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THURSDAY, AUGUST 15, 1872

THE BRITISH ASSOCIATION

THE recurrence of our annual Congress of Science naturally leads us to reflect on the position which the British Association occupies in our social economy, and on the part it is qualified and, perhaps, destined to play. Whilst other scientific societies occupy themselves in giving publicity to results and speculations, and in rewarding with their medals successful labours, the British Association alone systematically undertakes to distribute the greater part of its income, about 2,000*l.* per annum, in grants to enable men of science to conduct scientific investigations, and to institute inquiries with a view to possible future action. Stated briefly, this constitutes the broad distinction between the British Association and other scientific societies. As a publishing society it cannot vie with some other bodies, as, for instance, with the Royal Society. The great bulk of the papers it receives are published in abstract and but few *in extenso*; and it allows a greater latitude than other societies with regard to the reception of subjects which have been elsewhere made public, thus constituting its proceedings, to a great extent, a *résumé* of the year's work—a characteristic quite in keeping with its practice of meeting but once a year.

It is distinguished also by the wide range of subjects admissible in its various sections, by the facility with which membership is granted, by its attractiveness to foreign men of science, and, above all, by the tendency which its practice of meeting each year in a different town has to disseminate Science throughout the kingdom.

All these characteristics combine to make the British Association a truly national body. And the feeling that it is so has always made its leaders more ready to interest themselves in large national objects connected with Science, than, as members of other societies, the same men have elsewhere shown themselves to be. The British Association has thus always exhibited more self-assertion, and, in its communications with Government, more boldness, than other societies. In other scientific circles there is a disposition to regard any assistance given by the State as a favour to Science, and a timid reluctance to point out plainly cases in which the aid of the State is really necessary. The total Solar Eclipse of 1871 is a case in point. The Astronomical Society had not the heart to place before the Government a clear statemen of what was required. The duty of doing this, which obviously devolved on that Society, being thus neglected by it, was at once, without a moment's hesitation, successfully performed by the British Association, to the great benefit of knowledge.

But a more striking and far more important example of the wise vigour which has generally characterised its counsels, is afforded by the steps it took to obtain a thorough inquiry through a Royal Commission into the whole condition of scientific action and administration in England. Whether that inquiry leads to immediate reform and expansion of our scientific institutions, as it is generally expected it will, or not, the inquiry itself has already been productive of incalculable good. It will probably be found, when the evidence is published, that our "system" at present consists of a mass of inconsistencies, and deficiencies, of the existence of which not even those who originated the movement could have had any clear idea, much less any reliable proof. The utter absence of any guiding principle in the dealings of successive Governments with Science, and of any system for administering such imperfect and dislocated institutions as we possess, which we are convinced the inquiry must also establish, will so startle all thinking men, whether scientific or not, that sooner or later reform must come, although the causes of this state of things are not far to seek.

Another good result of the inquiry is that it has forced the large body of men of Science who have been examined to turn their attention from that too rapt contemplation each of his own labours to which English philosophers are addicted, towards the great fields which others are cultivating; and by forcing them to regard Science as a whole, to recognise and duly appreciate the individual value and the interdependence of its several parts. The change in scientific thought which has taken place in the course of the two years during which the Royal Commission has been sitting, is quite perceptible to those whose attention is turned to the subject.

But no change of thought is perceptible in the Ministry of the day. It is perfectly clear that now, as ever, any aid given to Science is a mere question of pressure. Sometimes it is yielded with apparent promptitude to the external force of numbers, importunity, or probable popularity. At other times it is as stubbornly refused. An example of each is of recent occurrence. Aid was given to expeditions to observe the two last total Solar Eclipses, in the shape of several thousand pounds, and the use of ships. The aid of 150*l.* was refused to the British Association for completing tidal investigations, on which that body had spent 600*l.* As it is impossible to refer these two acts to one and the same guiding principle, we must assume that different motives prompted each, and that, as no properly instructed mind could consider Eclipse Observations many times more important to a great naval and maritime country than Tidal Researches, compliance with the one demand cannot be set off against refusal of the other in assessing the real regard for Science to be credited to the Government.

These two well-marked cases, the miserable Hooker-Ayrton wrangle, the treatment of the Society of Antiquaries recorded in our last number, and the declaration of Mr. Gladstone at the Royal Society's anniversary dinner that Science must suffer if aided, or as he expressed himself, if "interfered with," by the State, are all indications that the Government do not yet know that it is possible to draw a boundary line separating the regions of scientific activity which should be occupied respectively by individuals or private bodies and by the State. And the truth does not yet seem to have dawned on them that the prolonged neglect of those scientific objects which State resources alone can attain is a positive dereliction of duty, the effect of which in overweighting England in the race of European civilisation is already perceptible.

Our immediate object in drawing attention to the unsettled and phlegmatic views of the Government with respect to Science is to raise the question whether the British Association cannot reinforce the healthy tone of thought they have brought about through the Royal Commission obtained by their influence. We believe that this question will be raised in a more formal manner at Brighton; but as it was first suggested in these columns,* we may, without impropriety, give it our advocacy.

It is thought by many that the perplexed and perplexing way in which the relative functions of individual and of State action in Science are now confounded, has its origin in a great measure in neglect of classification on the part of private persons and private bodies. And the opinion is spreading widely that the British Association itself has not sufficiently discriminated, in distributing its funds, between objects which individuals are perfectly able to compass, and which they should be encouraged to undertake, and those which the State alone can successfully grapple with, and which, by reason of their evident importance to the community at large, the State is therefore bound, as a matter of duty, effectively to provide for.

We shall not here attempt to indicate the tests by which these two classes of scientific objects may be distinguished. If the principle be but admitted that a distinction does exist, the necessary rules for enforcing it may safely be left to the wisdom of the Association to draw up. Its experience is very wide, and its records will supply ample materials for ascertaining what are the purposes which, with the best intentions, it has been unable to attain, and on which its grants have been virtually wasted. These will afford data sufficient for the construction of a code of rules applicable to almost every case that can come before it.

The next question is, how should these tests be applied? We are satisfied that the time has arrived when the Association may with perfect propriety, and with the certainty of the most beneficial consequences, decline to allot any portion of its funds to purposes which should by rights be undertaken by Government. We are far from counselling this step as a retaliation for such refusals of State help as that respecting, for instance, the tides. Any such feeling would be quite unworthy of such a body as the British Association. Its grounds for doing as is proposed would be perfectly clear, and entirely free from any suspicion of antagonism or irritability. First, many of the objects which the Association has attempted to attain have been distinctly proved to be too large for its resources, and to require official machinery which it cannot command. The question of Sewage is a marked example of this class. But the fact that the Association has taken up such a subject leads to the mistaken belief that it is properly provided for; and it is not till some years have elapsed that the truth breaks upon us that the time and money expended upon it have been almost wholly wasted, and that the question remains pretty much in its original condition—not appreciably advanced. The attempt to deal with such problems with insufficient means results, therefore, in delusion and delay. Secondly, to deal with all classes of scientific questions without discrimination, perpetuates and deepens the obscurity which prevails in England as to the duties of the State. No one has yet been bold enough to maintain that the State should do nothing whatever for Science, and that what is at present done should be discontinued; but scarcely any one seems to have a clear idea of the principles on which such duties should be defined, and on which expansion should proceed. If once such a definition is arrived at, the main difficulty will have been overcome. If it is once settled by competent authorities that certain inquiries, or experiments, or observations, should, by reason of their expensiveness, of their value to the community, or of the length of time they must occupy, be undertaken by Government, the first step will have been taken towards that organisation of State Science which it is clear must not be much longer delayed.

Now, there can be no more practical mode of arriving at such a definition than that of firmly refusing to apply private funds to public purposes, as here proposed. There will at first be some difficulty in effecting the necessary classification, and it will be well not to apply it at the outset too rigorously; but by degrees the difficulty will vanish, and it will be as easy to say what subjects devolve on the State as it now is to say what subjects appertain to particular sections of the Association.

Another great advantage which will ensue will be the amount of funds thus set free for assisting those objects which can be effectually attained by individual enterprise, the number of which is very great. It is well known, and much to be lamented, that many of these invaluable undertakings are starved for want of those very funds which are now spent in the vain endeavour to do the State's work.

The subjects which the Association may thus pronounce to be not within its province should not be lost sight of. An enumeration of them should be submitted annually to Government, and the resulting action taken on them by Government should be regularly reported to the Association and published. The effect of this would be to assist the Government in arriving at some measure of the scientific work which must be done by them, if done at all. This will soon be shown to be enormous in extent and variety. Attention will next be called to the machinery existing for such purposes. The first question will be, If such and such investigations are to be undertaken, which department shall be made responsible for them? And this must bring out prominently the absurdity of our present arrangements, whereby the various scientific institutions of the State are scattered amongst the various departments, and must lead to what is the fundamental requisite—concentration of all such institutions under one department responsible for the whole.

Nothing that the Association can do would, in our opinion, conduce so directly to this desired end as the classification of the applications made to them for funds in aid. The object is one in strict keeping with its traditions, and quite worthy of its ambition, destined as it is to be the High Court of Appeal in Science, and the prime mover in all that concerns material and philosophical progress. In the present day no considerable measure is undertaken by the State except in obedience to an impulse from without. This will clearly be the case with respect to Science. The first impulse has been given already by the British Association. We foresee that the issue will depend materially on the persistent firmness with which its first efforts are followed up by that powerful and useful body. Success must crown them at last, and sooner perhaps than some at present anticipate.

* *Vide* "The Tides and the Treasury," June 27, 1872. No. 139, vol. vi.

THE BEGINNINGS OF LIFE*

II.

LEADING on to the newer and more important observations in the latter portion of the work, we have a sketch of the relation of crystals and organisms, in which a variety of curious and suggestive facts are adduced, tending to show that there is a striking analogy, if nothing more, in their mode of origin. The influence of changed conditions is shown to produce very similar results to both, and the views of Mr. G. H. Lewes—that organisms are not always united by the link of a common heritage, but that many may owe their similarity to having originated under the influence of uniform organic laws acting under uniform conditions—is quoted with approval. Just as similar crystals are produced in similar liquids under like conditions, so may low organisms of similar or identical structure be produced; and just as the fragments of a crystal will, under favourable conditions, form each an entire and perfect crystal, so do low

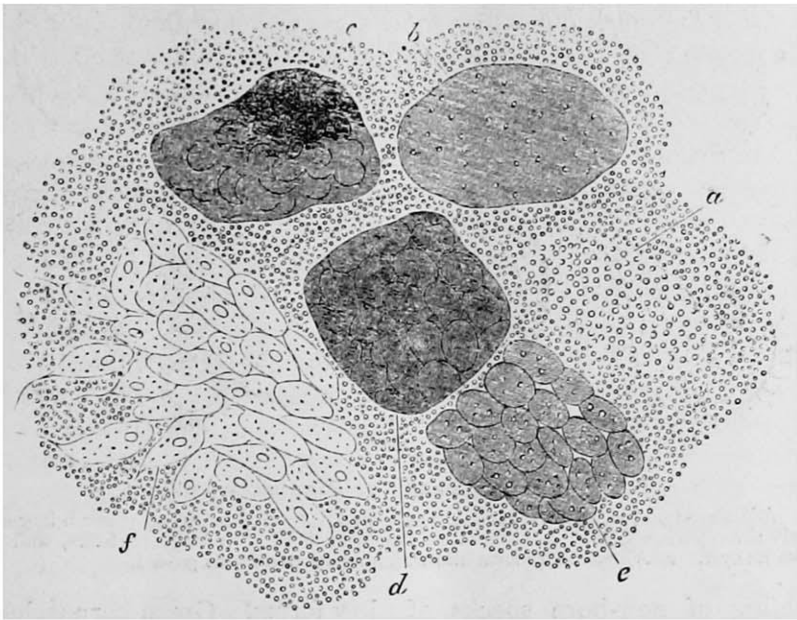

FIG. 1.—SEGMENTATION OF EMBRYONAL AREAS INTO MONADS—(× 1,670).

a. First stage of differentiation. *b.* Second stage; area almost homogeneous and refractive. *c.* First traces of segmentation. *d.* Segmentation more complete; units highly refractive. *e.* Units less refractive; forming tailless corpuscles. *f.* Fully developed Monads derived from such corpuscles.

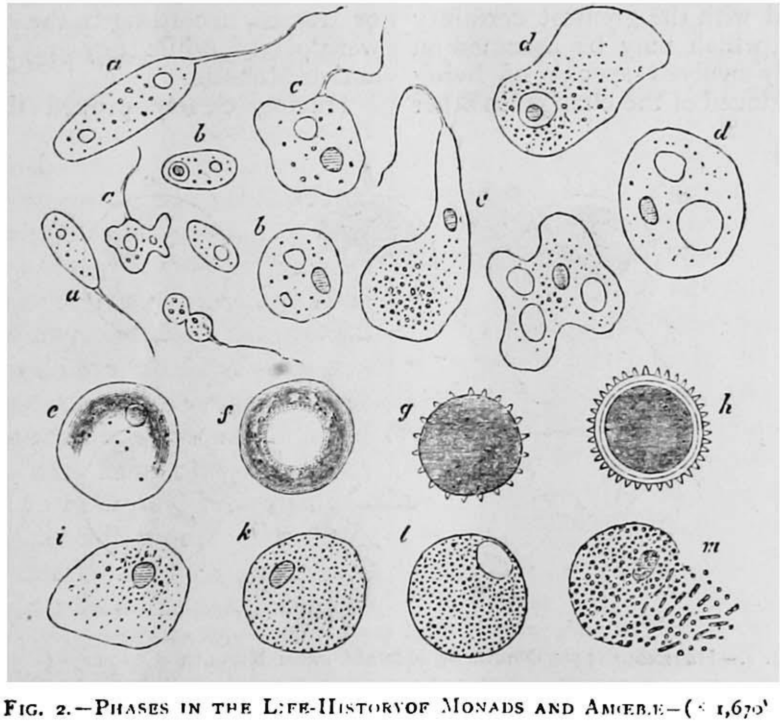

FIG. 2.—PHASES IN THE LIFE-HISTORY OF MONADS AND AMŒBÆ—(× 1,670).

a, a. Monads in different stages of growth. *b, b.* Similar Monads which have lost or retracted their flagella. *c, c.* Monads about to be transformed into Amœbæ. *d, d.* Resulting Amœbæ in active and motionless stages. *e, f, g, h.* Stages by which motionless Amœbæ become encysted. *i, k, l, m.* Stages by which other Amœbæ become resolved into Bacteria.

organisms multiply by fission, each part becoming a perfect whole. The difference between crystals and organisms is said to be less radical than has been supposed, and is mainly due to the much greater complexity and instability of the molecules which go to build up the latter. Crystals are statical; organisms, dynamical aggregations of molecules. Specks of new living matter soon aggregate into certain definite forms just as crystals do, but being much more complex and unstable, they are liable to much greater variations and successive modifications. The excessive variability and instability of low forms of life is dwelt upon as an anomaly on the ordinary theory, when viewed in connection with their supposed wonderful stability for immense periods of time. It is generally believed that every one of the lower animals is a descendant of other low forms which lived in ages far anterior to the Silurian epoch. Many of the foraminifera.

* "The Beginnings of Life: being some account of the Nature, Modes of Origin, and Transformations of Lower Organisms." By H. Charlton Bastian, M.A., M.D., F.R.S. (2 vols. London: Macmillan and Co. 1872.)

for example, have hardly undergone any essential change, the same forms and varieties recurring at very distant geological periods. If, however, living matter does continually come into existence, the lowest forms will probably have been very similar in all ages; and it is only as these forms developed into more complex organisms that the varying conditions of the different periods will have led to the development of specialised groups.

The nature and mode of development of the low organisms found in infusions is next elaborately discussed, with the following result:—"No other conclusion remains for us, but that the several organisms are products of the direct developmental unfolding of new-born specks of living matter. And yet among these forms we see Bacteria, Vibriones, Leptothrix, and Torulæ; Fungus filaments, with and without fructification; Protamœbæ and flagellated Monads; Pediastreæ and Algoid filaments. All these are therefore proved with the greatest certainty to be interchangeable forms, which may be assumed on different occasions by newly evolved specks of living matter." Evidence is also adduced of the changes in other low forms. Green corpuscles thrown off from a single Lichen have been seen by Dr. Hicks to assume the forms and mode of growth characteristic of no less than twenty-three supposed species of Algæ; while gonidia from an Alga or from a Moss were developed into Lichens, Algæ, or Mosses, according to the conditions under which they were placed, while they may sometimes give birth even to active Monads.

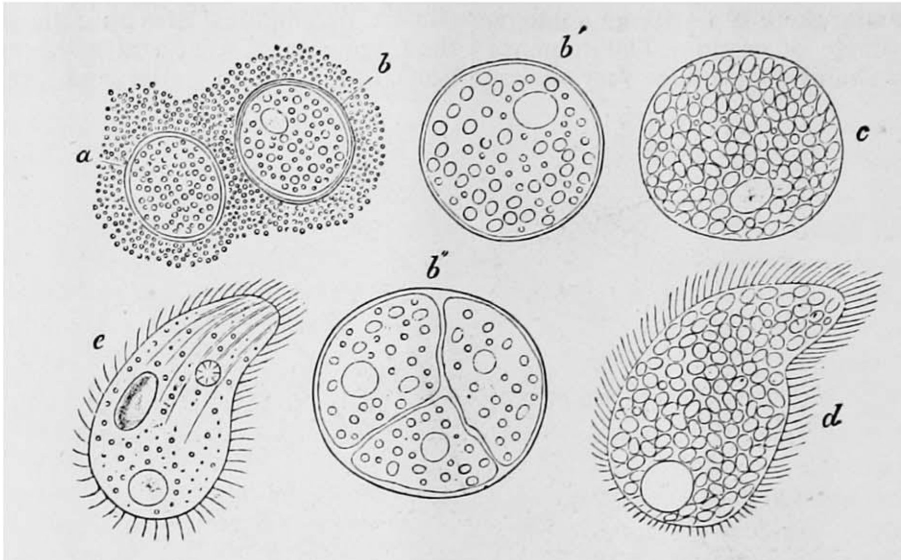

FIG. 3.—MODE OF ORIGIN OF PARAMECIA—(× 800).

a. First stage of differentiation. *b.* Later stage, in which vacuole has appeared. *b*¹. Similar stage of much larger embryo. *b*². Another embryo which has segmented into four (only three parts visible). *c.* Later stage; embryo filled with large particles, and revolving within its cyst. *d. Paramecium* after it emerges from its cyst. *e. Nassula*-like form into which many afterwards passed.

