THURSDAY, SEPTEMBER 12, 1901.

## CARNAC AND STONEHENGE.

(1) *The French Stonehenge: an Account of the Principal Megalithic Remains in the Morbihan Archipelago.* By T. Cato Worsfold, F.R.Hist.S., F.R.S.L. Pp. 44. (London: Bemrose and Sons, Ltd. No date.) Price 5*s*.

(2) *A Sentimental and Practical Guide to Amesbury and Stonehenge.* By Lady Antrobus. (Salisbury: Brown and Co., 1901.)

MR. WORSFOLD'S book, though the reason for its title, "Stonehenge," is not very apparent, gives a popular and interesting account of the wonderful megalithic works at Carnac, in Brittany. Those who desire to enter more deeply into the subject should consult "Les Alignments de Kermario," par James Miln Rennes (1881), a work which our author appears to have well studied, and "L'Astronomie Préhistorique," par F. Gaillard, dans la *Révue Mensuelle Internationale des Sciences Populaires* (15 Rue Lebrun, Paris).

Let us, however, accompany Mr. Worsfold in the slighter introduction which he gives. A lucid glance at the inhabitants of the district, which, with the peninsula of Quiberon, is part of the Department of Morbihan, is given in "Cæsar's Commentaries" (De Bello Gallico, iii. 3), describing his naval engagement with the Veneti, who seem to have been assisted by allies from Britain (Cæs., iv. 20). From this account it appears that the Veneti had evidently attained to no inconsiderable height of civilisation, as particularly shown in the construction and fittings of their ships. Our author adds the tradition that after their defeat the Veneti sailed away to the Mediterranean and founded the city of Venice. A description of Carnac follows.

The three great heads into which the megalithic remains may be divided are:—

(*a*) Menhirs, or single stones, in most cases upright, but occasionally overthrown. These, when they appear in circles, are called cromlechs.

(*b*) Dolmens, from Dol Men, a table stone consisting of a flat stone resting on two or more upright stones having subdivisions, "Dolmen a galérie" having an entrance way of sufficient height, and "Galgal," similar but smaller, "Dolmen à l'allée couverte" and "Kist-vaen," which should, indeed, be under another head, meaning stone coffins hewn out of one block.

(*c*) Alignments, which are lines of menhirs. These form the chief objects near Carnac. The finest dolmens are near Locmariaquer, a village about eight miles distant.

A large and lofty tumulus near Carnac, named Mont St. Michel, from a church so dedicated, which has been built upon it, has yielded on excavation many prehistoric relics; and near its base was found the remains of a Roman villa with Celtic or pre-Celtic relics arranged on shelves, showing that its Roman owner had been somewhat of an antiquarian. The name of the place is supposed to be derived from this tumulus or *cairn*. That this tumulus may have been connected originally with sun worship may be argued from the fact that the annual custom prevails of lighting a large bonfire on its summit at the time of the summer solstice, which is the signal for others to be kindled on similar prominent eminences for a distance of twenty or thirty miles round. These fires are called in the local patois "Tan Heol," and also by a later use, Tan St. Jean.

This practice prevailed also in Scotland under the name of Bel Tan, or Baal's Fire; the synonym for summer used by Sir Walter Scott in the "Lady of the Lake":—

> Ours is no sapling chance-sown by the fountain
> Blooming at Beltane in winter to fade.

The alignments of Carnac consist, firstly, of those known as "Le Menec," signifying *the place of stones*, or, by another interpretation, *the place of remembrance*. The next series are those of Kermario, or *the place of the dead*. The third series, Kerlescant, or *the place of burning*.

The alignments of Menec consist of eleven lines of menhirs, terminating towards the west in a cromlech, and notwithstanding that great numbers have been converted to other uses, still contain 1169 menhirs; some, however, do not exceed 18 inches in height, but others reach as much as 18 feet.

The alignments of Kermario in ten lines contain 989 menhirs. That of Kerlescant, which beginning with eleven rows is afterwards increased to thirteen, contains altogether 579 stones and thirty-nine in its cromlech, with some additional stones in a northerly direction. Each of these three alignments has its own orientation, and the large menhirs at the ends are supposed by some persons to mark the rising and setting of the sun, in some cases at the equinoxes and others at the solstices. In connection with this it is interesting to state that at Kerlescant the winter solstice is celebrated by a holiday, whilst Menec greets the summer solstice, and Kermario the equinoxes, with festivals. It appears that the adoration paid these stones remained strongly rooted for many centuries and yielded very slowly to Christianity. This is shown by the constant denunciations made against it in different countries. In the church history of Brittany in particular, the *Cultus Lapidum* was denounced in 658 A.D., and a decree was passed at Nantes that trees so worshipped should be torn up and burnt and stones cast down and hidden from those who sought to do them reverence. Indeed, the author quotes M. de Fréminville, who writes:—

> "On sait qu'au dix-septième siècle même l'idolâtrie était encore exercée dans l'isle d'Ouessant [*i.e.* Ushant] et dans plusieurs paroisses de l'évêché de Vannes." [Vannes is about twenty miles distant from Carnac.]

In connection with this it is stated that some years since a large number of these menhirs were lying prone on the ground exactly due north and south, and were subsequently restored to their original position by the French Government. These stones may have been overturned in compliance with the decree of 658 A.D. above referred to. Several of the loftier menhirs have been surmounted by crosses of stone or iron, so as to convert, at any rate, the appearance of veneration into an orthodox channel.

After a digression upon Stonehenge, presumably for the purpose of justifying the title of the book, but in which no very apparent analogy is pointed out, he cites Avebury, where it may be admitted that the character of the stones, but not the arrangement, is more suited for comparison. He returns to Morbihan and describes the monuments near Locmariaquer, and especially the "Dolmen des Marchands," about two miles from the village, where a granite block about 36 feet in length is poised in a horizontal position upon three others 16 feet high, the under surface being carved in a curious manner with undulating grooves and with hieroglyphics of axes interspersed. Then follows the account of a huge stone now lying on the ground which has been broken into five pieces, but presumably was once erect and stood 78 feet in height and 13 feet across at the base, weighing about 340 tons.[1]

Near Locmariaquer in the estuary named Riviere d'Auray, there is an island named Gavr' Inis, or Goat Island, which contains a good specimen of the kind of dolmen which has been named "Galgal."

"At the entrance our attention is at once arrested by the profusion of tracery which covers the walls. From the entrance to the wall facing us the distance is between 50 and 60 feet. The square chamber to which the gallery leads is composed of two huge slabs, the sides of the room and gallery being composed of upright stones, about a dozen on each side. The mystic lines and hieroglyphics similar to those above mentioned appear to have a decorative character."

An interesting feature of Gavr' Inis is its remarkable resemblance to the New Grange tumulus at Meath. In construction there is again a strong resemblance to Mæs-Howe, in the island of Orkney. There is also some resemblance in smaller details.

In excavations near Carnac and Locmariaquer, many curious prehistoric implements and ornaments have been discovered, which are preserved in the Miln museum at Carnac and in a museum at Vannes.

The main purpose of Lady Antrobus' sentimental and practical guide is to give a popular account of the interesting objects in the neighbourhood of Stonehenge, and especially of Stonehenge itself, and in this it succeeds admirably. The sentimental part is very well worth reading, but it is more in accordance with the intention of this article to proceed to the practical part, which is a very useful *résumé* of the chief authorities on the subject, and it is illustrated by some well-chosen photographs. The account begins with a translation from Diodorus Siculus, who lived about B.C. 8.

Hecatæus, the Milesian (who lived about 500 years B.C.), gives us the following story :—

"Over against Gaul, in the great ocean stream, is an island not less in extent than Sicily, stretching towards the North. The inhabitants are called Hyperboreans. It is said that the soil is very rich and fruitful, and the climate so favourable that there are two harvests in every year. There is in this island a magnificent temple to Apollo, circular in form, and adorned with many splendid offerings; and there is also a city sacred to Apollo, inhabited principally by harpers who in his temple sing sacred verses to the God. . . . Once in nineteen years (and this period is what we call the great year) they say that their God visits the island, and from the Vernal Equinox to the rising of the Pleiades (about May 1) all the night through expresses his satisfaction by dances and by playing on the harp."

The first author who is considered to make unmistakable mention of Stonehenge is Henry of Huntingdon (twelfth century). He speaks of it as the second wonder in England, and calls it Stanenges. Geoffry of Monmouth (A.D. 1138) wrote of it about the same time, as did his contemporary, Giraldus Cambrensis.

Langtoft, in his chronicle, tells a curious story :—

"A wonder wit of Wiltshire, rambling to Rome to gaze at antiquities, and there screwing himself into the company of antiquarians, they intreated him to illustrate to them that famous monument in his country called 'Stonage.' His answer was that he had never seen it, whereupon they kicked him out of doors and bade him go home and see Stonehenge."

Pepys says the stones are "as prodigious as any tales I have ever heard of them, and worth going this journey to see."

Mr. E. S. Maskelyne, in a lecture read 1897, called "The Age and Purpose of Stonehenge," fixes its date as 900 or 1000 B.C. (a date which seems sufficiently nearly confirmed by recent researches). Mr. Maskelyne proceeds :—

"I should like to add some reasons for my belief that Stonehenge was built by the Phœnicians. In the first place I cannot think of any other people that could either have designed or executed such a monument, which required both science for its conception and skill for its erection. The Phœnicians, with their familiarity with masts, cordage and pulleys could easily lift the imposts, and they must have known how the Egyptians raised masses of stone many times heavier.

"The trilithon standing clear seems to have had some fascination for the Phœnicians. They are found still standing in Tripoli in Libya, and specimens exist on the continent of Europe in Normandy and Brittany. One may be seen in the island of Ushant and another in St. Nazaire on the probable route they adopted for the carriage of tin. . . .

"About B.C. 400 the Greeks supplanted the Phœnicians in their trade with Britain. . . . Stonehenge must have been a noted temple, and I cannot doubt that Hecatæus did allude to it in the sixth century B.C. as the round temple of Apollo in the land of the Hyperboreans.

"As to the kinds of stone employed in the building, the whole of the outer circle and the four stones beyond that circle are undoubtedly 'Sarsen,' which are boulders left by the ice-sheet of the Glacial period on the Wiltshire Downs. In the inner circle are four stones of *whinstone*, an impure ironstone; the remainder are syenite, commonly called bluestones, and identical with those found on Dartmoor and many parts of Devon and Cornwall, the *altar-stone* being a kind of coarse blue marble, perhaps from Derbyshire."

Stonehenge stands about 440 feet above the sea-level. The outer circle measures 308 (330 externally). These stones formerly stood 14 feet above the surface of the ground. The uprights are unhewn, but have knobs or tenons on the top which fitted into mortise-holes on the underside of the horizontal stones, which were roughly squared.

Within this peristyle was the inner circle, composed mainly of unhewn syenite obeliscs, and then the great ellipse, formed of five (but some think seven) huge trilithons, which rose progressively in height from N.E. to S.W., the loftiest uprights being 25 feet above the ground. Of these remain two perfect trilithons and two of the upright stones, but of these one is much inclined from the vertical. The fall of one of the trilithons took place in A.D. 1620, owing to some injudicious excavations

[1] 240 in the text, but from other accounts this must be an error.

of the then Duke of Buckingham. Another fell in 1797, owing to the action of the weather upon its foundations.

The entrance to Stonehenge faced the N.E., and the road to it, or "Via Sacra," called the Avenue, can be traced by banks of earth which fall into those of the circumscribing circle of the earth bank which surrounds the whole structure, and which has a diameter of about 200 feet.

Prof. W. M. Flinders Petrie's account is cited as arriving at a date very much later than that given by Mr. Maskelyne, viz. as between 500 and 900 A.D. !

Lady Antrobus' book concludes with an account of the objects met with on the picturesque road which leads from Amesbury to Salisbury along the banks of the Avon.

## OUR BOOK SHELF.

*Polyphem ein Gorilla.* By Dr. Th. Zell. Pp. 184. (Berlin: W. Junk, 1901.) Price Mk. 2.50.

A BOOK of nearly two hundred octavo pages of close print, discussing the subject above mentioned, may, without exaggeration, be termed exhaustive. It should be at once added, however, that in his preface the author refers expressly to those of the fifteen chapters (viz. Nos. xiii and xiv) which contain the essential arguments in support of his views. Otherwise the reader should be prepared to find himself involved in the consideration of the behaviour of animals in relation to changes in the weather, or engaged in the study of the relative keenness of the special senses of the walrus. The effects of meteorological changes on animals are discussed in reference to the story, not of Polyphemus, but of Proteus, in the course of a general disquisition on the interpretation of Homeric myths, while the subject of the special senses appears in subtle connection with important questions raised by the peculiar cyclopian eye of Polyphemus. The author's enthusiasm and his desire to examine all sides of the question have led him to burden his work with a large amount of detail, which could have been largely avoided, without diminishing the value of the book, by the substitution of reference for quotation *in extenso*.

The contributions to Homeric literature consist, firstly, in the exposition of the view expressed in the title of the book, viz., that the story of Polyphemus is not, as Grimm and others believe, a mystic account of the strife of the elements or a solar myth, but rather the reminiscence of an encounter of early civilised man with a pre-human ancestor. To this ancestor Dr. Zell prefers to refer as a "gorilla-mensch," reminding one of Winwood Reade's implied suggestion that Caliban was a gorilla.

In the second place, Dr. Zell suggests that the term cyclopian indicates that the person or animal so designated had simply eyes of rounded appearance and was not necessarily the possessor of a single median organ of sight.

While agreeing with Dr. Zell that the explanation of the story of Polyphemus is to be sought in the actual adventures of early voyagers, rather than in an appeal to the unaided inventive faculty of a poet, it is thought that the term "gorilla-mensch," as well as the title of the book, are distinctly unfortunate, as tending to revive the now discarded view that the particular ape in question should be regarded as figuring in the line of human ancestry. If the large gorilla mentioned on p. 112 is that which has been recently represented in certain publications in England and Germany, it is thought that Dr. Zell has been misled; for the attitude of the specimen referred to is not warranted by the structural anatomy of the gorilla.

With regard to the explanation of the term κύκλωψ, it must be admitted that this is a subject for inquiry on the part of philologists rather than students of natural science. Two remarks may be made here. The ordinarily accepted significance is one of very ancient standing. At the same time it is far more essential to Grimm's explanation than to that of Dr. Zell. W. L. H. D.

*The Evolution of Consciousness.* By Leonard Hall, M.A. Pp. 152. (London: Williams and Norgate, 1901.) Price 3s. net.

THIS is one of those well-meaning but futile books which it is almost impossible to criticise. To write a history of the evolution of consciousness an author should be thoroughly well informed of the latest results in both psychology and physiology. Mr. Hall seems to depend for his knowledge of the two sciences principally on the late J. S. Mill, with an infusion of Mr. Herbert Spencer. His account of psychological development is, no doubt unconsciously, entirely at variance with the results which have been won in recent years by careful experimentation, especially in the important domains of animal psychology, the analysis of spatial perception and the investigation of the processes by which meaning is acquired. The physiological explanations in which the writer indulges most frequently amount to nothing more than the reiteration of the blessed words "integration" and "differentiation." His grand thesis is that human consciousness is the property of a dominant cell or monad, but he seems not to be aware of the practical dethronement of the cell by the neuron as the unit of nervous action, nor does he offer any valid reason for his belief that the sub-cortical and medullary cells have a minor consciousness of their own. The actual "transference of consciousness" from one cell to another of which he talks freely is, of course, nonsense. Like most writers whose knowledge of psychology is of the same kind as his own, he is a very dogmatic and determined adherent of the merely mechanical theory of human action.

*The Self-Educator in Chemistry.* By James Knight, M.A., B.Sc., F.C.S., F.G.S., F.E.I.S. Edited by John Adams, M.A., B.Sc. Pp. xxiv + 162. (London: Hodder and Stoughton, 1901.) Price 2s. 6d.

THE intention and hope of the series to which this book belongs is that "the most isolated student will be able, without other aid, to ground himself in the various subjects dealt with." It is much to be doubted whether Mr. Knight's book will achieve any such purpose. Grounding in a subject is usually held to mean the laying down of substantial foundations, whereas this book is calculated rather to give superficial and miscellaneous information. It will give the reader no idea of the methods by which the principles of chemistry have been established, how chemical knowledge grows, or how chemists work and think. Within the first four pages the reader is introduced to atoms and molecules, graphic formulæ and the mysteries of the nascent state, whilst on the fifth he is told, "the statement that the atomic weight of oxygen is 16, means that a cubic inch, say, of oxygen is 16 times as heavy as a cubic inch of hydrogen. The atomic weight of substances like copper and carbon, which are not gases at all, are got in a more round-about fashion."

The most that can be said for the book is that it aims at showing chemistry in its relation to the things and phenomena of daily life. But it is neither thorough nor accurate, and doubts must arise as to the claims of a populariser who, besides propounding theory in the manner illustrated above, gets so far wrong in matters of fact as, for example, to state (p. 42) that hydrogen is liberated when steam is passed over red-hot copper, and that water gas and producer gas have the same composition. A. S.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### Density and Figure of Close Binary Stars.

WHEN one star revolves round another in an orbit of which the plane is coincident, or nearly so, with the line of sight, an eclipse of one star by the other will take place every revolution. We have, in such circumstances, the well-known phenomenon of Algol variation.

It is evident that the duration, extent, and nature of the eclipse will depend upon the size and brightness of the component stars, and on the magnitude, inclination, and eccentricity of the orbit in which they move.

In practice, of course, the problem is the indirect one of determining the elements of orbital movement of the system from the observed variation in the star's brightness. That is to say, having ascertained, either photometrically or photographically, the manner in which the light of the star varies, we determine the physical conditions which have produced this variation.

There is no field of astronomical research in the present day so interesting, or so rich in future possibilities, as that which deals with close binary stars. We have only to instance the recent discovery, by Prof. W. W. Campbell, of the Lick Observatory, that at least one star in every five or six is a spectroscopic binary (*Astrophysical Journal*, vol. xiii. p. 89), as an indication of the vastness of the field.

If Prof. Campbell's estimate of the number of binary systems be correct, then there ought, on the consideration that such systems may revolve in any plane, to be at least 800 Algol variables brighter than the ninth magnitude. At present only twenty-two such systems are known to astronomers.

Then, again, the certainty that in variable stars of the Algol type—that is, binary stars revolving round one another almost or *actually in* contact—we have the first stage in the evolution of a stellar system, gives a unique interest to any investigation, whether spectroscopic or photometric, which has as its purpose the delineation of the conditions of magnitude and movement of such systems.

Of the many problems intimately related to a determination of the elements of orbital movement of any close binary system, two are, I think, of peculiar interest, as bearing directly on the evolution of such systems.

(1) When we have ascertained the size and brightness of the component stars of any system, and also the form, position, and magnitude of the orbit in which they revolve, we can directly deduce the mean density of the system. A full investigation of this and allied problems is given by Mérian in *Comptes rendus* (122, 1254).

A nomenclature adopted by the present writer, in the *Astrophysical Journal* (vol. x. p. 308), meets, I think, more directly the simpler conditions of the problem of the density of a close binary star.

Putting
$t$ = time, in days, of revolution;
$r$ = semi-axis major of the orbit of the system;
$p$ = ratio of the radius of companion (1) to semi-axis major;
$q$ = ratio of the radius of companion (2);
$m_1$ = mass of companion (1);
$m_2$ = mass of companion (2);
$\Delta_1$ = density of companion (1);
$\Delta_2$ = density of companion (2);

(the sun's radius, mass and density are taken as unity) then the simple relation

$$\Delta_1 = \frac{0{\cdot}0135}{p^3t^2} \cdot \frac{m_1}{m_1 + m_2}, \quad . \quad . \quad . \quad . \quad (1)$$

$$\Delta_2 = \frac{0{\cdot}0135}{q^3t^2} \cdot \frac{m_2}{m_1 + m_2}, \quad . \quad . \quad . \quad . \quad (2)$$

will determine the values of $\Delta_1$ and $\Delta_2$ when the relative masses of the two component stars are known.

If $p = q$,

then $$\Delta_1 + \Delta_2 = \frac{0{\cdot}0135}{p^3t^2}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

It will be at once evident that, since

$$\frac{m_1}{m_1 + m_2} \text{ and } \frac{m_2}{m_1 + m_2}$$

must always be less than unity,

$$\Delta_1 < \frac{0{\cdot}0135}{p^3t^2};$$

$$\Delta_2 < \frac{0{\cdot}0135}{q^3t^2}.$$

These two relations express the limit in one direction of the density of any binary system when the size of the component stars, and the period of variation, has been ascertained from an examination of the light-curve of the variable. In the *Astrophysical Journal* (vol. x. p. 315), Prof. H. N. Russell, of the Princeton University, from considerations similar to the foregoing, deals with the light-variation of seventeen out of the twenty-two Algol variables, deducing from their variation their densities.

He finds the mean density of the seventeen stars considered to be

0·19,

the density of water being unity.

In the same *Journal* (vol. x. p. 314), the writer discussed the light-changes of four southern Algol variables which had been under observation for some years at Lovedale, South Africa.

The mean density of these four stars was found to be 0·13 that of the sun. If the sun's density be taken as 1·44 times the density of water, then this result would yield as the mean density of the stars considered,

0·187.

Since this article was written, two new southern Algol variables have been discovered—one at the Cape Royal Observatory and the other at Lovedale. Further, a new photometric equatorial, specially constructed by Messrs. Cooke for variable star work, has made it possible to secure observations, at Lovedale, of all the eight southern Algol variables of considerable accuracy.

A reduction of these observations has just been completed, and an examination of the results gives, as the mean density of all the southern Algol variables at present known, viz. eight, the value

0·176.

Of the three investigations just given in brief, the first two were independently conceived, carried out, and completed. Yet the results are practically accordant.

There is just the barest possibility that this agreement may be fortuitous; such a remote possibility exists in all investigations.

It is much more probable, however, that the agreement between the results indicates the truth of the conclusion. And this conclusion is that the average density of close binary stars —that is, of bodies just forming, by the compulsion of their inherent forces, into a dual existence—is one-sixth that of water or one-eighth that of the sun.

It is not the purport of the present paper to follow the investigation to its legitimate termination—that is, to discover in what agreement is the result just obtained with the theoretical conditions of density consonant with a rotating ellipsoid on the limit of bipartition. A broad general agreement, however, is evident even on an elementary judgment, for if the result had been that the average density of close binary systems, or of the actual density of any one system, was, say, much greater than that of the earth, then it would be difficult to understand how separation could take place under these conditions. On the other hand, no violence is done to our appreciation of what is reasonable when we find that *all* close binary systems have a density much less than water; in some single cases, indeed, we meet with densities approaching that of a gaseous nebula. Such a condition of tenuity is evidently favourable to disruption.

Any investigation of the light-changes of an Algol variable, having extreme accuracy in view, must of necessity consider the form of the stars alternately eclipsing one another.

It is evident that the rate of decrease or increase of eclipse will be more rapid the more ellipsoidal in figure the component stars are.

In the case of two spheres eclipsing one another, the amount of obscuration, or the total amount of light emitted by the system, at any period of the eclipse, can be ascertained from the simplest geometrical considerations. In the case of two spheroids, the computation is not so simple.

Still a relation does exist between the amount of distortion due to the mutual attractions of two adjacent bodies, and the rate of obscuration in any eclipse, and this relation is capable of discernment and computation. The difficulty, however, does not lie in the computation: it lies in our inability to determine observations refined enough to respond to a demand so exacting as that which necessitates observations correct to within two-hundredths of a magnitude.

That this degree of accuracy in photometric measurement has been attained to by more than one observer brings the problem of the determination of the figure of a rotating binary system within a reasonable expectation of solution.

Of the twenty-two Algol variables at present known, five are binary systems the component stars of which revolve in contact. It is, therefore, evident that any investigation having as its purpose the figure of the component members of a close binary system should deal first with these five stars.

Particulars of these stars are as follows:—

| Chandler No. | Designation. | R.A. 1900. | Decl. 19c0. | Period. | Max. | Min. |
|---|---|---|---|---|---|---|
| | | h. m. s. | ° ′ | d. h. m. s. | m. | m. m. |
| 2852 | V Puppis . | 7 55 22 | −48 58·4 | 1 10 54 27 | 4·1 | 4·7—4·9 |
| 3055 | X Carinae . | 8 29 7 | −58 53·2 | 1 1 59 0 | 7·9 | 8·6—8·7 |
| 5099 | RR Centauri. | 14 9 55 | −57 23·3 | 0 14 32 7 | 7·4 | 7·8—7·85 |
| 6758 | β Lyræ . . | 18 46 23 | +33 14·8 | 12 21 46 58 | 3·4 | 3·9—4·5 |
| 8598 | U Pegasi . | 23 52 53 | +15 23·9 | 0 8 59 41 | 9·3 | 9·8—9·9 |

It may be objected that all along it has been assumed that Algol variables are binary systems. What evidence is there that this is so?

In the only cases where independent confirmation is possible—that is, in cases where the stars are bright enough to be dealt with spectroscopically—this confirmation is forthcoming.

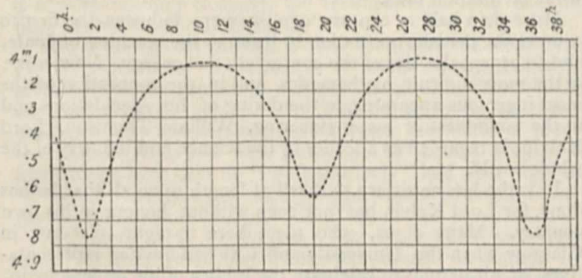

FIG. 1.—Light-curve of V Puppis, from observations made at Lovedale South Africa.

With regard to the foregoing five stars, the spectroscope reveals V Puppis and β Lyræ to be binary systems. The other stars are beyond the reach of spectroscopic examination, at least with its present limitations.

Apart from this, however, revolution and consequent eclipse is the only complete explanation of Algol variation.

In Fig. 1 is given the light-curve of V Puppis, the first star in the list, and this light-curve may be taken as typical of those of the other four stars. Indeed, the light-curve of U Pegasi (*Harvard Circular*, No. 23) is practically identical with that of V Puppis.

The figure of the β Lyræ system has been considered most fully and conclusively by Mr. Myers in the *Astrophysical Journal* (vol. vii. p. 1); one of his definite results being that each star forming the system is not a sphere but an ellipsoid of revolution. The amount of flattening is found to be 0·2 in both stars.

Mr. Myers also deals with the variation of U Pegasi in the same manner (*Astrophysical Journal*, vol. viii. p. 163), and finds that there is distinct evidence, in the form of the light-curve of this star, of an ellipsoidal figure in both components.

In the *Astrophysical Journal* (vol. xiii. p. 177), the writer considered the variation of V Puppis, the first of the five stars. Again it was found that while, to no insufficient extent, the view that the light-changes were produced by the eclipse of two spheres would meet the facts of variation, an assumption that both components were ellipsoidal in figure would satisfy the observations more fully.

Since the foregoing article was published, an examination of all the observations of X Carinæ and RR Centauri made at Lovedale has been completed. It is found that the twin stars of X Carinæ have parted company. They are no longer in actual contact, although a distance of only one-tenth of their diameters separates them. The observations of this star also do not indicate an unmistakable distortion of either component.

