last century we were so busy amassing and classifying plants that the great questions of botanical policy were left to solve themselves. Great herbaria became of the order of things: they received Government recognition, and they continue their work apart. Those who built up these great collections neglected to convince the schools of the importance of training a generation of botanists that would use them. The schools were free, and they have gone their own way, and that way does not lie in the direction of the systematic botany of the herbarium. So long as this tendency prevails the herbaria must languish. When I say languish, I do not mean that they will suffer from inefficient administration—their efficiency probably has never been greater than at the present time. But the effort involved in their construction and upkeep is altogether disproportionate to any service to which they are put. Work, of course, comes out of them; it is no question of the devotion or ability of individuals. It is the general position, the isolation of systematic botany, to which attention should be directed with a view to its alleviation.

If things are left to take their course there is the fear of atrophy through disuse. The operation of the ordinary economic laws will no doubt serve to fill vacancies on the staff as they arise, but the best men will be reluctant to enter. Of course the pendulum may begin to swing the other way, though no indication of such a change is yet apparent.

Let us now attempt an analysis of some of the causes which have led to this condition of affairs.

In the first place, our two national herbaria (Kew and the British Museum) stand apart from the ordinary botanical current. They are administered, the one as a portion of the Kew establishment under the Board of Agriculture, the other as a department of the British Museum under a Board of Trustees. Neither has any connection, direct or indirect, with any university organisation. The Keepers and Assistants as such have no educational functions allotted them; I mean positions in these herbaria carry no teaching duties with them. There are no facilities for teaching; there are no students. No machinery exists for training recruits or for interesting anybody in the ideals and methods of systematic botany. A recent event illustrates my meaning better than any words. My friend Dr. Rendle accepted the Keepership of the Botanical Department at the British Museum a few months ago. Previously, as Assistant, he had held a lectureship at a London college. One of the first consequences of his new appointment was his retirement from the teaching post. Now that was bad. Under the conditions which one would like to see there would have been no resignation. On the contrary, the Keepership should have entitled Dr. Rendle to promotion to a full professorship. I do not mean a great post, with elementary classes, organisation, and so on, but one in which he would be occupied with his own branch, giving a course for advanced students, let us say, once a year during the summer months. Nor is that all. Such are the vagaries of our university organisation in London that we run some risk of losing Dr. Rendle from the Board of Studies in Botany. Automatically he ceases to be a "recognised teacher," and unless some loophole can be found the connection will be severed.

Next we come to the question of routine duties. These are heavy in herbaria, and must include a great many that could be satisfactorily discharged by handy attendants. As in the case of those who work in laboratories, half a man's time should be at his own disposal for original investigations. It is important, for a variety of reasons, that the members of the staff should take a leading part in advancing systematic botany.

Then there is another way in which a great economy could be effected in effort, time, and money. This is the transfer of the collections and staff of the Botanical Department from the Museum to Kew. This is a very old proposal, first seriously entertained some fifty years ago after the death of Robert Brown. There must be endless files of reports and Blue Books in official pigeon-holes dealing with this question. The most recent report of a departmental committee is known to all interested in the matter. From the character of the evidence tendered it is not surprising that no action has been taken. I am at a loss to find any adequate reason for the continuance of two separate herbaria. It has been urged, no doubt, that botany would suffer if unrepresented in the Museum collections at South Kensington, and that the dried collections and herbarium staff are a necessary adjunct to the maintenance of a botanical museum. But there is little force in the contention. The specimens that go to make a herbarium are not proper subject-matter for museum display; nor is there anything about herbarium work which intrinsically fits the staff to engage in the arrangement of museum cases. The function of a botanical museum is to interest, stimulate, and attract. It should convey an idea of the current state of the science, and particularly of the problems that are to the front, in so far as it is possible to illustrate them. It requires a curator with imagination and ideas, as well as an all-round knowledge of his subject. He must also be an artist. Logically there is no reason why a museum should be part of the same organisation as systematic collections. There is, indeed, a danger of making the museum too exhaustive. I am speaking, of course, of a teaching museum, which belongs really to the province of a university, or university extension if you like. Systematic collections kept exposed under glass are luxuries. All the world agrees that the museum side is admirably done at South Kensington, and most people attribute this success to the systematic element which is paramount behind the scenes. But, as we have seen, this is a fallacy, and the "museum argument" for keeping the herbarium at South Kensington may be ignored.