Having clearly proved that Bacteria and other low organisms, which form a pellicle on the surface of infusions and other liquids, are produced *de novo* in such infusions, the third part of the work, entitled "Heterogenesis," is devoted to a history of the microscopical examination of the changes which take place in this pellicle, and of all that is at present known of the transformations of the various classes of organisms to which it gives birth. To make this part of the subject clearly intelligible, it will be necessary to reproduce a considerable number of the woodcuts by which these changes are illustrated. One of the most simple series of changes—this transformation of motionless corpuscles into ordinary Amœbæ—was closely watched by Dr. Bastian, and seen with the most perfect distinctness in thousands of instances. Fig. 4 shows the stages by which the more highly organised Monads are developed. The first step was an increase of the amount of gelatinous matter between the corpuscles or Bacteria, which gradually became less defined, and at last scarcely visible in the protoplasmic mass, in which segmentation then began to take place, and continued till it separated into active Monads. After a time, however, these again began to change into Amœbæ, and these latter, passing through a motionless and encysted stage, became resolved into Bacteria (Fig. 5). The whole

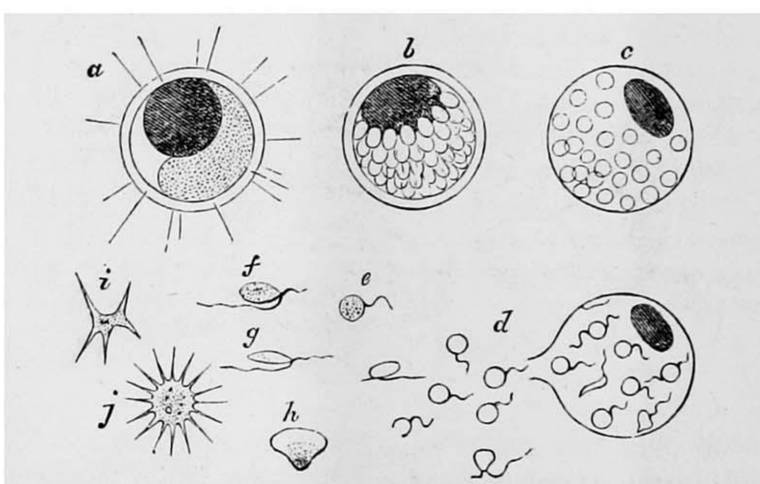

FIG. 4.—HETEROGENETIC ORIGIN OF MONADS FROM NITELLA (CARTER)—(× 350).

a. Contents of new-formed cyst separating into Protoplasm and dark brown refuse matter. *b, c, d.* Segmentation of the Protoplasm into Monads, which afterwards escape from the ruptured cyst. *e, f, g.* Different forms of the Monads. *h, i, j.* Forms of Amœbæ and Actinophrys which the Monads subsequently assume.

series of these changes occupied about ten days. In other cases similar corpuscles developed into fungi; while in some instances in the same pellicle the change into Amœbæ on the one hand, and into Fungus germs on the other, went on simultaneously. It was soon discovered that the temperature at which the infusion was made was of great importance. If it had been heated to 212° F. no development beyond Bacteria occurred; if at 149°—158° F Fungus germs arose; while an infusion in all other respects similar, but prepared at a temperature of 120°—130° F gave rise to actively-moving Monads.

A step further takes us to the "spontaneous eggs" of Pouchet, which are seen to be formed in the pellicle, and afterwards give birth to Paramecia—highly organised ciliated Infusoria. These never appear except in infusions made with cold water, and Dr. Bastian assures us that he

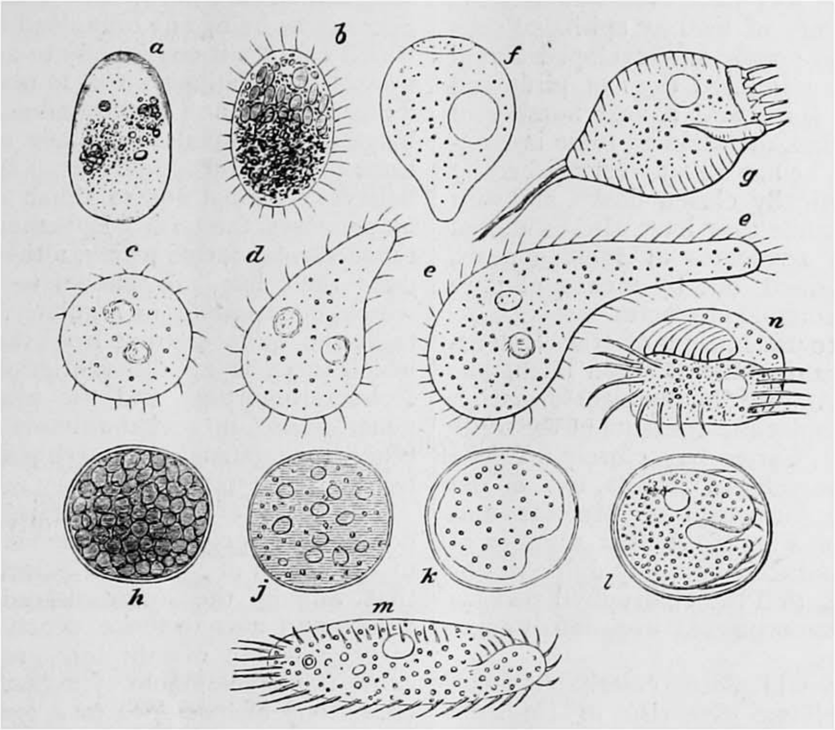

FIG. 5.—MODES OF ORIGIN AND DEVELOPMENT OF CILIATED INFUSORIA—(× 600).

a. A transforming Euglena with red "eye speck" still visible. *b* A similar body, having many of its chlorophyll corpuscles still green, fringed with almost motionless cilia. *c.* A completely decolourised sphere derived from a transformed Euglena, provided with a few partly motionless cilia. *d, e.* More advanced forms of a similar embryo developing into a Dileptus (?). *f.* Vorticella, soon after its emergence from a cyst of Euglena origin, which subsequently develops into a striated variety (*g*). *h.* A large Chlorococcus-vesicle whose contents gradually undergo decolourisation (*j*), and at last become converted into an animalised mass (*k*), which gradually shapes itself into the form of an Oxytricha (*l*); this after a time ruptures its cyst and soon takes on the characteristics shown at *m*. *n.* A form of Plæsconia derived from an embryo produced within other apparently similar Chlorococcus-vesicles.

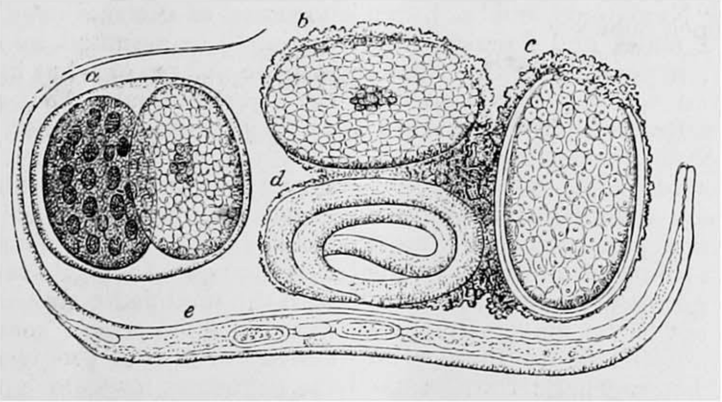

FIG. 6.—ORIGIN OF NEMATOIDS FROM EUGLENÆ (GROS).

a. A large Euglena which after encystment has undergone fission, whilst one of the halves has become decolourised. *b.* An Euglena which has become converted into a decolourised embryonic mass, leaving only a small coloured remainder. *e.* Another decolourised mass, which, after undergoing certain changes, becomes converted into a young Nematoid, as at (*d*). *e.* A female specimen of the developed Nematoid three weeks old, in whose ovaries two partially developed ova are seen.

has verified Pouchet's observations in all essential particulars, as represented in Fig. 6; and the still more complex Vorticellæ have been seen to arise in a similar manner. Now the germs of Ciliated Infusoria are comparatively very large and easily recognisable; they have never, or very rarely, been discovered in the atmosphere; and no competent observer could overlook them; so that it is almost impossible not to accept the fact of the origin of these organisms in the manner here described.

The course of the argument is at this point interrupted by a chapter on the Atmospheric Germ Theory, which, though exceedingly interesting and well written, is quite out of place here; and we then come to some curious observations on the production of organisms within the closed cells of various plants. M. Trecul, a distinguished French botanist, has watched the formation of Amylobacters, low organism allied to Bacteria, and minute Fungi within the closed cells of living plants. In *Ficus carica* he discovered fungoid organisms within the completely closed cells of the medullary tissue, which, he believes, "negatives all ideas as to the introduction of germs from without." Minute crystalline tetrahedrons in cells of the bark of common elder and other plants were actually seen to be transformed into Amylobacters. The transformation of milk globules and a film of diluted cream-cheese into Fungus germs has also been closely watched by several observers, and has convinced them that these organisms have not arisen from accidentally introduced spores, but by a true heterogenetic transformation of the substance examined. Dr. Lionel Beale has discovered lowly vegetable organisms "in the interior of the cells of animals, and in the very centre of cells with walls so thick and strong that it seems almost impossible that such soft bodies could have made their way through the surrounding medium." Many other observers have even watched the transformation of the contents of healthy epithelial cells into Bacteria and Vibriones; and well-developed Fungi have been found within the uninjured eggs of birds and serpents. Now, all these facts, and a vast number of others detailed by Dr. Bastian, are claimed to be in complete harmony with the facts he has already established by his experiments with hermetically closed flasks, and with the theory of Archebiosis, while they have always offered immense difficulties to the advocates of Homogenesis, and have never been explained but by means of pure assumptions of a most improbable character.

We next come to the consideration of true Heterogenesis among lower organisms. Dr. Braxton Hicks has observed the production of Amœbæ by the transformation of the chlorophyll and protoplasmic contents of the cells of Moss radicles. Mr. H. J. Carter has closely followed the changes occurring in the cells of Nitella, one of the Characeæ, resulting in the formation of Monads and Amœbæ, as represented in Fig. 1. A vast number of observations of a similar character by many different observers are detailed, showing that the chlorophyll vesicles of Algæ are sometimes metamorphosed even into Pediastreæ, Desmids, and Diatoms.

But we must pass on to still more remarkable facts. The cell contents of Confervæ give rise to Euglenæ and Astasiæ, beautiful green organisms which abound in stagnant water, and these undergo transformations into a variety of higher or lower organisms, such as Diatoms, Amœbæ, and Ciliated Infusoria, the latter process being represented in Fig. 2. But Ciliated Infusoria themselves undergo transformation into various forms of lower animals, among others into Rotifers. The low Euglenæ are also transformed into either Rotifers, Tardigrades, or Nematoids, and the latter even grew into well-developed males and females (Fig. 3). Still more extraordinary, if possible, is the transformation of the minute Algoid Chlorococcus into the large, complex, and well-known Rotifer, *Hydatina senta*. Concerning the reality of these transformations, astounding as they are, Dr. Bastian assures us he entertains not the slightest doubt, having traced them through all their stages. The extreme prevalence and almost universal distribution of certain common forms of Rotifers, Tardigrades, and Nematoids, whose germs or ova are unusually large, and have been proved not to be universally present in the atmosphere, is inexplicable to those who disbelieve in the occurrence of heterogenetic transformation. Not only is it said to be proved that such transformations occur among Algæ, Fungi, Lichens, and Mosses, in every group of animals belonging to the class Scolecida, and in some of the lowest Annelida, but also in some of the lowest Arthropoda. In concluding this part of his subject, our author remarks :—"The fact that animals with such distinct and specific organs should arise in this definite manner from the reproductive products of a plant, will doubtless seem to many to flavour more of fable than of fact. After the observations which have been detailed, however, we must accept the occurrence of such phenomena as established facts, just as we are compelled, and are now quite accustomed, unhesitatingly to believe in the reality of other equally inexplicable phenomena. When we are able really to explain the reason of the processes by which one minute vesicular mass of fatty and albuminoid particles develops into a man, another into a fish, and another into an insect, we may then, with a little more show of reason, think of rejecting other more or less similar facts because they are incomprehensible."

Passing now from facts and observations of which we have only been able to indicate the character and extent by a few examples, Dr. Bastian proceeds to discuss the nature of "individuals" and "species" by the aid of the new light these researches have thrown upon them. He adopts the definition of an "individual" given by Herbert Spencer as being any organised mass "having a structure which enables it continually to adjust its internal relations to external relations, so as to maintain the equilibrium of its functions," and would define species to be any assemblage of individuals which are enabled for many generations to reproduce their like. But between these two he believes we must now establish a third category, for which he proposes the term "Ephemeromorphs," to include all those various forms which, although they sometimes produce their like, are shown to be interchangeable, and which, occasionally or regularly, arise from, or give birth to, forms quite distinct from themselves. All groups in which there is no differentiation of sexes are probably Ephemeromorphs, and the phenomenon of "alternate generations" in sexual animals is thought to be a recurrence to a partially Heterogenetic mode of reproduction.

The facts of Heterogenesis, if established, will undoubtedly largely modify our views as to the universality of the action of "Natural Selection." They seem to show that among the lower organisms, unknown laws of "polarity" akin to those which influence the production of crystals, but of infinitely greater complexity, directly cause the development of a vast variety of forms; while conditions of existence to a great extent determine the special forms that shall arise in each individual case. For such creatures "laws of heredity" hardly exist, and if so, Natural Selection can have little or no power. If we consider the enormous variety of forms that have been here shown to arise by Heterogenesis, it becomes evident that the field of action for Natural Selection becomes thereby considerably reduced. Again, the experiments detailed by Dr. Bastian prove the overwhelming importance of external conditions in determining the form that shall be assumed by many of the lower organisms, just the reverse of what has been found to obtain among the higher animals. And, what is still more important, the varying conditions do not act by producing changes in the adult organism which may be transmitted to their offspring, but actually so modify the developing germs as from a similar starting point to produce organisms which would rank as of distinct species, genera, or even families. The change produced seems to be quite incommensurate with the modified conditions which lead to it, and we are thus forced to accept some form of belief in innate tendencies or laws of progressive development, dependent on the polarities, forms of equilibrium, and attractive or repulsive properties of the complex physiological units of which organisms are built up. Such views are generally repudiated by modern thinkers; but Dr. Bastian believes they are necessitated by the facts now brought forward, and that they are really not only in harmony with, but almost necessary deductions from, the principles of the philosophy of evolution.

The phenomena of Heterogenesis also lead us to conclusions as to the rate of change in time of lower organisms exactly the reverse of those generally held. From having mainly studied the higher forms of life, and from having ascertained that the complex actions and reactions of such organisms on each other have been more efficient in producing specific changes than mere variability or the influence of changed conditions, Mr. Darwin has been led to the conclusion that the rate of change of the early forms of life, which had far less complex actions and reactions among themselves, must have been exceedingly slow.

This has almost the appearance of a paradox, in view of the admitted fact of the extreme variability and instability of these lower forms; yet it has been generally accepted as a sound inference from the law of natural selection, and has greatly increased the difficulty that has been felt as to the enormous time required for the development of all forms of life from the supposed primordial germs. But if the facts of Archebiosis and Heterogenesis are true, and all the lower forms of life are continually being produced *de novo*, under the influence of unknown laws of development, then we may fairly conclude that, when once the earth had arrived at conditions favourable to the production of living organic matter, the process of development would be rapid, and an immense variety of low forms of animals and vegetables would soon people it. It is a fair inference, too, that if such complex organisms as Ciliated Infusoria, Rotifers, Nematoids, and even simple Acari, can be developed independently of the slowly modifying influence of natural selection, the same laws of development will continue to act a subordinate part much higher in the scale, and, by assisting natural selection in its work, may have enabled a much more rapid progress to be made.

It is very strongly argued by Dr. Bastian that the conception of an origin of living organisms at a single remote epoch in past time, and the lineal descent of all existing organisms from those primal forms, is one quite opposed to the uniformitarian and the evolutional philosophy, and in the highest degree difficult to accept. It is almost inconceivable that Bacteria, Moulds, Monads, Amœbæ, and a thousand other minute and simple organisms, should still exist so universally over the earth, and under such an infinite variety of simple forms, if all were descended from ancestors which could hardly have been more simple in the almost infinitely remote past, and which throughout all that time had been subject to those same causes of change and advance in complexity of organisation which have resulted in the varied forms of all the higher animals. Whatever laws and conditions led to the production of the earliest organisms, they are hardly likely to have been of so exceptional a nature as never to have occurred since. It does not seem probable that the very existence of life upon the earth depended on so rare and improbable a set of conditions that, having once occurred, they should never occur again in the whole period between some remote pre-Laurentian epoch and the present day. If, therefore, there is good evidence of the continued *de novo* production of lower forms of life, and of the direct transformation of these into various higher and more complex organisms, such a view will have many *a priori* considerations in its favour, and will tend to bring the whole series of life-phenomena into greater harmony with those of inorganic nature, without in any way diminishing the mysterious grandeur that surrounds them.

But if these views should be established, we shall have to form an entirely new conception of the genealogical history of the various existing organisms. We shall no longer have one "tree of life," but a vast number of such trees, all having their roots in a similar substratum of the lowest organisms, evolved at various periods of the earth's history, but differing greatly in their subsequent development. It is probable that by far the greater number of these "trees of life" have become extinct at various periods of their growth, and that all existing living things belong to portions of but a few "trees," some of which may be comparatively recent, while others may have their roots far back in the past, anterior to the earliest epochs of which geology affords us a record. But notwithstanding this diversity and separateness of origin, through the whole life-history of our globe the progress of organisation seems to have been essentially similar; which is readily explicable on the ground that living things, both as regards their origin and subsequent differentiation or development, are the immediate products of natural laws or material properties, which are probably the same now as they have ever been. Similar types of form may, therefore, again and again have arisen; and Dr. Bastian remarks, that even "the vertebrate grade of organisation *may* have been attained by ultimate branches of different trees of life." It remains to be seen how far this conception will throw light on obscure and difficult questions of biological classification, and on those facts of geological succession which are most difficult to reconcile with the usual view of all organisms whatever having originated from a single almost infinitely remote source.