In the case of RR Centauri we have a twin system similar to that of V Puppis, with this difference, that the form of the light-curve indicates beyond doubt a considerable ellipsoidal form of both stars. Indeed, a dumb-bell figure of equilibrium similar to that indicated in Prof. Darwin's treatise on "Figures of Equilibrium of Rotating Masses of Fluid" (p. 429) would produce variation of the same character as that of RR Centauri.

Of the interest which attaches to all investigations, whether by telescope or spectroscope, concerning these remarkable binary systems, there can be no manner of doubt. For we are dealing with the origin of stellar systems.

Hitherto, in theory only have we had cognisance of some great gaseous orb aggregating itself into two elongated spheroids, dividing after the lapse of long ages into two separate and distinct bodies.

By the action and interaction of their tidal forces, the gap between the component stars grows wider and wider: the system ceases to be a close binary star fulfilling its period in days; it takes months to complete its circuit.

And still the apocentric revolution goes on, until, at last, the star becomes a visual binary, one component separated from its fellow by the width of the whole solar system.

From V Puppis, on the one hand, a dumb-bell system speeding round in thirty-five hours, to the twin stars of Castor, completing their great round in one thousand years, we have a regular chain of sequences in distance.

The links of this chain are made evident by observation as well as by theory. It is not unreasonable, therefore, that the present trend of astrophysical research should be in the direction of discovering more fully and certainly the different stages of evolution and development in the architecture of the heavens.

ALEX. W. ROBERTS.

Lovedale, South Africa, August 9.

## A Plea for a Prehistoric Survey of Southern India.

ACCORDING to Mr. R. Bruce Foote, and no one is more competent to speak than he, the urgency for the establishment of a genuine prehistoric survey in Southern India is very great, if the study of this most fascinating branch of archæology is to be encouraged and the wanton destruction of prehistoric monuments checked. Such survey, if honestly carried out, would go far to procure much larger data than yet exist as to the distribution over the southernmost districts of the peninsula of the Palæolithic people whose remains in the shape of chipped stone implements have been found in so many localities in the Carnatic and Deccan plateau, embedded in Pleistocene deposits. Such data might help materially also to bridge over the great hiatus in time which now appears to exist between the era of those very rude people and that of the Neolithic tribes which followed them in the same country.

Further research in the southern districts especially might result in the finding of evidence as to the quarter from which the Dravidian tribes entered the Peninsula—a question of very high ethnological interest.

Another very important ethnological question might possibly be also answered by such investigations, namely, were the first Dravidian immigrants that settled in Southern India in a Neolithic stage of culture, or must the polished-stone people be considered as pre-Dravidian? If the question be answered in the latter way, a fresh immigration must be postulated, by which the true Dravidians reached their present country. If the answer affirms the former proposition, the idea of a further immigration may be dispensed with, for the early iron people appear to be the direct descendants of the Neolithic tribes and the ancestors of the present inhabitants.

Every year numbers of prehistoric burial places are destroyed by the rapacity of the "Waddars," the wandering tribe of tank diggers, who are allowed to annex the fine slabs composing the *kistvaens*, while independent archæologists are, by Government order, forbidden to open any old graves unless they are willing to make over to the Museum all their finds and bear their own expenses. The lapse of time and effects of weather greatly tend to diminish the remains of the old people in the sites they occupied. The action of the plough in many cases, and the trampling of herds of cattle in others, are active elements of destruction of pottery buried near the surface, and even of stone implements. These remarks apply with equal force to the old sites of the early iron age folk, both residential and sepulchral.

Mr. Foote further states in his "Catalogue of the Prehistoric Antiquities" in the Government Museum at Madras, that a full and exhaustive prehistoric survey of the country should be made by a really competent specialist, who shall be a geologist and an osteologist as well as a trained archæologist, and not a mere architectural surveyor. A knowledge of Sanskrit will be of no use in deciding as to the sources whence were derived the many foreign rocks and minerals found in the many old residential sites, which, up to date, have had only their surfaces examined, but which, doubtless, in many cases will yield rich finds to the careful excavator, who must be a man having the power to devote time to his work. A. C. HADDON.

Cambridge, September 3.

## THE BRITISH ASSOCIATION AT GLASGOW.

IN the previous articles, which appeared in NATURE, May 23, July 18, and August 22, particulars were given as to the local arrangements for the meeting in Glasgow, and a forecast of the papers to be read at the sectional meetings was published. The president, Principal A. W. Rücker, delivered his presidential address, which we print in this issue, as we went to press yesterday, and the business of the sections commenced this morning. A large number of British leading men of science are present at the meeting, and many well-known men of science are also present from abroad. Among others, the following are attending the meeting:—Prof. L. Kny, Berlin; Prof. George Quincke, Heidelberg; Prof. G. Mittag-Leffler, Stockholm; Dr. Gustav Cassil, Copenhagen; Prof. A. F. Renard, Ghent; Prof. Gustave Gilson, Louvain; Mr. A. Laurence Rotch, Readville, Mass., U.S.A.; Prof. R. H. Thurston, Cornell University; Dr. T. P. Lotsy, Arnheim, Holland; Dr. Theodor Beer, Vienna; Prof. J. J. Mackenzie, Toronto; Prof. E. W. Morley, Cleveland, Ohio; Prof. Joji Sakurai, Tokyo; His Excellency Don Arturo de Marcoartu, Bilbao; Prof. J. P. McMurrick, Michigan; Dr. V. Crémieu, Paris; Prof. Dr. W. Marikwald, Berlin; Prof. Paul Walden, Riga; Prof. Goebel, Munich; Dr. C. E. Guillaume, Sevres; Dr. Conventz, Danzig; Baron Varilla, Paris; Mr. Edward Atkinson, Brooklyn; Prof. Anitchkoff, Russia.

The meeting promises to be a successful one, both from the point of view of numbers and that of scientific interest.

### INAUGURAL ADDRESS BY PROF. ARTHUR W. RÜCKER, M.A., LL.D., D.SC., SEC.R.S., PRESIDENT OF THE ASSOCIATION.

THE first thought in the minds of all of us to-night is that since we met last year the great Queen, in whose reign nearly all the meetings of the British Association have been held, has passed to her rest.

To Sovereigns most honours and dignities come as of right; but for some of them is reserved the supreme honour of an old age softened by the love and benedictions of millions; of a path to the grave, not only magnificent, but watered by the tears both of their nearest and dearest, and of those who, at the most, have only seen them from afar.

This honour Queen Victoria won. All the world knows by what great abilities, by what patient labour, by what infinite tact and kindliness, the late Queen gained both the respect of the rulers of nations and the affection of her own subjects.

Her reign, glorious in many respects, was remarkable, outside these islands, for the growth of the Empire; within and without them, for the drawing nearer of the Crown and the people in mutual trust; while, during her lifetime, the developments of science and of scientific industry have altered the habits and the thoughts of the whole civilised world.

The representatives of science have already expressed in more formal ways their sorrow at the death of Queen Victoria, and the loyalty and confident hope for the future with which they welcome the accession of King Edward. But none the less, I feel sure that at this, the first meeting of the British Association held in his reign, I am only expressing the universal opinion of all our members when I say that no group of the King's subjects trusts more implicitly than we do in the ability, skill, and judgment which His Majesty has already shown in the exercise of the powers and duties of his august office; that none sympathise more deeply with the sorrows which two great nations have shared with their Sovereigns; and that none cry with more fervour, "Long live the King!"

But this meeting of the British Association is not only remarkable as being the first in a new reign. It is also the first in a new century. It is held in Glasgow at a time when your International Exhibition has in a special sense attracted the attention of the world to your city, and when the recent celebration of the ninth jubilee of your University has shown how deeply the prosperity of the present is rooted in the past. What wonder, then, if I take the Chair to which you have called me with some misgivings? Born and bred in the South, I am to preside over a meeting held in the largest city of Scotland. As your chosen mouthpiece I am to speak to you of science when we stand at the parting of the centuries, and when the achievements of the past and present, and the promise of the future, demand an interpreter with gifts of knowledge and divination to which I cannot pretend. Lastly, I am President of the British Association as a disciple in the home of the master, as a physicist in a city which a physicist has made for ever famous. Whatever the future may have in store for Glasgow, whether your enterprise is still to add wharf to wharf, factory to factory, and street to street, or whether some unforeseen "tide in the affairs of men" is to sweep energy and success elsewhere, fifty-three years in the history of your city will never be forgotten while civilisation lasts.

More than half a century ago, a mere lad was the first to compel the British Association to listen to the teaching of Joule, and to accept the law of the conservation of energy. Now, alike in the most difficult mathematics and in the conception of the most ingenious apparatus, in the daring of his speculations and in the soundness of his engineering, William Thomson, Lord Kelvin, is regarded as a leader by the science and industry of the whole world.

It is the less necessary to dwell at length upon all that he has done, for Lord Kelvin has not been without honour in his own country. Many of us, who meet here to-night, met last in Glasgow when the University and City had invited representatives of all nations to celebrate the Jubilee of his professorship. For those two or three days learning was surrounded with a pomp seldom to be seen outside a palace. The strange middle-age costumes of all the chief Universities of the world were jostling here, the outward signs that those who were themselves distinguished in the study of Nature had gathered to do honour to one of the most distinguished of them all.

Lord Kelvin's achievements were then described in addresses in every tongue, and therefore I will only remind you that we, assembled here to-night, owe him a heavy debt of gratitude; for the fact that the British Association enters on the twentieth century conscious of a work to do and of the vigour to do it is largely due to his constant presence at its meetings and to the support he has so ungrudgingly given. We have learned to know, not only the work of our great leader, but the man himself; and I count myself happy because in his life-long home, under the walls of the University he served so well, and at a meeting of the Association which his genius has so often illuminated, I am allowed, as your President, to assure him in your name of the admiration, respect, nay, of the affection, in which we all hold him.

I have already mentioned a number of circumstances which make our meeting this year noteworthy; to these I must add that for the first time we have a Section for Education, and the importance of this new departure, due largely to the energy of Prof. Armstrong, is emphasised by the fact that the Chair of that Section will be occupied by the Vice-President of the Committee of Council on Education—Sir John Gorst. I will not attempt to forecast the proceedings of the new Section. Education is passing through a transitional stage. The recent debates in Parliament; the great gifts of Mr. Carnegie; the discussion as to University organisation in the North of England; the reconstitution of the University of London; the increasing importance attached to the application of knowledge both to the investigation of Nature and to the purposes of industry, are all evidence of the growing conviction that without advance in education we cannot retain our position among the nations of the world. If the British Association can provide a platform on which these matters may be discussed in a scientific but practical spirit, free from the misrepresentations of the hustings and the exaggerations of the partisan, it will contribute in no slight measure to the national welfare.

But amid the old and new activities of our meeting the undertone of sadness, which is never absent from such gatherings, will be painfully apparent to many of us at Glasgow. The life-work of Prof. Tait has ended amid the gloom of the war-cloud. A bullet, fired thousands of miles away, struck him to the heart, so that in their deaths the father and the brave son, whom he loved so well, were not long divided. Within the last year, too, America has lost Rowland; Viriamu Jones, who did yeoman's service for education and for science, has succumbed to a long and painful illness; and one who last year at Bradford seconded the proposal that I should be your President at Glasgow, and who would unquestionably have occupied this Chair before long had he been spared to do so, has unexpectedly been called away. A few months ago we had no reason to doubt that George Francis FitzGerald had many years of health and work before him. He had gained in a remarkable way not only the admiration of the scientific world, but the affection of his friends, and we shall miss sadly one whom we all cared for, and who, we hoped, might yet add largely to the achievements which had made him famous.

*The Science of the Nineteenth Century.*

Turning from these sad thoughts to the retrospect of the century which has so lately ended, I have found it to be impossible to free myself from the influence of the moment and to avoid, even if it were desirable to avoid, the inclination to look backward from the standpoint of to-day.

Two years ago Sir Michael Foster dealt with the work of the century as a whole. Last year Sir William Turner discussed in greater detail the growth of a single branch of science. A third and humbler task remains, viz., to fix our attention on some of the hypotheses and assumptions on which the fabric of modern theoretical science has been built, and to inquire whether the foundations have been so "well and truly" laid that they may be trusted to sustain the mighty superstructure which is being raised upon them.

The moment is opportune. The three chief conceptions which for many years have dominated physical as distinct from biological science have been the theories of the existence of atoms, of the mechanical nature of heat, and of the existence of the ether.

Dalton's atomic theory was first given to the world by a Glasgow professor—Thomas Thomson—in the year 1807, Dalton having communicated it to him in 1804. Rumford's and Davy's experiments on the nature of heat were published in 1798 and 1799 respectively; and the celebrated Bakerian Lecture, in which Thomas Young established the undulatory theory by explaining the interference of light, appeared in the *Philosophical Transactions* in 1801. The keynotes of the physical science of the nineteenth century were thus struck, as the century began, by four of our fellow-countrymen, one of whom—Sir Benjamin Thompson, Count Rumford—preferred exile from the land of his birth to the loss of his birthright as a British citizen.

*Doubts as to Scientific Theories.*

It is well known that of late doubts have arisen as to whether the atomic theory, with which the mechanical theory of heat is closely bound up, and the theory of the existence of an ether have not served their purpose, and whether the time has not come to reconsider them.

The facts that Prof. Poincaré, addressing a congress of physicists in Paris, and Prof. Poynting, addressing the Physical Section of the Association, have recently discussed the true meaning of our scientific methods of interpretation; that Dr. James Ward has lately delivered an attack of great power on many positions which eminent scientific men have occupied; and that the approaching end of the nineteenth century led Prof. Haeckel to define in a more popular manner his own very definite views as to the solution of the "Riddle of the Universe," are perhaps a sufficient justification of an attempt to lay before you the difficulties which surround some of these questions.

To keep the discussion within reasonable limits, I shall illustrate the principles under review by means of the atomic theory, with comparatively little reference to the ether, and we may also at first confine our attention to inanimate objects.

*The Construction of a Model of Nature.*

A natural philosopher, to use the old phrase, even if only possessed of the most superficial knowledge, would attempt to bring some order into the results of his observation of Nature by grouping together statements with regard to phenomena which are obviously related. The aim of modern science goes far beyond this. It not only shows that many phenomena are related which at first sight have little or nothing in common, but, in so doing, also attempts to explain the relationship.

Without spending time on a discussion of the meaning of the word "explanation," it is sufficient to say that our efforts to establish relationships between phenomena often take the form of attempting to prove that, if a limited number of assumptions are granted as to the constitution of matter, or as to the existence of quasi-material entities, such as caloric, electricity, and the ether, a wide range of observed facts falls into order as a necessary consequence of the assumptions. The question at issue is whether the hypotheses which are at the base of the scientific theories now most generally accepted are to be regarded as accurate descriptions of the constitution of the universe around us, or merely as convenient fictions.

Convenient fictions be it observed, for even if they are fictions they are not useless. From the practical point of view it is a matter of secondary importance whether our theories and assumptions are correct, if only they guide us to results which are in accord with facts. The whole fabric of scientific theory may be regarded merely as a gigantic "aid to memory"; as a means for producing apparent order out of disorder by codifying the observed facts and laws in accordance with an artificial system, and thus arranging our knowledge under a comparatively small number of heads. The simplification introduced by a scheme which, however imperfect it may be, enables us to argue from a few first principles, makes theories of practical use. By means of them we can foresee the results of combinations of causes which would otherwise elude us. We can predict future events, and can even attempt to argue back from the present to the unknown past.

But it is possible that these advantages might be attained by means of axioms, assumptions, and theories based on very false ideas. A person who thought that a river was really a streak of blue paint might learn as much about its direction from a map as one who knew it as it is. It is thus conceivable that we might be able, not indeed to construct, but to imagine, something more than a mere map or diagram, something which might even be called a working model of inanimate objects, which was nevertheless very unlike the realities of Nature. Of course, the agreement between the action of the model and the behaviour of the things it was designed to represent would probably be imperfect, unless the one were a facsimile of the other; but it is conceivable that the correlation of natural phenomena could be imitated, with a large measure of success, by means of an imaginary machine, which shared with a map or diagram the characteristic that it was in many ways unlike the things it represented, but might be compared to a model in that the behaviour of the things represented could be predicted from that of the corresponding parts of the machine.

We might even go a step further. If the laws of the working of the model could be expressed by abstractions, as, for example, by mathematical formulæ, then, when the formulæ were obtained, the model might be discarded, as probably unlike that which it was made to imitate, as a mere aid in the construction of equations, to be thrown aside when the perfect structure of mathematical symbols was erected.

If this course were adopted, we should have given up the attempt to know more of the nature of the objects which surround us than can be gained by direct observation, but might nevertheless have learned how these objects would behave under given circumstances.

We should have abandoned the hope of a physical explanation of the properties of inanimate Nature, but should have secured a mathematical description of her operations.

There is no doubt that this is the easiest path to follow. Criticism is avoided if we admit from the first that we cannot go below the surface; cannot know anything about the constitution of material bodies; but must be content with formulating a description of their behaviour by means of laws of Nature expressed by equations.

But if this is to be the end of the study of Natuie, it is evident that the construction of the model is not an essential part of the process. The model is used merely as an aid to thinking; and if the relations of phenomena can be investigated without it, so much the better. The highest form of theory—it may be said—the widest kind of generalisation, is that which has given up the attempt to form clear mental pictures of the constitution of matter, which expresses the facts and the laws by language and symbols which lead to results that are true, whatever be our view as to the real nature of the objects with which we deal. From this point of view the atomic theory becomes not so much false as unnecessary; it may be regarded as an attempt to give an unnatural precision to ideas which are and must be vague.

Thus, when Rumford found that the mere friction of metals produced heat in unlimited quantity, and argued that heat was therefore a mode of motion, he formed a clear mental picture of what he believed to be occurring. But his experiments may be quoted as proving only that energy can be supplied to a body in indefinite quantity, and when supplied by doing work against friction it appears in the form of heat.

By using this phraseology we exchange a vivid conception of moving atoms for a colourless statement as to heat energy, the real nature of which we do not attempt to define; and methods which thus evade the problem of the nature of the things which the symbols in our equations represent have been prosecuted with striking success, at all events within the range of a limited class of phenomena. A great school of chemists, building upon the thermodynamics of Willard Gibbs and the intuition of Van t' Hoff, have shown with wonderful skill that, if a sufficient number of the data of experiment are assumed, it is possible, by the aid of thermodynamics, to trace the form of the relations between many physical and chemical phenomena without the help of the atomic theory.

But this method deals only with matter as our coarse senses know it; it does not pretend to penetrate beneath the surface.

It is therefore with the greatest respect for its authors, and with a full recognition of the enormous power of the weapons employed, that I venture to assert that the exposition of such a system of tactics cannot be regarded as the last word of science in the struggle for the truth.

Whether we grapple with them, or whether we shirk them; however much or however little we can accomplish without answering them, the questions still force themselves upon us: Is matter what it seems to be? Is interplanetary space full or empty? Can we argue back from the direct impressions of our senses to things which we cannot directly perceive; from the phenomena displayed by matter to the constitution of matter itself?

It is these questions which we are discussing to-night, and we may therefore, as far as the present address is concerned, put aside, once for all, methods of scientific exposition in which an attempt to form a mental picture of the constitution of matter is practically abandoned, and devote ourselves to the inquiries whether the effort to form such a picture is legitimate, and whether we have any reason to believe that the sketch which science has already drawn is to some extent a copy, and not a mere diagram, of the truth.

### *Successive Steps in the Analysis of Matter.*

In dealing, then, with the question of the constitution of matter and the possibility of representing it accurately, we may grant at once that the ultimate nature of things is, and must remain, unknown; but it does not follow that immediately below the complexities of the superficial phenomena which affect our senses there may not be a simpler machinery of the existence of which we can obtain evidence, indirect indeed, but conclusive.

The fact that the apparent unity which we call the atmosphere can be resolved into a number of different gases is admitted; though the ultimate nature of oxygen, nitrogen, argon, carbonic acid, and water vapour is as unintelligible as that of air as a whole, so that the analysis of air may be said to have substituted many incomprehensibles for one.

Nobody, however, looks at the question from this point of view. It is recognised that an investigation into the proximate constitution of things may be useful and successful, even if their ultimate nature is beyond our ken.

Nor need the analysis stop at the first step. Water vapour and carbonic acid, themselves constituents of the atmosphere, are in turn resolved into their elements, hydrogen, oxygen, and carbon, which, without a formal discussion of the criteria of reality, we may safely say are as real as air itself.

Now, at what point must this analysis stop if we are to avoid crossing the boundary between fact and fiction? Is there any fundamental difference between resolving air into a mixture of gases and resolving an elementary gas into a mixture of atoms and ether?

There are those who cry halt! at the point at which we divide a gas into molecules, and their first objection seems to be that molecules and atoms cannot be directly perceived, cannot be seen or handled, and are mere conceptions, which have their uses, but cannot be regarded as realities.

It is easiest to reply to this objection by an illustration.

The rings of Saturn appear to be continuous masses separated by circular rifts. This is the phenomenon which is observed through a telescope. By no known means can we ever approach or handle the rings; yet everybody who understands the evidence now believes that they are not what they appear to be, but consist of minute moonlets, closely packed indeed, but separate the one from the other.

In the first place, Maxwell proved mathematically that if a Saturnian ring were a continuous solid or fluid mass it would be unstable and would necessarily break into fragments. In the next place, if it were possible for the ring to revolve like a solid body, the inmost parts would move slowest, while a satellite moves faster the nearer it is to a planet. Now, spectroscopic observation, based on the beautiful method of Sir W. Huggins, shows not only that the inner portions of the ring move the more rapidly, but that the actual velocities of the outer and inner edges are in close accord with the theoretical velocities of satellites at like distances from the planet.

This and a hundred similar cases prove that it is possible to obtain convincing evidence of the constitution of bodies between whose separate parts we cannot directly distinguish, and I take it that a physicist who believes in the reality of atoms thinks that he has as good reason for dividing an apparently continuous gas into molecules as he has for dividing the apparently continuous Saturnian rings into satellites. If he is wrong, it is not the fact that molecules and satellites alike cannot be handled and cannot be seen as individuals, that constitutes the difference between the two cases.

It may, however, be urged that atoms and the ether are alleged to have properties different from those of matter in bulk, of which alone our senses take direct cognisance, and that therefore it is impossible to prove their existence by evidence of the same cogency as that which may prove the existence of a newly discovered variety of matter or of a portion of matter too small or too distant to be seen.

This point is so important that it requires full discussion, but in dealing with it, it is necessary to distinguish carefully between the validity of the arguments which support the earlier and more fundamental propositions of the theory; and the evidence brought forward to justify mere speculative applications of its doctrines which might be abandoned without discarding the theory itself. The proof of the theory must be carried out step by step.

The first step is concerned wholly with some of the most general properties of matter, and consists in the proof that those properties are either absolutely unintelligible, or that, in the case of matter of all kinds, we are subject to an illusion similar to that the results of which we admit in the case of Saturn's rings, clouds, smoke, and a number of similar instances. The believer in the atomic theory asserts that matter exists in a particular state; that it consists of parts which are separate and distinct the one from the other, and as such are capable of independent movements.

Up to this point no question arises as to whether the separate parts are, like grains of sand, mere fragments of matter; or whether, though they are the bricks of which matter is built, they have, as individuals, properties different from those of masses of matter large enough to be directly perceived. If they are mere fragments of ordinary matter, they cannot be used as aids in explaining those qualities of matter which they themselves share.

We cannot explain things by the things themselves. If it be true that the properties of matter are the product of an underlying machinery, that machinery cannot itself have the properties which it produces, and must, to that extent at all events, differ from matter in bulk as it is directly presented to the senses.

If, however, we can succeed in showing that if the separate parts have a limited number of properties (different, it may be, from those of matter in bulk), the many and complicated properties of matter can, to a considerable extent, be explained as consequences of the constitution of these separate parts; we shall have succeeded in establishing, with regard to quantitative properties, a simplification similar to that which the chemist has established with regard to varieties of matter. The many will have been reduced to the few.

The proofs of the physical reality of the entities discovered by means of the two analyses must necessarily be different. The chemist can actually produce the elementary constituents into which he has resolved a compound mass. No physicist or chemist can produce a single atom separated from all its fellows, and show that it possesses the elementary qualities he assigns to it. The cogency of the evidence for any suggested constitution of atoms must vary with the number of facts which the hypothesis that they possess that constitution explains.

Let us take, then, two steps in their proper order, and inquire, first, whether there is valid ground for believing that all matter is made up of discrete parts; and secondly, whether we can have any knowledge of the constitution or properties which those parts possess.

### *The Coarse-grainedness of Matter.*

Matter in bulk appears to be continuous. Such substances as water or air appear to the ordinary observer to be perfectly uniform in all their properties and qualities, in all their parts.

The hasty conclusion that these bodies are really uniform is, nevertheless, unthinkable.

In the first place, the phenomena of diffusion afford conclusive proof that matter when apparently quiescent is in fact in a state of internal commotion. I need not recapitulate the familiar evidence to prove that gases and many liquids when placed in communication interpenetrate or diffuse into each other; or that air, in contact with a surface of water, gradually becomes laden with water vapour, while the atmospheric gases in turn mingle with the water. Such phenomena are not exhibited by liquids and gases alone, or by solids at high temperatures only. Sir W. Roberts-Austen has placed pieces of gold and lead in contact at a temperature of 18° C. After four years the gold had travelled into the lead to such an extent that not only were the two metals united, but, on analysis, appreciable quantities of the gold were detected even at a distance of more than 5 millimetres from the common surface, while within a distance of three-quarters of a millimetre from the surface gold had penetrated into the lead to the extent of 1 oz. 6 dwts. per ton, an amount which could have been profitably extracted.

Whether it is or is not possible to devise any other intelligible account of the cause of such phenomena, it is certain that a simple and adequate explanation is found in the hypothesis that matter consists of discrete parts in a state of motion, which can penetrate into the spaces between the corresponding parts of surrounding bodies.

The hypothesis thus framed is also the only one which affords a rational explanation of other simple and well-known facts. If matter is regarded as a continuous medium, the phenomena of expansion are unintelligible. There is, apparently, no limit to the expansion of matter, or, to fix our attention on one kind of matter, let us say to the expansion of a gas; but it is inconceivable that a continuous material which fills or is present in every part of a given space could also be present in every part of a space a million times as great. Such a statement might be made of a mathematical abstraction; it cannot be true of any real substance or thing. If, however, matter consists of discrete particles, separated from each other either by empty space or by something different from themselves, we can at once understand that expansion and contraction may be nothing more than the mutual separation or approach of these particles.

Again, no clear mental picture can be formed of the phenomena of heat unless we suppose that heat is a mode of motion. In the words of Rumford, it is "extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated in [his] experiment [on friction] except it be motion" (*Phil. Trans.*, 1798, p. 99). And if heat be motion there can be no doubt that it is the fundamental particles of matter which are moving. For the motion is not visible, is not motion of the body as a whole, while diffusion, which is a movement of matter, goes on more quickly as the temperature rises, thereby proving that the internal motions have become more rapid, which is exactly the result which would follow if these were the movements which constitute sensible heat.