By the fusion of the herbaria at Kew one would look for increased economy and efficiency, more time for original work as distinguished from routine duties, and a more complete specialisation.

We now approach another aspect of the question. Much has been said on the value of anatomical characters in classification, and it is pretty generally conceded that they ought to be taken into consideration, though, like other characters, they are beset with their own special difficulties. As Dr. Scott—who has always urged their importance—says:[1] "Our knowledge of the comparative anatomy of plants, from this point of view, is still very backward, and it is quite possible that the introduction of such characters into the ordinary work of the herbarium may be premature; certainly it must be conducted with the greatest judgment and caution. We have not yet got our data, but every encouragement should be given to the collection of such data, so that our classification in the future may rest on the broad foundation of a comparison of the entire structure of plants." This passage was written ten years ago and we are still awaiting its realisation.

It is perfectly true that in the case of a recent proposal to found a new natural order of flowering plants anatomical characters find due consideration; still, on the whole, we are content to rely on the traditional methods that have been transmitted from Linnæus and the old taxonomists. So much material is always passing under the hands of our systematists that they cannot devote the time for the elaboration of a fresh method. In particular there are the new things which require docketing and provisional description. Circumstances, as ever, place obstacles in our way and tend to make us unprogressive.

Now it seems to be of the first importance that reform should come from within; that these problems, which are systematists' problems, should be solved by taxonomic specialists.

I am sanguine enough to believe that much might be done by a redistribution of duties, especially if this were accompanied by the fusion of the great herbaria, to which reference has already been made. But the greatest hope, I think, must lie in the possibility of some form of alliance or understanding between the authorities responsible for the administration of the herbaria on the one hand and the local university on the other. For directly you give the Keepers or Assistants in the former a status in the latter, you place at the disposal of the systematists a considerable

1 D. H. Scott, Presidential Address, Section K, Brit. Assoc. (1896).

supply of recruits in the form of advanced students possessing the requisite training to carry out investigations under direction. And if this be true of the herbaria, it holds equally in all the branches of knowledge represented in the National Museum. Really I fancy our Museum is rather anomalous in its isolation. In am confident that any understanding or arrangement that might be reached would be attended with great reciprocal advantage. Nor am I speaking without some data before me. The movement towards a closer relation between the museum and the university has already entered the experimental stage. For on several occasions during the last few years members of the Museum staff, from more than one department, have given courses of lectures in connection with the university schemes of advanced study. From all I hear, the experiment may be regarded as distinctly encouraging.

Before leaving this subject it may be appropriate to recall that the English edition of Solereder's great work on Systematic Plant-anatomy is rapidly approaching completion, and should be available very shortly. Its appearance cannot fail once more to arouse discussion as to the importance of anatomical characters. I hope the result produced may reward the devotion and labour with which Mr. L. A. Boodle and Dr. Fritsch have carried out their task.

In another and even more fundamental branch of systematic work the future seems brimful of promise. We are beginning to recognise that a vast number of the species of the systematist have no correspondence with the real units of nature, but are to be regarded rather as subjective groups or plexuses composed of closely similar units which possess a wide range of overlapping variability. That such might be the case was apparent to Linnæus, but the proof depends on the application of precise methods of analysis.

In the year 1870 our great taxonomist Bentham happened to meet Nägeli at Munich, and, as we find recorded in Mr. Daydon Jackson's interesting life, "had half an hour's conversation with him on his views that in systematic botany it is better to spend years in studying thoroughly two or three species, and thus really to contribute essentially to the science, than to review generally floras and groups of species." Bentham does not appear to have been convinced, for his comment runs: "He is otherwise, evidently, a man of great ability and zeal, and a constant and hard worker." At the time of this interview Bentham was seventy years old, Nägeli being seventeen years his junior. The views of the latter are now bearing fruit, as we see in the important results already obtained by De Vries and others, who are following the methods of experimental cultivation with so much success.