It will now be seen, even from the very imperfect sketch of its subject-matter, how many questions of the highest scientific importance rise out of the facts adduced in Dr. Bastian's work. It is not too much to say that, if its main conclusions are established, it will create a revolution in organic philosophy of equal importance with that which was effected by Mr. Darwin, whose observations and most important theories will, however, remain unaffected by it. That gentleman has himself remarked that "analogy is a deceitful guide," and it is only by analogy that he extends the laws he has established for the higher animals and plants to those lower forms with which Dr. Bastian deals; and the establishment of facts proving that they come under a different category will even relieve the theory of natural selection from some of its greatest difficulties, and neutralise some of the most serious objections that have been brought against it. The whole question, however, is primarily one of facts, and, however it may be ultimately decided, every lover of science must admire the courage and energy with which Dr. Bastian has taken up an unpopular subject, the skill and patience with which he has experimented, the labour which he has bestowed in collecting the records of widely scattered and almost forgotten observations, and the logical force as well as the philosophical spirit with which he has worked out his conclusions. It is a book that cannot be ignored, and must inevitably lead to renewed discussions and repeated observations, and through these to the establishment of truth.

ALFRED R. WALLACE

NOTES

THE Lords of the Committee of Council on Education having decided to transfer the instruction in Physics, Chemistry, and Natural History from the Royal School of Mines in Jermyn Street, and the College of Chemistry in Oxford Street, to the new buildings in Exhibition Road, South Kensington, notice has been given that in future the following courses of lectures and practical laboratory instruction will be given at South Kensington at the date specified:—Chemistry by Prof. Frankland, D.C.L., F.R.S. A course of forty lectures on Inorganic Chemistry commencing 21st of October, 1872. A course of thirty lectures on Organic Chemistry commencing 13th of January, 1873. Laboratory instruction consisting of an elementary and an advanced course commencing on 1st of October. Biology by Prof. Huxley, LL.D., F.R.S., a course of eighty lectures on Biology (or Natural History, including Palæontology) with laboratory instruction, commencing the 7th of October, 1872. Physics by Prof. Frederick Guthrie. The course will consist of lectures, with laboratory work on the subject of the lectures, divided as follows:—Twelve lectures on Molecular Physics, Sound, &c., commencing 24th of February, 1873; fifteen lectures on Heat, commencing on 24th of March; fifteen lectures on Light, commencing on 12th of April; twenty lectures on Electricity and Magnetism, commencing on 19th of May. Each course will be complete in itself, and may be taken separately.

THE Trustees of the British Museum have nominated Dr. Albert Günther, F.R.S., to the post of "Assistant Keeper" in the Zoological Department, vacant by the decease of the late Mr. G. R. Gray. It need hardly be said that no fitter appointment could have been made. The vacancy occasioned by Dr. Günther's promotion is, we understand, to be filled by Mr. R. B. Sharpe, F.Z.S., late Librarian to the Zoological Society, a young and rising Ornithologist, to whom the care of the National Collections of Birds will be entrusted.

IT is with great regret that we find there is too much truth in the report that the eminent astronomer and physicist M. Delaunay, Director of the Paris Observatory, has met his death by the upsetting of a boat while in an excursion on the Coast of Normandy. We hope in a future number to give a biography of this distinguished man. His loss is an irreparable one, not only to France, but to Science throughout the world.

THE meeting of the British Association for 1873 will be held at Bradford, under the presidency of Mr. J. P. Joule, D.C.L., F.R.S. At the present meeting the Association loses, with great regret, the services of one of its permanent officers, Dr. Thomas Thomson, one of the general secretaries, who will be succeeded by Dr. Michael Foster, F.R.S.

WE have to record the death of Sir Andrew Smith, K.C.B., Director-General of the Army Medical Department from 1851 to 1858, at his residence in Alexander Square, Brompton, at the at the age of 75. Sir Andrew Smith is well and favourably known to zoologists by his "Illustrations of the Zoology of South Africa." His complete and accurate knowledge of the various tribes of Southern Africa rendered his opinion of great value to successive Governments, and it was upon his representation and advice that the district of Natal was constituted a colony.

ALTHOUGH we can quite sympathise with those feelings which induced Mr. Fawcett to bring on the Ayrton-Hooker question in the House of Commons at all hazards, we consider that it is extremely unfortunate that Sir John Lubbock's determination to postpone it till next session—when the true opinion of the House, if necessary, would have been elicited—was not carried out. As it is, Mr. Ayrton has had an opportunity of exhibiting himself in his true character, which, however, was pretty well known before; and Mr. Gladstone has had an opportunity of again learning from the public press, in no hesitating tone, what is thought of his *protégé*; but the case itself has not progressed since the time we last referred to it.

IN his annual address as president of the Pharmaceutical Society, delivered at Brighton on Tuesday morning, Mr. H. B. Brady advocated the application of a portion of the surplus funds of the Society to the encouragement of scientific training. He enunciated the very sound proposition that beyond an investment sufficient to guarantee the means of carrying out the examining and governing functions entrusted to the Society by Government, there can be no excuse for the accumulation of wealth. Constantly recurring investments represent good left undone, opportunities unaccepted. Nor in this hoarding of money instead of science is the Pharmaceutical Society true to the spirit of its founders. The Society was formed to do in a collective capacity what could not be done by individuals. "It has seemed to me," he continued, "that the most substantial aid which could be rendered in the direction alluded to would be the setting apart of a number of free benches in the Society's laboratory for students who, having passed the Major examination with credit, might desire to continue their studies. These should even be endowed with a small annual income, under certain conditions, if found necessary. The only primary stipulation should be that, possessing the requisite preliminary knowledge, the recipient should be ready to work for the advancement of pharmacy under the direction of the professor. The effect of half-a-dozen or a dozen men so trained, sent out annually from Bloomsbury Square, would be to make a British school of pharmacy the like of which has never existed; and were this carried out, the most serious difficulties in the way of provincial education would resolve themselves in a few years." Should the Pharmaceutica Society follow this admirable advice of its president, it will place itself distinctly in the van of our learned societies, and will furnish an example which might be well followed by some of the others with their too abundant invested property.

A MONUMENT in honour of Jahn, the founder of the German Turnvereine, was unveiled on Saturday on the Haasenheide, near Berlin, amid enthusiastic acclamations. When shall we, as a nation, delight to honour in a similar manner the physical benefactors of mankind?

THE French Academy has elected M. Loewy, Titular Astronomer to the National Observatory, to a seat at the Bureau de Longitudes, vacant by the death of M. Laugier.

ONE of the biennial posts for practical work at the Laboratory of Cryptogamic Botany at Pavia is now open. There is attached to it a honorarium of 700 francs.

LORD NORTHBROOK has shown his appreciation of the value of scientific research by offering a gold medal for competition by the students of the Calcutta Medical College for the best essay on the exciting causes of fever, with special reference to the calamity which has for a long time devastated the Burdwan district in India, and the measures, sanitary or other, to be adopted for their remedy and prevention.

A METEOROLOGICAL Congress is being held at Leipzig from the 14th to the 16th inst. inclusive.

THE establishment of the College of Physical Science at Newcastle-on-Tyne has been followed by the formation of a similar scheme for another of our industrial centres, Birmingham, the necessary endowments being, on this occasion, given by the munificence of a single private individual, Mr. Josiah Mason, to whom Birmingham already owes so much in various ways. Being deeply convinced (Mr. Mason says, in his trust-deed) from long and varied experience in different branches of manufacture, of the necessity for and benefit of thorough systematic scientific instruction specially adapted to the practical, mechanical, and artistic requirements of the manufactures and industrial pursuits of the Midland district, and particularly of the boroughs of Birmingham and Kidderminster, he has determined to devote a portion of his remaining property to the foundation of an institution wherein such systematic scientific instruction may be given. With this object he assigns certain freehold and leasehold property situate in various parts of the town, which may be roughly estimated worth not less than 100,000*l*., to a body of trustees in trust for the purposes of the college. Out of the net income a sum not exceeding one-tenth may be set apart annually for providing scholarships, exhibitions, and other prizes, premiums, or gratuities, for the pupils, the remainder going to the general support of the college, the payment of professors, &c. Instruction is to be provided by means of classes in mathematics, physics, chemistry, the natural sciences (especially geology and mineralogy, with their application to mines and metallurgy), botany, zoology, physiology, the English, French, and German languages, mechanical drawing, and architecture. In addition to these means of instruction, the trustees may arrange for popular or unsystematic teaching by means of additional lectures or classes upon any subjects comprised in the regular curriculum. While no person is to be admitted to the benefit of the institution who is not for the time being wholly or principally dependent for a livelihood upon his own skill or labour, or upon the support of his parents, or upon some other person or persons, the poorer classes of the community are not to be considered as having any exclusive right to the benefit of the institution. An excellent site for the new college has already been secured in the immediate vicinity of the Birmingham Town-hall, and now that the list of trustees is completed, the college may be expected to assume form very shortly.

THE announcement, a year or two ago, of the purchase by an American of the celebrated Hay collection of Egyptian antiquities, at the time on exhibition at the Crystal Palace in London, created quite a sensation, in view of its intrinsic value and the desire which had been manifested to procure it for the British Museum. In the increasing rarity of objects of this kind, resulting from the great demand on the part of national museums throughout the world, it is believed quite unlikely that such a collection will again be brought together. Its richness in mummies, objects in bronze, marble, alabaster, &c., together with those of smaller size usually found in Egyptian tombs and elsewhere, is very great. While this collection does not embrace many statues or immense sarcophagi, it is believed to be equal to any in the completeness of its series of the smaller objects of religious and domestic Egyptian antiquity, and not inferior to the best collections of Paris, London, Berlin, or Leyden. It was purchased by Mr. Samuel A. Way, of Boston, and removed to that city, and offered to the Museum of Fine Arts, under certain conditions, which the directors did not think best to accept. At the death of Mr. Way, however, it passed into the possession of Mr. Charles Granville Way, himself an artist of merit, who has in turn offered it to the same establishment without condition other than it is to be kept in a room by itself, and to be called the Way Collection. This stipulation, we learn from *Harper's Weekly*, was gladly agreed to, and the collection accepted by the trustees, and its treasures will doubtless before long be opened to the public.

Harper's Weekly states that among some collections of specimens of natural history and ethnology lately presented by Governor W. M. F. Arny, of New Mexico, to the Smithsonian Institution, were some mastodon remains, which were submitted by Prof. Henry to Prof. Leidy for examination. These were found to indicate the existence of a very remarkable species of mastodon (*M. obscurus*), very different from the common *M. americanus*, and previously known only by a few fragments from California and a tooth found many years ago in the Miocene formation of Maryland. One peculiarity of this species consists in the existence of enamel on the tusks of the upper jaw, which does not occur in the more modern *M. americanus*. It also had tusks in the lower jaw, projecting from the prolongation of the jaw, as in the adult of the Miocene *Mastodon angustidens* of Europe, and known only in the young animal of *M. americanus*. The specimen referred to will be figured by Prof. Leidy in his forthcoming report to Dr. Hayden on the vertebrate fossils of the Western Territories.

THE second part of "Mycological Illustrations of New and Rare Fungi," edited by W. W. Saunders, F.R.S., with illustrations by Mr. Worthington Smith, will be ready in a few days. Although more than a twelvemonth has elapsed since the publication of the first part, it is hoped that in future the parts will be issued more regularly at about quarterly intervals.

WE have just received the "Monthly Record of Results of Observations in Meteorology, Terrestrial Magnetism, &c., taken at the Melbourne Observatory during March, 1872; together with Abstracts from Meteorological Observations obtained at various localities in Victoria, under the superintendence of Robert L. J. Ellery, Government Astronomer." Prefixed is a useful table of the averages and extremes of different meteorological elements at Melbourne and other localities for a number of years; and the tables which follow, showing the daily registrations for the month of March, seem sufficiently minute in detail and drawn up with great care.

THE BRITISH ASSOCIATION MEETING AT BRIGHTON

FROM Edinburgh to Brighton is a great leap, and the change is not merely one of clime and latitude. Two towns could hardly be found presenting a greater contrast. We exchange an ancient seat of learning for a modern watering place, the narrow streets and lofty houses of the Old Town for the palatial dwellings of the Steyne, Arthur's Seat for the New Chain Pier, the memory of Scott for that of the Prince Regent. So far the migration has little to recommend it; but then we have the set-off of being within easy reach of London; the British Association has, in fact, never before held its meetings so near the capital, and the present may be looked on as an experimental trial of a metropolitan meeting.

The list of officers of the meeting, and of the sections, has already been announced. The following is the diary of proceedings for each day:—

Wednesday, August 14—General Committee in the Town Hall, at 1 P.M.; Committees of Sections, at 2 P.M.; Inaugural Address by the President, in the Dome, at 8 P.M. Thursday, August 15—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; *soirée* in the Dome, Corn Exchange and Museum, at 8 P.M. Friday, August 16—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; discourse in the Dome by Prof. P. Martin Duncan, F.R.S., on the Metamorphoses of Insects, at half-past 8 P.M. Saturday, August 17—Committees of Sections, at 10 A.M.; Sections, at 11 A.M. Excursions: lecture to working men by Wm. Spottiswoode, LL.D., F.R.S., on Sunshine, Sea, and Sky, in the Dome, at 8 P.M. Monday, August 19—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; General Committee in the Town Hall, at 3 P.M.; discourse in the Dome by Prof. W. K. Clifford, on the Aims and Instruments of Scientific Thought, at half-past 8 P.M. Tuesday, August 20—Committees of Sections, at 10 A.M.; Sections, at 11 A.M.; *soirée* in the Dome, Corn Exchange and Museum, at 8 P.M. Wednesday, August 21—General Committee in the Town Hall, at 1 P.M.; concluding General Meeting in the Dome, at half-past 2 P.M. Thursday, August 22—Excursions.

The reception room will be in the New Museum and Library, Pavilion, and the following will be the rooms for meetings of Sections:—A, Mathematical and Physical Science, Albion Room; B, Chemical Science, Lecture Room, New Museum; C, Geology, Town Hall; D, Biology, Pavilion; E, Geography, Concert Hall, Middle Street; F, Economic Science and Statistics, Old Ship Assembly Rooms; G, Mechanical Science, Friends' Meeting House, Ship Street.

An Exhibition of objects of interest and works of art will be on view, during the meeting, in the Corn Exchange, New Museum and Library, and the following Excursions have been arranged:—Saturday afternoon, August 17—(1) To Glynde Station, for Glynde Place, the Chalk Pits, and Mount Caburn; to Glynde Station, for Firle Place and Beacon; (2) to Lewes, Southover, and Mount Harry, returning to Stanmer Park; (3) to Worthing, thence to Cissbury, for the excavations by Captain Oliver, and then to Findon; (4) to Bramber, then to Steyning and Wiston. Thursday, August 22—(1) To Pevensey, Hastings, and Battle Abbey, thence to the Sub-Wealden Boring, or to Norman Hurst (residence of Mr. Thomas Brassey, M.P.); (2) to Arundel, Amberley, and Parham; (3) to Chichester and Goodwood; (4) to Portsmouth, for steamer through the Solent for Alum Bay, the Needles and Freshwater Bay, Isle of Wight; to Portsmouth, for the Dockyard, Shipping, &c.; (5) to Hayward's Heath, for Paxhill, Wakehurst Place, West Hoathly Rocks, and Whiteman's Green Quarry, Cuckfield.

The Marine Aquarium, of which we shall take an opportunity of giving an account when it is in a more complete state, will of course be one of the chief objects of attraction to all the members of the Association, who will be admitted free, but "only a limited number daily." The great number of Lady Associates already announced is a prominent feature of the present meeting.

The following distinguished foreigners have already announced their intention of being present, viz. :—

Prof. Hébert, President of the Geological Society of France; Prof. van Benéden, of Louvain, and his son, a naturalist of great ability; Prof. Janssen, of Paris; Prof. Panceri, of Naples; Prof. H. A. Nicholson, of Toronto; Prof. Zengler, of Prague; Prof. Hale, of Albany, U.S.; while invitations have been sent to the following, who have been compelled reluctantly to decline the invitation :—

Prof. Hofmeister, Prof. Sir W. G. Logan, of Montreal; Prof. Clebsch, of Göttingen; Prof. Daubrée, of Paris; Prof. Young, of Dartmouth College, U.S.; Prof. Asa Gray, of Cambridge, U.S.; Prof. Gibbs, of Cambridge, U.S.; Principal Dawson, of Montreal; M. Quatrefages, of Paris; Prof. Kirchhoff, of Heidelberg; Prof. Helmholtz, of Berlin; Prof. Shaler, of Harvard College, U.S.

The customary courtesy of the officers of the Association has enabled us to give our readers this week the President's Address, as well as the opening addresses in Sections A, B, C, and D.

INAUGURAL ADDRESS OF DR. WILLIAM CARPENTER, F.R.S., PRESIDENT

THIRTY-SIX years have now elapsed since at the first and (I regret to say) the only meeting of this Association held in Bristol—which Ancient City followed immediately upon our National Universities in giving it a welcome—I enjoyed the privilege which I hold it one of the most valuable functions of these Annual assemblages to bestow; that of coming into personal relation with those distinguished Men whose names are to every cultivator of Science as "household words," and the light of whose brilliant example, and the warmth of whose cordial encouragement are the most precious influences by which his own aspirations can be fostered and directed. Under the Presidency of the Marquis of Lansdowne, with Conybeare and Prichard as Vice-Presidents, with Vernon Harcourt as General Secretary, and John Phillips as Assistant Secretary, were gathered together Whewell and Peacock, James Forbes and Sir W. Rowan Hamilton, Murchison and Sedgwick, Buckland and De la Beche, Henslow and Daubeny, Roget, Richardson, and Edward Forbes, with many others, perhaps not less distinguished, of whom my own recollection is less vivid.