Combining, then, the phenomena of diffusion, expansion, and heat, it is not too much to say that no hypotheses which make them intelligible have ever been framed other than those which are at the basis of the atomic theory.

Many other considerations also point to the same conclusion. Many years ago Lord Kelvin gave independent arguments, based on the properties of gases, on the constitution of the surfaces of liquids, and on the electric properties of metals, all of which indicate that matter is, to use his own phrase, coarse-grained—that it is not identical in constitution throughout, but that adjacent minute parts are distinguishable from each other by being either of different natures or in different states.

And here it is necessary to insist that all these fundamental proofs are independent of the nature of the particles or granules into which matter must be divided.

The particles, for instance, need not be different in kind from the medium which surrounds and separates them. It would suffice if they were what may be called singular parts of the medium itself, differing from the rest only in some peculiar state of internal motion or of distortion, or by being in some other way earmarked as distinct individuals. The view that the constitution of matter is atomic may and does receive support from theories in which definite assumptions are made as to the constitution of the atoms; but when, as is often the case, these assumptions introduce new and more recondite difficulties, it must be remembered that the fundamental hypothesis—that matter consists of discrete parts, capable of independent motions—is forced upon us by facts and arguments which are altogether independent of what the nature and properties of these separate parts may be.

As a matter of history, the two theories, which are not by any means mutually exclusive, that atoms are particles which can be treated as distinct in kind from the medium which surrounds them, and that they are parts of that medium existing in a special state, have both played a large part in the theoretical development of the atomic hypothesis. The atoms of Waterston, Clausius, and Maxwell were particles. The vortex-atoms of Lord Kelvin, and the strain-atoms (if I may call them so) suggested by Mr. Larmor, are states of a primary medium which constitutes a physical connection between them, and through which their mutual actions arise and are transmitted.

### *Properties of the Basis of Matter.*

It is easy to show that, whichever alternative be adopted, we are dealing with something, whether we consider it under the guise of separate particles or of differentiated portions of the medium, which has properties different from those of matter in bulk.

For if the basis of matter had the same constitution as matter, the irregular heat movements could hardly be maintained either against the viscosity of the medium or the frittering away of energy of motion which would occur during the collisions between the particles. Thus, even in the case in which a hot body is prevented from losing heat to surrounding objects, its sensible heat should spontaneously decay by a process of self-cooling. No such phenomenon is known, and though on this, as on all other points, the limits of our knowledge are fixed by the uncertainty of experiment, we are compelled to admit that, to all appearance, the fundamental medium, if it exists, is unlike a material medium, in that it is non-viscous; and that the particles, if they exist, are so constituted that energy is not frittered away when they collide. In either case, we are dealing with something different from matter itself in the sense that, though it is the basis of matter, it is not identical in all its properties with matter.

The idea therefore that entities exist possessing properties different from those of matter in bulk is not introduced at the end of a long and recondite investigation to explain facts with which none but experts are acquainted. It is forced upon us at the very threshold of our study of Nature. Either the properties of matter in bulk cannot be referred to any simpler structure, or that simpler structure must have properties different from those of matter in bulk as we directly knew it—properties which can only be inferred from the results which they produce.

No *a priori* argument against the possibility of our discovering the existence of quasi-material substances, which are nevertheless different from matter, can prove the negative proposition that such substances cannot exist. It is not a self-evident truth that no substance other than ordinary matter can have an existence as real as that of matter itself. It is not axiomatic that matter cannot be composed of parts whose properties are different from those of the whole. To assert that even if such substances and such parts exist no evidence however cogent could convince us of their existence is to beg the whole question at issue; to decide the cause before it has been heard.

We must therefore adhere to the standpoint adopted by most scientific men, viz., that the question of the existence of ultra-physical entities, such as atoms and the ether, is to be settled by the evidence, and must not be ruled out as inadmissible on *a priori* grounds.

On the other hand, it is impossible to deny that, if the mere entry on the search for the concealed causes of physical phenomena is not a trespass on ground we have no right to explore, it is at all events the beginning of a dangerous journey.

The wraiths of phlogiston, caloric, luminiferous corpuscles, and a crowd of other phantoms haunt the investigator, and as the grim host vanishes into nothingness he cannot but wonder if his own conceptions of atoms and of the ether

> "shall dissolve,
> And, like this insubstantial pageant faded,
> Leave not a wrack behind."

But though science, like Bunyan's hero, has sometimes had to pass through the "Valley of Humiliation," the spectres which meet it there are not really dangerous if they are boldly faced. The facts that mistakes have been made, that theories have been propounded, and for a time accepted, which later investigations have disproved, do not necessarily discredit the method adopted. In scientific theories, as in the world around us, there is a survival of the fittest, and Dr. James Ward's unsympathetic account of the blunders of those whose work, after all, has shed glory on the nineteenth century, might *mutatis mutandis* stand for a description of the history of the advance of civilisation. "The story of the progress so far," he tells us, "is briefly this: Divergence between theory and fact one part of the way, the wreckage of abandoned fictions for the rest, and an unattainable goal of phenomenal nihilism and ultra-physical mechanism beyond" ("Naturalism and Agnosticism," vol. i. p. 153).

"The path of progress," says Prof. Karl Pearson, "is strewn with the wreck of nations. Traces are everywhere to be seen of the hecatombs of inferior races, and of victims who found not the narrow way to the greater perfection. Yet these dead peoples are, in very truth, the stepping-stones on which mankind has arisen to the higher intellectual and deeper emotional life of to-day" ("National Life from the Standpoint of Science," p. 62).

It is only necessary to add that the progress of society is directed towards an unattainable goal of universal contentment, to make the parallel complete.

And so, in the one case as in the other, we may leave "the dead to bury their dead." The question before us is not whether we too may not be trusting to false ideas, erroneous experiments, evanescent theories. No doubt we are; but, without making an insolent claim to be better than our fathers, we may fairly contend that, amid much that is uncertain and temporary, some of the fundamental conceptions, some of the root-ideas of science, are so grounded on reason and fact that we cannot but regard them as an aspect of the very truth.

Enough has, perhaps, now been said on this point for my immediate purpose. The argument as to the constitution of matter could be developed further in the manner I have hitherto adopted, viz. by series of propositions, the proof of each of which is based upon a few crucial phenomena. In particular, if matter is divided into moving granules or particles, the phenomenon of cohesion proves that there must be mutual actions between them analogous to those which take place between large masses of matter, and which we ascribe to force, thereby indicating the regular, unvarying operation of active machinery which we have not yet the means of adequately understanding. For the moment, I do not wish to extend the line of reasoning that has been followed. My main object is to show that the notion of the existence of ultra-physical entities and the leading outlines of the atomic theory are forced upon us at the beginning of our study of Nature, not only by *a priori* considerations, but in the attempt to comprehend the results of even the simplest observation. These outlines cannot be effaced by the difficulties which undoubtedly arise in filling up the picture. The cogency of the proof that matter is coarse-grained is in no way affected by the fact that we may have grave doubts as to the nature of the granules. Nay, it is of the first importance to recognise that, though the fundamental assumptions of the atomic theory receive overwhelming support from a number of more detailed arguments, they are themselves almost of the nature of axioms, in that the simplest phenomena are unintelligible if they are abandoned.

### *The Range of the Atomic Theory.*

It would be most unfair, however, to the atomic theory to represent it as depending on one line of reasoning only, or to treat its evidence as bounded by the very general propositions I have discussed.

It is true that as the range of the theory is extended the fundamental conception that matter is granular must be expanded and filled in by supplementary hypotheses as to the constitution of the granules. It may also be admitted that no complete or wholly satisfactory description of that constitution can as yet be given; that perfection has not yet been attained here or in any other branch of science; but the number of facts which can be accounted for by the theory is very large compared with the number of additional hypotheses which are introduced; and the cumulative weight of the additional evidence obtained by the study of details is such as to add greatly to the strength of the conviction that, in its leading outlines, the theory is true.

It was originally suggested by the facts of chemistry, and though, as we have seen, a school of chemists now thrusts it into the background, it is none the less true, in the words of Dr. Thorpe, that "every great advance in chemical knowledge during the last ninety years finds its interpretation in [Dalton's] theory" ("Essays on Historical Chemistry," 1894, p. 368).

The principal mechanical and thermal properties of gases have been explained, and in large part discovered, by the aid of the atomic theory; and, though there are outstanding difficulties, they are, for the most part, related to the nature of the atoms and molecules, and do not affect the question as to whether they exist.

The fact that different kinds of light all travel at the same speed in interplanetary space, while they move at different rates in matter, is explained if matter is coarse-grained. But to attempt to sum up all this evidence would be to recite a text-book on physics. It must suffice to say that it is enormous in extent and varied in character, and that the atomic theory imparts a unity to all the physical sciences which has been attained in no other way.

I must, however, give a couple of instances of the wonderful success which has been achieved in the explanation of physical phenomena by the theory we are considering, and I select them because they are in harmony with the line of argument I have been pursuing.

When a piece of iron is magnetised, its behaviour is different according as the magnetic force applied to it is weak, moderate, or strong. When a certain limit is passed, the iron behaves as a non-magnetic substance to all further additions of magnetic force. With strong forces it does and with very weak forces it does not remain magnetised when the force ceases to act. Prof. Ewing has imitated all the minute details of these complicated properties by an arrangement of small isolated compass needles to represent the molecules. It may fairly be said that as far as this particular set of phenomena is concerned a most instructive working model based on the molecular theory has not only been imagined but constructed.

The next illustration is no less striking. We may liken a crowd of molecules to a fog; but while the fog is admitted by everybody to be made up of separate globules of water, the critics of scientific method are sometimes apt to regard the molecules as mere fictions of the imagination. If, however, we could throw the molecules of a highly rarefied gas into such a state that vapour condensed on them, so that each became the centre of a water-drop, till the host of invisible molecules was, as it were, magnified by accretion into a visible mist, surely no stronger proof of their reality could be desired. Yet there is every reason to believe that something very like this has been accomplished by Mr. C. T. R. Wilson and Prof. J. J. Thomson.

It is known that it is comparatively difficult to produce a fog in damp air if the mixture consists of air and water-vapour alone. The presence of particles of very fine dust facilitates the process. It is evident that the vapour condenses on the dust particles, and that a nucleus of some kind is necessary on which each drop may form. But electrified particles also act as nuclei; for if a highly charged body from which electricity is escaping be placed near a steam jet, the steam condenses; and a cloud is also formed in dust-free air more easily than would otherwise be the case if electricity is discharged into it.

Again, according to accepted theory, when a current of electricity flows through a gas, some of the atoms are divided into parts which carry positive and negative charges as they move in opposite directions, and unless this breaking-up occurs a gas does not conduct electricity. But a gas can be made a conductor merely by allowing the Röntgen rays or the radiation given off by uranium to fall upon it. A careful study of the facts shows that it is probable that some of the atoms have been broken up by the radiation, and that their oppositely electrified parts are scattered among their unaltered fellows. Such a gas is said to be ionised.

Thus by these two distinct lines of argument we come to the conclusions:—1st, that the presence of electrified particles promotes the formation of mist, and 2nd, that in an ionised gas such electrified particles are provided by the breaking-up of atoms.

The two conclusions will mutually support each other if it can be shown that a mist is easily formed in ionised air. This was tested by Mr. Wilson, who showed that in such air mist is formed as though nuclei were present, and thus in the cloud we have visible evidence of the presence of the divided atoms. If, then, we cannot handle the individual molecules, we have at least some reason to believe that a method is known of seizing individuals, or parts of individuals, which are in a special state, and of wrapping other matter round them till each one is the centre of a discrete particle of a visible fog.

I have purposely chosen this illustration, because the explanation is based on a theory—that of ionisation—which is at present subjected to hostile criticism. It assumes that an electrical current is nothing more than the movement of charges of electricity. But magnets placed near to an electric current tend to set themselves at right angles to its direction; a fact on which the construction of telegraphic instruments is based. Hence if the theory be true, a similar effect ought to be produced by a moving charge of electricity. This experiment was tried many years ago in the laboratory of Helmholtz by Rowland, who caused a charged disc to spin rapidly near a magnet. The result was in accord with the theory; the magnet moved as though acted upon by an electric current. Of late, however, M. Crémieu has investigated the matter afresh, and has obtained results which, according to his interpretation, were inconsistent with that of Rowland.

M. Crémieu's results are already the subject of controversy,[1] and are, I believe, likely to be discussed in the Section of Physics. This is not the occasion to enter upon a critical discussion of the question at issue, and I refer to it only to point out that though, if M. Crémieu's result were upheld, our views as to electricity would have to be modified, the foundations of the atomic theory would not be shaken.

It is, however, from the theory of ions that the most far-reaching speculations of science have recently received unexpected support. The dream that matter of all kinds will some day be proved to be fundamentally the same has survived many shocks. The opinion is consistent with the great generalisation that the properties of elements are a periodic function of their atomic weights. Sir Norman Lockyer has long been a prominent exponent of the view that the spectra of the stars indicate the reduction of our so-called elements to simpler forms, and now Prof. J. J. Thomson believes that we can break off from an atom a part, the mass of which is not more than one-thousandth of the whole, and that these corpuscles, as he has named them, are the carriers of the negative charge in an electric current. If atoms are thus complex, not only is the *a priori* probability increased that the different structures which we call elements may all be built of similar bricks, but the discovery by Lenard that the ease with which the corpuscles penetrate different bodies depends only on the density of the obstacles, and not on their chemical constitution, is held by Prof. Thomson to be "a strong confirmation of the view that the atoms of the elementary substances are made up of simpler parts, all of which are alike."[1] On the present occasion, however, we are occupied rather with the foundations than with these ultimate ramifications of the atomic theory; and having shown how wide its range is, I must, to a certain extent, retrace my steps and return to the main line of my argument.

[1] See *Phil. Mag.*, July 1901, p. 144; and *Johns Hopkins University Circulars*, xx. No. 152, May–June 1901, p. 78.

### *The Properties of Atoms and Molecules.*

For if it be granted that the evidence that matter is coarse-grained and is formed of separate atoms and molecules is too strong to be resisted, it may still be contended that we can know little or nothing of the sizes and properties of the molecules.

It must be admitted that, though the fundamental postulates are always the same, different aspects of the theory, which have not in all cases been successfully combined, have to be developed when it is applied to different problems; but in spite of this there is little doubt that we have some fairly accurate knowledge of molecular motions and magnitudes.

If a liquid is stretched into a very thin film, such as a soap-bubble, we should expect indications of a change in its properties when the thickness of the film is not a very large multiple of the average distance between two neighbouring molecules. In 1890 Sohncke (*Wied. Ann.*, 1890, xl. pp. 345–355) detected evidence of such a change in films of the average thickness of 106 millionths of a millimetre ($\mu\mu$), and quite recently Rudolph Weber found it in an oil-film when the thickness was 115 $\mu\mu$ (*Annalen der Physik*, 1901, iv. pp. 706–721).

Taking the mean of these numbers and combining the results of different variants of the theory, we may conclude that a film should become unstable and tend to rupture spontaneously somewhere between the thicknesses of 110 and 55 $\mu\mu$, and Prof. Reinold and I found by experiment that this instability is actually exhibited between the thicknesses of 96 and 45 $\mu\mu$ (*Phil. Trans.*, 1893, 184, pp. 505–529). There can therefore be little doubt that the first approach to molecular magnitudes is signalled when the thickness of a film is somewhat less than 100 $\mu\mu$, or 4 millionths of an inch.

Thirteen years ago I had the honour of laying before the Chemical Society a *résumé* of what was then known on these subjects (*Chem. Soc. Trans.*, liii., March 1888, pp. 222–262), and I must refer to that lecture or to the most recent edition of O. E. Meyer's work on the kinetic theory of gases ("Kinetic Theory of Gases," O. E. Meyer, 1899; translated by R. E. Baynes) for the evidence that various independent lines of argument enable us to estimate quantities very much less than 4 millionths of an inch, which is perhaps from 500 to 1000 times greater than the magnitude which, in the present state of our knowledge, we can best describe as the diameter of a molecule.

Confining our attention, however, to the larger quantities, I will give one example to show how strong is the cumulative force of the evidence as to our knowledge of the magnitudes of molecular quantities.

We have every reason to believe that though the molecules in a gas frequently collide with each other, yet in the case of the more perfect gases the time occupied in collisions is small compared with that in which each molecule travels undisturbed by its fellows. The average distance travelled between two successive encounters is called the mean free path, and, for the reason just given, the question of the magnitude of this distance can be attacked without any precise knowledge of what a molecule is, or of what happens during an encounter.

Thus the mean free path can be determined, by the aid of the theory, either from the viscosity of the gas or from the thermal conductivity. Using figures given in the latest work on the subject (Meyer's "Kinetic Theory of Gases"; see above), and dealing with one gas only, as a fair sample of the rest, the lengths of the mean free path of hydrogen as determined by these two independent methods differ only by about 3 per cent. Further, the mean of the values which I gave in the lecture already referred to differed only by about 6 per cent. from the best modern result, so that no great change has been introduced during the last thirteen years.

[1] For the most recent account of this subject see an article on "Bodies Smaller than Atoms," by Prof. J. J. Thomson in the *Popular Science Monthly* (The Science Press), August 1901.

It may, however, be argued that these concordant values are all obtained by means of the same theory, and that a common error may affect them all. In particular, some critics have of late been inclined to discredit the atomic theory by pointing out that the strong statements which have sometimes been made as to the equality, among themselves, of atoms or molecules of the same kind may not be justified, as the equality may be that of averages only, and be consistent with a considerable variation in the sizes of individuals.

Allowing this argument more weight than it perhaps deserves, it is easy to show that it cannot affect seriously our knowledge of the length of the mean free path.

Prof. George Darwin (*Phil. Trans.*, 180) has handled the problem of a mixture of unequal spherical bodies in the particular case in which the sizes are distributed according to the law of errors, which would involve far greater inequalities than can occur among atoms. Without discussing the precise details of his problem, it is sufficient to say that in the case considered by him the length of the mean free path is 7/11 of what it would be if the particles were equal. Hence, were the inequalities of atoms as great as in this extreme case, the reduction of the mean free path in hydrogen could only be from 185 to 119 $\mu\mu$; but they must be far less, and therefore the error, if any, due to this cause could not approach this amount. It is probably inappreciable.

Such examples might be multiplied, but the one I have selected is perhaps sufficient to illustrate my point, viz., that considerable and fairly accurate knowledge can be obtained as to molecular quantities by the aid of theories the details of which are provisional, and are admittedly capable of improvement.

### *Is the Model Unique?*

But the argument that a correct result may sometimes be obtained by reasoning on imperfect hypotheses raises the question as to whether another danger may not be imminent. To be satisfactory, our model of Nature must be unique, and it must be impossible to imagine any other which agrees equally well with the facts of experiment. If a large number of hypotheses could be framed with equal claims to validity, that fact would alone raise grave doubts as to whether it were possible to distinguish between the true and the false. Thus Prof. Poincaré has shown that an infinite number of dynamical explanations can be found for any phenomenon which satisfies certain conditions. But though this consideration warns us against the too ready acceptance of explanations of isolated phenomena, it has no weight against a theory which embraces so vast a number of facts as those included by the atomic theory. It does not follow that, because a number of solutions are all formally dynamical, they are therefore all equally admissible. The pressure of a gas may be explained as the result of a shower of blows delivered by molecules, or by a repulsion between the various parts of a continuous medium. Both solutions are expressed in dynamical language; but one is, and the other is not, compatible with the observed phenomena of expansion. The atomic theory must hold the field until another can be found which is not inferior as an explanation of the fundamental difficulties as to the constitution of matter, and is, at the same time, not less comprehensive.

On the whole, then, the question as to whether we are attempting to solve a problem which has an infinite number of solutions may be put aside until one solution has been found which is satisfactory in all its details. We are in a sufficient difficulty about that to make the rivalry of a second of the same type very improbable.

### *The Phenomena of Life.*

But it may be asked—nay, it has been asked—may not the type of our theories be radically changed? If this question does not merely imply a certain distrust in our own powers of reasoning, it should be supported by some indication of the kind of change which is conceivable.

Perhaps the chief objection which can be brought against physical theories is that they deal only with the inanimate side of Nature, and largely ignore the phenomena of life. It is therefore in this direction, if in any, that a change of type may be expected. I do not propose to enter at length upon so difficult a question, but, however we may explain or explain away the characteristics of life, the argument for the truth of the atomic theory would only be affected if it could be shown that living matter does not possess the thermal and mechanical properties, to explain which the atomic theory has been framed. This is so notoriously not the case that there is the gravest doubt whether life can in any way interfere with the action within the organism of the laws of matter in bulk belonging to the domain of mechanics, physics, and chemistry.

Probably the most cautious opinion that could now be expressed on this question is that, in spite of some outstanding difficulties which have recently given rise to what is called Neo-vitalism, there is no conclusive evidence that living matter can suspend or modify any of the natural laws which would affect it if it were to cease to live. It is possible that though subject to these laws the organism while living may be able to employ, or even to direct, their action within itself for its own benefit, just as it unquestionably does make use of the processes of external nature for its own purposes; but if this be so, the seat of the controlling influence is so withdrawn from view that, on the one hand, its very existence may be denied, while, on the other hand, Prof. Haeckel, following Vogt, has recently asserted that "matter and ether are not dead, and only moved by extrinsic force; but they are endowed with sensation and will; they experience an inclination for condensation, a dislike for strain; they strive after the one and struggle against the other" ("Riddle of the Universe," English translation, 1900, p. 380).

But neither unproved assertions of this kind nor the more refined attempts that have been made by others to bring the phenomena of life and of dead matter under a common formula touch the evidence for the atomic theory. The question as to whether matter consists of elements capable of independent motion is prior to and independent of the further questions as to what these elements are, and whether they are alive or dead.

The physicist, if he keeps to his business, asserts, as the bases of the atomic theory, nothing more than that he who declines to admit that matter consists of separate moving parts must regard many of the simplest phenomena as irreconcilable and unintelligible, in spite of the fact that means of reconciling them are known to everybody, in spite of the fact that the reconciling theory gives a general correlation of an enormous number of phenomena in every branch of science, and that the outstanding difficulties are connected, not so much with the fundamental hypotheses that matter is composed of distinguishable entities which are capable of separate motions as with the much more difficult problem of what these entities are.

On these grounds the physicist may believe that, though he cannot handle or see them, the atoms and molecules are as real as the ice crystals in a cirrus cloud which he cannot reach; as real as the unseen members of a meteoric swarm whose death-glow is lost in the sunshine, or which sweep past us, unentangled, in the night.

If the confidence that his methods are weapons with which he can fight his way to the truth were taken from the scientific explorer, the paralysis which overcomes those who believe that they are engaged in a hopeless task would fall upon him.

Physiology has specially flourished since physiologists have believed that it is possible to master the physics and chemistry of the framework of living things, and since they have abandoned the attitude of those who placed in the foreground the doctrine of the vital force. To supporters of that doctrine the principle of life was not a hidden directing power which could perhaps whisper an order that the flood-gates of reservoirs of energy should now be opened and now closed, and could, at the most, work only under immutable conditions to which the living and the dead must alike submit. On the contrary, their vital force pervaded the organism in all its parts. It was an active and energetic opponent of the laws of physics and chemistry. It maintained its own existence not by obeying but by defying them; and though destined to be finally overcome in the separate campaigns of which each individual living creature is the scene, yet like some guerilla chieftain it was defeated here only to reappear there with unabated confidence and apparently undiminished force.

This attitude of mind checked the advance of knowledge. Difficulty could be evaded by a verbal formula of explanation which in fact explained nothing. If the mechanical, or physical, or chemical causes of a phenomenon did not lie obviously upon the surface, the investigator was tempted to forego the toil of searching for them below; it was easier to say that the vital force was the cause of the discrepancy, and that it was hopeless to attempt to account for the action of a principle which was incomprehensible in its nature.

For the physicist the danger is no less serious, though it lies in a somewhat different direction. At present he is checked in his theories by the necessity of making them agree with a comparatively small number of fundamental hypotheses. If this check were removed, his fancy might run riot in the wildest speculations, which would be held to be legitimate if only they led to formulæ in harmony with facts. But the very habit of regarding the end as everything, and the means by which it was attained as unimportant, would prevent the discovery of those fragments of truth which can only be uncovered by the painful process of trying to make inconsistent theories agree, and using all facts, however remote, as the tests of our central generalisation.

"Science," said Helmholtz, "Science, whose very object it it is to comprehend Nature, must start with the assumption that Nature is comprehensible." And again, "The first principle of the investigator of Nature is to assume that Nature is intelligible to us, since otherwise it would be foolish to attempt the investigation at all." These axioms do not assume that all the secrets of the universe will ultimately be laid bare, but that a search for them is hopeless if we undertake the quest with the conviction that it will be in vain. As applied to life, they do not deny that in living matter something may be hidden which neither physics nor chemistry can explain, but they assert that theaction of physical and chemical forces in living bodies can never be understood, if at every difficulty and at every check in our investigations we desist from further attempts in the belief that the laws of physics and chemistry have been interfered with by an incomprehensible vital force. As applied to physics and chemistry, they do not mean that all the phenomena of life and death will ultimately be included in some simple and self-sufficing mechanical theory: they do mean that we are not to sit down contented with paradoxes such as that the same thing can fill both a large space and a little one; that matter can act where it is not, and the like, if by some reasonable hypothesis, capable of being tested by experiment, we can avoid the acceptance of these absurdities. Something will have been gained if the more obvious difficulties are removed, even if we have to admit that in the background there is much that we cannot grasp.

### *The Limits of Physical Theories.*

And this brings me to my last point. It is a mistake to treat physical theories in general, and the atomic theory in particular, as though they were parts of a scheme which has failed if it leaves anything unexplained, which must be carried on indefinitely on exactly the same principles, whether the ultimate results are, or are not, repugnant to common-sense.

Physical theories begin at the surface with phenomena which directly affect our senses. When they are used in the attempt to penetrate deeper into the secrets of Nature, it is more than probable that they will meet with insuperable barriers, but this fact does not demonstrate that the fundamental assumptions are false, and the question as to whether any particular obstacle will be for ever insuperable can rarely be answered with certainty.

Those who belittle the ideas which have of late governed the advance of scientific theory too often assume that there is no alternative between the opposing assertions that atoms and the ether are mere figments of the scientific imagination, or that, on the other hand, a mechanical theory of the atoms and of the ether, which is now confessedly imperfect, would, if it could be perfected, give us a full and adequate representation of the underlying realities.

For my own part I believe that there is a *via media*.

A man peering into a darkened room, and describing what he thinks he sees, may be right as to the general outline of the objects he discerns, wrong as to their nature and their precise forms. In his description fact and fancy may be blended, and it may be difficult to say where the one ends and the other begins; but even the fancies will not be worthless if they are based on a fragment of truth, which will prevent the explorer from walking into a looking-glass or stumbling over the furniture. He who saw "men as trees walking" had at least a perception of the fundamental fact that something was in motion around him.