The supposed slowness of change has been a difficulty to many. This was one of the "lions" left by Darwin in the way, and it has driven back many a "Timorous" and "Mistrust." Now, as we are gradually perceiving, it is only a chained lion after all; a thing to avoid and pass by. The detection of the origin of species and varieties by sudden mutation opens out new vistas to the systematist, and along these he will pursue his way. It will take many years of arduous work this reinvestigation of the species question. The collections of our herbaria form the provisional sorting-out from which we must start afresh. In the long run it may be that our present collections will prove obsolete, but that will not deter us. The scrap-heap is the sign and measure of all progress.

The Garden thus becomes an instrument of supreme importance in conjunction with the herbarium, and that is another reason for the transfer of South Kensington to Kew. The resources of the latter could then be directed more fully than ever to the advancement of scientific botany, and the Gardens would be revealed in a new light. For the operations and results of experimental inquiries would form a new feature, very acceptable to the specialist and public alike. And, as I am on the subject, it may not be out of place to remark that we all look forward eagerly to the time when the multifarious activities of Kew will permit the development of other features of which traces are already discernible. The arrangement of the living collections is at present based largely on horticultural convenience, geographic origin and systematic affinity, happily subordinated to an artistic or decorative treatment. In time we shall go further than that and attempt in some degree to reflect current botanical ideas in the grouping of our plants. Let me illustrate my meaning by a good example. The Succulent House is generally conceded to form one of the most interesting and stimulating exhibits to be seen at Kew—not merely from the weird and grotesque forms assumed by the individual plants, but chiefly because here you have assembled together plants of the most varied affinity having the common bond of similar adaptations to a like type of environment. The principles that underlie the arrangement of the best sort of museum may be applied with advantage in the case of a garden, and with tenfold effect; for is not a live dandelion better than a dead Welwitschia? This feature, introduced as it would be with moderation and discretion, would immensely enhance the value of the Gardens both to the student and general visitor.

But to return from this digression: on the whole the time seems ripe for the new departure. Fresh lines are opening up in systematic botany that call for special provision. Now it was evident from the circumstances of the botanical renaissance twenty-five years ago that when it acquired strength some readjustment between the old and the new would have to be made. The thing was inevitable. The administrative acts of recent years all point in the same direction. The founding of the Jodrell Laboratory, the enhanced efficiency of the Gardens, the great extension of the Herbarium building, all help to pave the way. But more is wanted. Reference has been made to the advantages that would attend the migration from the Natural History Museum. But it is most important of all to devise a mechanism for securing a flow of recruits to carry on the work. This would follow in the wake of a *rapprochement* with the schools on the lines already sketched out. Difficulties, no doubt, will be encountered in the initial stages of a reorganisation, but these are inseparable from our bureaucratic system. A very hopeful sign is the readiness which the Government has shown in instituting inquiries in the past. That nothing has come of them may be attributed primarily to the attitude of botanists themselves. If they can unite on any common policy, there should be no serious delay in giving it effect.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE resignation of Dr. A. E. Dolbear, professor of physics at Tufts College since 1874, is announced.

DR. KUNO FISCHER has resigned the professorship of philosophy at the University of Heidelberg in consequence of ill-health.

SIR WALTER LAWRY BULLER, F.R.S., has left on trust 1000*l.* to found a Maori scholarship, to be called the Buller scholarship, tenable by Maoris, but not by Europeans or half-castes.

DR. A. G. RUTHVEN, who is at present collecting reptiles and studying their field relations for the American Museum of Natural History, has been appointed curator of the museum of the University of Michigan.

THE Physical Society, Frankfurt a. M., has fitted up an electrotechnical instructional and experimental institution in which young people after finishing their apprenticeship may go through a further course in order to qualify themselves as works managers, &c.

DR. J. K. H. INGLIS, of University College, London, has been appointed principal lecturer in chemistry at University College, Reading; and Mr. F. J. Cole, of the University of Liverpool, has been appointed principal lecturer in zoology at the same institution.

PLANS are being prepared for a building for operative surgery and experimental pharmacy, and for the new university hospital in connection with the college of medicine and surgery, the University of Minnesota, this having been made possible by the recent bequest by Dr. A. F. Elliott of 30,000*l.*

THE Austrian Government has sanctioned the granting of the title of "Doktor der Bodenkultur" to be conferred upon those students of the Vienna High School for Agri-