In his honoured old age, Sedgwick still retains, in the Academic home of his life, all his pristine interest in whatever bears on the advance of the Science he has adorned as well as enriched; and Phillips still cultivates with all his old enthusiasm the congenial soil to which he has been transplanted. But the rest—our fathers and elder brother,—"Where are they?" It is for us of the present generation to show that they live in our lives; to carry forward the work which they commenced; and to transmit the influence of their own example to our own successors.

There is one of these great men, whose departure from among us since last we met claims a special notice, and whose life—full as it was of years and honours—we should have all desired to see prolonged for a few months, could its feebleness have been unattended with suffering. For we should all then have sympathised with Murchison, in the delight with which he would have received the intelligence of the safety of the friend in whose scientific labours and personal welfare he felt to the last the keenest interest. That this intelligence, which our own Expedition for the relief of Livingstone would have obtained (we will hope) a few months later, should have been brought to us through the generosity of one, and the enterprising ability—may I not use our peculiarly English word, the "pluck"—of another of our American brethren, cannot but be a matter of national regret to us. But let us bury that regret in the common joy which both Nations feel in the result; and while we give a cordial welcome to Mr. Stanley, let us glory in the prospect now opening, that England and America will co-operate in that noble object which —far more than the discovery of the Sources of the Nile—our great Traveller has set before himself as his true mission, the Extinction of the Slave Trade.

At the last Meeting of this Association I had the pleasure of being able to announce that I had received from the First Lord of the Admiralty a favourable reply to a representation I had ventured to make to him, as to the importance of prosecuting on a more extended scale the course of inquiry into the Physical and Biological conditions of the Deep Sea, on which, with my colleagues Prof. Wyville Thomson and Mr. J. Gwyn Jeffreys, I had been engaged for the three preceding years. That for which I had asked was a Circumnavigating Expedition of at least three years' duration, provided with an adequate Scientific Staff, and with the most complete Equipment that our experience could devise. The Council of the Royal Society having been led by the encouraging tenor of the answer I had received, to make a formal application to this effect, the liberal arrangements of the Government have been carried out under the advice of a Scientific Committee which included Representatives of this Association. H.M. ship *Challenger*, a vessel in every way suitable for the purpose, is now being fitted out at Sheerness; the command of the Expedition is intrusted to Captain Nares, an Officer of whose high qualifications I have myself the fullest assurance; while the Scientific charge of it will be taken by my excellent friend Prof. Wyville Thomson, at whose suggestion it was that these investigations were originally commenced, and whose zeal for the efficient prosecution of them is shown by his relinquishment for a time of the important Academic position he at present fills. It is anticipated that the Expedition will sail in November next; and I feel sure that the good wishes of all of you will go along with it.

The confident anticipation expressed by my predecessor, that for the utilisation of the total Eclipse of the Sun then impending, our Government would "exercise the same wise liberality as heretofore in the interests of Science," has been amply fulfilled. An Eclipse-Expedition to India was organised at the charge of the Home Government, and placed under the direction of Mr. Lockyer; the Indian Government contributed its quota to the work; and a most valuable body of results was obtained, of which, with those of the previous year, a Report is now being prepared under the direction of the Council of the Astronomical Society.

It has been customary with successive occupants of this Chair, distinguished as Leaders in their several divisions of the noble Army of Science, to open the proceedings of the Meetings over which they respectively presided, with a Discourse on some aspect of Nature in Relation to Man. But I am not aware that any one of them has taken up the other side of the inquiry—that which concerns Man as the "Interpreter of Nature;" and I have therefore thought it not inappropriate to lead you to the consideration of the Mental processes, by which are formed those fundamental conceptions of Matter and Force, of Cause and Effect, of Law and Order, which furnish the basis of all scientific reasoning, and constitute the *Philosophia prima* of Bacon. There is a great deal of what I cannot but regard as fallacious and misleading Philosophy—"oppositions of Science falsely so called" —abroad in the world at the present time. And I hope to satisfy you, that those who set up *their own conceptions* of the Orderly Sequence which they discern in the Phenomena of Nature, as fixed and determinate *Laws*, by which those phenomena not only *are* within all Human experience, but always *have been*, and always *must be*, invariably governed, are really guilty of the Intellectual arrogance they condemn in the Systems of the Ancients, and place themselves in diametrical antagonism to those real Philosophers, by whose comprehensive grasp and penetrating insight that Order has been so far disclosed. For what love of the Truth, as it is in Nature, was ever more conspicuous than that which Kepler displayed in his abandonment of each of the ingenious conceptions of the Planetary System which his fertile Imagination had successively devised, so soon as it proved to be inconsistent with the facts disclosed by observation? In that almost admiring description of the way in which his enemy Mars, "whom he had left at home as a despised Captive," had "burst all the chains of the equations, and broke forth from the prisons of the tables," who does not recognise the justice of Schiller's definition of the real Philosopher, as one who always loves Truth better than his System? And when at last he had gained the full assurance of a success so complete that (as he says) he thought he must be dreaming, or that he had been reasoning in a circle, who does not feel the almost sublimity of the self-abnegation, with which, after attaining what was in his own estimation such a glorious reward of his life of toil, disappointment, and self-sacrifice he abstains from claiming the applause of his contemporaries, but leaves his fame to after ages in these noble words :—"The book is written; to be read either now or by posterity, I care not which. It may well wait a century for a reader, as God has waited six thousand years for an observer."

And when a yet greater than Kepler was bringing to its final issue that grandest of all Scientific Conceptions, long pondered over by his almost superhuman intellect—which linked together the Heavens and the Earth, the Planets and the Sun, the Primaries and their Satellites, and included even the vagrant Comets, in the *nexus* of a Universal Attraction—establishing for all time the truth for whose utterance Galileo had been condemned, and giving to Kepler's Laws a significance of which their author had never dreamed—what was the meaning of that agitation which prevented the Philosopher from completing his computation, and compelled him to hand it over to his friend? That it was not the thought of his own greatness, but the glimpse of the grand Universal Order thus revealed to his mental vision, which shook the serene and massive soul of Newton to its foundations, we have the proof in that beautiful comparison in which he likened himself to a Child picking up shells on the shore of the vast Ocean of Truth—a comparison which will be evidence to all time at once of his true Philosophy with his profound Humility.

Though it is with the Intellectual Representation of Nature which we call *Science*, that we are primarily concerned, it will not be without its use to cast a glance in the first instance at the other two principal characters under which Man acts as her Interpreter—those, namely, of the Artist and of the Poet.

The Artist serves as the Interpeter of Nature, not when he works as the mere copyist, delineating that which he sees with his bodily eyes, and which we could see as well as ourselves; but when he endeavours to awaken within us the perception of those beauties and harmonies which his own trained sense has recognised, and thus impart to us the pleasure he has himself derived from their contemplation. As no two Artists agree in the original constitutions and acquired habits of their Minds, all look at Nature with different (mental) eyes; so that to each, *Nature is what he individually sees in her.*

The Poet, again, serves as the Interpreter of Nature, not so much when by skilful word-painting (whether in prose or verse) he calls up before our mental vision the picture of some actual or ideal scene, however beautiful; as when, by rendering into appropriate forms those deeper impressions made by the Nature around him on the Moral and Emotional part of his own Nature, he transfers these impressions to the corresponding part of ours. For it is the attribute of the true Poet to penetrate the secret of those mysterious influences which we all unknowingly experience; and having discovered this to himself, to bring others, by the power he thus wields, into the like sympathetic relation with Nature—evoking with skilful touch the varied response of the Soul's finest chords, heightening its joys, assuaging its griefs, and elevating its aspirations. Whilst, then, the Artist aims to picture what he *sees* in Nature, it is the object of the Poet to represent what he *feels* in Nature; and to each true Poet, *Nature is what he individually finds in her.*

The Philosopher's interpretation of Nature *seems* less individual than that of the Artist or the Poet, because it is based on facts which any one may verify, and is elaborated by reasoning processes of which all admit the validity. He looks at the Universe as a vast Book lying open before him, of which he has in the first place to learn the characters, then to master the language, and finally to apprehend the ideas which that language conveys. In that Book there are many Chapters, treating of different subjects; and as Life is too short for any one man to grasp the whole, the Scientific interpretation of this Book comes to be the work of many Intellects, differing not merely in the range but also in the character of their powers. But whilst there are "diversities of gifts," there is "the same spirit." While each takes his special direction, the general Method of study is the same for all. And it is a testimony alike to the truth of that Method and to the Unity of Nature, that there is an ever-increasing tendency towards agreement among those who use it aright—temporary differences of interpretation being removed, sometimes by a more complete mastery of her language, sometimes by a better apprehension of her ideas—and lines of pursuit which had seemed entirely distinct or even widely divergent, being found to lead at last to one common goal. And it is this agreement which gives rise to the general belief—in many, to the confident assurance—that the Scientific interpretation of Nature represents her not merely as she *seems*, but as she *really is.*

When, however, we carefully examine the foundation of that assurance, we find reason to distrust its security; for it can be shown to be no less true of the Scientific conception of Nature, than it is of the Artistic or the Poetic, that it is *a representation framed by the Mind itself* out of the materials supplied by the impressions which external objects make upon the Senses; so that to each Man of Science, *Nature is what he individually believes her to be.* And that belief will rest on very different bases, and will have very unequal values, in different departments of Science. Thus, in what are commonly known as the "exact" Sciences, of which Astronomy may be taken as the type, the data afforded by precise methods of observation can be made the basis of reasoning, in every step of which the Mathematician feels the fullest assurance of certainty; and the final deduction is justified either by its conformity to known or ascertainable facts —as when Kepler determined the elliptic orbit of Mars; or by the fulfilment of the predictions it has sanctioned—as in the occurrence of an Eclipse or an Occultation at the precise moment specified many years previously; or, still more emphatically, by the actual discovery of phenomena till then unrecognised—as when the Perturbations of the planets, shown by Newton to be the necessary results of their mutual attraction, were proved by observation to have a real existence; or as when the unknown disturber of Uranus was found in the place assigned to him by the computations of Adams and Le Verrier.

We are accustomed, and I think most rightly, to speak of these achievements as triumphs of the Human Intellect. But the very phase implies that the work is done by Mental Agency; and the coincidence of its results with the facts of observation is far from proving the Intellectual process to have been correct. For we learn from the honest confession of Kepler, that he was led to the discovery of the Elliptic orbit of Mars by a series of happy accidents, which turned his erroneous guesses into the right direction; and to that of the passage of the Radius Vector over *equal areas in equal times*, by the notion of a whirling force emanating from the Sun, which we now regard as an entirely wrong conception of the cause of orbital revolution.* It should always be remembered, moreover, that the Ptolemaic system of Astronomy, with all its cumbrous ideal mechanism of "Centric and Excentric, Cycle and Epicycle, Orb in Orb," did intellectually represent all that the Astronomer, prior to the invention of the Telescope, could see from his actual standpoint, the Earth, with an accuracy which was proved by the fulfilment of his anticipations. And in that last and most memorable prediction which has given an imperishable fame to our two illustrious contemporaries, the inadequacy of the basis afforded by actual observation of the perturbations of Uranus required that it should be supplemented by an assumption of the probable distance of the disturbing Planet beyond, which has been shown by subsequent observation to have been only an approximation to the truth.

Even in this most exact of Sciences, therefore, we cannot proceed a step without translating the actual Phenomena of Nature into Intellectual Representations of those phenomena; and it is because the Newtonian conception is not only the most simple, but is also, up to the extent of our present knowledge, *universal* in its conformity to the facts of observation, that we accept it as the only Scheme of the Universe yet promulgated, which satisfies our Intellectual requirements.

When, under the reign of the Ptolemaic System, any new inequality was discovered in the motion of a Planet, a new wheel had to be added to the ideal Mechanism—as Ptolemy said, "to save appearances." If it should prove, a century hence, that the motion of Neptune himself is disturbed by some other attraction than that exerted by the interior Planets, we should confidently expect that not an *ideal* but a *real* cause for that disturbance will be found in the existence of another Planet beyond. But I trust that I have now made it evident to you, that this confident expectation is not justified by any absolute necessity of Nature, but arises entirely out of *our belief* in her Uniformity; and into the grounds of this and other Primary Beliefs, which serve as the foundation of all Scientific reasoning, we shall presently inquire.

There is another class of cases, in which an equal certainty is generally claimed for conclusions that seem to flow immediately from observed facts, though really evolved by Intellectual processes; the apparent simplicity and directness of those processes either causing them to be entirely overlooked, or veiling the assumptions on which they are based. Thus Mr. Lockyer

* See Drinkwater's "Life of Kepler," in the Library of Useful Knowledge, pp. 26 35.

speaks as confidently of the Sun's Chromosphere of incandescent Hydrogen, and of the local outbursts which cause it to send forth projections tens of thousands of miles high, as if he had been able to capture a flask of this gas, and had generated water by causing it to unite with oxygen. Yet this confidence is entirely based on the assumption that a certain line which is seen in the Spectrum of a hydrogen flame *means* hydrogen also when seen in the spectrum of the Sun's chromosphere; and high as is the probability of that assumption, it cannot be regarded as a demonstrative certainty, since it is by no means inconceivable that the same line *might* be produced by *some other* substance at present unknown. And so when Dr. Huggins deduces from the different relative positions of certain lines in the spectra of different Stars, that these Stars are moving from or towards us in space, his admirable train of reasoning is based on the assumption that these lines have *the same meaning*—that is, that they *represent the same elements*—in every luminary. That assumption, like the preceding, may be regarded as possessing a sufficiently high probability to justify the reasoning based upon it; more especially since, by the other researches of that excellent observer, the same Chemical elements have been detected as vapours in those filmy cloudlets which seem to be stars in an early stage of consolidation. But when Frankland and Lockyer, seeing in the spectrum of the yellow Solar prominences a certain bright line not identifiable with that of any known Terrestrial flame, attribute this to a hypothetical new substance which they propose to call Helium, it is obvious that their assumption rests on a far less secure foundation; until it shall have received that verification, which, in the case of Mr. Crookes's researches on Thallium, was afforded by the actual discovery of the new metal, whose presence had been indicated by him by a line in the Spectrum not attributable to any substance then known.

In a large number of other cases, moreover, our Scientific interpretations are clearly matters of *judgment*; and this is eminently a *personal act*, the value of its results depending in each case upon the qualifications of the *individual* for arriving at a correct decision. The surest of such judgments are those dictated by what we term "Common Sense," as to matters on which there seems no room for difference of opinion, because every sane person comes to the same conclusion, although he may be able to give no other reason for it than that it appears to him "self-evident." Thus while Philosophers have raised a thick cloud of dust in the discussion of the basis of our belief in the existence of the world external to ourselves—of the Non Ego, as distinct from the Ego—and while every Logician claims to have found some flaw in the proof advanced by every other—the Common Sense of Mankind has arrived at a decision that is practically worth all the arguments of all the Philosophers who have fought again and again over this battle-ground. And I think it can be shown that the trustworthiness of this Common Sense decision arises from its dependence, not on any one set of Experiments, but upon *our unconscious co-ordination of the whole aggregate of our Experiences*—not on the conclusiveness of any one train of Reasoning, but on *the convergence of all our lines of thought towards this one centre.*

Now this "Common Sense," disciplined and enlarged by appropriate culture, becomes one of our most valuable instruments of Scientific inquiry; affording in many instances the best, and sometimes the only, basis for a rational conclusion. Let us take as a typical case, in which no special knowledge is required, what we are accustomed to call the "flint implements" of the Abbeville and Amiens gravel-beds. No logical proof can be adduced that the peculiar shapes of these flints were given to them by Human hands; but does any unprejudiced person now doubt it? The evidence of *design*, to which, after an examination of one or two such specimens, we should only be justified in attaching a probable value, derives an irresistible cogency from accumulation. On the other hand, the *im*probability that these flints acquired their peculiar shape by *accident*, becomes to our minds greater and greater as more and more such specimens are found; until at last this hypothesis, although it cannot be directly disproved, is felt to be almost inconceivable, except by minds previously "possessed" by the "dominant idea" of the modern origin of Man. And thus what was in the first instance a matter of discussion, has now become one of those "self-evident" propositions, which claim the unhesitating assent of all whose opinion on the subject is entitled to the least weight.

We proceed upwards, however, from such questions as the Common Sense of Mankind generally is competent to decide, to those in which special knowledge is required to give value to the judgment; and thus the interpretation of Nature by the use of that faculty comes to be more and more *individual*; things being perfectly "self-evident" to men of special culture, which ordinary men, or men whose training has lain in a different direction, do not apprehend as such. Of all departments of Science, Geology seems to me to be the one that most depends on this specially-trained "Common Sense;" which brings as it were into one focus the light afforded by a great variety of studies—Physical and Chemical, Geographical and Biological; and throws it on the pages of that Great Stone Book, on which the past history of our Globe is recorded. And whilst Astronomy is of all Sciences that which may be considered as most nearly representing Nature as she really is, Geology is that which most completely represents her as seen through the medium of the interpreting mind; the meaning of the phenomena that constitute its data being in almost every instance open to question, and the judgments passed upon the same facts being often different according to the qualifications of the several judges. No one who has even a general acquaintance with the history of this department of Science, can fail to see that the Geology of each epoch has been the reflection of the Minds by which its study was then directed; and that its true progress dates from the time when that "Common Sense" method of interpretation came to be *generally* adopted, which consists in seeking the explanation of past changes in the Forces at present in operation, instead of invoking the aid of extraordinary and mysterious agencies, as the older Geologists were wont to do, whenever they wanted—like the Ptolemaic Astronomers—"to save appearances." The whole tendency of the ever-widening range of modern Geological inquiry has been to show how little reliance can be placed upon the so-called "Laws" of Stratigraphical and Palæontological Succession, and how much allowance has to be made for local conditions. So that while the Astronomer is constantly enabled to point to the fulfilment of his predictions as an evidence of the correctness of his method, the Geologist is almost entirely destitute of any such means of verification. For the value of any prediction that he may hazard—as in regard to the existence or non-existence of Coal in any given area—depends not only upon the truth of the general doctrines of Geology in regard to the succession of Stratified Deposits, but still more upon the detailed knowledge which he may have acquired of the distribution of those Deposits in the particular locality. Hence no reasonably-judging man would discredit either the general doctrines or the methods of Geology, because the prediction proves untrue in such a case as that now about to be brought in this neighbourhood to the trial of experience.