And so, at the beginning of the twentieth century, we are neither forced to abandon the claim to have penetrated below the surface of Nature, nor have we, with all our searching, torn the veil of mystery from the world around us.

The range of our speculations is limited both in space and time: in space, for we have no right to claim, as is sometimes done, a knowledge of the "infinite universe"; in time, for the cumulative effects of actions which might pass undetected in the short span of years of which we have knowledge, may, if continued long enough, modify our most profound generalisations. If some such theory as the vortex-atom theory were true, the faintest trace of viscosity in the primordial medium would ultimately destroy matter of every kind. It is thus a duty to state what we believe we know in the most cautious terms, but it is equally a duty not to yield to mere vague doubts as to whether we can know anything.

If no other conception of matter is possible than that it consists of distinct physical units—and no other conception has been formulated which does not blur what are otherwise clear and definite outlines—if it is certain, as it is, that vibrations travel through space which cannot be propagated by matter, the two foundations of physical theory are well and truly laid. It may be granted that we have not yet framed a consistent image either of the nature of the atoms or of the ether in which they exist; but I have tried to show that in spite of the tentative nature of some of our theories, in spite of many outstanding difficulties, the atomic theory unifies so many facts, simplifies so much that is complicated, that we have a right to insist—at all events till an equally intelligible rival hypothesis is produced—that the main structure of our theory is true; that atoms are not merely helps to puzzled mathematicians, but physical realities.

---

## SECTION A.

### MATHEMATICS AND PHYSICS.

### OPENING ADDRESS BY MAJOR P. A. MACMAHON, D.SC., F.R.S., PRESIDENT OF THE SECTION.

DURING the seventy meetings of the Association a pure mathematician has been president of Section A on ten or a dozen occasions. A theme taken by many has been a defence of the study of pure mathematics. I take Cayley's view expressed before the whole Association at Southport in 1883, that no defence is necessary, but were it otherwise, I feel that nothing need be added to the eloquent words of Sylvester in 1869 and of Forsyth in 1897. I intend, therefore, to make some remarks on several matters which may be interesting to the Section even at the risk of being considered unduly desultory.

Before commencing I must remark that during the twelve months that have elapsed since the Bradford Meeting we have lost several great men whose lives were devoted to the subjects of this Section. Hermite, the veteran mathematician of France, has left behind him a splendid record of purely scientific work. His name will be always connected with the herculean achievement of solving the general quintic equation by means of elliptic modular functions. Other work, if less striking, is equally of the highest order, and his treatise "Cours d'Analyse" is a model of style. Of Fitzgerald of Dublin it is not easy to speak in this room without emotion. For many years he was the life and soul of this Section. His enthusiasm in regard to all branches of molecular physics, the force and profundity of his speech, the vigour of his advocacy of particular theories, the acute thinking which enabled him to formulate desiderata, his warm interest in the work of others, and the unselfish aid he was so willing to give, are fresh in our remembrance. Rowland was in the forefront of the ranks of physicists. His death at a comparatively early age terminates the important series of discoveries which were proclaimed from his laboratory in the Johns Hopkins University at Baltimore. In Viriamu Jones we have lost an assiduous worker at physics whose valuable contributions to knowledge indicated his power to do much more for science. In Tait, Scotland possessed a powerful and original investigator. The extent and variety of his papers are alike remarkable, and in his collected works there exists an imperishable monument to his fame.

It is interesting, in this the first year of the new century, to take a rapid glance at the position that mathematicians of this country held amongst mathematicians a hundred years ago. During the greater part of the eighteenth century the study of mathematics in England, Scotland and Ireland had been at a very low ebb. Whereas in 1801 on the Continent there were the leaders Lagrange, Laplace and Legendre, and of rising men, Fourier, Ampère, Poisson and Gauss, we could only claim Thomas Young and Ivory as men who were doing notable work in research. Amongst schoolboys of various ages we note Fresnel, Bessel, Cauchy, Chasles, Lamé, Möbius, v. Staudt and Steiner on the Continent, and Babbage, Peacock, John Herschel, Henry Parr Hamilton and George Green in this country. It was not, indeed, till about 1845, or a little later, that we could point to the great names of William Rowan Hamilton, MacCullagh, Adams, Boole, Salmon, Stokes, Sylvester, Cayley, William Thomson, H. J. S. Smith and Clerk Maxwell as adequate representatives of mathematical science. It is worthy of note that this date, 1845, marks also the year of the dissolution of a very interesting society, the Mathematical Society of Spitalfields; and I would like to pause a moment, and, if I may say so, rescue it from the oblivion which seems to threaten it. In 1801 it was already a venerable institution, having been founded by Joseph Middleton, a writer of mathematical text-books, in 1717.[1] The members of the Society at the beginning were for the most part silk-weavers of French extraction; it was little more than a working man's club, at which questions of mathematics and natural philosophy were discussed every Saturday evening. The number of members was limited to the "square of seven," but later it was increased to the "square of eight," and later still to the "square of nine." In 1725 the place of meeting was changed from the Monmouth's Head to the White Horse in Wheeler Street, and in 1735 to the Ben Jonson's Head in Pelham Street. The subscription was six-and-sixpence a quarter, or sixpence a week, and entrance was gained by production of a metal ticket, which had the proposition of Pythagoras engraved on one side and a sighted quadrant with level on the other. The funds, largely augmented by an elaborate system of fines, were chiefly used for the purchase of books and philosophical apparatus. A president, treasurer, inspector of instruments, and secretary were appointed annually, and there were, besides, four stewards, six auditors and six trustees. By the constitution of the Society it was the duty of every member, if he were asked any mathematical or philosophical question by another member, to instruct him to the best of his ability. It was the custom for each member in rotation to lecture or perform experiments on each evening of meeting. There was a fine of half-a-crown for introducing controverted points of divinity or politics. The members dined together twice annually, viz. on the second Friday in January in London in commemoration of the birth of Sir Isaac Newton (this feast frequently took place at the Black Swan, Brown's Lane, Spitalfields), and on the second Friday in July "at a convenient distance in the country in commemoration of the birth of the founder." The second dinner frequently fell through because the members could not agree as to the locality. It was found necessary to introduce a rule fining members sixpence for letting off fireworks in the place of meeting. Every member present was entitled to a pint of beer at the common expense, and, further, every five members were entitled to call for a quart for consumption at the meeting. Such were some of the quaint regulations in force when about the year 1750 the Society moved to larger apartments in Crispin Street, where it remained without interruption till 1843. It appears from the old minute books that about the year 1750 the Society absorbed a small mathematical society which used to meet at the Black Swan, Brown's Lane, above mentioned, and that in 1783 an ancient historical society was also incorporated with it. By the year 1800 the class of the members had become improved, and we find some well-known names, such as Dolland, Simpson, Saunderson, Crossley, Paroissen and Gompertz. At the time lectures were given in all branches of science by the members in the Society's rooms, which on these occasions were open to the public on payment of one shilling. The arrangements for the session 1822–23 included lectures in mechanics, hydrostatics and hydraulics, pneumatics, optics, astronomy, chemistry, electricity, galvanism, magnetism and botany, illustrated by experiments. On account of these lectures the Society had to fight an action-at-law, and although the case was won, its slender resources were crippled for many years. In 1827 Benjamin Gompertz, F.R.S., succeeded to the presidency on the death of the Rev. George Paroissen. From the year 1830 onwards the membership gradually declined and the financial outlook became serious. In 1843 there was a crisis; the Society left Crispin Street for cheaper rooms at 9 Devonshire Street, Bishopsgate Street, and finally, in 1845, after a futile negotiation with the London Institution, it was taken over by the Royal Astronomical Society, which had been founded in 1821. The library and documents were accepted and the few surviving members were made life members of the Astronomical Society without payment. So perished this curious old institution; it had amassed a really valuable library containing books on all branches of science. The Astronomical Society has retained the greater part, but some have found their way to the libraries of the Chemical and other societies. An inspection of the documents establishes that it was mainly a society devoted to physics, chemistry and natural history. It had an extensive museum of curiosities and specimens of natural history, presented by individual members, which seems to have disappeared when the rooms in Crispin Street were vacated. It seems a pity that more effort was not made to keep the old institution alive. The fact is that at that date the Royal Society had no sympathy with special societies and did all in its power to discourage them. The Astronomical Society was only formed in 1821 in the teeth of the opposition of the Royal Society.

Reverting now to the date 1845, it may be said that from this period to 1866 much good work emanated from this country, but no Mathematical Society existed in London. At the latter date the present Society was formed, with De Morgan as its first president. Gompertz was an original member, and the only person who belonged to both the old and new societies. The thirty-three volumes of *Proceedings* that have appeared give a fair indication of the nature of the mathematical work that has issued from the pens of our countrymen. All will admit that it is the duty of any one engaged in a particular line of research to keep himself abreast of discoveries, inventions, methods and ideas, which are being brought forward in that line in his own and other countries. In pure science this is easier of accomplishment by the individual worker than in the case of applied science. In pure mathematics the stately edifice of the Theory of Functions has, during the latter part of the century which has expired, been slowly rising from its foundations on the continent of Europe. It had reached a considerable height and presented an imposing appearance before it attracted more than superficial notice in this country and in America. It is satisfactory to note that during recent years much of the leeway has been made up. English-speaking mathematicians have introduced the first notions into elementary text-books; they have written advanced treatises on the whole subject; they have encouraged the younger men to attend courses of lectures in foreign universities; so that to-day the best students in our universities can attend courses at home given by competent persons, and have the opportunity of acquiring adequate knowledge, and of themselves contributing to the general advance. The Theory of Functions, being concerned with the functions that satisfy differential equations, has attracted particularly the attention of those whose bent seemed to be towards applied mathematics and mathematical physics, and there is no doubt, in analogy with the work of Poincaré in celestial dynamics, those sciences will ultimately derive great benefit from the new study. If, on the other hand, one were asked to specify a department of pure mathematics which has been treated somewhat coldly in this country during the last quarter of the last century, one could point to geometry in general, and to pure geometry, descriptive geometry and the theory of surfaces in particular. This may doubtless be explained by the circumstance that, at the present time, the theory of differential equations and the problems that present themselves in their discussion are of such commanding importance from the point of view of the general advance of mathematical science that those subjects naturally prove to be most attractive.

As regards organisation and cooperation in mathematics, Germany, I believe, stands first. The custom of offering prizes for the solutions of definite problems which are necessary to the general advance obtains more in Germany and in France than here, where, I believe, the Adams Prize stands alone. The idea has an indirect value in pointing out some of the more pressing desiderata to young and enthusiastic students, and a direct importance in frequently, as it proves, producing remarkable dissertations on the proposed questions. The field is so vast that any comprehensive scheme of cooperation is scarcely possible, though much more might be done with advantage.

If we turn our eyes to the world of astronomy we find there a grand scheme of cooperation which other departments may indeed envy. The gravitation formula has been recognised from the time of Newton as ruling the dynamics of the heavens, and the exact agreement of the facts derived from observation with

[1] Its first place of meeting was the Monmouth's Head, Monmouth Street, Spitalfields. This street has long disappeared. From a map of London of 1746 it appears to have run parallel to the present Brick Lane and to have corresponded to the present Wilks Street.

the simple theory has established astronomy as the most exact of all the departments of applied science. Men who devote themselves to science are actuated either by a pure love of truth or because they desire to apply natural knowledge to the benefit of mankind. Astronomers belong, as a rule, to the first category, which, it must be admitted, is the more purely scientific. We not only find international cooperation in systematically mapping the universe of stars and keeping all portions of the universe under constant observation, but also when a particular object in the heavens presents itself under circumstances of peculiar interest or importance, the observatories of the world combine to ascertain the facts in a manner which is truly remarkable. As an illustration, I will instance the tiny planet Eros discovered a few years ago by De Witt. Recently the planet was in opposition and more favourably situated for observation than it will be again for thirty years. It was determined, at a conference held in Paris in July 1900, that combined work should be undertaken by no fewer than fifty observatories in all parts of the world. Beyond the fixing of the elements of the mean motion and of the perturbations of orbit due to the major planets, the principal object in view is the more accurate determination of solar parallax. To my mind this concert of the world, this cosmopolitan association of fine intellects, fine instruments, and the best known methods, is a deeply impressive spectacle and a grand example of an ideal scientific spirit. Other sciences are not so favourably circumstanced as is astronomy for work of a similar kind undertaken in a similar spirit. If in comparison they appear to be in a chaotic state, the reason in part must be sought for in conditions inherent to their study, which make combined work more difficult, and the results of such combined work as there is less striking to spectators. Still, the illustration I have given is a useful object-lesson to all men of science, and may encourage those who have the ability and the opportunity to make strenuous efforts to further progress by bringing the work of many to a single focus.

In pure science we look for a free interchange of ideas, but in applied physics the case is otherwise, owing to the fact that the commercial spirit largely enters into them. In a recent address, Prof. Perry has stated that the standard of knowledge in electrical engineering in this country is not as high as it is elsewhere, and all men of science and many men in the street know that he is right. This is a serious state of affairs, to which the members of this Section cannot be in any sense indifferent. We cannot urge that it is a matter with which another Section of the Association is concerned to a larger degree. It is our duty to take an active, and not merely passive attitude towards this serious blot on the page of applied science in England. For this many reasons might be given, but it is sufficient to instance one, and to state that neglect of electrical engineering has a baneful effect upon research in pure science in this country. It hinders investigations in pure physics by veiling from observation new phenomena which arise naturally, and by putting out of our reach means of experimenting with new combinations on a large scale. Prof. Perry has assigned several reasons for the present *impasse*, viz., a want of knowledge of mathematics on the part of the rising generation of engineers; the bad teaching of mathematics, and antiquated methods of education generally; want of recognition of the fact that engineering is not on stereotyped lines, but, in its electrical aspect, is advancing at a prodigious rate; municipal procrastination, and so on. He confesses, moreover, that he does not see his way out of the difficulty, and is evidently in a condition of gloomy apprehension.

It is, I think, undoubted that science has been neglected in this country, and that we are reaping as we have sowed. The importance of science teaching in secondary schools has been overlooked. Those concerned in our industries have not seen the advantage of treating their workshops and manufactories as laboratories of research. The Government has given too meagre an endowment to scientific institutions, and has failed to adequately encourage scientific men and to attract a satisfactory quota of the best intellects of the country to the study of science. Moreover, private benefactors have not been as numerous as in some other countries in respect of those departments of scientific work which are either non-utilitarian or not immediately or obviously so. We have been lacking alike in science organisation and in effective cooperation in work.

It has been attempted to overcome defects in training for scientific pursuits by the construction of royal roads to scientific knowledge. Engineering students have been urged to forego the study of Euclid, and, as a substitute, to practise drawing triangles and squares; it has been pointed out to them that mathematical study has but one object, viz., the practical carrying out of mathematical operations; that a collection of mathematical rules of thumb is what they should aim at; that a knowledge of the meaning of processes may be left out of account so long as a sufficient grasp of the application of the resulting rules is acquired. In particular, it has been stated that the study of the fundamental principles of the infinitesimal calculus may be profitably deferred indefinitely so long as the student is able to differentiate and integrate a few of the simplest functions that are met with in pure and applied physics. The advocates of these views are, to my mind, urging a process of "cramming" for the work of life which compares unfavourably with that adopted by the so-called "crammers" for examinations; the latter I believe to be, as a rule, much maligned individuals, who succeed by good organisation, hard work and personal influence, where the majority of public and private schools fail; the examinations for which their students compete encourage them to teach their pupils to think, and not to rely principally upon remembering rules. The best objects of education, I believe, are the habits of thought and observation, the teaching of how to think, and the cultivation of the memory; and examiners of experience are able to a considerable extent to influence the teaching in these respects; they show the teachers the direction in which they should look for success. The result has been that the "crammer" for examinations, if he ever existed, has disappeared. But what can be said for the principle of cramming for the work of one's life? Here an examination would be no check, for examiners imbued with the same notion would be a necessary part of the system; the awakening of the student would come, perhaps slowly, but none the less inevitably; he might exist for a while on his formulæ and his methods, but with the march of events, resulting in new ideas, new apparatus, new designs, new inventions, new materials requiring the utmost development of the powers of the mind, he will certainly find himself hopelessly at sea and in constant danger of discovering that he is not alone in thinking himself an impostor. And an impostor he will be if he does not by his own assiduity cancel the pernicious effects of the system upon which he has been educated. I do not, I repeat, believe in royal roads, though I appreciate the advantage of easy coaches in kindred sciences. In the science to which a man expects to devote his life, the progress of which he hopes to further, and in which he looks for his life's success, there is no royal road. The neglect of science is not to be remedied by any method so repugnant to the scientific spirit; we must take the greater, knowing that it includes the less, not the less, hoping that in some happy-go-lucky way the greater will follow.

At the beginning of the nineteenth century it was possible for most workers to be well acquainted with nearly all important theories in any division of science; the number of workers was not great, and the results of their labours were for the most part concentrated in treatises and in a few publications especially devoted to science; it was comparatively easy to follow what was being done. At the present time the state of affairs is different. The number of workers is very large; the treatises and periodical scientific journals are very numerous; the ramifications of investigation are so complicated that it is scarcely possible to acquire a competent knowledge of the progress that is being made in more than a few of the subdivisions of any division of science. Hence the so-called specialist has come into being.

Evident though it be that this is necessarily an age of specialists, it is curious to note that the word "specialist" is often used as a term of opprobrium, or as a symbol of narrow-mindedness. It has been stated that most specialists run after scientific truth in intellectual blinkers; that they wilfully restrain themselves from observing the work of others who may be even in the immediate neighbourhood; that even when the line of pursuit intersects obviously other lines, such intersection is passed by without remark; that no attention is paid to the existence or the construction of connecting lines; that the necessity for collaboration is overlooked; that the general advance of the body of scientific truth is treated as of no concern; that absolute independence of aim is the thing most to be desired. I propose to inquire into the possibility of such an individual existing as a scientific man.

I take as a provisional definition of a specialist in science one who devotes a very large proportion of his energies to original research in a particular subdivision of his subject. It will be sufficient to consider the subjects that come under the purview of Section A, though it will be obvious that a similar train of reasoning would have equal validity in connection with the subjects included in any of the other sections. I take the word "specialist" to denote a man who makes original discoveries in some branch of science, and I deny that any other man has the right, in the modern meaning of the word, to be called by others, or to call himself, a specialist. I would not wish to be understood to imply a belief that a truly scientific man is necessarily a specialist; I do believe that a scientific man of high type is almost invariably an original discoverer in one or more special branches of science; but I can conceive that a man may study the mutual relations of different sciences and of different branches of the same science and may throw such an amount of light upon the underlying principles as to be in the highest degree scientific. I will now advance the proposition that, with this exception, all scientific workers are specialists; it is merely a question of degree. An extreme specialist is that man who makes discoveries in only one branch, perhaps a very narrow branch, of his subject. I shall consider that in defending him I am *a fortiori* defending the man who is a specialist, but not of this extreme character.

A subject of study may acquire the reputation of being narrow either because it has for some reason or other not attracted workers and is in reality virgin soil only awaiting the arrival of a husbandman with the necessary skill, or because it is an extremely difficult subject which has resisted previous attempts to elucidate it. In the latter case, it is not likely that a scientific man will obstinately persist in trying to force an entrance through a bare blank wall. Either from weariness in striving or from the exercise of his judgment he will turn to some other subdivision which appears to give greater promise of success. When the subject is narrow merely because it has been overlooked, the specialist has a grand opportunity for widening it and freeing it from the reproach of being narrow; when it is narrow from its inherent difficulty he has the opportunity of exerting his full strength to pierce the barriers which close the way to discoveries. In either case the specialist, before he can determine the particular subject which is to engage his thoughts, must have a fairly wide knowledge of the whole of his subject. If he does not possess this he will most likely make a bad choice of particular subjects, or, having made a wise selection, he will lack an essential part of the mental equipment necessary for a successful investigation. Again, though the subject may be a narrow one, it by no means follows that the appropriate or possible methods of research are prescribed within narrow limits. I will instance the Theory of Numbers, which, in comparatively recent times, was a subject of small extent and of restricted application to other branches of science. The problems that presented themselves naturally, or were brought into prominence by the imaginations of great intellects, were fraught with difficulty. There seemed to be an absence, partial or complete, of the law and order that investigators had been accustomed to find in the wide realm of continuous quantity. The country to be explored was found to be full of pitfalls for the unwary. Many a lesson concerning the danger of hasty generalisation had to be learnt and taken to heart. Many a false step had to be retraced. Many a road which a first reconnaissance had shown to be straight for a short distance was found, on further exploration, to change suddenly its direction and to break up into a number of paths which wandered in a fitful manner in country of increasing natural difficulty. There were few vanishing points in the perspective. Few, also, and insignificant were the peaks from which a general notion could be gathered of any considerable portion of the country. The surveying instruments were inadequate to cope with the physical characters of the land. The province of the Theory of Numbers was forbidding. Many a man returned empty-handed and baffled from the pursuit, or else was drawn into the vortex of a kind of maelström and had his heart crushed out of him. But early in the last century the dawn of a brighter day was breaking. A combination of great intellects—Legendre, Gauss, Eisenstein, Stephen Smith, &c.—succeeded in adapting some of the existing instruments of research in continuous quantity to effective use in discontinuous quantity. These adaptations are of so difficult and ingenious a nature that they are to-day, at the commencement of a new century, the wonder and, I may add, the delight of beholders. True it is that the beholders are few. To attain to the point of vantage is an arduous task demanding alike devotion and courage. I am reminded, to take a geographical analogy, of the Hamilton Falls, near Hamilton Inlet, in Labrador. I have been informed that to obtain a view of this wonderful natural feature demands so much time and intrepidity, and necessitates so many collateral arrangements, that a few years ago only nine white men had feasted their eyes on falls which are finer than those of Niagara. The labours of the mathematicians named have resulted in the formation of a large body of doctrine in the Theory of Numbers. Much that, to the superficial observer, appears to lie on the threshold of the subject is found to be deeply set in it and to be only capable of attack after problems at first sight much more complicated have been solved. The mirage that distorted the scenery and obscured the perspective has been to some extent dissipated; certain vanishing points have been ascertained; certain elevated spots giving extensive views have been either found or constructed. The point I wish to urge is that these specialists in the Theory of Numbers were successful for the reason that they were not specialists at all in any narrow meaning of the word. Success was only possible because of the wide learning of the investigator; because of his accurate knowledge of the instruments that had been made effective in other branches; because he had grasped the underlying principles which caused those instruments to be effective in particular cases. I am confident that many a worker who has been the mark of sneer and of sarcasm from the supposed extremely special character of his researches would be found to have devoted the larger portion of his time to the study of methods which had been available in other branches, perhaps remote from the one which was particularly attracting his attention. He would be found to have realised that analogy is often the finger-post that points the way to useful advance; that his mind had been trained and his work assisted by studying exhaustively the successes and failures of his fellow-workers. But it is not only existing methods that may be available in a special research.

Furthermore, a special study frequently creates new methods which may be subsequently found applicable in other branches. The Theory of Numbers furnishes several beautiful illustrations of this. Generally, the method is more important than the immediate result. Though the result is the offspring of the method, the method is the offspring of the search after the result. The Law of Quadratic Reciprocity, a cornerstone of the edifice, stands out not only for the influence it has exerted in many branches, but also for the number of new methods to which it has given birth, which are now a portion of the stock-in-trade of a mathematician. Euler, Legendre, Gauss, Eisenstein, Jacobi, Kronecker, Poincaré and Klein are great names that will be for ever associated with it. Who can forget the work of H. J. S. Smith on homogeneous forms and on the five-square theorem, work which gave rise to processes that have proved invaluable over a wide field, and which supplied many connecting links between departments which were previously in more or less complete isolation?

In this connection I will further mention two branches with which I may claim to have a special acquaintance—the theory of invariants and the combinatorial analysis. The theory of invariants was evolved by the combined efforts of Boole, Cayley, Sylvester and Salmon, and has progressed during the last sixty years with the cooperation, amongst others, of Aronhold, Clebsch, Gordan, Brioschi, Lie, Klein, Poincaré, Forsyth, Hilbert, Elliott and Young. It involves a principle which is of wide significance in all the subject-matters of inorganic science, of organic science, and of mental, moral and political philosophy. In any subject of inquiry there are certain entities, the mutual relations of which under various conditions it is desirable to ascertain. A certain combination of these entities may be found to have an unalterable value when the entities are submitted to certain processes or are made the subjects of certain operations. The theory of invariants in its widest scientific meaning determines these combinations, elucidates their properties, and expresses results when possible in terms of them. Many of the general principles of political science and economics can be expressed by means of invariantive relations connecting the factors which enter as entities into the special problems. The great principle of chemical science which asserts that when elementary or compound bodies combine with one another the total weight of the materials is unchanged, is another case in point. Again, in physics, a given mass of gas under the operation of varying pressure and temperature has the well-known invariant, pressure multiplied by volume and divided by absolute temperature. Examples might be multiplied. In mathematics the entities under examination may be arithmetical, algebraical, or geometrical; the processes to which they are subjected may be any of those which are met with in mathematical work. It is the *principle* which is so valuable. It is the *idea* of invariance that pervades to-day all branches of mathematics. It is found that in investigations the invariantive fractions are those which persist in presenting themselves, even when the processes involved are not such as to ensure the invariance of those functions. Guided by analogy may we not anticipate similar phenomena in other fields of work?

The combinatorial analysis may be described as occupying an extensive region between the algebras of discontinuous and continuous quantity. It is to a certain extent a science of enumeration, of measurement by means of integers as opposed to measurement of quantities which vary by infinitesimal increments. It is also concerned with arrangements in which difference of quality and relative position in one, two, or three dimensions are factors. Its chief problem is the formation of connecting roads between the sciences of discontinuous and continuous quantity. To enable, on the one hand, the treatment of quantities which vary *per saltum*, either in magnitude or position, by the methods of the science of continuously varying quantity and position, and on the other hand to reduce problems of continuity to the resources available for the management of discontinuity. These two roads of research should be regarded as penetrating deeply into the domains which they connect.