We have thus considered Man's function as the Scientific Interpreter of Nature in two departments of Natural Knowledge; one of which affords an example of the strictest, and the other of the freest method, which Man can employ in constructing his Intellectual representation of the Universe. And as it would be found that in the study of all other departments the same methods are used, either separately or in combination, we may pass at once to the other side of our inquiry, namely, the origin of those Primary Beliefs which constitute the groundwork of all Scientific reasoning.

The whole fabric of Geometry rests upon certain Axioms which every one accepts as true, but of which it is necessary that the truth should be *assumed*, because they are incapable of demonstration. So, too, the deliverances of our "Common Sense" derive their trustworthiness from what we consider the "self-evidence" of the propositions affirmed.

This inquiry brings us face to face with one of the great Philosophical problems of our day, which has been discussed by Logicians and Metaphysicians of the very highest ability as Leaders of opposing Schools, with the one result of showing how much can be said on each side. By the *Intuitionalists* it is asserted that the tendency to form these Primary Beliefs is inborn in Man, an original part of his mental organisation; so that they grow up spontaneously in his Mind as its faculties are gradually unfolded and developed, requiring no other Experience for their genesis, than that which suffices to call these faculties into exercise. But by the advocates of the doctrine which regards *Experience* as the basis of all our knowledge, it is maintained that the Primary Beliefs of each individual are nothing else than generalisations which he formed of such experiences as he has either himself acquired or has consciously learned from others; and they deny that there is any original or intuitive tendency to the formation of such beliefs, beyond that which consists in the power of retaining and generalising experiences.

I have not introduced this subject with any idea of placing before you even a summary of the ingenious arguments by which these opposing doctrines have been respectively supported; nor should I have touched on the question at all, if I did not believe that a means of reconcilement between them can be found in the idea that the *Intellectual Intuitions of any one Generation are the embodied Experiences of the previous Race.* For, as it appears to me there has been a progressive improvement in the *Thinking Power* of Man; every product of the culture which has preceded serving to prepare the soil for yet more abundant harvests in the future.

Now as there can be no doubt of the Hereditary transmission in Man of acquired constitutional peculiarities, which manifest themselves alike in tendencies to Bodily and Mental disease, so it seems equally certain that *acquired mental habitudes* often impress themselves on his organisation, with sufficient force and permanence to occasion their transmission to the offspring as *tendencies to similar modes of thought.* And thus, while all admit that *Knowledge* cannot thus descend from one generation to another, an increased *aptitude* for the acquirement, either of knowledge generally, or of some particular kind of it, may be thus inherited. These tendencies and aptitudes will acquire additional strength, expansion, and permanance, in each new generation, from their habitual exercise upon the materials supplied by a continually enlarged experience; and thus the *acquired* habitudes produced by the Intellectual culture of ages, will become "a second nature" to every one who inherits them.*

We have an illustration of this progress in the fact of continual occurrence, that conceptions which prove inadmissable to the minds of one generation, in consequence either of their want of intellectual power to apprehend them, or of their preoccupation by older habits of thought, subsequently find a universal acceptance, and even come to be approved as "self-evident." Thus the First Law of Motion, divined by the genius of Newton, though opposed by many Philosophers of his time as contrary to all experience, is now accepted by common consent, not merely as a legitimate inference from Experiment, but as the expression of a necessary and universal truth; and the same Axiomatic value is extended to the still more general doctrine, that Energy of *any* kind, whether manifested in the "molar" motion of masses, or consisting in the "molecular" motion of atoms, *must* continue under some form or other without abatement or decay; what all admit in regard to the indestructibility of Matter, being accepted as no less true of Force, namely, that as *ex nihilo nil fit*, so *nil fit ad nihilum* †

But, it may be urged, the very conception of these and similar great truths is in itself a typical example of Intuition. The men who divined and enunciated them stand out above their fellows, as possessed of a Genius which could not only combine but create, of an Insight which could clearly discern what Reason could but dimly shadow forth. Granting this freely, I think it may be shown that the Intuitions of individual Genius are but specially exalted forms of endowments which are the general property of the Race at the time, and which have come to be so in virtue of its whole previous culture. Who, for example, could refuse to the marvellous aptitude for perceiving the relations of Numbers, which displayed itself in the untutored boyhood of George Bidder and Zerah Colburn, the title of an Intuitive gift? But who, on the other hand, can believe that a Bidder or a Colburn could suddenly arise in a race of Savages who cannot count five? Or, again, in the history of the very earliest days of Mozart, who cannot fail to recognise the dawn of that glorious Genius, whose brilliant but brief career left its imperishable impress on the Art it enriched? But who would be bold enough to affirm that an infant Mozart could be born amongst a tribe whose only musical instrument is a tom-tom whose only song is a monotonous chant?

Again, by tracing the gradual *genesis* of some of those Ideas which we now accept as "self-evident"—such, for example, as that of the "Uniformity of Nature"—we are able to recognise them as the expressions of certain Intellectual tendencies, which have progressively augmented in force in successive generations, and now manifest themselves as Mental Instincts that penetrate and direct our ordinary course of Thought. Such Instincts constitute a precious heritage, which has been transmitted to us with ever-increasing value through the long succession of preceding generations; and which it is for us to transmit to those who shall come after us, with all that further increase which our higher Culture and wider range of Knowledge can impart.

And now, having studied the working action of the Human Intellect in the Scientific Interpretation of Nature, we shall examine the general character of its products; and the first of these with which we shall deal is our conception of *Matter* and of its relation to *Force.*

The Psychologist of the present day views Matter entirely through the light of his own Consciousness: his idea of Matter in the abstract being that it is a "something" which has a permanent power of exciting Sensations; his idea of any "property" of Matter being the mental representation of some kind of sensory impression he has received from it; and his idea of any particular kind of Matter being the representation of the whole aggregate of the Sense-perceptions which its presence has called up in his Mind. Thus, when I press my hand against this table, I recognise its unyieldingness through the conjoint medium of my sense of Touch, my Muscular sense, and my Mental sense of Effort, to which it will be convenient to give the general designation of the Tactile Sense; and I attribute to that table a *hardness* which resists the effort I make to press my hand into its substance, whilst I also recognise the fact that the force I have employed is not sufficient to move its mass. But I press my hand against a lump of dough; and finding that its substance yields under my pressure I call it *soft.* Or, again, I press my hand against this desk; and I find that although I do not thereby change its *form*, I change its *place*; and so I get the Tactile idea of *Motion.* Again, by the impression received through the same Sensorial apparatus, when I lift this book in my hand, I am led to attach to it the motion of *weight* or ponderosity; and by lifting different solids of about the same size, I am enabled, by the different degrees of exertion I find myself obliged to make in order to sustain them, to distinguish some of them as *light*, and others as *heavy.* Through the medium of another set of Sense-perceptions which some regard as belonging to a different category, we distinguish between bodies that *feel* "hot" and those that *feel* "cold;" and in this manner we arrive at the notion of differences of Temperature. And it is through the medium of our Tactile Sense, without any aid from Vision, that we first gain the idea of *solid form*, or the Three Dimensions of Space.

Again, by the extension of our Tactile experiences, we acquire the notion of *liquids*, as forms of matter yielding readily to pressure, but possessing a sensible weight which may equal that of solids: and of *air*, whose resisting power is much slighter, and whose weight is so small that it can only be made sensible by artificial means. Thus, then, we arrive at the notions of *resistance* and weight as properties common to all forms of Matter; and now that we have got rid of that idea of Light and Heat, Electricity and Magnetism, as "imponderable fluids," which used to vex our souls in our Scientific Childhood, and of which the popular term "Electric Fluid is a "survival," we accept these properties as affording the practical distinction between the "material" and the "immaterial."

Turning, now, to that other great portal of Sensation, the Sight, through which we receive most of the messages sent to us from the Universe around, we recognise the same truth. Thus it is agreed alike by Physicists and Physiologists, that *Colour* does not exist *as such* in the object itself; which has merely the power of reflecting or transmitting a certain number of millions of undulations in a second; and these only produce that affection of our consciousness which we call Colour, when they fall upon the retina of the living Percipient. And if there

* I am glad to be able to append the following extract from a letter which Mr. John Mill, the great Master of the Experimental School, was good enough to write to me a few months since, with reference to the attempt I had made to place "Common Sense" upon this basis (*Contemporary Review*, Feb. 1872):—"When states of mind in no respect inate or instinctive have been frequently repeated, the mind acquires, as is proved by the power of Habit, a greatly increased facility of passing into those states; and this increased facility must be owing to some change of a physical character in the organic action of the Brain. There is also considerable evidence that such acquired facilities of passing into certain modes of cerebral action can in many cases be transmitted, more or less completely, by inheritance. The limits of this power of transmission, and the conditions on which it depends, are a subject now fairly before the scientific world; and we shall doubtless in time know much more about them than we do now. But so far as my imperfect knowledge of the subject qualifies me to have an opinion, I take much the same view of it that you do, at least in principle."

† This is the form in which the doctrine now known as that of the "Conservation of Energy" was enunciated by Dr. Mayer, in the very remarkable Essay published by him in 1845, entitled "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel."

be that defect either in the retina or in the apparatus behind it, which we call "colour-blindness" or Daltonism, some particular hues cannot be distinguished, or there may even be no power of distinguishing any colour whatever. If we were all like Dalton, we should see no difference, except in form, between ripe cherries hanging on a tree, and the green leaves around them; if we were all affected with the severest form of colour-blindness, the fair face of Nature would be seen by us as in the chiaroscuro of an Engraving of one of Turner's Landscapes, not as in the glowing hues of the wondrous Picture itself. And in regard to our Visual conceptions it may be stated with perfect certainty, as the result of very numerous observations made upon persons who have acquired sight for the first time, that these do *not* serve for the recognition even of those objects with which the individual had become most familiar through the Touch, until the two sets of sense-perceptions have been co-ordinated by experience.*

When once this co-ordination has been effected, however, the composite perception of Form which we derive from the Visual sense alone is so complete, that we seldom require to fall back upon the Touch for any further information respecting the quality of the object. So again, while it is from the co-ordination of the two dissimilar pictures formed by any solid or projecting object upon our two retinæ, that (as Sir Charles Wheatstone's admirable investigations have shown) we ordinarily derive through the Sight alone a correct notion of its *solid* form, there is adequate evidence that this notion, also, is a mental *judgment* based on the experience we have acquired in early infancy by the consentaneous exercise of the Visual and Tactile senses.

Take, again, the case of those wonderful instruments by which our Visual range is extended almost into the infinity of Space, or into the infinity of Minuteness. It is the mental not the bodily eye, that takes cognizance of what the Telescope and Microscope reveal to us. For we should have no well-grounded confidence in their revelations as to the *unknown*, if we had not first acquired experience in distinguishing the true from the false by applying them to *known* objects; and every interpretation of what we see through their instrumentality is a *mental judgment* as to the probable form, size, and movement of bodies removed by either their distance or their minuteness from being cognosced by our sense of Touch.

The case is still stronger in regard to that last addition to our Scientific *armamentum*, which promises to be not inferior in value either to the Telescope or the Microscope; for it may be truly said of the Spectroscope, that it has not merely extended the range of our Vision, but has almost given us a new sense, by enabling us to recognise distinctive properties in the Chemical Elements which were previously quite unknown. And who shall now say that we know all that is to be known as to any form of Matter; or that the science of the *fourth* quarter of this century may not furnish us with as great an enlargement of our knowledge of its Properties, and of our power of recognising them, as that of its *third* has done?

But, it may be said, is not this view of the Material Universe open to the imputation that it is "evolved out of the depths of our own consciousness"—a projection of our own Intellect into what surrounds us—an *ideal* rather than a *real* World? If all we know of Matter be an "Intellectual Conception," how are we to distinguish this from such as we form in our Dreams?—for these, as our Laureate no less happily than philosophically expresses it, are "true while they last." Here our "Common Sense" comes to the rescue. We "awake, and behold it was a dream." Every healthy mind is conscious of the difference between his waking and his dreaming experiences; or, if he is now and then puzzled to answer the question, "Did this really happen, or did I dream it?" the perplexity arises from the consciousness that it *might* have happened. And every healthy mind, finding its own experiences of its waking state not only self-consistent, but consistent with the experiences of others, accepts them as the basis of his beliefs, in preference to even the most vivid recollections of his dreams.

The Lunatic Pauper who regards himself as a King, the Asylum in which he is confined as a Palace of regal splendour, and his Keepers as obsequious attendants, is so "possessed" by the conception framed by his disordered intellect, that he *does* project it out of himself into his surroundings; his refusal to admit the corrective teaching of Common Sense being the very essence of his malady. And there are not a few persons abroad in the world, who equally resist the teachings of Educated Common Sense, whenever they run counter to their own preconceptions; and who may be regarded as—in so far—affected with what I once heard Mr. Carlyle pithily characterise as a "diluted Insanity."

It has been asserted over and over again, of late years, by a class of men who claim to be the only true Interpreters of Nature, that we know nothing but Matter and the Laws of Matter, and that Force is a mere fiction of the Imagination. May it not be affirmed, on the other hand, that while our notion of *Matter* is a Conception of the Intellect, *Force* is that of which we have the *most* direct—perhaps even the *only* direct—cognizance? As I have already shown you, the knowledge of Resistance and of Weight which we gain through our Tactile Sense is derived from our own perception of *exertion*; and in Vision, as in Hearing, it is the Force with which the undulations strike the sensitive surface, that affects our consciousness with Sights or Sounds. True it is that in our Visual and Auditory Sensations, we do not, as in our Tactile, *directly* cognosce the Force which produces them; but the Physicist has no difficulty in making sensible to us indirectly the undulations by which Sound is propagated, and in proving to our Intellect that the Force concerned in the transmission of Light is really enormous.*

It seems strange that those who make the loudest appeal to Experience as the basis of all knowledge, should thus disregard the most constant, the most fundamental, the most direct of all experiences; as to which the Common Sense of Mankind affords a guiding light much clearer than any that can be seen through the dust of Philosophical discussion. For, as Sir John Herschel most truly remarked, the universal Consciousness of mankind is as much in accord in regard to the existence of a real and intimate connection between Cause and Effect, as it is in regard to the existence of an Eternal World; and that consciousness arises to every one out of his own sense of *personal* exertion in the origination of changes by his individual agency.

Now while fully accepting the Logical definition of Cause as the "antecedent or concurrence of antecedents on which the Effect is invariably and unconditionally consequent," we can always single out one *dynamical* antecedent—the Power which does the work—from the aggregate of *material conditions* under which that Power may be distributed and applied. No doubt the term Cause is very loosely employed in popular phraseology; often (as Mr. Mill has shown) to designate the occurrence that immediately preceded the effect—as when it is said that the spark which falls into a barrel of gunpowder is the cause of its explosion, or that the slipping of a man's foot off the rung of a ladder is the cause of his fall. But even a very slightly trained Intelligence can distinguish the Power which acts in each case, from the Conditions under which it acts. The Force which produces the explosion is locked up (as it were) in the powder; and ignition merely liberates it, by bringing about new Chemical combinations. The fall of the man from the ladder is due to the Gravity which was equally pulling him down while he rested on it; and the loss of support, either by the slipping of his foot, or by the breaking of the rung, is merely that change in the material conditions which gives the Power a new action.

Many of you have doubtless viewed with admiring interest that truly wonderful work of Human Design, the Walter Printing Machine. You first examine it at rest; presently comes a man who simply pulls a handle towards him; and the whole inert mechanism becomes instinct with life—the blank paper continuously rolling off the cylinder at one end, being delivered at the other, without any intermediate human agency, as large sheets of print, at the rate of 15,000 in an hour. Now what is the *Cause* of this most marvellous effect? Surely it lies essentially in the Power of Force which the pulling of the handle brought to bear on the machine from some extraneous source of Power, which we in this instance know to be a Steam-engine on the other side of the wall. This Force it is, which, distributed through the various parts of the Mechanism, really performs the action of which each is the instrument; *they* only supply the vehicle for its transmission and application. The man comes again, pushes the handle in the opposite direction, detaches the Machine from the Steam-engine, and the whole

* Thus, in a recently recorded case in which sight was imparted by operation to a young woman who had been blind from birth, but who had nevertheless learned to work well with her needle, when the pair of scissors she had been accustomed to use was placed before her, though she described their shape, colour, and glistening metallic character, she was utterly unable to recognise them *as scissors* until she put her finger on them, when she at once named them, laughing at her own stupidity (as she called it) in not having made them out before.

* See Sir John Herschel's "Familiar Lectures on Scientific Subjects."

comes to a stand; and so it remains, like an inanimate corpse, until recalled to activity by the renewal of its Moving Power.

But, say the Reasoners who deny that Force is anything else than a fiction of the imagination, the revolving shaft of the Steam-engine is "Matter in Motion;" and when the connection is established between that shaft and the one that drives the Machine, the *Motion* is communicated from the former to the latter, and thence distributed to the several parts of the Mechanism. This account of the operation is just what an observer might give, who had looked on with entire ignorance of every thing but what his *eyes* could *see;* the moment he puts his *hand* upon any part of the machinery, and tries to stop its motion, he takes as direct cognizance, through his sense of the Effort required to resist it, of the *force* which produces that motion, as he does through his *eye* of the motion itself.

Now since it is universally admitted that our notion of the External World would be not only incomplete, but erroneous, if our Visual perceptions were not supplemented by our Tactile, so, as it seems so me, our interpretation of the Phenomena of the Universe must be very inadequate, if we do not mentally coordinate the idea of Force with that of Motion, and recognise it as the "efficient cause" of those phenomena—the "material conditions" constituting (to use the old Scholastic term) only "their formal cause." And I lay the greater stress on this point, because the Mechanical Philosophy of the present day tends more and more to express itself in terms of *Motion* rather than in terms of *Force*—to become *Kinetics* instead of *Dynamics*.

Thus from whatever side we look at this question—whether the Common Sense of Mankind, the Logical Analysis of the relation between Cause and Effect, or the Study of the working of our own Intellects in the interpretation of Nature—we seem led to the same conclusion; that the notion of Force is one of those elementary Forms of Thought with which we can no more dispense, than we can with the notion of Space or of Succession. And I shall now, in the last place, endeavour to show you that it is the substitution of the Dynamical for the mere Phenomenal idea, which gives their highest value to our conceptions of that Order of Nature, which is worshipped as itself a God by the class of Interpreters whose doctrine I call in question.

The most illustrative as well as the most illustrious example of the difference between the mere Generalisation of Phenomena and the Dynamical conception that applies to them, is furnished by the contrast between the so-called Laws of Planetary Motion discovered by the persevering ingenuity of Kepler, and the interpretation of that Motion given us by the profound insight of Newton. Kepler's three Laws were nothing more than comprehensive statements of certain groups of Phenomena determined by observation. The *first*, that of the revolution of the Planets in Elliptical orbits, was based on the study of the observed places of Mars alone; it might or might not be true of the other Planets; for so far as Kepler knew, there was no reason why the orbits of some of them might not be the excentric circles which he had first supposed that of Mars to be. So Kepler's *second* law of the passage of the Radius Vector over equal areas in equal times, so long as it was simply a generalisation of facts in the case of that one Planet, carried with it no *reason* for its applicability to other cases, except that which it might derive from his erroneous conception of a whirling force. And his *third* law was in a like manner simply an expression of certain Harmonic relation which he had discovered between the times and the distances of the Planets, having no more *rational* value than any other of his numerous hypotheses.

Now the Newtonian "Laws" are often spoken of as if they were merely *higher generalisations* in which Kepler's are included; to me they seem to possess an altogether different character. For starting with the conception of two *Forces*, one of them tending to produce continuous uniform motion in a straight line, the other tending to produce a uniformly accelerated motion towards a fixed point, Newton's wonderful mastery of Geometrical reasoning enabled him to show that, if these *Dynamical* assumptions be granted, Kepler's *phenomenal* "Laws," being necessary consequences of them, must be *universally* true. And while that demonstration would have been alone sufficient to give him an imperishable renown, it was his still greater glory to divine that the fall of the Moon towards the Earth—that is, the deflection of her path from a tangential line to an ellipse—*is a phenomenon of the same order* as the fall of a stone to the ground; and thus to show the applicability to the entire Universe, of those simple Dynamical conceptions which constitute the basis of the Geometry of the Principia.

Thus, then, whilst no "Law" which is simply *a generalisation of Phenomena* can be considered as having any *coercive* action, we may assign that value to Laws which express *the universal conditions of the action of a Force*, the existence of which we learn from the testimony of our own consciousness. The assurance we feel that the Attraction of Gravitation *must* act under all circumstances according to its one simple Law, is of a very different order from that which we have in regard (for example) to the Laws of Chemical Attraction, which are as yet only generalisations of phenomena. And yet even in that strong assurance, we are required by our examination of the basis on which it rests, to admit a *reserve* of the *possibility* of something different; a reserve which we may well believe that Newton himself must have entertained.

A most valuable lesson as to the allowance we ought always to make for the unknown "possibilities of Nature," is taught us by an exceptional phenomenon so familiar that it does not attract the notice it has a right to claim. Next to the Law of the Universal Attraction of Masses of Matter, there is none that has a wider range than that of the *Expansion of Bodies by Heat.* Excluding Water and one or two other substances, the fact of such expansion might be said to be *invariable*; and, as regards bodies whose Gaseous condition is known, the Law of Expansion can be stated in a form no less simple and definite than the Law of Gravitation. Supposing those exceptions, then, to be unknown, the Law would be *universal* in its range But it comes to be discovered that Water, whilst conforming to it in its expansion from $39\frac{1}{2}°$ *upwards* to its boiling-point, as also, when it passes into Steam, to the special law of Expansion of Vapours, is exceptional in its *expansion* also from $39\frac{1}{2}°$ *downwards* to its Freezing-point; and of this failure in the Universality of the Law, no rationale can be given. Still more strange is it, that by dissolving a little *salt* in water, we should remove this exceptional peculiarity; for *sea*-water continues to contract from $39\frac{1}{2}°$ downwards to its Freezing-point 12° or 14° lower, just as it does with reduction of temperature at higher ranges.

Thus from our study of the mode in which we arrive at those conceptions of the Orderly Sequence observable in the Phenomena of Nature which we call "Laws," we are led to the conclusion that they are Human conceptions, subject to Human fallibility; and that they *may* or *may not* express the Ideas of the Great Author of Nature. To set up these Laws as self-acting, and as either excluding or rendering unnecessary the Power which alone can give them effect, appears to me as arrogant as it is unphilosophical. To speak of *any* Law as "regulating" or "governing" phenomena, is only permissible on the assumption that the Law is the expression of the *modus operandi* of a Governing Power. I was once in a great City which for two days was in the hands of a lawless mob. Magisterial authority was suspended by timidity and doubt; the force at its command was paralysed by want of resolute direction. The "Laws" were on the Statute book, but there was no Power to enforce them. And so the Powers of evil did their terrible work; and fire and rapine continued to destroy life and property without check, until new Power came in, when the Reign of Law was restored.

And thus we are led to the culminating point of Man's Intellectual Interpretation of Nature—his recognition of the Unity of the Power, of which her Phenomena are the diversified manifestations. Towards this point all Scientific inquiry now tends. The Convertibility of the Physical Forces, the Correlation of these with the Vital, and the intimacy of that *nexus* between Mental and Bodily activity, which, explain it as we may, cannot be denied, all lead upward towards one and the same conclusion; and the pyramid of which that Philosophical conclusion is the apex, has its foundation in the Primitive Instincts of Humanity.

By our own remote Progenitors, as by the untutored Savage of the present day, every change in which Human agency was not apparent was referred to a particular Animating Intelligence. And thus they attributed not only the movement of the Heavenly bodies, but all the phenomena of Nature, each to its own Deity. These Deities were invested with more than Human power; but they were also supposed capable of Human passions, and subject to Human capriciousness. As the Uniformities of Nature came to be more distinctly recognised, some of these Deities were invested with a dominant control, while others were supposed to be their subordinate ministers. A serene Majesty was attributed to the greater Gods who sit above the clouds; whilst their inferiors might "come down to Earth in the likeness of Men." With the growth of the Scientific Study of Nature, the conception of its Harmony and Unity gained ever-increasing strength. And so among the most enlightened of the Greek and Roman Philosophers, we find a distinct recognition of the idea of the Unity of the Directing Mind from which the Order of Nature proceeds; for they obviously believed that, as our modern Poet has expressed it—

All are but parts of one stupendous whole,
Whose body Nature is, and God the Soul.

The Science of Modern times, however, has taken a more special direction. Fixing its attention exclusively on the *Order* of Nature, it has separated itself wholly from Theology, whose function it is to seek after its *Cause*. In this, Science is fully justified, alike by the entire independence of its objects, and by the historical fact that it has been continually hampered and impeded in its search for the Truth as it is in Nature, by the restraints which Theologians have attempted to impose upon its inquiries. But when Science, passing beyond its own limits, assumes to take the place of Theology, and sets up its own conception of the *Order* of Nature as a sufficient account of its *Cause*, it is invading a province of Thought to which it has no claim, and not unreasonably provokes the hostility of those who ought to be its best friends.

For whilst the deep-seated instincts of Humanity, and the profoundest researches of Philosophy, alike point to Mind as the one and only source of Power, it is the high prerogative of Science to demonstrate the *Unity* of the Power which is operating through the limitless extent and variety of the Universe, and to trace its *Continuity* through the vast series of Ages that have been occupied in its Evolution.

SECTION A

MATHEMATICAL AND PHYSICAL SCIENCE

Opening Address by the President, Mr. Warren De La Rue, D.C.L., F.R.S.

My predecessors in this chair have addressed you on many subjects of high interest in Mathematical and Physical Science: I do not contemplate passing in review the recent discoveries in Astronomy and Physical Science, but intend to confine myself, in the main, to Astronomical Photography; and in selecting this branch of science as the subject of this introductory discourse, I think that I shall have your approval, not only because I have given special attention to that subject, but also because it is about to be applied to the determination of a fundamental element of our system, the solar parallax, by observations of the transit of Venus in 1874, and probably also in 1882.

Nothing is so lastingly injurious to the progress of science as false data, for they endure often through many centuries. False views, even if supported by some amount of evidence, do comparatively little harm, for every one takes a salutary interest in proving their falseness, and when this is done the path to error is closed, and the road to truth is opened at the same moment.

It will be conceded that Photography, when applied to scientific observation, undoubtedly preserves facts. But the question has sometimes been raised, Are photographic records absolutely trustworthy representations of the phenomena recorded? If not, what is the extent of truth, and where are the inlets for errors and mistakes? Not only has photographic observation gained a wide range of applications in astronomy, but in every other branch of physical science its help is daily more and more taken advantage of; and although, in speaking of this art, I shall confine myself to astronomy, the observations which I propose to make may be suggestive with reference to other branches of physics.

As an instance of the application of this art to optical physics, I may in this place call attention to the very successful delineation of the solar spectrum by Mr. Lewis M. Rutherfurd, of the United States. In Mr. Rutherfurd's spectrum, obtained by the camera, many portions and lines are shown (in the ultra-violet for instance), which, while imperceptible to the retina of the eye, impress themselves on the sensitive film. As a fact, lines which are single in Angström's and Kirchhoff's maps have been recorded by photography as well-marked double lines. I will now review the application of the art to astronomy.

Stellar photography was for some time applied at Harvard College Observatory, U.S., to double stars, for the purpose of determining by micrometric measurement their relative angle of position and distance. The zero of the angle of position was found by moving the telescope in right ascension after an impression had been taken, and taking a second one on the same plate; this process gave two sets of photographic images on the same plate, and the right line passing through the series gave the direction of the daily motion of the heavens. The probable error of a single measurement of the photographic distance of the images was found to be $\pm 0''\cdot 12$, or somewhat smaller than that of a direct measurement with the common filar micrometer. The late Prof. Bond, who applied photography to stellar astronomy, confining himself to stars brighter than the seventh magnitude, discussed the results in various numbers of the "Astronomische Nachrichten." No astronomer more unbiassed could have been selected to decide on the comparative value of the photographic and direct observational method. His discussion shows that the probable error of the centre of an image was $\pm 0''\cdot 051$, and that of the distance of two such centres was $\pm 0''\cdot 072$. Adopting the estimate of Struve, $\pm 0''\cdot 217$, as the probable error of a single measurement of a double star of this class with a filar micrometer, Prof. Bond shows that the measurement of the photographic images would have a relative value three times as great. He derived the further important conclusion, that deficiency of light can be more than compensated for by proportionate increase in the time of exposure. A star of the ninth magnitude would give a photographic image, after an exposure of ten minutes, with the Cambridge equatorial.

In the reproduction of stars by photography, recently undertaken by Mr. Rutherfurd, the objects to be secured being so minute, special precautions were found to be necessary in depicting them upon the sensitive film, so that their impressions might be distinguishable from accidental specks in the collodion plate. To prevent any such chance of mistake, Mr. Rutherfurd secures a double image of each luminary, the motion of the telescope being stopped for a short time (half a minute) between a first and second exposure of the plate; so that each star is represented by two close specks, so to speak, upon the negative, and is clearly to be distinguished by this contrivance from any accidental speck in the film. A map of the heavens is thus secured, very clear though delicate in its nature, but yet one upon which implicit reliance can be placed for the purposes of measurement. Prof. Peirce aptly says, "This addition to astronomical research is unsurpassed by any step of the kind that has ever been taken. The photographs afford just as good an opportunity for new and original investigation of the relative position of near stars as could be derived from the stars themselves as seen through the most powerful telescopes. They are indisputable facts, unbiassed by personal defects of observation, and which convey to all future times the actual places of the stars when the photographs were taken."

Mr. Asaph Hall, who shared with Prof. Bond the work of measuring the photographic images, and of reducing the measurements, has very recently subjected the photographic method to a critical comparison, with a view to deciding on its value when applied to the observation of the transit of Venus. He appears, as regards its application to stellar observations, to under-estimate the photographic method in consequence of want of rapidity; but he admits that in the case of a solar eclipse, or of the transit of a planet over the sun's disc, it has very great advantages, especially over eye-observations of contacts, inner and outer, of the planet and the sun's limb, and that the errors to which it is subject are worthy of the most thorough investigation. The observation of a contact is uncertain on account of irradiation, and is also only momentary: so that, if missed from any cause, the record of the event is irretrievably lost at a particular station, and long and costly preparations rendered futile. On the other hand, when the sky is clear, a photographic image can be obtained in an instant, and repeated throughout the progress of the transit, and even if all the contacts be lost, equally valuable results will be secured, if the data collected on the photographic plates can be correctly reduced, as will be proved hereafter to be undoubtedly possible. That the transit of Venus will be recorded by photography may now be announced as certain, as preparations are energetically progressing in England, France, Russia, and America for obtaining photographic records. There is also a possibility of Portugal taking part in these observations; for it is contemplated by Señor Capello to transport the Lisbon photoheliograph to Macao. There are at present five photoheliographs in process of construction for the observing parties to be sent out by the British Government, under the direction of the Astronomer Royal, Sir George B. Airy. The Russian Government will supply their own parties with three similar instruments; and I am also having constructed one of my own for this purpose and for future solar observations. All these instruments, made precisely alike, will embody the results of our experience gained during the last ten years in photoheliography at the Kew Observatory whilst belonging to this Association. One only of them, namely, the photoheliograph which has been at work for some years at Wilna, is of somewhat older pattern; but how great an advance even this instrument is on the original at Kew is proved by the delightful definition of the most delicate markings of the sun in the pictures which have reached this country from Wilna.

Hitherto sun-pictures have been taken on wet collodion; but a question has been raised whether it would not be better to use dry plates. On this point M. Struve informs us that, in two places—at Wilna, under the direction of Colonel Smysloff, and at Bothkamp, in Holstein, under Dr. Vogel—they have perfectly succeeded in taking instantaneous photographs of the sun with dry plates.

As far, however, as my own experience has gone, I still believe that the wet collodion is preferable to the dry for such observations.

Now, with reference to contact observations—which, it must be remembered, are by no means indispensable as far as photography is concerned—it may be conceded that there will attach to the record of the internal contact a certain amount of uncertainty, although not so great as that which affects optical observation. The photography which first shows contact may possibly not be that taken when the thread of light between Venus and the sun's disc is first completed, but the first taken after it has become thick enough to be shown on the plate; and this thickness is somewhat dependent on incidental circumstances—for example, a haziness of the sky, which, although almost imperceptible, yet diminishes the actinic brilliancy of the sun, and might render the photographic image of the small extent of the limb which is concerned in the phenomenon too faint for future measurements. On the other hand, having a series of photographs of the sun with Venus on the disc, we can, with a suitable micrometer—such as I contrived for measuring the eclipse pictures of 1860, and which, since then, has been in continuous use in measuring the Kew solar photograms *—fix the position of the centre of each body with great precision. But the reduction of the measured distances of the centre to their values in arc is not without difficulty. Irradiation may possibly enlarge the diameter of the sun in photographic pictures, and it may diminish the size of the disc of a planet crossing the sun, as is the case with eye-observations; but if the images depicted are nearly of the same size at all stations whose results are to be included in any set of discussions, then the ratio of the diameters of Venus and the sun will be the same in all the plates, and it will be safe to assume that they are equally affected by irradiation. The advantage which, therefore, will result by employing no less than eight instruments precisely alike, as are those now being made by Mr. Dallmeyer on the improved Kew model, is quite obvious. If other forms of instruments, such as will hereafter be alluded to, be used, it will be essential that a sufficient number of them be employed in selected localities to give also connected sets for discussion.

To give some idea of the relative apparent magnitudes of the sun and Venus, I may mention that at the epoch of the transit of 1874 the solar disc would, in the Kew photoheliograph, have a semi-diameter of 1965·8 thousandths of an inch, or nearly two inches; Venus a semi-diameter of 63·33 of these units; and the parallax of Venus referred to the sun would be represented by 47·85 such units, the maximum possible displacement being 95·7 units, or nearly $\frac{1}{10}$th of an inch.

When the photographs have been secured, the micrometric measurements which will have to be performed consist in the determination of the sun's semi-diameter in units of the scale of the micrometer, the angle of position of the successive situations of the planet on the disc, as shown on the series of photographs, and finally the distances of the centres of the planet and the sun. These data determine absolutely the chord along which the transit has been observed to within $0''\cdot1$; and an error of $1''$ in the measurement would give an error of only $0''\cdot185$ in the deduced solar parallax. Moreover the epoch of each photographic record is determinable with the utmost accuracy, the time of the exposure being from $\frac{1}{80}$ to $\frac{1}{100}$ of a second, or even less.

Now, although the truth of the foregoing remarks will be fully admitted, it will yet be well to point out in this place the inherent or supposed defects of the photographic method. These defects may principally be comprised under the head of Possibility of Distortion; and the importance of an investigation into this source of error will appear at once obvious in all cases where the position of a definite point with reference to a system of co-ordinates has to be determined from measured photographs, especially in such a refined application of it as that which it will have in the determination of the solar parallax.

The distortion of a photographic image, if such exist, may be either extrinsic or intrinsic—that is, either optical or mechanical. The instrumental apparatus for producing the image may produce optical irregularities before it reaches the sensitive plate; or an image optically correct may, by irregular contraction of the sensitive film in the process of drying, and other incidents of the process, present on the plate a faulty delineation. *

In general, two ways present themselves for clearing observations from errors. Either methods may be devised for determining the numerical amount of every error from any source, or by special contrivances the source of error may be contracted to such insignificant limits that its effect in a special case is too minute to exert any influence upon the result. Both these roads have been followed in the inquiry into the optical distortion of photographic images.