In the early days of the revival of mathematical learning in Europe the subject of "combinations" cannot be said to have rested upon a scientific basis. It was brought forward in the shape of a number of isolated questions of arrangement, which were solved by mere counting. Their solutions did not further the general progress, but were merely valuable in connection with the special problems. Life and form, however, were infused when it was recognised by De Moivre, Bernoulli and others that it was possible to create a science of probability on the basis of enumeration and arrangement. Jacob Bernoulli, in his "Ars Conjectandi," 1713, established the fundamental principles of the Calculus of Probabilities. A systematic advance in certain questions which depend upon the partitions of numbers was only possible when Euler showed that the identity $x^a x^b = x^{a+b}$ reduced arithmetical addition to algebraical multiplication and *vice versâ*. Starting with this notion, Euler developed a theory of generating functions on the expansion of which depended the formal solutions of many problems. The subsequent work of Cayley and Sylvester rested on the same idea, and gave rise to many improvements. The combinations under enumeration had all to do with what may be termed arrangements on a line subject to certain laws. The results were important algebraically, as throwing light on the theory of Algebraic series, but another large class of problems remained untouched, and was considered as being both outside of the scope and beyond the power of the method. I propose to give some account of these problems, and to give a short history of the way in which a method of solution has been reached. It will be gathered from remarks made above that I regard any department of scientific work which seems to be narrow or isolated as a proper subject for research. I do not believe in any branch of science or subject of scientific work being destitute of connection with other branches. If it appears to be so, it is especially marked out for investigation by the very unity of science. There is no necessarily pathless desert separating different regions. Now a department of pure mathematics which appeared to be somewhat in this forlorn condition a few years ago was that which included problems of the nature of the magic square of the ancients. Conceive a rectangular lattice or generalised chess board (*cf.* "Gitter," Klein), whose compartments are situations for given numbers or quantities, so that there is a rectangular array of certain entities. The general problem is the enumeration of the arrays when both the rows and the columns of the lattice satisfy certain conditions. With the simplest of such problems certain progress had undoubtedly been made. The article on Magic Squares in the "Encyclopædia Britannica" and others on the same subject in various scientific publications are examples of such progress, but the position of isolation was not sensibly ameliorated. Again, the well-known "problème des rencontres" is an instance in point. Here the problem is to place a number of different entities in an assigned order in a line and beneath them the same entities in a different order subject to the condition that the entities in the same vertical line are to be different. This easy question has been solved by generating functions, finite differences, and in many other ways. In fact, when the number of rows is restricted to two the difficulties inherent in the problem when more than two rows are in question do not present themselves. The problem of the Latin Square is concerned with a square of order $n$ and $n$ different quantities which have to be placed one in each of the $n^2$ compartments in such wise that each row and each column contains each of the quantities. The enumeration of such arrangements was studied by mathematicians from Euler to Cayley without any real progress being made. In reply to the remark "Cui bono?" I should say that such arrangements have presented themselves for investigation in other branches of mathematics. Symbolical algebras and in particular the theory of discontinuous groups of operations have their laws defined by what Cayley has termed a multiplication table. Such multiplication tables are necessarily Latin Squares, though it is not conversely true that every Latin Square corresponds to a multiplication table. One of the most important questions awaiting solution in connection with the theory of finite discontinuous groups is the enumeration of the types of groups of given order or of Latin Squares which satisfy additional conditions. It thus comes about that the subject of Latin Squares is important in mathematics, and some new method of dealing with them seemed imperative.

A fundamental idea was that it might be possible to find some mathematical operation of which a particular Latin Square might be the diagrammatic representative. If, then, a one-to-one correspondence could be established between such mathematical operations and the Latin Squares, the enumeration might conceivably follow. Bearing this notion in mind, consider the differentiation of $x^n$ with regard to $x$. Noticing that the result is $nx^{n-1}$ ($n$ an integer), let us inquire whether we can break up the operation of differentiation into $n$ elementary portions, each of which will contribute a unit to the resulting coefficient $n$. If we write down $x^n$ as the product of $n$ letters, viz. $xxxx \ldots$, it is obvious that if we substitute unity in place of a single $x$ in all possible ways, and add together the results, we shall obtain $nx^{n-1}$. We have, therefore, $n$ different elementary operations, each of which consists in substituting unity for $x$. We may denote these diagrammatically by

| 1 | | | | ...    | | 1 | | | ...

| | | 1 | | ... .....

and from this point of view $\frac{d}{dx}$ is a combinatorial symbol, and denotes by the coefficient $n$ the number of ways of selecting one out of $n$ different things.

Similarly, the higher differentiations give rise to diagrams of two or more rows, the numbers of which are given by the coefficients which result from such differentiations. Following up this clue much progress has been made. For a particular problem success depends upon the design, on the one hand, of a function, on the other hand, of an operation such that diagrams make their appearance which have a one-to-one correspondence with the entities whose enumeration is sought. For a general investigation, however, it is more scientific to start by designing functions and operations, and to then ascertain the problems of which the solution is furnished. The difficulties connected with the Latin Square and with other more general questions have in this way been completely overcome.

The second new method in analysis that I desire to bring before the Section had its origin in the theory of partition. Diophantus was accustomed to consider algebraical questions in which the symbols of quantity were subject to certain conditions, such, for instance, that they must denote positive numbers or integer numbers. A usual condition with him was that the quantities must denote positive integers. All such problems, and particularly those last specified, are qualified by the adjective Diophantine. The partition of numbers is then on all fours with the Diophantine equation

$$\alpha+\beta+\gamma+ \ldots +\nu=n,$$

a further condition being that one solution only is given by a group of numbers $\alpha, \beta, \gamma$ . . . satisfying the equation; that in fact permutations amongst the quantities $\alpha, \beta, \gamma$ . . . are not to be taken into account. This further condition is brought in analytically by adding the Diophantine inequalities

$$\alpha \geq \beta \geq \gamma \geq . . . \geq \nu \geq 0$$

$\nu$ in number. The importation of this idea leads to valuable results in the theory of the subject which suggested it. A generating function can be formed which involves in its construction the Diophantine equation and inequalities, and leads after treatment to a representative as well as enumerative solution of the problem. It enables further the establishment of a group of fundamental parts of the partitions from which all possible partitions of numbers can be formed by addition with repetition. In the case of simple unrestricted partition it gives directly the composition by rows of units which is in fact carried out by the Ferrers-Sylvester graphical representation and has led in the hands of the latter to important results in connection with algebraical series which present themselves in elliptic functions and in other departments of mathematics. Other branches of analysis and geometry supply instances of the value of extreme specialisation.

What we require is not the disparagement of the specialist but the stamping out of narrow-mindedness and of ignorance of the nature of the scientific spirit and of the life-work of those who devote their lives to scientific research. The specialist who wishes to accomplish work of the highest excellence must be learned in the resources of science and have constantly in mind its grandeur and its unity.

## SECTION D.

### ZOOLOGY.

OPENING ADDRESS BY PROF. J. COSSAR EWART, M.D., F.R.S., PRESIDENT OF THE SECTION.

*The Experimental Study of Variation.*

THE study of variation may be said to consist (1) in noting and classifying the differences between parents and their offspring; and (2) in determining by observation and experiment the causes of these differences, especially why only some of them are transmitted to future generations. The facts of variation having been dealt with at considerable length in a recent work by Mr. Bateson, I shall discuss chiefly the causes of variation.

Though for untold ages parents have doubtless observed differences in the form and temperament of their children, and though breeders have long noted unlooked-for traits in their flocks and herds, the systematic study of variation is of very recent date. This is not surprising, for while the belief in the immutability of species prevailed, there was no special incentive either to collect the facts or inquire into the causes of variation; and since the appearance in 1859 of the "Origin of Species" biologists have been mainly occupied in discussing the theory of natural selection. Now that discussions as to the nature and origin of species no longer occupy the chief attention of biologists, variability—the fountain and origin of progressive development—is likely to receive an ever-increasing amount of notice. Strange as it may appear, naturalists at the end of the eighteenth century concerned themselves more with the causes of variation than their successors at the end of the nineteenth. Buffon, who discussed at some length nearly all the great problems that interest naturalists to-day, after considering variation arrived at the conclusion that it was due to the direct action of the environment, and even invented a theory (strangely like Darwin's theory of pangenesis) to explain how somatic were converted into germinal variations. Erasmus Darwin and Lamarck also had views as to the causes of variation. Erasmus Darwin believed variability resulted from the efforts of the individual, new structures being gradually evolved by the organisms constantly endeavouring to adapt themselves to their surroundings. Lamarck about the same time endeavoured to prove that changes in the environment produced new needs, which in turn led to the formation of new organs and the modification of old ones, use being specially potent in perfecting the new, disuse in suppressing the old. Both Erasmus Darwin and Lamarck, without attempting, or apparently even seeing the need of, any such explanation as pangenesis offered, assumed that definite acquired modifications were transmitted to the offspring, and they both further assumed that variations occurred not in many but in a single definite direction; hence they had no need to postulate selection. The speculations of Erasmus Darwin and Lamarck having had little influence, it fell to Charles Darwin to construct new and more lasting foundations for the evolution theory.

Charles Darwin, clearly realising that variation occurs in many different directions, arrived at the far-reaching conclusion that the best adapted varieties are selected by the environment, and thus have a chance of giving rise to new species. Though impressed with the paramount importance of selection, Charles Darwin realised that "its action absolutely depends on what we in our ignorance call spontaneous or accidental variation."[1] Darwin, however, concerned himself to the last more with selection than with variation, doubtless because he believed variability sinks to a quite subordinate position when compared with natural selection. As variations stand in very much the same relation to selection as bricks and other formed material stand to the builder, Darwin was perhaps justified in rating so high the importance of the principle with which his name will ever be intimately associated. Though Darwin considered variability of secondary importance, it may be noted that he did more than any other naturalist to collect the facts of variation, and he moreover considered at some length the causes of variation. He regarded with most favour the view "that variations of all kinds and degrees are directly or indirectly caused by the conditions of life to which each being or more especially its ancestors have been exposed."[2] Of all the causes which induce variability, he believed excess of food was probably the most powerful.[3] In addition to variations which arise spontaneously in obedience to fixed and immutable laws, Darwin believed with Buffon that variations were produced by the direct action of the environment, and with Lamarck by the use and disuse of parts; and he accounted for the inheritance of such variations by his theory of pangenesis. Darwin seems always to have regarded the direct action of the environment and use and disuse as, at the most, subsidiary causes of variation; but Mr. Herbert Spencer and his followers regard "use-inheritance" as an all-important factor in evolution; while Cope and his followers in America, by a mixture of "use-inheritance" (Konetogenesis) and Lamarck's neck-stretching theory (Archæsthetism), apparently see their way to account for the evolution of animals with but little help from natural selection.

Prof. Weismann and others, however, have recently given strong reasons for the belief that all variation is the result of changes in the germ-plasm ultimately due to external stimuli, the environment acting directly on unicellular, indirectly on multicellular organism. It is convenient to speak of biologists who believe with Mr. Herbert Spencer in the law of use and disuse (use-inheritance) as Neo-Lamarckians, and of those who with Weismann refuse to accept the doctrine of the transmission of definite acquired characters, and in the case of multicellular organisms the direct influence of the environment as a cause of variation, as Neo-Darwinians. In discussing variability I shall assume that all variations are transmitted by the germ-cells; that the primary cause of variation is always the effect of external influences, such as food, temperature, moisture, &c.; and that "the origin of a variation is equally independent of selection and amphimixis" (Weismann, "The Germ-Plasm," p. 431), amphimixis being simply the means by which effect is given to differences inherited, and to the differences acquired by the germ-cells during their growth and maturation.

Theoretically the offspring should be an equal blend of the parents and (because of the tendency to reversion) of their respective ancestors. In as far as the offspring depart either in an old or in a new direction from this ideal intermediate condition they may be said to have undergone variation. The more obvious variations consist of a difference in form, size and colour, in the rate of growth, in the period at which maturity is reached, in the fertility, in the power to withstand disease and changes in the surroundings, of differences in temperament and instincts, and in the aptitude to learn. In the members of a human family there may be great dissimilarity, and the dissimilarity may be even greater in the members of a single brood or litter of domestic animals, especially if the parents belong to slightly different breeds.

[1] "Animals and Plants," vol. ii, p. 206.

[2] *Ibid.*, vol. ii, p. 240. Elsewhere he says we are "driven to the conclusion that in most cases the conditions of life play a subordinate part in causing any particular modification."

[3] *Ibid.*, vol. ii, p. 282.

Frequently some of the offspring closely resemble the immediate ancestors, while others suggest one or more of the remote ancestors, are nearly intermediate between the parents, or present quite new characters. Similarly seedlings from the same capsule often differ. Can we by way of accounting for these differences only with Darwin say variations are due to fixed and immutable laws, or at the most subscribe to the assertion of Weismann, that they are "due to the constant recurrence of slight inequalities of nutrition of the germ-plasm"? ("Germ-Plasm," p. 431). Weismann accounts for ordinary variation by saying that the reduction of the germ-plasm during the maturation of the germ-cell is qualitative as well as quantitative, *i.e.* that the germ-plasm retained in the ovum to form the female pro-nucleus is different from the germ-plasm discharged in the second polar body. He accounts for discontinuous variation and "sports" by "the permanent action of uniform changes in nutrition" ("Germ-Plasm," p. 431). These uniform changes in nutrition by modifying in a constant direction susceptible groups of germ-units (determinants) after a time giving rise to new, it may be pronounced variation. Must we rest satisfied with these assumptions, or is it possible to account for some of the variability met with by, say, differences in the maturity of the parents or of the germ-cells, by the germ-cells having been influenced by interbreeding or intercrossing, or by the soma in which they are lodged having been invigorated by a change of food, or habitat, or deteriorated by unfavourable surroundings or disease? In other words, are there valid reasons for believing that the germ-cells are extremely sensitive to changes in their immediate environment, *i.e.*, to modifications of the body, or soma containing them, and that the characters of the offspring depend to a considerable extent on whether the germ-cells have recently undergone rejuvenescence?

Obviously if the offspring, other things being equal, vary with the age of the parents, the ripeness of the germ-cells and with the bodily welfare, the qualitative division of the nucleus on which Weismann so much relies as an explanation of ordinary variation will prove inadequate.

### *Is Age a Cause of Variation?*

During the course of my experiments on Variation I endeavoured to find an answer to the question, "Is Age a Cause of Variation?" During development and while nearly all the available nourishment is required for building up the organs and tissues of the body, the germ-cells remain in a state of quiescence. Sooner or later, however, they begin to mature, and eventually in most cases escape from the germ-glands. I find the first germ-cells ripened often prove infertile. When, *e.g.*, pigeons from the same nest are isolated and allowed to breed as soon as mature, they seldom hatch out birds from the first pair of eggs, and though quite vigorous in appearance they may only hatch a single bird from the second pair of eggs. The same result generally follows mating very young but quite unrelated pigeons; but when a young hen bird is mated with a vigorous, well-matured male, or a young male is mated with a vigorous, well-matured female, the eggs generally prove fertile from the first. The germ-cells are, as far as can be determined, structurally perfect from the outset; and that they only fail in vigour is practically proved by the fact that though the conjugation of germ-cells from two young birds leads to nothing, the conjugation of germ-cells from quite young birds with germ-cells from mature birds generally at once results in offspring.

The following experiments indicate how age may prove a cause of variation. Last autumn I received from Islay two young male blue-rock pigeons which, though bred in captivity, were believed to be as pure as the wild birds of the Islay caves. In February last one of the young blue-rocks, while still immature, was placed with an inbred white fantail, the other with an extremely vigorous well-matured black barb. In course of time a pure-white bird was reared by the white fantail, and two dark birds by the black barb. Owing probably to the fantail being inbred and the blue-rock being still barely mature, the young white bird died soon after leaving the nest. No birds were hatched from the second and third pairs of eggs laid by the fantail, but from the fourth pair two birds were hatched which are now nearly full-grown. These young birds are of a darker shade of blue, and look larger and more vigorous than their blue-rock sire. As in the Indian variety of the blue-rock pigeon the croup is blue, and, as in some of the Eastern blue-rocks, the wings are slightly chequered. They, however, only essentially differ from their sire in having four extra feathers in the tail. The first pair of birds hatched by the black barb when they reached maturity early in August might have passed for young barbs with somewhat long beaks. Since the first pair were hatched in March the blue-rock and black barb have reared six other birds. One of the second brood closely resembles the first birds hatched; the other is of a greyish colour, with slightly mottled wings, a long beak, and a tail bar. The birds of the third nest are both of a greyish colour, but have indistinct wing bars as well as a tail bar. Of the fourth pair of young, one is greyish like the birds of the third nest, the other is of a dark blue colour with slightly chequered wings, and a head, beak and bars as in its blue-rock sire. The gradual change from black to dark blue in the blue-rock barb crosses is very remarkable. I can only account for the almost mathematical regularity of the change by supposing it has kept pace with a gradual increase in the vigour or prepotency in the young blue-rock. Eventually the offspring of the blue-rock mated to the black barb, like the offspring of its brother with the white fantail, may be of a slaty blue colour, and otherwise resemble a wild blue-rock pigeon. Many breeders would explain the offspring taking more and more after the sire by the doctrine of Saturation—a doctrine that finds much favour amongst breeders—but as identical results were obtained when young females were mated with well-matured males the saturation explanation falls to the ground.

Like results were obtained by breeding young grey quarter-wild rabbits with an old white Angora buck: the first young were white, the subsequent young were white, grey and bluish grey. From these results it follows that when old and young but slightly different members of a variety or species are marked a wonderfully perfect series of intermediate forms is likely to be produced. Amongst wild animals the young males rarely have a chance of breeding with the young females; hence amongst wild animals, owing to age being a cause of variation, a considerable amount of material is doubtless constantly provided for selection, thus affording a variety an additional chance of adapting itself to slight fluctuations in the environment.

In the results obtained by crossing mature, vigorous, and, in some cases, inbred males with barely mature females, an explanation may be found why in some families the same features have persisted almost unaltered for many generations; why in his features the squire of to-day sometimes exactly reproduces the lines of his ancestors, as seen in portraits and monumental brasses. It should, however, be borne in mind that highly prepotent forms are capable from the first of so completely controlling the development that they transmit their peculiar traits to all their offspring.

### *Is Ripeness of the Germ-Cells a Cause of Variation?*

While difference in age may sometimes account for the earlier broods and litters resembling one of the parents, it fails to account for the very pronounced variation often found in a single brood or litter, and for much of the dissimilarity between members of the same human family. When a *single* fertilised germ-cell, as occasionally happens, gives rise to twins, they are always identical; hence it may be assumed the differences in members of the same family have their source in differences in the germ-cells from which they spring. If the offspring vary with the maturity of the soma, it may also vary with the maturity of the germ-cells, or at least with their condition at the moment of conjugation.

Some years ago Mr. H. M. Vernon, when hybridising echinoderms, discovered that "the characteristics of the hybrid offspring depend directly on the relative degrees of maturity of the sexual products" (*Proceedings* Royal Society, vol. lxiii. May 1898). Mr. Vernon found subsequently that over-ripe (stale) ova fertilised with fresh sperms gave very different results from fresh ova fertilised with over-ripe (stale) sperms, from which he inferred that over-ripeness (staleness) is a very potent cause of variation (*ibid.*, vol. lxv. November 1899).

I find that if a well-matured rabbit doe is prematurely (*i.e.*, some time before ovulation is due) fertilised by a buck of a different strain, the young take after the sire; when the fertilisation takes place at the usual time, some of the young resemble the buck, some the doe, while some present new characters or reproduce more or less accurately one or more of the ancestors. When, however, the mating is delayed for about thirty hours beyond the normal time, all the young, as a rule, resemble the doe. It may hence be inferred that in mammals, as in echinoderms, the characters of the offspring are related to the condition of the germ-cells at the moment of conjugation, the offspring resulting from the union of equally ripe germ-cells differing from the offspring developed from the conjugation of ripe and unripe germ-cells, and still more from the union of fresh and over-ripe germ-cells. This conclusion may be said to be in harmony with the view expressed by Darwin, that the causes which induce variability probably act "on the sex elements before impregnation has been effected" ("Animals and Plants," vol. ii. p. 259). The results already obtained, though far from answering the question why there is often great dissimilarity between members of the same family, may lead to further experiment, and especially to more complete records being kept by breeders. It is unnecessary to point out what a gain it would be were breeders able to regulate, even to a small extent, the characters of the offspring.

*Is the Condition of the Soma a Cause of Variation?*

There is a considerable amount of evidence in support of the view that changes in any part of the body or soma which affect the general welfare influence the germ-cells. This is but what might be expected if the soma in the metazoa is to the germ-cells what the immediate surroundings are to the protozoa. The soma from the first forms a convenient nidus for the germ-cells, and when sufficiently old and sufficiently nourished it provides the stimuli by which the ripening (maturing) of the germ-cells is effected. If in the case of the protozoa variation is due to the direct action of the environment, it may be inferred that in the metazoa variations of the germ-cells result from the direct action of the soma, *i.e.*, from the direct action on the germ-cells of their immediate environment. This, however, is quite a different thing from saying that definite somatic variations are incorporated in the germ-cells (converted into germinal variations) and transmitted to the offspring.

It may first be asked, Does disease in as far as it reduces the general vigour or interferes with the nutrition of the germ-cells act as a cause of variation? I recently received a number of blue-rock pigeons from India infected with a blood parasite (Halteridium) not unlike the organism now so generally associated with malaria. In some pigeons the parasites were very few in number, in others they were extremely numerous. The eggs of a pair of these Indian birds with numerous parasites in the blood proved infertile. Eggs from a hen with numerous parasites fertilised by a cock with a few parasites proved fertile, but the young died before ready to leave the nest. An old male Indian bird, however, with comparatively few parasites mated with a mature half-bred English turbit produced a single bird. The half-bred turbit has reddish wings and shoulders, but is otherwise white. The young bird by the Indian blue-rock is of a reddish colour nearly all over, but in make not unlike the cross-bred turbit hen.

Some time before the second pair of eggs were laid the parasites had completely disappeared from the Indian bird, and he looked as if he had quite recovered from his long journey, as well as from the fever. In due time a pair of young were hatched from the second eggs, and as they approached maturity it became more and more evident that they would eventually present all the distinctive points of the wild-rock pigeon.[1] The striking difference between the first bird reared and the birds of the second nest might, however, be due not to the malaria parasites but to the change of habitat.

Against this view, however, is the fact that another Indian bird infected to about the same extent as the mate of the half-bred red turbit counted for little when mated with a second half-bred turbit; while two Indian birds in which extremely few parasites were found at once produced blue-rock-like birds when bred—one with a fantail, the other with a tumbler.

Another possible explanation of the difference between the bird of the first and the birds of the second nest is that the germ-cells were for a time infected by the minute protozoan Halteridium in very much the same way as the germ-cells of ticks are infected by the parasite of Texas fever. But of this there is no evidence, for even in the half-grown birds hatched by the pure-bred malarious Indian rocks the most careful examination failed to detect any parasites in the blood. In all probability Halteridium can only be conveyed from one pigeon to another by Culex or some other gnat.

These results with pigeons suffering from malaria seem to indicate that the germ-cells are liable to be influenced by fevers and other forms of disease that for the time being diminish the vitality of the parents. Further experiments may show that the germ-cells are influenced in different ways by different diseases.

Sometimes the germ-cells suffer from the direct action of their immediate environment, from disturbance in or around the germ-glands. If, for example, inflammation by the ducts or other channels reaches the germ-glands, the vitality of the germ-cells may be considerably diminished; if serious or prolonged, the germ-cells may be as effectively sterilised as are the bacteria of milk by boiling.

In 1900 two mares produced foals to a bay Arab which had previously suffered from a somewhat serious illness involving the germ-glands. These foals in no way suggest their sire. This year I have three foals by the same Arab after he had quite recovered: one promises to be the image of his sire, and the other two are decidedly Arab-like both in make and action.

While the germ-cells are liable to suffer when the soma is the subject of disease, there is no evidence that they are capable of being so influenced that they transmit definite or particular modifications (unless directly infected with bacteria or other minute organisms); that, *e.g.*, the germ-cells of gouty subjects necessarily give rise to gouty offspring. Doubtless if the germ-cells, because of their unfavourable immediate surroundings, suffer in vigour or vitality, the offspring derived from them are likely to be less vigorous, and hence more likely than their immediate ancestors to suffer from gout and other diseases.

It would be an easy matter to give instances of the offspring varying with the condition or fitness of the parents; but it will suffice if, before discussing intercrossing, I refer to the influence of a change of habitat.

*Is Change of Habitat a Cause of Variation?*

It has long been recognised that a change of surroundings may profoundly influence the reproductive system, in some cases increasing the fertility, in others leading to complete sterility. Exotic plants, sterile it may be at first, often become extremely fertile, and when thoroughly established give rise to new varieties. In the case of mares obtained from Iceland and the south of England sometimes a year elapses before they breed. An Arab-Kattiawar pony which arrived during April from India proved during the first three months quite sterile, owing, I believe, to loss of vigour on the part of the germ-cells, their vitality being only about one-tenth that of a home-bred hackney pony. But the fertility is apparently greatly impaired by even comparatively slight changes of environment. Lions which breed freely in Dublin seem to be sterile in London, and I heard recently that when bulls are changed from one district to another in the north of Ireland complete sterility is sometimes the result. The tendency of some exotic plants to "sport" after they become acclimatised is doubtless due to the fact that their new habitat is unusually favourable, their general vigour—so essential for new developments—is increased, and, probably because certain groups of germ units are constantly stimulated by the new food available, they give rise abruptly or gradually to new and it may be unexpected characters. No one doubts that the bodily vigour is liable to be impaired by fevers and other diseases, by changes in the habitat, unsuitable food, rapid and unseasonable changes of temperature, and the like; hence it will not be surprising if further investigations prove that changes in the soma, beneficial as well as injurious, are reflected in the germ-cells, and thus indirectly induce variation. Moreover there are excellent reasons for believing that the germ-cells are influenced by seasonable changes, such as moulting in birds and changing the coat in mammals. In the case of pigeons, *e.g.*, the young bred in early summer are, other things being equal, larger and more vigorous, and mature more rapidly, than birds hatched in the late summer or autumn. But however sensitive the germ-cells may be to the changes of their immediate environment, *i.e.*, the soma or body in which they are lodged, there is no evidence whatever that (as Buffon asserted and Darwin thought possible) definite changes of the soma, due to the direct action of the environment, can be imprinted on the germ-cells. By the direct action of the environment—food, temperature, moisture, &c.—the body in whole or in part may be dwarfed, increased, or otherwise modified; but such changes only influence the germ-cells in as far as they lead to modifications of the body as a whole. They may expedite or delay maturity, after the length of the reproductive period, interfere with the nutrition of the germ-cells, or retard the development of the embryo, but they seem incapable of giving rise to definite structural or functional variations in the offspring.

[1] In these young birds the breast and some of the wing feathers are imperfect. Fanciers regard this condition of the feathers as evidence of constitutional weakness.

*Intercrossing and Interbreeding as Causes of Variation.*

The belief was once common amongst naturalists that variability was wholly due to crossing, and at the present day naturalists and breeders alike agree that intercrossing is a potent cause of variability, and are unanimous in regarding interbreeding as an equally potent means of checking variability. The opinion is also general that intercrossing has a swamping influence; that having brought forth new forms it forthwith proceeds to destroy them. Darwin, when discussing reversion, points out that intercrossing often speedily leads to almost complete reversion to a long-lost ancestor, *i.e.*, to the loss of recently acquired and the reappearance of long lost characters "Animals and Plants," vol. i. p. 22). When, however, he comes to deal with variability, he states that "crossing, like any other change in the conditions of life, seems to be an element, probably a potent one, in causing variability (*ibid.*, vol. ii. p. 254), the offspring of the first generation being generally uniform, but those subsequently produced displaying an almost infinite diversity of character. As to the influence of inbreeding he says, "close interbreeding, if not carried to an injurious extreme, far from causing variability, tends to fix the character of each breed (*ibid.*, vol. ii. p. 251).

These statements may be quoted in support of the very common belief that intercrossing is both a potent cause of variation and of reversion; that it produces new varieties one moment and swamps them the next. Whether intercrossing may be regarded as the immediate cause of variation or of reversion (it can hardly be both) depends on what is implied by variation. Obviously variation may be either progressive or retrogressive, *i.e.*, the offspring may differ from their parents in having quite new characters or in presenting ancestral characters, or in being characterised by traits neither new nor old, due to new combinations of characters already recognised as belonging to the variety or species. When intercrossing results in the restoration of old characters, we have reversion or retrogressive variation; when to new combinations of already existing characters like new combinations in a kaleidoscope, we have new variations of a non-progressive kind, almost always characterised by more or less reversion; when, however, intercrossing results in the characters of one variety being engrafted on another, or to the appearance of characters quite new to the species, we have progressive variation. Judging from the results I have obtained, intercrossing of two distinct varieties results, as a rule, in the loss of the more striking characters of both parents, *i.e.* in more or less marked reversion, the extent of the loss generally depending on the difference between the forms crossed. For example, if an owl pigeon is crossed with a pigeon known among fanciers as an archangel, nondescript birds are obtained, which may at once, with a white fantail, give birds almost identical with a blue-rock—the common ancestor of all our breeds of pigeons. Intercrossing, on the other hand, rarely leads to the blending in one individual of the unaltered characters of two or more varieties, and it never results in the appearance of characters absolutely new to the species. In a word, the immediate result of intercrossing distinct varieties is, as a rule, more or less marked reversion. But though intercrossing usually results in retrogressive variation, it is *indirectly* an extremely potent cause of progressive variation. This is due to the fact (better realised by botanists than zoologists) that cross-bred offspring (first crosses) are (unless the parents have been enfeebled by interbreeding) endowed with an unusual amount of vigour, *i.e.*, intercrossing is of supreme importance, not only because it leads to the co-mingling of germ-plasms having different tendencies, but also and perhaps chiefly because of its rejuvenating influence. The importance of this rejuvenation is usually at once evident if intercrossing is immediately followed by interbreeding. The interbreeding of closely related forms generally reduces the vigour, and, as Darwin points out, "far from causing variability, tends to fix the character of each breed" ("Animals and Plants," vol. ii. p. 251); but the intercrossing of first crosses (or of highly vigorous individuals closely related in either the direct or the collateral line) without appreciably weakening the constitution, often results in offspring displaying, to use Darwin's words, "an almost infinite diversity of character" (*ibid.*, vol. ii. p. 256). The epidemics of variation, so often the outcome of interbreeding first or at least vigorous recently produced crosses, are apparently partly due to the union of individuals having a similar tendency checking reversion, and partly to the vigour acquired by recent intercrossing. This much may be inferred from the fact, that when interbreeding is persisted in the variability dwindles as the vigour ebbs.

Breeders agree with Darwin that first crosses are generally uniform, and that the subsequent offspring usually vary immensely; yet neither breeders nor naturalists seem to have clearly realised that interbreeding at the right moment is the *direct* cause of variation, while intercrossing is, except in very rare cases, at the most an *indirect* cause of variation.

It may be here said that it is impossible to over-estimate the importance of vigour in studying variation. Without vigour no race or breed can maintain its position; without renewed vigour it is hardly likely to develop new characters. The new vigour, as already explained, may be obtained by intercrossing; but it may also be acquired, especially in plants, by a change of surroundings accompanied by a plentiful supply of suitable food.

With rigid selection the gradual loss of vigour may escape notice, but when selection is suspended rapid deterioration (from the fancier's standpoint) is the inevitable result. If, *e.g.*, a number of pigeons, good specimens of a distinct breed, are isolated and left unmolested for a few years, they rapidly degenerate, *i.e.*, they lose their show points (be they peaks, frills, ruffs, or metallic tints) and reassume the more fixed ancestral characters. If, however, the less characteristic birds are eliminated, and high-class birds are from time to time introduced from another loft, the vigour and the distinctive traits are indefinitely preserved.

If the age and condition of the soma and the state of ripeness of the germ-cells are potent factors, and especially if vigour counts for much, the difficulties of breeders become intelligible, and the unlikeliness of intercrossing being a direct cause of variation all the more evident. The most that can be expected from intercrossing is the engrafting on one breed the characters of another. Even this rarely happens, and is only possible when the two breeds are somewhat allied. It is impossible, *e.g.*, to unite in one individual all the points of a fantail and a pouter, or of a fantail and a jacobin; but given healthy, vigorous birds, the points of an owl may be engrafted on a barb. Or to take another example, the black ears, feet, &c., of a Himalaya rabbit may be combined with the characteristic form, long hair and habits of an Angora. It may be impossible to predict what will happen when intercrossing is resorted to, but if pure-bred members of a distinct variety are experimented with —and it is useless working with either plants or animals of unknown origin—characters not already present in one of the varieties need not be looked for.

But while interbreeding at the right moment may be a cause of progressive variation, at other times it leads to what is perhaps best described as degeneration. When, *e.g.*, very young members of the same brood or litter, or unhealthy, closely related individuals, or quite mature and apparently vigorous but for several generations closely related animals are interbred, the offspring frequently differ from their parents. They are often delicate and highly sensitive, and unable to survive unless provided with highly nutritious food; and though they mature numerous germ-cells they rear but few offspring, and, what is still more striking, they are sometimes either white or all but devoid of pigment. Offspring thus characterised, especially when white or nearly white in colour, *e.g.*, nearly white pheasants, partridges, and woodcock, white specimens of the brown hare, white squirrels, &c., are sometimes regarded as distinct varieties, but when the departure from the normal colour, &c., is the result of close inbreeding it is best regarded as degeneration.

In the spring of 1900 I crossed a quarter-wild grey doe rabbit with a closely inbred black-and-white buck. The young obtained varied considerably in colour: to one of her offspring coloured like the sire, the grey doe produced a second litter, all but one decidedly lighter in colour than the sire. Two of the darker members of this litter produced almost white young, and to one of them the original grey doe has recently produced a light-coloured litter consisting of two pure white specimens, two with only a narrow dorsal band, two fawn-coloured and one black. Close interbreeding with goats and pigeons yields similar results. Birds on small remote Pacific islands are sometimes marked with irregularly disposed white patches. These pie-bald birds, like light-coloured pheasants, cream-coloured partridges and dun-coloured rooks, may also be the victims of close inbreeding.

*The Swamping Effects of Intercrossing.*

The question "Are new varieties liable to be swamped by intercrossing?" is perhaps the most important now pressing for an answer from biologists. What would happen, for example, if specimens of all the different breeds of cattle were set free and left unmolested on a large area? Would they some centuries hence be represented by several breeds or by one? Many would answer this question by saying that unless some of them in course of time were isolated by mountains, deserts, or other physical barriers, they would eventually through intercrossing give rise to a single breed. To this question Darwin would, I think, have given a somewhat different answer, for, while admitting "that isolation is of considerable importance in the production of new species," he was, on the whole, "inclined to believe that largeness of area is of more importance ("Origin of Species," p. 104). Unfortunately Darwin nowhere indicates how he supposed new varieties escape being swamped by intercrossing. His silence on this important point is difficult to explain, for during his lifetime the influence of intercrossing in checking progress except in one direction was often enough insisted on. Huxley tells us that in his earliest criticisms of the "Origin" "he ventured to point out that its logical foundation was insecure so long as experiments in selective breeding had not produced varieties which were more or less infertile" ("Life of Professor Huxley," p. 170). Later Moritz Wagner and others pointed out the important part physical isolation had played in the origin of species; and later still Romanes endeavoured to show how the blighting influence of free intercrossing might be overcome by physiological selection, Romanes, like Huxley, believing several varieties might be evolved in the same area if more or less mutually infertile. Evidence of the importance of physical isolation is plentiful enough; but neither has experimental nor selective breeding proved that physiological isolation has been instrumental in arresting the swamping effects of intercrossing. Hence, according to Huxley and others, the foundation of Darwin's doctrine of natural selection must still be regarded as insecure. Is intersterility the only possible means by which new varieties can be saved from premature extinction, from being destroyed before they have a chance of proving their fitness to survive? In other words, are barriers as essential among wild as among domestic animals? It does not seem to have occurred to the biologists who so fully realised the need of isolation that the old varieties instead of swamping might be swamped by the new, and that several varieties might sometimes be sufficiently exclusive to flourish and eventually give rise to a like number of species in the same area. If on an island two new varieties of sheep appeared sufficiently vigorous, or, as we say, sufficiently prepotent, to swamp all the other varieties—as the ill-favoured lean kine did eat up the fat ones—and yet so exclusive that their cross-bred offspring invariably belonged to the one new variety or the other, for their preservation fences and other barriers would be superfluous.

Is there any evidence that by prepotency the swamping of new varieties is sometimes checked, and that by exclusive inheritance two or more varieties, though mutually fertile, may persist in the same area, occasionally intercrossing with each other, but neither giving up nor taking from each other any of their distinctive characters? I have in my possession a skewbald Iceland pony that produces richly striped hybrids to a zebra, but skewbald offspring the image of herself in make, colour, and temperament to whole-coloured bay Arab and Shetland ponies. This pony instead of being swamped invariably swamps older breeds. A number of prepotent skewbald ponies, wherever placed, would (especially with the help of preferential mating) in all probability soon give rise to a distinct race such as once existed in the East. What is true of the Equidæ is equally true of other groups. Black hornless Galloway bulls are often so prepotent that their offspring with long-horned brightly-coloured Highland heifers readily pass for pure-bred Galloways. The wolf is so prepotent over the dog, as the wild rabbit, rat, and mouse are prepotent over their tame relatives. As an instance of prepotency in rabbits, I may give the results of an interbreeding experiment with a grey doe, the granddaughter of a wild rabbit, and an inbred buck richly spotted like a Dalmatian hound. Of six young in the first litter three were like the sire. To one of her sons, the grey doe next produced eight young, all richly spotted, and subsequently to one of her spotted grandsons she produced two spotted, two white, and two grey offspring. Similar results are obtained with plants; hybrid orchids, *e.g.*, sometimes reproduce all the characters of one of the parents.

It need hardly be insisted on that if new varieties, well adapted for their environment, are not only sufficiently prepotent to escape being swamped by other varieties, but are also, like the spotted rabbit, able to hand on the prepotency almost unimpaired to a majority of their descendants, progressive development along a definite line will be possible. But of even more importance than prepotency is what for want of a better name may be known as exclusive inheritance. Recently a vigorous mature Indian blue-rock pigeon mated with an inbred and equally mature fantail, hatched and reared two birds, one exactly like a blue-rock, but with fourteen instead of twelve tail feathers; the other characterised by all the points of a high-class fantail, the tail feathers being thirty in number—two fewer than in the fantail parent, but eighteen more than in the blue-rock parent. In this case the blue-rock was the exclusive bird, the fantail having previously produced birds with only sixteen feathers in the tail when mated with an ordinary dovecot pigeon. A still more striking example of exclusive inheritance we have in the crow family. The carrion crow and the hooded crow are so unlike in colour that they were long regarded as two distinct species; now they are said to be two varieties of the same species. The carrion crow is black all over, but in the hooded crow the breast and back are grey. These two crows cross freely (but for this they would probably still rank as distinct species); but in the crossbred young there is never any blending—they are either black or grey, usually both varieties occurring in the same nest. Similar exclusiveness occurs amongst mammals. When distinct varieties of cats are crossed, some of the young usually resemble one breed, some the other, and the distinctions may persist for several generations. A white crossed with a tabby-coloured Persian cat produced a pair of white and a pair of tabby-coloured young; the two white cats when interbred also produced two white and two tabby-coloured individuals. I find cats are far more exclusive than rabbits; perhaps it is partly for this reason we have so many species and varieties of wild cats, so few species and varieties of wild rabbits. Another very striking instance of exclusiveness we have in the Ancon or "Otter" sheep common in New England at the end of the eighteenth century. This breed, which was characterised by short crooked legs and a long back like a turnspit dog, descended from a ram-lamb born in Massachusetts in 1791. The offspring of this "sport" were never intermediate in their characters; they were either like the original Ancon ram or like the breeds, some thirteen in number, with which he was mated. Frequently in the case of twins one was otter-like, the other an ordinary lamb. More remarkable still, the Ancon-like crosses, generation after generation, were as exclusive as their crooked-legged ancestor.

Another familiar example of exclusiveness we have in the peppered moth, a dark variety of which in a few years swamped the older light variety throughout a considerable part of England, and is now extending its range on the Continent. It thus appears that when a new variety is sufficiently prepotent, instead of being swamped it may actually swamp the old-established variety; and that when two or more varieties are sufficiently exclusive they may flourish side by side, and eventually give rise to two or more distinct species.

Prepotency may hence be said to supplement and complete the work of the environment. The environment seems to be mainly concerned in eliminating the unfit; whether any of the survivors persist depends not so much on their surroundings as on whether they are sufficiently prepotent and exclusive to escape being swamped by intercrossing. This way of accounting for progress in one or more directions may prove as inadequate as the one suggested by isolationists, but it has the merit of being more easily tested by experiment. It not only gets rid of the swamping bugbear, but makes it matter of indifference whether (to quote from the President's address at the last Oxford meeting of the Association) "the advantageously varied bridegroom at the one end of the wood meets the bride, who, by a happy contingency, had been advantageously varied in the same direction, and at the same time, at the other end of the wood." Further, as a highly prepotent vigorous variety can well afford to maintain a number of budding organs, it helps us to understand how luminous, electric, and certain other structures were nursed up to the point when they began to count in the struggle for existence.

*Doubtful Causes of Variation.*

Having indicated how maturity of the soma and of the germ-cells, and how bodily welfare and interbreeding may act as causes of variation, and also how swamping of the new variations may be checked, I shall now refer to certain supposed causes of variation.

*Maternal Impressions.*

I may begin with the widespread belief that the offspring are capable of being influenced in form, colour, and temperament by maternal impressions—the belief we associate with the skilful shepherd who peeled wands and stuck them up before the fulsome ewes. Muller ("Elements of Physiology," vol. ii. p. 1405) more than half a century ago, conclusively argued against the belief in maternal impressions, but the belief still prevails. I know of two able naturalists who subscribe to the maternal impression doctrine, and it is firmly held by many breeders and by not a few physicians. A writer in a recent number of a quarterly (*Bibby's Quarterly*, Autumn Number, 1900, p. 163), which circulates widely amongst farmers and stock-keepers, boldly asserts that the existence of impressions which affect progeny (more especially in colour) is a settled fact. This writer supports his case by referring to a highly successful breeder of polled Angus cattle, who considered it necessary to surround his herd "with a tight black fence in order to keep the females from dropping red calves because they saw the red herds of his neighbours." Reference is also made by this writer to the belief, common in certain parts of England, that whitewashed byres, regardless of the colour of the parents, produce light-coloured calves; that the colour of foals is often more influenced by the stable companion of the dam than by her own colour or that of the sire; and that even the colour of birds varies with the immediate surroundings, fowls, *e.g.*, however carefully penned, hatching birds resembling in colour the hens they habitually see in a neighbouring run. If maternal impressions thus influence the offspring they must be one of the most effective causes of variation. During the last six years I have bred many hundreds of animals, but the nearest approach to an instance of maternal impressions was a dark pup with a white ring half round the neck, which suggested the white metal collar sometimes worn by his sire. But similar rings round the legs and tail rather discredited the view that the white neck-ring was in any way related to the sire's nickel-plated collar. Telegony was sometimes said to be due to maternal impressions. It was doubtless for this reason that I was urged some years ago to carefully prevent the mares used in my experiments from seeing too much of the zebras. But though numerous foals have been bred from mares stabled with zebras or grazing with richly striped zebra hybrids, not a particle of evidence have I found in support of the maternal impression doctrine. The foals have neither stripes nor upright manes, and do not even attempt to mock the weird barking call of the zebra. Sheep and cattle, goats, rabbits and guinea-pigs, fowls and pigeons, have simply confirmed the results obtained with horses. This being the case, grooms may very well omit following the practice (considered so essential in Spain during the Middle Ages, and still often religiously observed in England and America) of setting "before the mares . . . the most goodly beasts" by way of hinting to them the kind of foals they are expected to produce.

*The Needs of the Organism as a Cause of Variation.*

No recent biologists are perhaps prepared to believe like Lamarck that the wings of birds were developed by their remote ancestors making efforts to fly; that by stretching its toes the otter acquired webbed feet; nor are they prepared to find in our new mammal, the Ocapi, evidence in support of Lamarck's contention that to meet new needs the giraffe by much stretching gradually lengthened his neck. Yet it is difficult sometimes to see any real difference between the beliefs of the new Lamarckians and the old. It is maintained, for example, "that when a certain functional activity produces a certain change in one generation it will produce it more easily the next," that, *e.g.*, flounders and their allies by constant efforts generation after generation have dragged the left eye to the right side, while by similar efforts in the turbot and certain other flat fishes the right eye has been shifted to the left side. It is not alleged by Neo-Lamarckians that globe fishes resulted from round fishes blowing themselves out, or that flounders resulted from round fishes generation after generation making efforts to flatten themselves. If by germinal variation and selection flounders were evolved out of round fishes, is it not straining at a gnat and swallowing a camel to refuse to admit that by the same factors the left eye of the flounder has been transferred from the left to the right side of the head? In the flat fishes it is not difficult to imagine how by variation and selection the eyes originally acquired the power of responding to certain external stimuli.

*The Direct Action of the Environment and Use-Inheritance as Causes of Variation.*

Of the doctrine of the transmission of acquired characters, still so often the subject of discussion, I need say little more than that I have failed to discover any evidence in its favour. Writing in 1876, Darwin says, "In my opinion the greatest error which I have committed has been not allowing sufficient weight to the direct action of the environment, *i.e.*, food, climate, &c., independently of natural selection" ("Life and Letters": Letter to Moritz Wagner). Darwin not only in his later years reverted to the teaching of Buffon, but in as far as he continued to believe in the "inherited effects of use and disuse" he adopted the views of Erasmus Darwin and Lamarck. While admitting that the direct action of the environment on the soma and use-inheritance are indirect—it may be potent—causes of variation, I do not believe there is any trustworthy evidence in support of the view that definite somatic variations are ever transmitted.

*Telegony as a Cause of Variation.*

The belief in telegony is less deserving of consideration than the doctrine of the transmission of acquired characters. Nevertheless I perhaps ought to refer to it at greater length, not so much because of its scientific importance, but because it interests all sorts and conditions of men in many different parts of the world. Telegony ("infection of the germ" of older writers) means that not only the immediate parents but also the previous mates (if any) contribute to the characters of the offspring; that, *e.g.*, a mare which had produced foals to, say, "Ladas" and "Persimmon" might thereafter give birth to a foal by "Flying Fox," to which "Ladas" and "Persimmon," as well as the actual sire, contributed some of their characteristics. Many even think a sire may transmit definite structural characters from one mate to another. If there is such a thing as telegony, if it is possible to blend without the risks of intercrossing the characteristics of several individuals or varieties, progressive development would be greatly accelerated. Though the doctrine of "infection" has probably long formed part of the breeder's creed, it received but little attention from men of science until in 1820 Lord Morton communicated a case of infection to the Royal Society, which in due time was published in the *Philosophical Transactions*. In this the most credible and best authenticated of all the cases of telegony on record a chestnut mare, after rearing a quagga hybrid, produced to a black Arabian horse three foals of a peculiar bay colour, one of them (a filly) showing more stripes than the quagga hybrid, and, according to the stud groom in charge of "the colts," characterised by a mane "which from the first was short, stiff, and upright" (*Phil. Trans.*, 1820, p. 21). Darwin, after fully considering Lord Morton's case, came to the conclusion that the chestnut mare had been infected, and this case along with others led him to believe that the first male influenced "the progeny subsequently borne by the mother to other males" ("Animals and Plants," vol. ii, pp. 435, 436.) If the upright zebra-like mane in one of the pure-bred colts and the markings on all three were the result of the chestnut mare having been first mated with a quagga, there is undoubtedly such a thing as telegony, and the presumption is that other mares first mated with a quagga or zebra and then with a black Arabian would give birth to striped offspring with a stiff if not quite upright mane. The evidence that from the first the mane of the filly was short, stiff, and upright is most unsatisfactory. It consists of an allegation by a stud groom. That the mane was upright, as in the quagga and zebra, is *a priori* improbable (1) because the mane of the quagga hybrid instead of being short and stiff was long and lank enough to arch to one side of the neck; (2) because the mane of zebra hybrids throughout the greater part of the year is so long that it falls to one or it may be both sides of the neck; and (3) because in the Equidæ an upright mane is always accompanied by a tail deficient of hairs at the root—in the filly the tail is as perfect as that of her Arab sire. We have still stronger evidence that the allegation of the groom was unfounded from drawings (of the chestnut mare, her three "colts," the black Arab, the quagga, and the quagga hybrid) by Agasse, a very trustworthy animal painter of the early part of last century. In the drawing of the filly the mane is represented as lying to one side, as in Arabs and other well-bred horses. The pictures (now in the Museum of the Royal College of Surgeons, London) were made because the subsequent foals were believed to prove the truth of the "infection" doctrine. Had the mane of the filly been erect it would hardly have escaped the keen eyes of the artist. But had Agasse by any chance missed this all-important detail, Lord Morton or some of those interested would doubtless have called his attention to the matter. If the mane of an Arab is completely removed early in the spring it is stiff, and upright in the autumn, but hanging to one side close to the neck in the following summer. When the whole circumstances are taken into consideration, there seems to me no escape from the conclusion that the mane of the filly was upright when seen by Lord Morton in August, 1820, and lying to one side when painted by Agasse the following summer, because it had been regularly cropped or at least hogged some months before Lord Morton's visit. But whatever be the explanation of the want of agreement between the mane as seen by Lord Morton and as depicted by Agasse, it will, I think, be admitted that the evidence afforded by the mane of the filly is hardly sufficient to establish the truth of the doctrine of telegony. Of still less value is the evidence afforded by the make, coat, colour and markings which were apparently too indistinct to deserve the name of stripes. The colts were decidedly Arab-like, of a bay colour marked more or less "in a darker tint." Judging from Agasse's drawings they closely resemble Arab-Indian crosses; they are, in fact, in make very like the Arab-Kattiawar horse already referred to. I have seen a bay Highland cob with as many stripes as Lord Morton's colts, and pure-bred Arabs of a dun colour with stripes on the neck and far more distinct leg bars than those depicted by Agasse. I believe the colts owed their stripes and colour not to "infection" of their dam by her previous mate the quagga, but to reversion. It is quite possible the black Arabian horse was of mixed origin; that the chestnut mare was crossbred is admitted. As in the west of Ireland the offspring of black and chestnut ponies are sometimes of a decidedly dun colour, it is not surprising that the black Arab and the half-bred chestnut had bay offspring. Neither are the stripes surprising. I recently ascertained that the chestnut mare was presented to Lord Morton (while serving with his regiment in India) by one of his officers—Mr. Boswell, of Deeside, Aberdeenshire—and that she was most likely a cross between an Arab and a country-bred pony. In Kattiawar the ponies when pure-bred are of a rufous grey colour and more or less richly striped. If in the chestnut mare there was any Kattiawar or even any native pony blood its offspring to a black sire might have been expected to be of a dun colour and striped. In a word, there is no reason for assuming that the foals would have been less striped if the chestnut mare had been mated with the black Arab first and the quagga afterwards.

By way of testing the truth of the "infection" doctrine I started, in 1895, a number of experiments, and especially arranged to repeat as accurately as possible, what is commonly called Lord Morton's experiment. Since then twelve mares after producing sixteen zebra hybrids, a mule, and a hinny have had an opportunity of supporting the telegony hypothesis by giving birth to twenty-two pure-bred foals.

During the same period Baron de Parana of Brazil has bred at least six zebra hybrids, and some of the dams of these hybrids subsequently produced ordinary foals. Further, Baron de Parana has for a number of years been engaged in crossing cattle and in watching the results obtained in several mule-breeding establishments, where from 400 to 1000 brood mares are kept. As in these establishments the mares breed mules and horses alternately—two or three mules and then a horse foal—there has been carried on for some years, under the observation of Baron de Parana, a telegony experiment on a gigantic scale.

The single hybrid bred by Lord Morton had extremely few stripes, and only in a remote way suggested a member of the zebra family. All my hybrids, like those bred in Brazil, have more stripes than their zebra sire, and in some of them the bands are nearly as conspicuous as in some of the zebras, thus proving that both the mares (which varied in colour and breed) and the two zebra stallions used were well adapted for the experiment. The results of my experiments, not only with the Equidæ but also with other domestic quadrupeds and birds, all point to the conclusion that there is no such thing as telegony, and the same conclusion has been independently arrived at by Baron de Parana in Brazil. Believers in telegony—they are numerous in America, India, and Australasia, as well as in England—almost always say of the many experiments recently made with a view to giving "infection" a chance of showing itself that they have only yielded negative results, and they generally add, it is impossible to prove a negative. After carefully considering all the more striking so-called cases of "infection," I have no hesitation in saying that there is no satisfactory evidence that there has ever been, either in the human family or amongst domestic animals, a single instance of infection.

I have in a hurried and imperfect manner indicated that we are not likely to find either in maternal impressions, the direct action of the environment, use-inheritance, or telegony a true cause of variation. I have endeavoured to point out that, instead of simply stating that variation is due to the constant recurrence of slight inequalities of nutrition of the germ-cells, we may with some confidence assert that differences in the age, vigour, and health of the parents and differences in the ripeness of the germ-cells are potent causes of variation.

I have also endeavoured to prove that intercrossing, though a *direct* cause of retrogressive variation, is only an *indirect* cause of progressive variation, while interbreeding (in-and-in-breeding) at the right moment is a cause of progressive variation.

Further, I have discussed at some length the swamping effects of intercrossing, chiefly with the object of showing (1) that progress in a single direction is probably often due to new varieties swamping old, it may be long established, varieties; and (2) that several varieties may be sufficiently exclusive to flourish side by side in the same area, and eventually (partly owing to their aloofness, *i.e.*, to differential mating) give rise to several new species.

I have only now to add that I was mainly led to select "The Experimental Study of Variation" as the subject of my address that I might indirectly indicate that the time had come when a well-equipped institute should be provided for biological and other experiments.

## NOTES.

THE *Times* announces that the Indian Government have adopted the suggestion of the Royal Society for the carrying out of a magnetic survey. The existing magnetic observatories at Bombay and Calcutta being inadequate as base stations for the vast area the survey will cover, similar observatories are in course of construction at Dehra Dun, at Kodaikanal, and at Rangoon. The Dehra Dun observatory will be under the supervision of Colonel Gore, R.E., the Surveyor-General of the Indian Survey (whose headquarters are located there); but the other four will be in charge of Mr. John Eliot, the meteorological reporter to the Government. The Survey and Meteorological Departments will, in fact, be jointly responsible for the investigations. The field observations will be carried out by six or seven detachments of the Survey Department, and these will be controlled by Captain Fraser, R.E., who has recently been arranging in England for the purchase of the necessary instruments. Sind and the Punjab will first be taken in hand; and, as the country is now intersected with railways in all directions, enabling field detachments to quickly cover the distances from one observing station to another, it is anticipated that five years will suffice to complete the field work of the preliminary magnetic survey.