As regards the first, let it be supposed that, as in the Kew instrument, the primary image is magnified by a system of lenses before reaching the sensitive plate. The defects inherent to the optical arrangement will clearly affect every photographic picture produced by the same instrument; and hence a method suggests itself for determining absolutely the numerical effect of distortion at every point of the field. Let us assume that the same object, which may be a rod of unalterable and known length, be photographed in precisely the same manner in which celestial events are photographically recorded, the object being at a considerable distance; it may successively be brought into all possible positions in the field of the photoheliograph, and the length of the image on the photograph may be measured afterwards at leisure by means of a micrometer. These lengths will change relatively wherever distortion takes place; but by laying down these varying lengths we shall obtain an optical distortion-map of the particular instrument; and tables may be constructed giving in absolute numbers the corrections to be applied to measurements of positions on account of the influence of optical distortion. In this way the optical distortion of the combined object-glass and secondary magnifier is ascertained. The chief source of distortion, if such exist, will be in the secondary magnifier; and in order to ascertain its amount, a reticule of lines drawn at equal distances upon glass may (as has been done recently by Paschen and Dallmeyer) be placed in the common focus of the object-glass and secondary magnifier. The required data are then immediately given by the measurement of the resulting pictures of the parallelograms on the reticule. Mr. Dallmeyer has ascertained in this manner that no sensible distortion exists in the secondary magnifier constructed by him. The truth of the principle being granted, it was applied to a preliminary series for finding the distortion which affects the Kew instrument, which is not nearly so perfect as those more recently constructed; and the results were so far satisfactory that, instead of a single rod, a proper scale, fifteen feet in length, representing a series of rectangles distributed over half the radius of the field has been erected; and the process of absolutely determining the optical distortion of the Kew photoheliograph is now in active progress, and will be used for the new instruments to be employed in observing the transits of Venus.

The second method of dealing with optical distortion aims at total exclusion of this source of error. It has been proposed by American astronomers, who intend taking part in the coming observations of the transit of Venus, to exclude the secondary magnifier, and, in order to obtain an image of sufficient diameter, to employ a lens of considerable focal length, say 40 ft., which would give an image as large as with the Kew photoheliograph—

* In this micrometer, which is capable of giving radial distances, angles of position, and also rectangular co-ordinates, the accuracy of linear measurements does not depend on the doubtful results given by a long run of a micrometer screw.

* It has been proposed, in order to obviate any possible alteration of the sensitive surface, to use the Daguerreotype instead of the collodion process. The former, however, is so little practised, and, moreover, is so much more troublesome, that it does not seem to be advisable to adopt it, especially as the subsequent measurements would present greater difficulties than occur with collodion pictures.

namely, 4 in. in diameter. As it would be inconvenient to mount such an instrument equatorially, it is proposed to fix it in the meridian in a horizontal position, and reflect the sun in the direction of its axis by means of a flat mirror moved by a heliostat. There cannot be any doubt about the fact that the image so produced would be nearly free from optical distortion, if the interposed mirror did not introduce a new source of error. The difficulty of producing a plane mirror is well known; and there is a difficulty in maintaining its true figure in all positions; there is also a liability of the disturbance of the rays by currents of heated air between the mirror and object-glass: moreover, with such an instrument, position-wires could not be defined with sharpness on the photographs. On the whole, greater reliance may be placed on a method which admits the existence of a distorting influence, but has at the same time means of checking and controlling it numerically.

Great attention has been paid by me at various times to those effects of distortion which might arise from the process of drying. The results to which the experiments lead seem to prove that there is no appreciable contraction except in thickness, and that the collodion film does not become distorted, provided the rims of the glass plates have been well ground: this point is a fundamental one. But in such observations as that of the transit of Venus, no refinement of correction ought to be neglected; hence fresh experiments will be undertaken to set at rest the question whether distortion of the film really takes place when proper precautions are taken. This will be done both by the method I have employed before, and also in accordance with M. Paschen's proposal to measure images of such reticules as above described: this reticule might, as he has suggested, be photographed during the transit of Venus, so that each plate would thus bear data for the correction due to unequal shrinking, if such were to take place.

It has been objected by some astronomers who have casually examined solar photograms that the limb of the sun appears as a consequence of the gradual shading off, even under a small magnifying power, not bounded by a sharp contour; but the measurements of such photograms which have been made during the last ten years of pictures, taken under the most varying conditions which influence definition, have proved that even the worst picture leads to a very satisfactory determination of the sun's semi-diameter and centre; moreover, an independent examination of this question by M. Paschen gave as the result that the mean error of a determination is only ± 0·008 millimetre with a sun-picture of 4 Paris inches in diameter; this corresponds to ± 0″·135, and it is nearly three times less than that resulting from a measurement with the Köningsberg heliometer.

Nevertheless it will be seen from the foregoing remarks that I have not hesitated to arouse your attention to the fact that Astronomical Photography is about to be put to the severest test possible in dealing with such a fundamental problem of astronomy as the determination of the sun's distance from the earth. An intimate knowledge of the subject, however, and experience with respect to work already accomplished in the Kew ten-year solar observations, inspire me with a confident anticipation that it will prove fully equal to the occasion.

So much for performances to be looked forward to in the future: now let me briefly review what Astronomical Photography has already undoubtedly accomplished.

In the first instance the possibility proved of giving to the photographic method of observation a trustworthiness which direct observations can never quite obtain, will render the results of our discussion of the ten years' solar observations at Kew more free from doubts than those observational series on the Sun's spots which have preceded ours. The evidence of a probable connection between planetary positions and solar activity, and the evidence which we have published on the nature of spots as depressions of solar matter, could never have been brought forward but for the preservation of true records of the phenomena through a number of years, while the closer agreement of the calculated results in reference to solar elements is itself evidence of the intrinsic truthfulness of the method, and gives the highest promise that our final deductions, which will be completed in the course of the ensuing year, will not be unworthy the exertions which I, in conjunction with my friends B. Stewart and B. Loewy, have constantly devoted to this work during a period of fully ten years. Not only will some doubtful questions be set finally at rest by it, but new facts of the greatest interest will result, bearing on the laws which appear to govern solar activity.

By nothing, however, would the claims of photographic observation, as one of the most important instruments of scientific research, seem to be so thoroughly well established as by the history of recent solar eclipses. It will be recollected that in 1860 for the first time the solar origin of the prominences was placed beyond doubt solely by photography, which preserved a faithful record of the moon's motion in relation to these protuberances. The photographs of Tennant at Guntour, and of Vogel at Aden, 1868, and also those of the American astronomers at Burlington and Ottumwa, Iowa, in 1869, under Profs. Morton and Mayer, have fully confirmed those results. In a similar manner the great problem of the solar origin of that portion of the corona which extends more than a million of miles beyond the body of the sun has been, by the photographic observations of Col. Tennant and Lord Lindsay in 1871, set finally at rest, after having been the subject of a great amount of discussion for some years.

The spectroscopic discovery in 1869 of the now famous green line, 1474 K, demonstrated undoubtedly the self-luminosity, and hence the solar origin of part of the corona. Those who denied the possibility of any extensive atmosphere above the chromosphere received the observation with great suspicion; but in 1870 and again in 1871 it was fully verified. So far, therefore, the testimony of spectroscopic observations was in favour of the solar origin of the inner corona.

Indeed the observations of 1871 have proved hydrogen to be also an essential constitutent of the "coronal atmosphere," as Janssen proposes to call it—hydrogen at a lower temperature and density, of course, than in the chromosphere. Janssen was further so fortunate as to catch glimpses of some of the dark lines of the solar spectrum in the coronal light, an observation which goes far to show that in the upper atmosphere of the sun there are also solid or liquid particles, like smoke or cloud, which reflect the sunlight from below. Many problems, however, even with reference to the admittedly solar part of the corona, are unsettled. The first relates to the nature of the substance which produces the line 1474 K. Since it coincides with a line in the spectrum of iron, it is by many considered due to that metal; but then we must suppose either that iron vapour is less dense than hydrogen gas, or that it is subject to some peculiar solar repulsion which maintains it at its elevation, or other hypotheses may be suggested for explaining the fact. Since the line is one of the least conspicuous in the spectrum of iron and the shortest, and as none of the others are found associated with it in the coronal spectrum, it seems natural, as many have done, to assume at once that it is due to some new kind of matter. But the observations of Angström, Roscoe, and Clifton, and recently those of Schuster regarding the spectrum of nitrogen, render it probable that elementary bodies have only one spectrum, and since in all experimental spectra we necessarily operate only on a small thickness of a substance, we cannot say what new lines may be given out in cases where there is an immense thickness of vapour; and hence we canno conclude with certainty that because there is an unknown line in the chromosphere or corona, it implies a new substance. Another problem, the most perplexing of all, is the reconciliation of the strangely discordant observations upon the polarisation of the coronal light; but I will at once proceed to the points on which photography alone can give us decisive information.

The nature and conditions of the outer corona (the assemblage of dark rifts and bright rays which overlies and surrounds the inner corona) was very incompletely studied; and the question whether it is solar was not finally settled in the opinions of astronomers of high repute. Some believed it to be caused by some action of our atmosphere; and others supposed it due to cosmical dust between us and the moon. The bright light of the corona and the prominences most undoubtedly cause a certain amount of atmospheric glare; and although it is difficult to see how this is to account for the rays and rifts, it would be rash to deny that it may do so in some manner yet to be discovered. It is quite certain that some of the phenomena observed just at the beginning and end of totality are really caused by it. A light haze of meteoric dust between us and the moon might give results much resembling those observed; but when we come to details this theory seems to be doubtful.

Here photography steps in to pave the way out of the existing doubts. If the rays and rifts were really atmospheric, it would hardly be possible that they should present the same appearance at different stations along the line of totality; indeed, they would probably change their appearance every moment, even at the same station. If they are cislunar, the same appearances could not be recorded at distant stations. It is universally admitted that proof of the invariability of these markings, and especially of their identity as seen at widely separated stations, would amount to a demonstration of their extra-terrestrial origin. Eye-sketches cannot be depended on ; the drawings made by persons standing side by side differ often to an extent that is most perplexing. Now photographs have, undoubtedty, as yet failed to catch many of the faint markings and delicate details ; but their testimony, as far as it goes, is unimpeachable. In 1870, Lord Lindsay at Santa Maria, Prof. Winlock at Jerez, Mr. Brothers at Syracuse, obtained pictures some of which, on account partly of the unsatisfactory state of the weather, could not compare with Mr. Brothers' picture obtained with an instrument of special construction ; * but all show one deep rift especially, which seemed to cut down through both the outer and inner corona clear to the limb of the moon. Even to the naked eye it was one of the most conspicuous features of the eclipse. Many other points of detail also come out identical in the Spanish and Sicilian pictures ; but whatever doubts may have still existed in regard of the inner corona were finally dispelled by the pictures taken in India, in 1871, by Col. Tennant and Lord Lindsay's photographic assistant, Mr. Davis.

None of the photographs of 1871 show so great an extension of the corona as is seen in Mr. Brothers' photograph, taken at Syracuse in 1870 ; but, on the other hand, the coronal features are perfectly defined on the several pictures, and the number of the photographs renders the value of the series singularly great. The agreement between the views, as well those taken at different times during totality as those taken at different stations, fully proves the solar theory of the inner corona. We have in all the views the same extensive corona, with persistent rifts similarly situated. Moreover there is additional evidence indicated by the motion of the moon across the solar atmospheric appendages, proving in a similar manner as in 1860, in reference to the protuberances, the solar origin of that part of the corona.

It will be well here to mention a difficulty which occurs in recording the fainter solar appendages, namely the encroachment of the prominences and the corona on the lunar disc when the plates have to be over-exposed in order to bring out the faint details of the corona. It is satisfactory to find that whenever a difficulty arises it can be mastered by proper attention. Lord Lindsay and Mr. Ranyard have successfully devoted themselves to experiments on the subject. They tested whether reflections from the back surface of the plate played any part in the production of the fringes : for this purpose plates of ebonite and the so-called nonactinic yellow glass were prepared ; and it was immediately found that the outer haze had completely disappeared in the photographs taken on ebonite, while on the yellow glass plates it is much fainter than on ordinary white glass plates. By placing a piece of wetted black paper at the back of an unground plate, the outer haze was greatly reduced ; but by grinding both the back and the front surfaces of a yellow glass plate, and covering the back with a coating of black varnish, it was rendered quite imperceptible, thus showing the greatest part of the so-called photographic irradiation to be due to reflection from the second surface.

In connection with the solution of the most prominent questions connected with the solar envelopes, it may not be without great interest to allude to another point conclusively decided during the last annular eclipse of the sun, observed by Mr. Pogson on the 6th of June of this year, as described by him in a letter to Sir G. B. Airy. In 1870 Prof. Young was the first to observe the reversal of the Frauenhofer lines in the stratum closest to the sun. Now, in 1871 doubts were thrown upon the subject. It appears that the reversed lines seem to have been satisfactorily observed by Captain Maclear at Bekul, Colonel Tennant at Dodabetta, and Captain Fyers at Jaffna. The observations of Pringle at Bekul, Respighi at Poodocottah, and Pogson at Avenashi were doubtful, while Mosely at Trincomalee saw nothing of this reversal, which is, according to all accounts, a most striking phenomenon, although of very short duration. Mr. Lockyer missed it by an accidental derangement of the telescope. The reversal and the physical deductions from it are placed beyond doubt by Mr. Pogson's observation of the annular eclipse on June 6. At the first internal contact, just after a peep in the finder had shown the moon's limb lighted up by the corona, he saw all the dark lines reversed and bright, but for less than two seconds. The sight of beauty above all was, however, the reversion of the lines at the breaking-up of the limb. The duration was astonishing—five to seven seconds ; and the fading-out was gradual, not momentary. This does not accord with Captain Maclear's observations in 1870, who reports the disappearance of the bright spectrum as "not instantly, but so rapidly that I could not make out the order of their going." Prof. Young again says that "they flashed out like the stars from a rocket-head." But discrepancies in this minor point may be accounted for by supposing differences in quietude of that portion of the sun's limb last covered by the moon.

The mention of the solar appendages recalls to mind another instance in which photography has befriended the scientific investigator. I allude to the promising attempt which has been made by Prof. Young to photograph the protuberances of the sun in ordinary daylight. A distinct reproduction of some of the double-headed prominences on the sun's limb was obtained ; and although as a picture the impression may be of little value, still there is every reason to believe, now that the possibility of the operation is known, that with better and more suitable apparatus an exceedingly valuable and reliable record may be secured. Prof. Young employed for the purpose a spectroscope containing seven prisms, fitted to a telescope of $6\frac{1}{2}$-inch aperture, after the eye-piece of the same had been removed. A camera, with the sensitive plate, was attached to the end of the spectroscope, the eye-piece of which acted in the capacity of a photographic lens, and projected the image on the collodion film. The exposure was necessarily a long one, amounting to three minutes and a half. The eye-piece of the spectroscope was unsuitable for photographic purposes, and only in the centre yielded a true reproduction of the lines free from any distortion. A larger telescope, with a suitable secondary magnifier, will be required, in order to secure a more defined image.

I have hitherto spoken of the successful applications of photography to astronomy ; but I must point out also some cases where it has failed. Nebulæ and comets have not yet been brought within the grasp of this art, although, perhaps, no branch of astronomy would gain more if we should hereafter succeed in extending to these bodies that mode of observing them. There is theoretically, and even practically, no real limit to the sensitiveness of a plate. Similarly with reference to planets great difficulties still exist, which must be overcome before their phases and physical features can be recorded to some purpose by photography ; yet there is great hope that the difficulties may be ultimately surmounted. The main obstacle to success arises from atmospheric currents, which are continually altering the position of the image on the sensitive plate ; the structure of the sensitive film is also an interfering cause for such small objects. A photograph taken at Cranford of the occultation of Saturn by the moon some time ago exhibits the ring of the planet in a manner which holds out some promise for the future.

The moon, on the other hand, has been for some time past very successfully photographed ; but no use has hitherto been made of lunar photographs for the purposes of measurement.

The photographs of the moon are free from distortion, and offer therefore material of incalculable value as the basis of a selenographic map of absolute trustworthiness, and also for the solution of the great problem of the moon's physical libration. This question can be solved with certainty by a series of systematic measurements of the distance of definite lunar points from the limb. Mr. Ellery, Director of the Observatory of Melbourne, has sent over an enlargement of a lunar photograph taken with the Great Melbourne Telescope, in which the primary image is $3\frac{3}{16}$ inches in diameter. Such lunar negatives would be admirably adapted for working out the problem of the physical libration, and also for fundamental measurements for a selenographic map ; the more minute details, however, would have to be supplied by eye-observations, as the best photograph fails to depict all that the eye sees with the help of optical appliances. On the other hand, selenographic positions would be afforded more free from error than those to be obtained by direct micrometrical measurements.

Although, as I have stated, I do not contemplate passing in review recent discoveries in astronomy, I must not omit to call your attention to some few subjects of engrossing interest. First,

* Mr. Brothers had, in 1870, the happy idea to employ a so-called rapid rectilinear photographic lens, made by Dallmeyer, of 4 inches aperture and 30 inches focal length, mounted equatorially, and driven by clockwork ; and he was followed in this matter by both Col. Tennant and Lord Lindsay in 1871. The focal image produced, however, is far too small ($\frac{3}{16}$ of an inch, about) ; therefore it will be desirable in future to prepare lenses of similar construction, but of longer focal length and corresponding aperture.

with reference to the more recent work of Dr. Huggins. In his observations he found that the brightest line of the three bright lines which constitute the spectrum of the gaseous nebulæ was coincident with the brightest of the lines of the spectrum of nitrogen; but the aperture of his telescope did not permit him to ascertain whether the line in the nebulæ was double, as is the case with the line of Nitrogen. With the large telescope placed in his hands by the Royal Society, he has found that the line in the nebulæ is not double, and in the case of the great nebula in Orion it coincides in position with the less refrangible of the two lines which make up the corresponding nitrogen-line. He has not yet been able to find a condition of luminous nitrogen in which the line of this gas is single and narrow and defined like the nebular line.