IT is stated that a committee is being formed at Amalfi to arrange for the celebration of the fourth centenary of the invention of the compass. The Duke of the Abruzzi has agreed to be president of the committee, and Signor Morin, the Minister of Marine, the vice-president. The celebration will take place next year.

To obtain information for the Lightning Research Committee organised by the Royal Institute of British Architects and the Surveyors' Institution, the Institution of Electrical Engineers has sent papers of questions to the secretaries of local branches abroad, and it is hoped that members resident in districts outside Great Britain will cooperate in the work.

THE gold medal of the Italian Science Society has been presented to Mr. Marconi in London for his services in the invention of wireless telegraphy. The medal was entrusted by the Minister for Foreign Affairs in Rome to the Marquis Luigi Solari di Loreto, an officer in the Italian navy. Count Vinci, Chargé d'Affaires of Italy in London, and Cav. P. T. Righetti, Vice-Consul, were also present. The Marquis Solari, in handing the medal to Mr. Marconi, expressed the admiration of Italy for her distinguished son, whom the whole country remembered with pride and delight. Mr. Marconi, in reply, stated that among the many marks of distinction conferred on him he prized none so much as those which came from his beloved native land.

M. SANTOS DUMONT made an ascent with his new steerable balloon on Friday last, and though he was successful in navigating the balloon, an accident occurred owing to one of the guide-ropes getting caught among some trees. He hopes to make another ascent in a week or so. The Paris correspondent of the *Times* says that the scientific committee of the Aëro Club has resolved that the competitors for the Deutsch Prize must not only return to the starting-point within twenty minutes after rounding the Eiffel Tower, but must actually touch ground in the Aëro Club enclosure. Mr. William Beedle, whose balloon is now being rapidly got ready at the Spencer Works, intends to be in Paris towards the end of October to try for the prize. He has a more powerful motor than that of M. Santos Dumont, 28-horse power, and a tougher envelope protected, and, it is believed, so arranged under an aluminium framework as to secure perfect rigidity for all practical purposes.

THE fifth International Congress of Criminal Anthropology was opened at Amsterdam on Monday last. Among the papers contributed was one by Prof. Lombroso, on the latest anatomical researches into degeneration and on tattooing.

WE regret to notice the announcement of the death, on the 7th inst., of Dr. John Louis William Thudichum, well known for his researches in organic and physiological chemistry.

THE late Mr. H. M. Courage, of Snowdenham Hall, Bramley, has left the whole of his valuable collection of birds, numbering between 6000 and 7000 specimens, to the governors of Cheltenham College. A few years before his death, which occurred last month, Mr. Courage presented a representative collection of British birds to the Hobart Museum, Tasmania.

THE Baumgartner prize of the value of 2000 crowns will be awarded at the end of 1903 by the Vienna Academy of Sciences for a research enlarging our knowledge of the invisible radiations.

*Science* announces that the Veitch silver medal for distinguished services in botany and horticulture has been awarded to Mr. Thomas Meehan, of Philadelphia.

THE Surrey County Council have taken a practical step in the direction of the prevention of tuberculosis by the issue of a leaflet dealing with the character of the disease, its great mortality, the manner in which it is spread, and the precautions which should be taken to prevent infection. This leaflet has been drawn up by the county medical officer, Dr. Seaton, acting on the instructions of the sanitary committee of the County Council, and local authorities throughout the county have been asked to cooperate in the dissemination of this information to every household in Surrey. The committee recommend all sanitary authorities to inform medical practitioners in their districts that, after the death or removal of a patient suffering from the disease, they will undertake disinfection of the premises at their own cost.

AN analysis of dust which fell at Fiume, Hungary, on March 10–11, and was described as showers of "red or blood rain" over a large part of Southern and Central Europe, was made by M. M. Barac, and the results are given in the number of the *Journal* of the Royal Meteorological Society just issued (vol. xxvii. No. 119). It will be remembered that some of the dust was collected by Prof. Rücker at Taormina, and described by Prof. Judd in these columns on March 28 (p. 514). M. Barac's chemical analysis gave the following percentage composition for the material :—Silica, 49·49 ; iron sesquioxide, 9·96 ; alumina, 12·10 ; manganese peroxide, 1·99 ; lime, 11·46 ; magnesia, 0·40 ; carbonic acid, 8·96 ; organic matter, 5·48 ; traces of soda, sulphuric acid, hydrochloric acid, &c., 0·16. Under the microscope, with a power of 640, M. Barac found the main mass to consist principally of colourless and, in less degree, of coloured particles, of irregular shape, partly angular fragments of crystals, and also mineral particles. In addition there were siliceous skeletons of micro-organisms, and, finally, particles of soot. There were a few well-formed rhombohedra of calcite and cubes of common salt ; and both the calcite and the quartz crystals exhibited chromatic polarisation. As regards magnitude, the minimum was 0·001 mm., the average 0·017 mm., and the maximum among the crystalline particles 0·051 mm. ; while the yellow structureless mineral particles attained the size of 0·113 mm.

MAJOR RONALD ROSS, F.R.S., has sent a letter to Mr. A. L. Jones giving some particulars as to the results of his visit to the Gold Coast and the work of the fifth expedition of the Liverpool School of Tropical Medicine in Sierra Leone. He says that at Sierra Leone he found Dr. Logan Taylor pushing on the operations against mosquitoes with great vigour. A report received a few days ago from Dr. Logan Taylor states that 5000 houses in Freetown have been cleared of vessels of every description which previously served as breeding places for mosquitoes. What a serious blow this will be to the prevalence of the Culex mosquito in Freetown will be readily understood. The result is already well marked, and there is undoubtedly a great reduction in the number of these insects in the centre of the town generally. Of course, the insects will still occur for some time here and there, but their breeding places can be easily detected and abolished. To judge of the value of these operations it must be remembered that besides causing constant annoyance to everyone, the insects carry the germs of yellow fever, elephantiasis, and perhaps other diseases. Operations against the Anopheles mosquito (which breed in puddles on the ground) are also being well pushed by Drs. Taylor and Berkeley. Hollows in the ground are everywhere being drained away or filled up with rubble and earth. Others are being filled with the empty bottles and tins found in the houses. Many of the worst streets, which formerly were practically marshes in the rains, have been reclaimed. Major Ross says it is now a matter of some difficulty to catch Anopheles for scientific examination, and it seems that a little perseverance will ultimately abolish these malaria-bearing insects as a disease factor in Freetown. Major Ross remarks in conclusion that the unhealthiness of the Coast has been much exaggerated. True, there is a considerable amount of malaria among Europeans ; but then there is little or no typhoid. He says that in nine cases out of ten if a man contracts malarial infection it is his own fault.

THE *Pioneer Mail*, Allahabad, states that Mr. Rea, superintendent of the Archæological Survey of Madras and Coorg, has discovered a field for exploration in the Tinnevelly district which promises to be of much interest and importance. The site, which is near Adichanallar, has been, Mr. Rea thinks, at one time a very large town. "The deposits, if fully excavated, would, I have not the slightest doubt," says Mr. Rea, "stock several museums with unique objects of the most interesting description, for almost every excavation brings to light something not heretofore found. I have examined many prehistoric sites, but have never seen one so extensive and varied in its results as this. Extensive tracts are yet untouched. . . . Over 114 acres are now reserved, but the remains extend even beyond that area. It would require several years' steady work to completely explore the place. That this is by far the most important and extensive prehistoric burial place as yet discovered in Madras, I can certainly state." Some eighteen hundred curious objects in bronze, iron and pottery, as well as seven pure gold oval-shaped ornaments, have already been unearthed.

THE Grand Trunk Railway of Canada, according to the *Railway and Engineering Review*, has recently constructed, and commenced using, a car specially adapted for the distribution of live fish to waters along its lines. The interior of the car is arranged with a series of galvanised iron tanks to hold from 1000 to 1500 fish. At one end of the car is an upper and lower berth, like those in a Pullman car, to accommodate two men. Ice for keeping the water at a certain temperature is carried in two compartments built for this purpose and holding about one ton each. Arrangements have been made for replenishing the water in the tanks, *en route*, by attaching a hose to any of the hydrants at stations on the road.

THE Museum of the Literary and Philosophical Society of Hull has recently been taken over by the Corporation, and one of the first results is a note (*Yorkshire Naturalist* for August) by the Curator (Mr. T. Sheppard) on the type skeleton of Sibbald's rorqual, which forms one of the treasures of the collection. The animal to which this skeleton belonged was stranded in the Humber so long ago as 1835.

THE September issue of the *Entomologist's Monthly Magazine* contains the commencement of a series of articles on the insect fauna of the Balearic Islands, mainly based on collections made by Prof. Poulton and Messrs. Pocock and Thomas of the British Museum. Prof. Poulton himself contributes the introduction to the series, and he is followed by Mr. E. Saunders, who describes the bees, wasps, and their allies. In Majorca much of the original insect fauna appears to have been exterminated by agriculture, although much of interest will, it is hoped, still be found. In Minorca, where cultivation is not carried on to such an extent, insect life is probably much richer.

THE polychætous annelids of the Puget Sound region form the subject of a communication by Mr. H. P. Johnson published in vol. xxix. (No. 18) of the *Proceedings* of the Boston Natural History Society. Including two species from British Columbia sent by Prof. Herdman, the collection at the author's disposal comprises fifty-one species (many of which are new), classed in thirty-four genera. Nearly all the forms were collected between tide-marks, and only one is common to the Japanese coast. This latter fact is not surprising when it is borne in mind that the Puget Sound fauna is boreal, while the forms collected in Japan pertain to the Indo-Pacific fauna.

SOME time ago Prof. E. B. Poulton announced in these columns (vol. lx. p. 591) the discovery of two species of peripatus in the Siamese Malay States, this being the first record of the occurrence of this group on the Asiatic mainland. These two new forms, together with a third from Selangor, are described by Mr. R. Evans in the August number of the *Quarterly Journal of Microscopical Science*. This description has involved a reclassification of the group (Onychophora), and the author proposes the new generic title Eoperipatus for the Malayan (inclusive of the Sumatran) forms. Curiously enough, these are more nearly related to the Central American than to any other members of the group, although they are connected to a certain extent with the African forms through a species which is now assigned to a second new genus (Mesoperipatus). It is concluded that the birthplace of this very archaic group was probably Africa. In another article in the same journal Mr. S. B. Mitra, of Calcutta, discusses the function of the so-called "crystalline style" of the bivalve molluscs. After reviewing previous theories, the author comes to the conclusion that this remarkable rod-like body (which in the common pond-mussel is three-fourths the entire length of the animal) really acts as a digestive ferment whose function is to convert starch into sugar.

THE services which anthropometry renders to physical education are dealt with by Major Dr. Paul Godin in the *Bulletins et Mémoires* de la Société d'Anthropologie de Paris (5<sup>e</sup> série, tome ii. 1902, fascic. 2, p. 110), whose paper is fortified by numerous tables and graphic curves.

THE development of illumination, or rather the evolution of artificial illumination, is the subject of a short paper by Mr. Walter Hough in the *American Anthropologist* (N.S., vol. iii. 1901, p. 342), in which he epitomises the stages in the development of the candle and of the lamp. It is only comparatively recently that the latter has improved beyond a very simple and inefficient contrivance; at present the destiny of illumination is in the hands of the investigator and inventor.

UNDER the title of "Les Peuplades de Guinée" (*Revue Scientifique*, 4e Sér. T. 16, No. 8, p. 233), M. A. Vergely gives an account more particularly of the Soussous (Susu), who he thinks have been greatly calumniated. He describes their appearance, mental traits, clothing, mode of life, morality, and other social characteristics, and contrasts with them the Foullas (Fulah); the former are true negroes, the latter are Hamites. This comparison of two very different people living under the same conditions is very suggestive.

THERE have been several theories for the origin of the word "Surrey." Mr. T. le Marchant Douse, in the *Home Counties Magazine* (vol. iii. No. 11, July 1901, p. 198), follows up the suggestion of Kluge, and produces an array of evidence that supports his contention that it means the land of the South Rige, who are identified with the Rugii of Tacitus. The oldest known habitat of the Rugi was by the mouth and lower course of the Oder, probably to the east of it. Very early in our era the Goths wholly or in part dispossessed them; some migrated southward, others westward and north-westward, and it is extremely probable that the Baltic Rugi in the fifth century joined other adventurers, but under their own chief or king, and settled in England. Surrey continued to be called a "kingdom" long after it had ceased to have a king to itself. Eastry, near Sandwich in Kent, is now a large village and parish, but was formerly a town and district; in a charter of 788 this is spoken of as "the district of the Eastriges."

IN the current number of the *Bulletins et Mémoires* de la Soc. d'Anthropologie de Paris (5e série, tome ii. 1902, fascic. 2) there are two illustrated papers, by Dr. Atgier, on deformed heads of living subjects: the one is a case of oxycephaly or acrocephaly and the other of scaphocephaly. The discussions on these cases is as important as the original papers. M. Pelletier proposes (p. 188) a new method of obtaining the cubic index of the skull. It is sometimes impossible to measure the cubic capacity of a skull by the ordinary methods, and always to do so in the case of the living, so recourse has to be made to an estimation of the capacity from certain measurements. Those in vogue are the glabella-occipital length, the greatest breadth and the basio-bregmatic height. The author proposes the ophryo-occipital length, the greatest breadth and the auriculo-bregmatic height; for, as he justly observes, these can also be made approximately on the living. We are glad to find that the auricular height, which has been employed by one or two British anthropologists, is recognised as of more value than the basial height. Those who are interested in this subject should also consult the recent noteworthy investigations by Alice Lee and Karl Pearson on the determination of capacity of the human skull from external measurements (*Phil. Trans. Roy. Soc.* series A, vol. cxcvi. pp. 225-264, "Data for the Problem of Evolution in Man; vi. A First Study of the Correlation of the Human Skull").

AN appreciative article on the life and work of Prof. T. G. Bonney, F.R.S., appears in the September number of the *Geological Magazine*, being the first of what is apparently to be a series of biographies of eminent living geologists. The article is accompanied by a portrait of Prof. Bonney, reproduced as a full-page plate.

THE first part of a work on European butterflies—"Die Schmetterlinge Europas"—by Dr. A. Spuler, forming the third edition of E. Hoffmann's treatise, has been received. It is intended to complete the work in thirty-eight parts, which together will contain descriptive text and nearly a hundred plates having about 2700 coloured pictures of butterflies upon them. The book will be noticed when all the parts have been received. Messrs. Heyne Brothers are the English publishers.

THE Annual Report of Mr. J. C. Smock, the State Geologist of New Jersey, contains important articles on the Portland Cement Industry and on the Iron and Copper Mines by Dr. H. B. Kümmel, and on Artesian Wells by Mr. Lewis Woolman. One boring in Atlantic City has been carried to a depth of 2285 feet, and is still being drilled; so far without success. Mr. W. S. Myers contributes a short article on Chlorine in the Natural Waters of the State, and draws attention to its importance in the examination of waters suspected of contamination by sewage.

THERE is an interesting sketch of the vestiges of the ancient settlement of the Northmen in the Isle of Man, by Anton Weis, in *Globus* (Band lxxx. No. 7, p. 113), but the author makes a remarkable slip when he states that "this little island is only four to five miles long and two miles broad."

THE additions to the Zoological Society's Gardens during the past week include a Jaguar (*Felis onca*, ♀) from South America, presented by Mr. F. W. Barrow; a Vulpine Phalanger (*Trichosurus vulpecula*) from Australia, presented by Mr. A. N. Owen; an Egyptian Jerboa (*Dipus aegyptius*) from Egypt, presented by Miss A. Moore; a Tawny Owl (*Syrnium aluco*), British, presented by Mr. T. E. Gunn; a Green Turtle (*Chelone mydas*) from the Tropical Seas, presented by Captain Stevenson; a Madagascar Tree Boa (*Corallus madagascariensis*), a Madagascar Boa (*Boa madagascariensis*), eight Sharp-headed Snakes (*Lioheterodon madagascariensis*) from Madagascar, a Chameleon (*Chamaeleon vulgaris*) from North Africa, deposited.

## OUR ASTRONOMICAL COLUMN.

OPPOSITION OF EROS IN 1903.—The planet Eros has now so nearly approached the sun that further observations of its light have become impossible; but as the amount of material accumulated since the discovery of its variability in brightness is not sufficient for determining satisfactorily the laws governing it, full advantage of all the future opportunities of observing the body should be taken.

Since three times the tropical period is about seven years, the favourable conditions of 1893 and 1900 will not be repeated until 1907. The coming opposition in 1903 will be similar to that of 1896, and although not specially favourable, may possibly give opportunities for useful measurements. With this end in view, Prof. Pickering has issued an ephemeris showing the computed path of the planet during the years 1901, 1902 and 1903, the accidental errors of which have been eliminated as far as possible. The magnitudes given are not corrected for phase, and are based on the assumption that the magnitude at unit distance = 11·39. From these tables it appears that the next most favourable time of observation will be during the spring of 1903, and preparations are being made for an extensive series of photometric measures at Arequipa during that period. (*Harvard College Observatory Circular*, No. 61).

RADIAL VELOCITY OF 1830 GROOMBRIDGE.—An interesting investigation is reported by Prof. Campbell, bearing on the spectroscopic determination of the velocity in the line of sight of 1830 Groombridge, the star which, until lately, had the largest known proper motion (7″·05 per year). Although the various determinations of the parallax of this star differ somewhat in value, they all agree in placing the star at a great distance, Newcomb's adopted parallax being 0″·14. Assuming this as the true value, the component of the star's velocity *perpendicular* to the line of sight will be 240 kilometres (150 miles) per second. The component of its velocity *in* the line of sight has been determined from *four* photographs of its spectrum taken with the Mills spectrograph; the results from all are substantially in agreement; the best values are:—

| Date. | Velocity. Kilometres. | Miles. |
|---|---|---|
| 1901 March 18 | −93 | −58 |
| April 1 | −97 | −60 |

The mean value of the radial velocity is taken as $-95 \pm 5$ kilometres per second (equivalent to 59 miles per second approach). The spectrum is approximately of the solar type, inclining rather to the characteristics of Procyon or α Persei. The best photograph was obtained with an exposure of two hours (*Lick Observatory Bulletin*, No. 4).

NOVA PERSEI.—MM. Flammarion and Antoniadi give some further particulars in the *Astronomische Nachrichten* (Bd. 156, No. 3736) respecting the peculiar appearance of the Nova. The photographs of the star region were obtained with an Hermagis photographic objective of 16 cm. aperture and 70 cm. focal length.

Three proofs on paper have been examined, both enlargements and direct prints. That from the plate obtained on August 19, with an exposure of 30 minutes, shows that the image of the new star is very different in appearance to the images of neighbouring stars, being surrounded by a strong penumbra with a sharp edge, the mean diameter of which is about 2′ of arc. Another from a negative which was exposed for a much longer period, 3h. 20m., shows the image of the star encroaching on the first penumbra, but beyond this there is shown a much larger aureole, some 6′ of arc in diameter, and the appearance is said to resemble the umbra and penumbra of a sunspot.

VARIABLE RADIAL VELOCITY OF δ ORIONIS.—The variable velocity of this star was discovered by M. Deslandres from observations made with a spectroscope attached to the great Meudon refractor. The star is not quite suited for this type of investigation, as the lines are broad, but three observations secured during 1900 confirm M. Deslandres' results.

The velocities reduced from these were:—

| | |
|---|---|
| 1900 August 12 | +3 kilometres per sec. |
| " 21 | +51 " " |
| Sept. 17 | −69 " " |

(*Lick Observatory Bulletin*, No. 4.)

## IRON AND STEEL INSTITUTE.

THE autumn meeting of the Iron and Steel Institute was held on September 3 and 4, in conjunction with the International Engineering Congress, at the University of Glasgow, and was very largely attended. After speeches of welcome, the president, Mr. W. Whitwell, delivered a short introductory address, in which he dwelt upon the advantages to be expected from the fact that the Iron and Steel Institute met for the first time in its history in conjunction with eight other societies, forming one great International Engineering Congress. In the overwhelming mass of matter published by these societies there was, he considered, a certain amount of overlapping that the Congress might tend to obviate in the future. Some of the papers, too, at first sight might appear to be of little practical importance. This criticism had frequently been applied to many of the papers read before the Iron and Steel Institute. It must be remembered, however, that this had been from time immemorial the favourite objection to the work of pioneers of thought.

The 30,000 pages published by the Iron and Steel Institute since its inauguration in 1871 afforded fruitful examples of the subsequent value of scientific researches, which, when first presented, were received with coolness and suspicion. Numerous examples might be cited. For instance, the microscopic method of investigating the structure of steel, created by Sorby, Martens, Osmond, Howe and Stead, had become an indispensable auxiliary to chemical analysis and physical tests in steelworks. The abstruse memoirs on the heat treatment of steel, and on pyrometry, had led to important practical applications, and the phase rule enunciated by the American professor, Gibbs, and applied by Sir William Roberts-Austen, Baron Jüptner, Le Chatelier and Stansfield would no doubt eventually prove of extreme value in elucidating some of the more intricate problems confronting the metallurgist.

The first paper was read by Mr. Walter Dixon. It contained a concise account of the iron and steel industries of the west of Scotland, drawn up by a committee of the local metallurgical society, pig iron being dealt with by Mr. Henry Bumby, wrought iron by Mr. W. Wylie, and steel by Mr. H. Archibald.

The second paper was also the report of a committee, presented by Mr. Bennett H. Brough, the secretary. In view of the fact that with the development of metallography the nomenclature was becoming more and more involved, the Iron and Steel Institute appointed a committee to consider the matter and to ascertain whether it would be possible to take steps to make the terminology less complicated and more precise. A glossary was submitted, containing the more important terms used by authors of memoirs dealing with metallography, in the hope of obtaining criticisms and suggestions in order that the committee might have before them data upon which to base their judgment. In each case the equivalents in French and German were added.

Mr. A. Wahlberg (Stockholm), then read a paper on variations of carbon and phosphorus in steel ingots. The object of his research was to establish the limits of variation of carbon and phosphorus in steel which has been cast into 10-inch ingots and then rolled into 4-inch billets, and to ascertain to what extent chemical analyses of identical samples vary in their results as regard the percentage of carbon and of phosphorus when made by different chemists. The material tested was procured from four works and was analysed in four laboratories. The variation in chemical composition in different portions of the billet and the divergent results obtained by different analysts were well shown in a number of tables. In the discussion which followed the reading of the paper, the need for standard methods of analysis was urged.

The meeting then adjourned until September 4, when Mr. C. H. Ridsdale read a lengthy paper of great practical interest on the correct treatment of steel. After a full discussion of this paper, Mr. J. E. Stead read an abstract of two papers. In the first, on copper and iron alloys, he reviewed the contradictory evidence in metallurgical text-books, and gave the results of his recent work. Copper and iron, he showed, alloyed most readily by direct fusion in all proportions. Such alloys might be classed in three main sections: (*a*) with traces to 2·73 per cent. of iron and 97·2 per cent. of copper, (*b*) with 8·0 per cent. of copper and 91·5 per cent. of iron, and (*c*) alloys intermediate between the two. The alloys of the first two sections are practically homogeneous, class *a* consisting of copper with iron in solid solution and class *b* consisting of iron with copper in solution. The alloys of the third class contain saturated solid solutions, copper in iron and iron in copper, separate from each other but in micro-juxtaposition. In solidifying the portion first to fall out of solution was the iron containing copper in solid solution. The conflicting character of evidence previously published was probably due to the fact that some of the investigators in the past had not taken the precaution to use iron free from carbon. The effect of carbon is marked. On heating the alloys containing more than 7·5 per cent. of copper to whiteness with charcoal, copper containing about 10 per cent. of iron is thrown out of solution and falls to the bottom, leaving a layer of carburised iron on the surface containing about 7·5 per cent. of copper.

In the next paper, Mr. J. E. Stead and Mr. F. H. Wigham described experiments on a series of steels with and without copper prepared by dividing the finished steel in each series when in a fluid state into two parts, to one of which copper was added in amounts varying between 0·46 and 2·0 per cent. Elaborate tests showed that copper in such large quantities does not improve the quality of the wire, but generally has a deteriorating influence, particularly in the presence of high carbon. The only good property exhibited by cupreous steel wire is that it resists corrosion.

Mr. G. Watson Gray then read a paper recording the occurrence of calcium in a ferro-silicon. He gave analyses of ferro-silicons containing 0·79 to 14·40 per cent. of calcium, and described a new method for the analysis of ferro-silicon.

A lengthy paper on the profitable utilisation of power from blast-furnace gases was read by Mr. B. H. Thwaite. One of the results following the use of blast-furnace gas for the direct production of power in internal combustion engines has been marked progress in the mechanical perfection of power capacities and in the thermodynamic efficiency of this engine. A new scheme for obtaining all the power possible from the blast furnace, devised by the author, includes the recovery of the sensible heat that is otherwise lost in cooling the blast-furnace gases, in heating the air to convert coal into gas in producers, and in supporting the combustion of the gases thus produced in hot-blast stoves. The various outlets for electric power that could be generated by the new system are described. The production of silicon and calcium carbides, of chromium, nickel and aluminium are instanced as being exceptionally suitable as associated industries for ironworks.

Prof. W. N. Hartley and Mr. H. Ramage next gave the results of an investigation of the spectra of flames at different periods during the basic Bessemer blow. The conclusion arrived at was that the phenomena of the basic Bessemer blow differ considerably from those of the acid process, in the following respects:—

First, a flame is visible from the commencement of blowing, or as soon as the cloud of lime dust has dispersed. We conclude that the immediate production of this flame is caused by carbonaceous matter in the lining of the vessel, that its luminosity is due partly to the volatilisation of the alkalis, and to the incandescence of lime dust carried out by the blast.

Secondly, volatilisation of metal occurs largely at an early period in the blow, and is due to the difference in composition of the metal blown, chiefly to the smaller quantity of silicon. There is practically no distinct period when siliceous slags are formed in the "basic" process, and metals are volatilised readily in the reducing atmosphere, rich in carbon monoxide.

Thirdly, a very large amount of fume is formed towards the close of the second period. This arises from the oxidation of metal and of phosphorus in the iron phosphide being productive of a high temperature, but little or no carbon remaining. The flame is comparatively short, and the metallic vapours carried up are burnt by the blast.

Fourthly, the "over-blow" is characterised by a very powerful illumination from what appears to be a brilliant yellow flame; a dense fume is produced at this time composed of oxidised metallic vapours, chiefly iron. These particles are undoubtedly of very minute dimensions, as is proved by the fact that they scatter the light which falls on them, and the cloud casts a brown shadow, and, on a still day, ascends to a great height. In a given flame the brilliancy of the line spectrum of potassium is increased by diminishing the quantity of metallic vapour in the flame; this does not appear to depend altogether on the weakening of the continuous spectrum which accompanies the line spectrum of potassium; some experiments made with various salts of potassium show that it is probably due, in part at least, to the increased freedom of motion permitted to the molecules of the metal.

Mr. A. Wahlberg (Stockholm) submitted the second portion of his elaborate memoir on Brinell's method of determining hardness and other properties of iron and steel. The first portion was read at the May meeting, and the two together constitute a monograph of about one hundred pages. The second portion dealt more particularly with the influence of different methods of annealing and hardening on the tensile properties of iron and steel determined by means of tensile tests, and with researches undertaken for the purpose of ascertaining the influence of chemical composition and various modes of treatment on the resistance to impact in iron and steel at ordinary and low temperatures.

Mr. Arthur Wingham submitted a very suggestive paper on the internal strains of iron and steel and their bearing upon fracture.

The object of the paper was to assist the elucidation of some of the mysteries attendant upon the physical behaviour of metals generally, and of iron and steel in particular, and to throw light upon the cause of the sudden and unexpected breakages of metal used for machinery and other purposes. Its reasonings were based upon the following facts and hypotheses:—That there are two kinds of equilibrium to which a metal attains, viz., chemical and physical; that the natural tendency of a complex metal is to assume its most simple forms of combination preferentially capable of existing at a given temperature; that its rapidity of cooling, even under the slowest conditions, is too great to allow this to reach finality; that the equilibrium is further repeatedly interfered with by changes of atmospheric and other conditions; that the adjustment to physical equilibrium tends to assist the adjustment to chemical equilibrium; that adjustment which is assisted by slightly raised temperatures, also, as a consequence, takes place in the cold; and that the eutectic is the medium through which the chemical or molecular change takes place, working, of course, in conjunction with the vibration of the molecules.

## *RELATIONS BETWEEN CLIMATE AND CROPS.*[1]

THE weather exerts a tacit, though relentless, tyranny over the labour and the thought of the agriculturist. The probable influences of the present and prospective weather upon the growing crops are seldom absent from his mind. But science teaches that climate is rhythmic, not capricious. Laplace has shown that the mean temperature of the mass of the earth cannot have changed in any appreciable measure during the entire period of astronomical calculation, and that while the planetary movements remain as at present no such change can occur. "Astronomical permanency," he says, "implies an absolute fixedness of the quantity of heat for the mass of the earth." And the sun's heat is the leading element of climate; all other conditions depend in the long run upon that. Hence, the sun's heat being constant, all the changes we observe are periodic as regards the astronomical units, the day and the year; and non-periodic in all other cases, the averages returning always to a line of absolute permanency.

Climate is the average of seasonal atmospheric conditions, and as corn is an annual plant, these fluctuating seasonal factors must affect its growth. The crop season is in fact the climatic unit with respect to this cereal. No season exactly repeats itself; there are perturbations within relatively narrow limits; the plant strives perpetually to adjust itself to perfect correspondence with its environment. As this environment—that is, climate and food supply—vibrates now one way, now another, about a fixed mean, the consequent variations of the plant will be compensatory, and so there should be no final permanent modification of the plant in a given locality.

Aside from its direct control of the amount and quality of the crop, climatic variations, by vitiating experience, impede agricultural progress. This fact is most apparent in the agricultural history of a new country, where experience acquired in one district is in many cases not only useless, but positively pernicious, when applied to a distant district. In the United States millions of dollars have been lost through the efforts of new settlers to learn by experience the climatic peculiarities of their adopted home. It is the province of agricultural science to teach how to profit by the experience that has been so dearly bought in the past.

Agricultural climatology considers the relations between the meteorological elements measured in terms of plant development. As intimate as these relations are known to be, and as interesting and promising a field as their study is known to offer, it is rather surprising that no adequate organised effort has yet been expended in this direction. But we are coming to see that the very fact of this intimate reciprocal dependence may be turned to advantage, and that by methods of correlation the facts of each science may be made to illumine the other.

The laws of biological and of meteorological phenomena separately considered are extremely subtle and complex, and any attempt to study them in their manifold reciprocal relations is sufficiently difficult to deter any but the best equipped and most zealous students. This difficulty of properly interpreting the separate effects upon vegetation of heat, light, moisture, and the gases of the atmosphere is enhanced by the fact that a change in one meteorological condition ordinarily disturbs all the other elements. For example, rain is accompanied by cloudiness, decrease in light and heat, and, it may be, by an increase of warmth in the soil, if the rain be a warm one.

Extended and elaborate meteorological observations have been conducted in the United States and in Europe, but instruments measure only detached elements of climate; plants alone record its composite or cumulative effects. Hence, the insistence on the part of leading agricultural investigators that climate should be studied in terms of plant life. Such study is termed phenology, and while it has led to some valuable generalisations, the fragmentary character of the data vitiates many of its conclusions. It appears that in the past phenologists have given the element of heat undue, if not almost exclusive, weight. It is becoming more and more evident that the real function and value of light have been neglected and undervalued. A fundamental theory which has been held by botanists for more than a century is, briefly, that a certain life event takes place in any species whenever that species has been exposed to a certain sum total of heat, which is called the physiological constant or thermal constant. In harmony with this theory, Blodgett, in his "Climatology of the United States," says with regard to corn that its period of growth is precisely proportional to the abruptness of the temperature curve; that its unusual elasticity of constitution admits it to all regions where the temperature reaches a certain point, however brief the duration of this warm period may be. He defines the extreme northern limits of Indian corn as coincident with the isotherm of 67° for July, though a somewhat higher mean for one summer month is required, and he attributes the increase of productiveness at the north mainly to "the hasty growth, the excess of heat while it lasts, and the hastened ripening period." The seemingly insignificant item of a deficiency of two degrees on the mean of a single summer month practically excludes this crop from the British Isles, where it is grown, when grown at all, only as a forage crop, seldom maturing any grain. This statement of the subject has been for fifty years the popular and the current theory. Temperature being the most easily measured of the solar manifestations, it has quite naturally been regarded as the dominant one. Then, too, the rudimentary state of climatology made necessary such a simplification as is afforded by the consideration of heat alone.

The trend of recent opinion is summarised by Prof. Abbe in his extensive manuscript report of June 1891, on the "Relations of Climate and Crops," where, after reviewing the investigations of Tisserand, he concludes:—

That the temperature of the air has apparently little to do, in and of itself, with the duration of time from sowing to ripening, but that this depends principally on the sunshine. The temperature of the air controls the chemical composition of the seed, but the effective sunshine seems to be the productive climatic element; it furnishes the total energy at the disposal of the plant, but it is also the one least studied and understood.

Prof. Sturtevant, of New York, from tests with 128 varieties, concludes that "actinism has an influence scarcely secondary to temperature." So it would seem wise in the light of recent study to attribute much of the hostility of a climate like that of England to the greater degree of cloudiness, and the congeniality of the climate of the Western States to the habitually clear skies of summer.

In discussing climate and corn it will be convenient to treat their relations first historically and then analytically; a cursory glance at the more evident accumulated results of climatic modification and limitation will prepare the way for an outline of the individual factors that constitute environment and the principles that govern the life of the plant.

The original home of corn or maize is now quite certainly known to have been in Central Mexico, and hence it is the only one of our cereals that is indigenous to the New World. It has been so long and so thoroughly domesticated that no truly wild varieties are known. In geographical range and elasticity of habit it probably surpasses every other cultivated plant. From its original tropical home it has spread to the temperate as well as the tropical regions of the world. Introduced into

[1] Abridged from a paper by Mr. H. B. Wren in the U.S. *Monthly Weather Review*.

Europe soon after the conquest of Mexico, it finds a genial home only in the warm valleys of the south and central portions of that continent; it is extensively grown in Africa, and in India it thrives everywhere throughout the hill country; it appears to flourish as well in the temperate as the tropical regions, and at altitudes of from sea-level to 7000 feet or more. Corn is, however, as it has always been and will undoubtedly remain, a distinctive and characteristic American product. It is cultivated from Canada to Patagonia, over 7000 miles of latitude. It has been known to ripen as far north as 63°, and has been found a profitable crop in latitude 51° north. In response to the multifarious conditions which this great range imposes, countless varieties have been developed, there being more than 200 in the United States alone.

The effects of climate on maize may be appropriately classified as immediate, intermediate, and incidental. Prof. Storer has tersely said that the prime object of agriculture is to collect for purposes of human aggrandisement as much as may be possible of the energy that comes from the sun in form of light and heat. Now the working capacity of sunshine is, according to Kelvin, one horse-power for every seven square feet of surface. Measured by the standards of mechanics, how inefficient and wasteful an engine is our agriculture at its best. The atmosphere is directly the source of 95 per cent. of the material in the total plant and of 98 per cent. of the matter in the grain of corn. The plant is an elaborate machine that absorbs and transforms energy, utilising solar radiation to digest carbon dioxide in the leaves and to combine into vegetable organs and tissues the gases of the air with the elements supplied by the soil. When we remember that the amount of energy available, the food supply, and, consequently, the amount of matter stored, all depend directly upon meteorological conditions, we realise how overwhelming is the influence of climate.

A grain of maize once matured is as inert as a pebble until heat and moisture are applied; then a sprout and a root appear, each for a separate function, the one for absorbing ethereal waves, the other for absorbing water. In addition to heat and moisture, oxygen is absolutely essential to germination, as well as to all subsequent growth. The importance of moisture will be appreciated when we recall that water performs at least four distinct offices: first, directly as a food, being united in the leaves with carbon to form the carbohydrates; second, as a solvent for the nutritive matters in the soil; third, as the vehicle which transports the soluble food through the roots and stems to the leaves; and, finally, as a cooling device, since, through evaporation, water largely controls the temperature of the plant. The "free water of vegetation," as it is called, or the water of the juices, comprises from 70 to 90 per cent. of corn in the fodder stage, while the "combined water of vegetation," or the water that remains after the plant is air-dried, is 12 per cent. in a kernel of corn.

The immediate effects of climate will be better understood by glancing first at its intermediate effects through the medium of the soil and through the food-supply. Climate originates soil and all the capacities of the earth for tillage, and it is at the same time more than soil or tillage. For in a truly "good year" the worst tilled soil returns a more bountiful harvest than it is possible with all our industry to extort from the best tilled soil in a "bad year." The oasis differs from the desert only in the item of water supply, and a given climate does not result primarily from the nature of the earth's surface; on the contrary, that surface is determined almost wholly by climate. The agencies that produce, and are producing arable areas from the seemingly impervious and indurate rocks, must continue their action perennially if the soil is to maintain itself. Indeed, the reverse metamorphosis is constantly at work. The greater part of the known rock formations were once in the form of soil, and chemical, physical, and even vital forces are continually engaged in the work of rock making, as well as rock breaking, so that an important office of agriculture is to oppose this cyclic law of nature, and to counteract the retrogressive tendency from soil to rock.

Primarily, the soil is a reservoir of moisture and plant food; but hardly secondary is its office as a vast laboratory, wherein during the warmer seasons countless complex chemical agencies and numberless microscopic organisms operate unceasingly. Indeed, the relations of climate to the plant through the medium of the soil are so intimate and vital that no just idea of their importance can be given here. These relations may be classed as physical, chemical and biological.

The physical texture of the soil determines its conductivity for heat and its content of water and air, both of which in proper proportions are essential to the chemical and biological functions. Moreover, the water content, through its power to absorb, transform, and conserve radiant energy, controls the temperature of the soil. Finally, soil temperature is far more effective than the temperature of the air. Heat is well known to accelerate diffusion, solution, osmotic action, and evaporation. Now these physical processes are precisely those that perform the chief, almost the entire work involved in plant nutrition and growth. Hence, a high soil temperature is essential not only for the life of the plant itself, but also for the ventilation and the life of the soil, a healthy soil being very appropriately called a living mass. On an average 40 per cent. of the radiant energy incident on the soil is absorbed, conducted downward, and stored in the form of heat, 60 per cent. being lost to the soil by reflection, radiation and evaporation.

Oxygen is as indispensable to the chemical life of the soil as it is to animal life. Both oxygen and nitrogen are essential to the biological processes, and both the chemical and biological activities in the soil are as indispensable to the crop as are sunshine and showers.

The importance of right proportions of water and air in the soil is further shown by the fact that the process of decay, whereby organic material is turned into humus and made available to the plant, cannot go on without an abundant supply of oxygen. A soil that contains too much water contains too little air. The ferments thrive best at a temperature of 85° to 95°, and when the soil contains from one-half to one-third the amount of water required for saturation. The ultimate source of the nitrogen found in vegetable matter is the air, and plants are unable directly to utilise it in a free state. The bacteria, which are chiefly concerned in maintaining the available supply of nitrogen in the soil, are able to work only during the warm seasons, and their activity depends directly on the temperature of the soil, being a maximum at 98°. On the other hand, light is inimical to the life and activity of these soil bacteria, a fact that may have some bearing on the rapid growth of corn during hot nights, inasmuch as the work of the micro-organisms in feeding the roots is then facilitated. That corn germinates best at the high temperature of 98° to 100° is, undoubtedly, due to its tropical origin. For Prof. Davenport shows that the attunement of plants to environment as regards temperature has its origin, not in processes of selection, but in the modifications of protoplasm by temperature itself.

Granted that the soil is porous enough and dry enough to admit the air readily, ventilation is facilitated by the unequal heating of night and day, and by non-periodic temperature changes as well. As the air within the soil is heated it expands, and some of it is forced downward to the deeper layers; when it cools it contracts, and free air is drawn into the soil. The same effect is produced by barometric changes; the passage of areas of high and low pressure has been found to influence the flow of water from drains to the extent of 15 per cent., thus showing an unexpected movement of air in the soil. The corn belt lies entirely within the region of maximum frequency and intensity of barometric oscillations in the United States. Strong, and particularly gusty winds, by a measurable aspiratory action, have also a significant influence on soil breathing.

Having seen how heat, light, moisture, and the supply of gases operate to control the supply of those ingredients that are furnished by the soil and that constitute in the main the ash of the plant, we return now to the immediate effect of these elements on the vital processes and assimilation.

While light is indispensable to the assimilation of carbon dioxide, it undoubtedly exerts a directly retarding influence on growth proper, or cell multiplication, but the beneficial effects of the higher temperature that accompanies daylight more than counteract this. Sachs showed that for many plants, when kept at a uniform temperature, the rate of growth gradually increases during the night and is a maximum shortly after daybreak. This effect of light is opposed to the effect of the diurnal temperature; heat and light increase transpiration, which means a loss of water, and hence less growth. This sensitiveness and response of protoplasm to light is the result of the chemical changes wrought therein by the light.

By osmotic action the root hairs imbibe the liquid food that surrounds them; capillary and osmotic actions carry this supply to every part of the plant, to the tip of every blade, which is not only bathed in air, but has its microscopic interstices permeated with it. Here, in the leaf cells, the carbon dioxide of the air, which is practically an invariable quantity, comes in contact with the water that has been brought from the roots. Here, too, the energy of the ether waves, which we call light, but which the vegetable cell recognises only as force, or a mode of motion, causes the carbon dioxide to part with some of its oxygen in exchange for some of the hydrogen contained in the water. Thus, there is formed within the cell a substance composed of carbon, hydrogen, and oxygen, the exact molecular structure of which is not known; in this process some of the oxygen is freed and thrown off by transpiration. By the introduction of the molecule of carbon dioxide into the cell the equilibrium in the atmosphere of that gas is disturbed and another molecule diffuses into its place; for this gas exists and behaves as if it were the only gas present in the space under consideration, the same law being true for each of the gaseous elements whose mixture constitutes what is called the atmosphere. The consumption of carbon dioxide tends constantly to produce a vacuum in the carbon dioxide atmosphere, and the law of diffusion as constantly tends to maintain the supply. If molecules of hydrogen are withdrawn from the fluid contents of the cell, instantly osmosis and diffusion tend to replace them; the same is true of the solid particles in solution. Assimilation within the cells of the leaves perpetually destroys the equilibrium of osmotic pressure, hence this pressure creates a constant flow toward the seat of demand. Evaporation from the leaves, which is proportional to temperature and is accelerated by winds, as is the supply of carbon dioxide, operates in the same direction, viz. to destroy the equilibrium in the leaf cells and channels, and consequently the tiny streams from the rootlets are hastened onward with their precious stores of food. Cold not only stiffens the sap and retards its flow, but also slackens molecular motion and hinders the chemical reorganisation of the elements. The process of evaporation proper is, however, almost independent of the processes of nutrition, and is rather a "necessary evil." The most rapid growth frequently occurs under precisely those conditions that make evaporation least rapid.

The quantity of water that passes through the plant and is transpired and evaporated is enormous. The average is about three hundred parts of water to one of dry matter. According to experiments by Prof. King of the Wisconsin Experiment Station, dent corn used three hundred and ten tons and flint corn two hundred and thirty-four tons of water for each ton of dry matter produced. This same experimenter supplied growing corn with water as fast as it could be used to advantage, and found that the crop consumed during its season of growth water equivalent to a rainfall of 34·3 inches, and yielded more than four times as bountifully as a very large crop grown under the best natural conditions of rainfall in Wisconsin. And he concludes that "large as this movement of water is, it is seldom great enough to enable a moderately fertile field to produce its largest crops." And these tests in Wisconsin merely confirm a conclusion that is becoming quite general, and is prompting the advocacy of irrigation even in the humid regions. Moreover, the quantity of water producing a given result increases with the fertility of the soil, and, according to Wollny, the soil moisture produces its maximum results only when the plants are grown in the strongest light. The value of a given quantity of rainfall for the crop increases as the number of rains, and what has been called the useful remainder of rainfall is only 20 per cent. of the total amount, percolation and evaporation accounting for 80 per cent. Percolation is a fertile source of loss of the valuable soil nitrates, especially in the wet fall and winter seasons, when the corn-field is bare and a large proportion of the water escapes downward. Rain, like snow, is the "poor man's fertiliser," bringing down, per acre, in the course of a year, at Rothamsted, England, twenty-four pounds of salt, four and a half pounds of nitrogen, eighteen pounds of sulphuric acid, and much carbon dioxide, which is a valuable solvent.

Some of the less important incidental relations of climate and corn, such as electricity, winds, frost, insect enemies, and diseases remain to be mentioned. Electricity artificially applied to the roots by charging the soil, and to the leaves by means of the electric light, have both repeatedly been found to stimulate the growth, and, in some instances, greatly to accelerate it. Recent experiments show that when green leaves are exposed to direct sunlight there is developed a difference of electrical potential between the illumined and the shaded surfaces, amounting in some cases to ·02 volt, but the bearing of this fact upon assimilation is not well known. Atmospheric electricity is a fertile source of ozone, or condensed oxygen, which is particularly active in the production of nitric acid. Electricity stimulates protoplasm, the ultimate vital principle, and may determine the character of its activities, but under natural conditions this element is believed to have but slight influence.

Seasonal characteristics have practical connection, too, with the insect pests and diseases of corn. Not only during the crop season are these pests largely at the mercy of the elements, but fitful winters are sure to prove destructive to them, for during the bright, warm days eggs are hatched, chrysalides matured, and insects lured from their retreats, only to be caught and destroyed by the sudden cold waves. The fungus diseases, such as rust and smut, are carried by winds, and are favoured by wet seasons, dews, and moist atmosphere.

So sensitive is the plant to the changes of climate that even the ordinary seasonal irregularities have a strong influence; the general disposition acquired by the seed in a single dry or wet, warm or cold, early or late, season prepares it by virtue of that experience to become the best seed for planting in anticipation of another such season as that in which the seed was matured. This tendency is illustrated by the well-known fact that dwarfed varieties of corn from northern latitudes, when cultivated to the southward, mature earlier, are hardier, and more prolific than the native varieties. A corollary of great practical promise is that in a region habitually or frequently dry, corn raised in the driest years should be preserved for seed, as likely to be far better than any that may be brought from a distance. Hence the common, if not universal, practice of using seed grown in the preceding year is strongly condemned. By always utilising seed that has been raised in the driest years one may hope speedily to develop varieties whose vegetating period will be so short that the crop will rarely be injured by the hot winds of July or August. And a similar rule would apply for any desired disposition we may seek to impress upon the seed.

In the light of these facts it is suggested that irrigation may come to be used as a temporary device to promote the evolution of new varieties that can be cultivated without irrigation. On the other hand, recent careful work in France has demonstrated that when the plants are forced to their maximum yield by irrigation the seed thereby suffers a marked deterioration, and that for continued maximum results the seed must be raised on dry soil.

Climate being inviolable and inexorable, what hope is there that the agriculturist shall be emancipated from the tyranny of frost and drought? Clearly, he must attain this by work on the soil and on the plant. By utilising vast stores of energy in the form of fuel man banishes the rigours of winter, thus creating artificial conditions of shelter and heat, by aid of which he has supplemented the process of acclimatisation. Thus, also, must he co-operate with Nature in behalf of the plant: he must combat her malignant aspects by intelligent selection; by scientific methods of culture he must supplement her beneficent efforts on behalf of the human race.

## *UNIVERSITY AND EDUCATIONAL INTELLIGENCE.*

MR. J. W. BULLERWELL, assistant lecturer at the Durham College of Science, Newcastle-on-Tyne, has been appointed assistant lecturer in mathematics at the Hartley College, Southampton.

FOLLOWING the usual custom, addresses will be given at many of the metropolitan and provincial medical schools, at the opening of the new session early in October. At St. George's Hospital an introductory address will be given by Dr. P. W. Latham, of Cambridge. The first meeting of the Physical Society of Guy's Hospital will be held on October 5, in the new physiological theatre, when Sir Samuel Wilks, F.R.S., will preside. At St. Mary's Hospital the session will begin on October 1 with an introductory lecture by Dr. William Hill. The session at the Middlesex Hospital will also begin on October 1, when Mr. T. H. Kellock will give an introductory address. The session of the Faculty of Medicine of University College will be opened with an introductory lecture by Prof. J. Risien Russell. The session of the London (Royal Free Hospital) School of Medicine for Women will be opened with an introductory lecture by Dr. F. W. Andrews. The winter session at Charing Cross Hospital will open on Wednesday, October 2, when an introductory address will be delivered by Prof. J. W. Taylor. At the inauguration of the sixtieth session of the School of Pharmacy, the Hanbury gold medal will be presented, and the inaugural address will be delivered by Dr. Arthur P. Luff. At the Royal Veterinary College the sessional course of instruction will be opened with an introductory address delivered by Dr. E. M. Crookshank. The winter session at Yorkshire College, Leeds, will open on October 1, when an introductory address will be delivered by Sir W. S. Church. At University College, Sheffield, the session will be opened with an introductory address by Sir Thomas Barlow.

SEVERAL prospectuses and calendars of technical institutions, showing the courses of work for the session just commencing, have been received. The London Polytechnics give prominence to the announcement that are recognised as qualified institutions from which students who have matriculated in the University of London may be presented for the new engineering degrees of the University. Courses of work suitable for such students have been arranged, and they should be the means of extending the knowledge of the science of engineering. In the prospectus of the Battersea Polytechnic, trade students are rightly warned against only attending classes connected with their occupations. It is pointed out that the principles of science must be studied, as well as technical subjects, if a thoroughly sound knowledge is desired. Without a working acquaintance with mathematics, mensuration and geometry it is almost impossible to make any real and useful advance in science and technology. This ought to be clearly understood, and it is worth while to consider whether students should not be compelled to give evidence of such knowledge before being permitted to join technological classes, where their presence is often a hindrance to progress. The calendar of the Northampton Institute, Clerkenwell, also contains much good advice as to the choice of studies, and the objects to be borne in mind. Among the noteworthy characteristics of the work of this Institute are the electrochemical laboratory, which has been equipped in a very complete manner, the attention given to horological engineering, and the department of optical and scientific instruments. But while there is evidence of progress in the work of our polytechnics and technical schools, there is still much to be done before they reach the standard of similar institutes in Germany and the United States, such, for instance, as the Rose Polytechnic Institute, Terre Haute, Indiana, the new calendar of which is before us. Each of the courses in this Institute occupies four years of three terms each, and no undergraduate student is permitted to elect any special or partial course. All students must take full work in one of the courses, and each member of the senior class must present a thesis recording an independent investigation at the close of the year. The value of this educational policy is indicated by the high positions which the alumni of the Institute occupy as professional engineers.

## SOCIETIES AND ACADEMIES.

### LONDON.

**Royal Society,** June 20.—"On the Resistance and Electromotive Forces of the Electric Arc." By W. Duddell, Whitworth Scholar. Communicated by Prof. W. E. Ayrton, F.R.S.

The author considers that the new facts given in the paper assist in formulating a consistent explanation of the resistance and back E.M.F. of the arc. The values found for the resistance of the vapour column and for the contacts between it and the electrodes offer no serious difficulties. The greater part of the two E.M.F.'s are considered as being most probably due to thermo-electric forces, and experiments in support of this view are described, in which it was found possible to obtain a P.D. of 0·6 volt by unequally heating two *solid* carbon electrodes with a blow-pipe flame, the voltmeter indicating that the hotter carbon was positive to the cooler. By using *cored* carbons and adding potassium salts, this P.D. was increased to 1·5 *volts*. It is pointed out that the differences of temperature existing in the arc must be many times as great as those which it is possible to produce with the blow-pipe, as the cooler electrode must be red-hot, or else it does not seem to make contact with the surrounding flame.

*On the Resistance of an Electrolyte.*—In measuring the resistance of an electrolyte by the Kohlrausch method, it is often assumed that the errors due to polarisation are avoided if the frequency of the alternating or interrupted current used is as high as a few hundred periods per second. Experiments made to test this point lead to the conclusion that unless other methods are adopted to eliminate the effects of polarisation, *it must not be assumed that the use of alternating currents of ordinary frequencies of a few hundred periods per second eliminates the possibility of errors due to polarisation.*

### PARIS.

**Academy of Sciences,** September 2.—M. Bouquet de la Grye in the chair.—On the application of the equations of Lagrange to electrodynamic and electromagnetic phenomena, by M. E. Sarrau. The application of the method of Lagrange to electrical phenomena leads to results which are naturally in accord with the principle of energy, since this principle is only one form of the theorem of kinetic energy, and this is a consequence of the general equations. But for this agreement to exist it appears to be necessary to admit that the internal energy of a system of currents and magnets is purely kinetic, no part of it being potential.—On the quadratic transformation of Abelian functions, by M. Georges Humbert.—Observations of the Encke comet made at the Observatory of Algiers, by MM. Rambaud and Sy. Observations of the magnitude, apparent positions of comparison stars, and apparent positions of the comet taken between the 9th and 18th of August.—On the continuous deformation of surfaces, by M. G. Tzitzéica.—Outline of a general theory of mechanisms, by M. G. Koenigs.—On the equilibrium of elastic bodies, by M. R. Liouville.—Evaluation of the resistance of steel to traction deduced from the resistance to shearing, by M. Ch. Fremont. The curve for resistance to shearing per square millimetre plotted as ordinates against the resistance to extension as abscissæ is a straight line.—On the first stages of development of some Polycystidea, by MM. L. Leger and O. Duboscq. Observations on three groups of Polycystidea, *Actinocephalides*, *Dactylophorides*, and *Clepsidrinides*, show that the typical evolution allows no intracellular stage. They differ in this respect from the intestinal Monocystidia, as has been shown by Caullery and Mesnil.—On scissiparity in the Hydroides, by M. Armand Billard.—On the appearance of the white rot (*Charrinia Diplodiella*) in Algeria, by MM. J. D. Catta and A. Maige.—On a case of sexual determinism produced by mixed grafting, by M. A. Jurie.

## CONTENTS.