He has extended the method of detecting a star's motion in the line of sight by a change of refrangibility in the line of a terrestrial substance existing on the star to about 30 stars besides Sirius. The comparisons have been made with lines of hydrogen, magnesium, and sodium. In consequence of the extreme difficulty of the investigation, the numerical velocities of the stars have been obtained by estimation, and are to be regarded as provisional only. It will be observed that, speaking generally, the stars which the spectroscope shows to be moving from the earth, as Sirius, Betelgeux, Rigel, Procyon, are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing; whilst the stars in the neighbourhood of this region, as Arcturus, Vega, and α Cygni, show a motion of approach. There are, however, in the stars already observed, exceptions to this general statement; and there are some other considerations, as the relative velocities of the stars, which appear to show that the sun's motion in space is not the only or even in all cases the chief cause of the observed proper motions of the stars. In the observed stellar motions we have to do probably with two other independent motions—namely, a movement common to certain groups of stars and also a motion peculiar to each star. Thus the stars, β γ δ ε ζ, of the Great Bear, which have similar proper motions, have a common motion of recession; while the star α of the same constellation, which has a proper motion in the opposite direction, is shown by the spectroscope to be approaching the earth. From further researches in this direction, and from an investigation of the motions of stars in the line of sight in conjunction with their proper motions at right angles to the visual direction obtained by the ordinary methods, we may hope to gain some definite knowledge of the constitution of the heavens.

This discovery supports, in a somewhat striking manner, the views which Mr. Proctor has been urging respecting the distribution of the stars in space. According to these views there exist within the sidereal system subordinate systems of stars forming distinct aggregations, in which many orders of real magnitude exist, while around them is relatively barren space. He had inferred the existence of such systems from the results of processes of equal-surface charting applied successively to stars of gradually diminishing orders of brightness. He found the same regions of aggregation, whether the charts included stars to the sixth order only or were extended, as in his chart of the northern heavens, to the tenth and eleventh orders; and these regions of aggregation are the very regions where the elder Herschel found the faintest telescopic stars to congregate. Applying a new system of charting to show the proper motions of the stars, he found further evidence in favour of these views. The charts indicated the existence of concurrent motions among the members of several groups or sets of stars. Selecting one of the more striking instances as affording what appeared to him a crucial test of the reality of this *star-drift*, Mr. Proctor announced his belief that whenever the spectroscopic method of determining stellar motions of recess or approach should be applied to the five stars, β γ δ ε and ζ Ursæ Majoris, these orbs (which formed a drifting set in the chart of proper motions) would be found to be drifting collectively either towards or from the earth: this has been confirmed.

The time has now come for more closely investigating the various theories which have been propounded by such profound thinkers as Tyndall, Tait, Reynolds, and others, to account for the phenomena of Comets. I do not propose to enter into a statement of these theories; but I venture to call attention to Zöllner's views, which have recently given rise to a great amount of controversy. In doing so, I am solely influenced by a desire to give information on this subject, without implying thereby that I give my adherence, or even preference, to his theory.*

* See Appendix, p. 12.

The vaporisation of even solid bodies at low temperatures suggests that a mass of matter in space will ultimately surround itself with its own vapour, the tension of which will depend upon the mass of the body (that is, upon its gravitative energy) and the temperature. If the mass of the body is so small that its attractive force is insufficient to give to the enveloping vapour its maximum tension for the existing temperature, the evolution of vapour will be continuous until the whole mass is converted into it. It is proved by analysis that such mass of gas or vapour in empty and unlimited space is in condition of unstable equilibrium, and must become dissipated by continual expansion and consequent decrease of density. It follows that celestial spaces, at least within the limits of the stellar universe, must be filled with matter in the form of gas.

A fluid mass existing in space at a distance from the sun or other body radiating heat would, if its mass were not too great, be converted entirely into vapour after the lapse of sufficient time. But if the fluid mass approach the sun, solar heat would occasion a more rapid development of vapour on the sunward side; and the total vaporisation would require an incomparably short time with reference to the interval necessary in the former case; this time would be shorter the smaller the mass of the body. Prof. Zöllner points to the smaller comets, which often appear as spherical masses of vapour, as examples of such bodies, while the spectra of some of the nebulæ and smaller comets render the existence of fluid masses giving out vapour highly probable.

The self-luminosity and train of comets he refers to other causes. Two causes only are known through the operation of which gases become self-luminous—elevation of temperature (as by combustion), or electrical excitement. Setting aside the first as involving theoretical difficulties, the second cause is demonstrated by him to be sufficient to account for the self-luminosity and the formation of the train, provided it be granted that electricity may be developed by the action of solar heat, if not in the process of evaporation, at least in the mechanical and molecular disturbances resulting from it. The production of electricity by such processes within the limits of our experience, must be admitted as a well-known fact. The spectrum of the vaporous envelope of a comet, illuminated in this manner, must necessarily be that produced by the passage of an electrical discharge through vapour identical in substance with a portion of the comet's nucleus, from which the envelope is derived. As, according to this supposition, water and liquid hydrocarbons are important constituents of these bodies, the spectra of the comets should be such as belong to the vapours of these substances; and in this manner the resemblance and partial coincidence of the observed cometic spectra with those of gaseous hydrocarbons is explained.

The form and direction of the train indicate undoubtedly the action of a repulsive force; and Prof. Zöllner asserts that the assumption of an electrical action of the sun upon bodies of the solar system is necessary and sufficient to account for all the essential and characteristic phenomena of the vaporous envelope and the train. The direction of the train, towards or from the sun, is, according to this theory, to be easily explained by the supposition of a variability in the mutual electrical conditions. This accords perfectly with the phenomena observed in the development of electricity by vapour-streams in the hydroelectric machine, where the sign of electricity depends upon the presence or absence of various substances in the boiler or the tubes.

The theory acquires an additional interest from Schiaparelli's remarkable discovery of the identity of the paths of certain comets with great meteor-streams, since the meteoric masses must inevitably be converted into vapour on approaching the sun, with the exhibition of the characteristic appearances of the comets.

The intimate connection of planetary configuration and solar spots, of the latter and terrestrial magnetism and auroral phenomena, must tend to establish also a connection between solar spots and solar radiation. It is demonstrated by the researches of Piazzi Smyth, Stone, and Cleveland Abbe, that there is a connection between the amount of heat received from the sun and the prevalence of spots—a result clearly in harmony with those derived from recent investigations into the nature of the solar atmosphere. Further, in a paper by Mr. Meldrum, of Mauritius, which will be read before you during this session, most remarkable evidence is given on the close connection of these phenomena. It appears that the cyclones of the Indian Ocean have a periodicity corresponding with the sun-spot periodicity; so that if an observer in another planet could see and measure the sun-spots and cyclones (earth-spots), he would find a close harmony between them. Such a connection will probably be found to exist over the globe generally; but with reference to the Indian Ocean it may be stated as a matter of fact, from Mr. Meldrum's discussion of twenty-five years' observations, that in the area lying between the equator and 25° south latitude, and between 40° and 110° east longitude, the frequency of cyclones has varied during that period directly as the amounts of sun-spots. I am glad to be able announce that Mr. Meldrum, in order to place the deductions on a still broader foundation, proposes to investigate these laws on a plan perfectly in agreement with our method of determining the areas of solar disturbances, the results of which have been published from time to time during the last ten years. Moreover the observations on the periodic changes of Jupiter's appearance, and the observations of Mr. Baxendell that the convection currents of our earth vary according to the sunspot period—all these results, seemingly solitary, but truly in mysterious harmony, point to the absolute necessity for establishing constant photographic records of solar and terrestrial phenomena all over the world. No astronomer or physicist should lose any opportunity of assisting in this great aim, by which alone unbiassed truthful records of phenomena can be preserved. What is more, no system of observations can be carried on at a less expense.

We have hopes of seeing the photographic method as applied to sun-observations joined to the work of the Greenwich Observatory; but what is further wanted is the erection of instruments for photographic records and of spectroscopes in a number of observatories throughout the world, so as to obtain daily records of the sun and to observe magnetical and meteorological phenomena continuously in connection with solar activity. Meteorological observation is storing up useful facts; but they can only be dealt with effectually if investigated in close parallelism with other cosmical phenomena. Only when this is done may we hope to penetrate the maze of local meteorological phenomena and elevate meteorology to the rank of a science. The time has really come not only for relieving private observers from the systematic observation of solar phenomena, but for drawing close ties between all scattered scientific observations, so as to let one grand scheme embrace the whole; and no method seems to be so well adapted to bring about this great achievement than the method of photographing the phenomena of nature, which in its very principle carries with it all extinction of individual bias.

In conclusion I cannot refrain from making a passing allusion to a Royal Commision, presided over by the Duke of Devonshire, which has been sitting for some time past; for I believe that its labours will have an important bearing on all that relates to scientific education and the promotion of science in this country. The time has come when the cultivation of science must be protected and fostered by the State; it can no longer be safely left to individual efforts. If England is to continue to hold a high position among civilised nations, the most anxious care must be given to the establishment by the State of such an organised system for the advancement of science and the utilisation of the work of scientific men as will be in harmony with similar organisations in neighbouring states — for examples, France, Germany, and Russia.

Appendix

Certain conclusions at which Prof. Zöllner arrives in the investigation of several points bearing on the theory which he defends are, quite independent of the latter, of high scientific value.

First, with reference to the density of atmospheric air, which (in accordance with the considerations mentioned in stating his views) he supposes to fill the interstellar space everywhere, he assumes for the purposes of calculation that the temperature of space is that of melting ice, and finds that the lower limit of density for a portion of gas in space is $\frac{1}{10^{246}}$ of that of the air at the earth's surface, a value so small that if a mass of air which, at its ordinary density upon the earth's surface, occupies a volume of one cubic decimetre (a litre), were reduced to the density expressed by this fraction, it would fill a sphere whose radius would not be traversed by a ray of light in less than 10^{98} years. These values indicate a density which would have no appreciable effect whatever upon rays of light or upon the motion of bodies in space, and which would become still less if the temperature of space be taken, with Fourier, at −60° C., or with Pouillet, at −132° C. But as every solid body must, by virtue of its gravitative energy, condense the gas into an atmospheric envelope round itself, the density of the latter will solely depend on the size and mass of the body. Prof. Zöllner finds by calculation that, for instance, the density of air thus forming an atmosphere round the moon must be $\frac{1}{10^{332}}$ of that of the air of the earth's surface. This is in accord with the fact that no trace of a lunar atmosphere has as yet been detected. But the values become very great for the larger planets, quite great enough to manifest absorptive effects upon the light reflected from them. Considering that there are peculiarities in the spectra of Uranus, Neptune, and also of Jupiter, which appear to indicate atmospheric influences, Prof. Zöllner's results are not without deep interest, and certainly suggestive of further inquiry.

Secondly, with reference to the supposition that a body may be at the same time under the influence of gravitative and electrical agencies, it was necessary for the author of this theory to discuse the quantitive difference in their effect upon ponderable masses at a distance. The discussion shows that, if the mass increases, gravitation preponderates over electricity; if the mass decreases sufficiently, the contrary takes place. It follows that the cometary nuclei, as masses, are subject to gravitation, while the attenuated vapours developed from them yield to the action of free electricity of the sun. Prof. Zöllner has based upon Hankel's numerous and careful researches on the determination of atmospheric electricity, in absolute measure, an analytical inquiry into the motion of a small sphere under the action of gravity and atmospheric electricity, which leads to some remarkable results. Supposing the free electricity of the sun to be not greater than that repeatedly observed on the earth's surface, and to be uniformly distributed, it would communicate to a sphere having a diameter of 11 millimetres and a weight of $\frac{1}{100}$ of a milligramme, and starting from the sun, by the time it had moved as far away as the mean distance of Mercury, a velocity per second of 3,027,000 metres, or 408·4 German geographical miles.* This velocity is such that in two days it would pass over a space of 70,540,000 German geographical miles, a magnitude quite of the same order as those recorded by cometary astronomy. The discussion was undertaken to prove that there is no need for assuming the existence of any unknown repulsive agency, but that electrical energy not greater than that observed on the earth's surface is amply sufficient to account satisfactorily for the phenomena presented by cometic trains.

SECTION B

CHEMICAL SCIENCE

Opening Address by the President, Dr. J. Hall Gladstone, F.R.S.

One of my fellow-students in the laboratory of the late Prof. Graham began the study of Chemistry because he wanted to be a Geologist, and he had read in some Geological Catechism that, in order to be versed in that science, it was necessary, as a preliminary step, to gain a knowledge of Chemistry, Mineralogy, Zoology, Botany, and I know not what besides. My friend became a chemist, and found that enough for the exercise of his faculties. Yet the catechism had truth on its side; for so intertwined are the various branches of observational or experimental research, that a perfect understanding of one can only be obtained through an acquaintance with the whole cycle of knowledge.

Yet, on the other hand, who can survey the whole field even of modern Chemistry? There was a time, doubtless, in the recollection of the more venerable of my auditors, when it was not impossible to learn all that chemists had to teach; but now that our "Hand Book" has grown so large that it would take a Briareus to carry it—and it requires a small army of abstractors to give the Chemical Society the substance of what is done abroad—we are compelled to become specialists in spite of ourselves. He who studies the general laws of Chemistry may well turn in despair from the ever-growing myriads of transformations among the compounds of carbon; we have agricultural, physiological, and technical chemists; one man builds up new substances, another new formulæ; while some love the rarer metals, and others find their whole soul engrossed by the phenyl compounds.

How is this necessity of specialisation to be reconciled with the necessity of general knowledge? By our forming a home

* Fifteen to a degree of longitude on the Equator.

for ourselves in some particular region, and becoming intimately conversant with every feature of the locality and their choicest associations, while at the same time we learn the general map of the country, so as to know the relative position and importance of our favourite resort, and to be able—when we desire it —to make excursions elsewhere.

To facilitate this is one of the great objects of the British Association. The different Sections are like different countries, and leaving the insular seclusion of our special studies, we can pass from one to the other, and gain the advantages of foreign travel.

From this chair I must of course regard Chemistry as the centre of the universe, and in speaking of other sections I must think of them only in their relation to ourselves. There is that rich and ancient country, Section A, which, according to the Annual Report, comprises several provinces—Mathematics, Astronomy, Optics, Heat, Electricity, and Meteorology.

Mathematics and Astronomy.—It was when the idea of exact weights and measures was projected into it that Alchemy was transmuted into Chemistry: as our science has become more refined in its methods its numerical laws have become more and more significant, and it may safely be predicted that the more closely it is allied with general physics, the greater will be the mathematical knowledge demanded of its votary. But till lately the Chemist and the Astronomer seemed far asunder as the heavens and the earth, and none could have foretold that we should now be analysing the atmospheres of the sun and stars, or throwing light on the chemical composition of planetary nebulæ and the heads of comets. There is in this, too, as in other things, a reciprocal benefit; for we are encouraged to hope that this celestial chemistry will reveal to us elements which have not yet been detected among the constituents of our globe.

Light, Heat, and Electricity.—How intimately are these associated with the chemical force, or rather how easily are these Protean forces transformed into one another! The rays of the sun coming upon our earth are like a chemist entering his laboratory; they start strange decompositions and combinations not only in the vegetable kingdom, but also among inorganic gases and salts; they are absorbed selectively by different bodies which they penetrate, or are refracted, dispersed, and polarised according to the chemical composition and structure of the substance. All this has been the subject recently of much scientific research; and I need scarcely remind you of the beautiful art of photography as one of the results of photo-chemistry, or of the benefits that have arisen from a study of circular polarisation, indices of refraction, and especially spectrum-analysis. In regard to the latter, however, I would remark that while the optical examination of the rays emitted by luminous vapours has yielded most brilliant results, there is another kind of spectrum-analysis —that of the rays absorbed by various terrestrial gases, liquids, and solids—which has already borne valuable fruit, and which, as it is far more extensively applicable than the other, may perhaps play a still more important part in the Chemistry of the future. The dispersion of the rays of the spectrum is certainly due to the chemical nature of the body through which they pass, but this is as yet almost unbroken ground waiting for an explorer. As to heat, it has ever been the tool of the chemist; and it would be difficult to over-estimate the significance of researches into the specific heat, or the melting- and boiling-points of elements and their compounds. The laws of chemical combination have been elucidated lately by thermochemical researches; it has been sought to establish a connection between the absorption or radiation of heat and the complexity of the chemical constitution of the active body; while the power of conducting heat, or of expanding under its influence, offers a promising field of inquiry. As to electrical science, one department of it—Galvanism—is strictly chemical: the electrolytic cell does our work; and indeed we claim half the electric telegraph, for while the needle may oscillate in Section A, the battery belongs to B.

Last in Section A comes Meteorology; and there are chemical questions concerning the constitution of the atmosphere, its changes, and the effect of its occasional constituents upon vegetable and animal life, which merit the deepest attention of the physiologist, philanthropist, and statesman.

If we turn to Section C, there is an outlying province belonging to us, namely, Mineralogy, which lies on the frontiers of Geology. A vast and very promising region is the origin and mode of formation of different minerals: this has attracted some explorers during the past year; but in order to investigate it properly the geologist and the chemist must travel hand in hand. Geology, in demanding of us the analysis of earths and ores, rocks and precious stones, repays us by bringing to our knowledge many a rare element and strange combination.

When we pass from C to D, that is, from the crust of the globe to the organised beings that inhabit and adorn it, we are introduced into new regions of research. When organic chemistry was young, Cuvier said of it, "Dans cette nouvelle magie, le chimistre n'a presque qu'à vouloir: tout peut se changer en tout peut l'extraire de tout"; and though we have now learnt much of the laws by which these magical transformations proceed, they far transcend the dreams of the French philosopher; there is yet no visible limit to the multitude of products to be derived from the vegetable and animal world, and their changes seem to afford boundless scope for chemical ingenuity. The benefit here is also reciprocal; for the physiologist enters by our aid into the wonderful laboratory of the living plant or animal, and learns to estimate the mode of action of different foods and medicines. There have lately been some good researches of this character; the difficulties are great, but the results to be achieved are worthy of any effort.

There may be little intercourse between us and the geographers in E, but we stand in no distant relationship with many of the subjects discussed in F. Economic science embraces the chemical arts from cookery upwards; such imperial questions as that of the national standards, or the patent laws, interest us greatly; the yield of our corn-fields is increased through our knowledge of the constituents of soils and manures, and upon many of the chemical manufactures depend in no small degree the commerce and the wealth of Britain.

In this most important branch of technical chemistry we need the skill of the mechanician; and this introduces us to Section G. One of the questions of the day will illustrate the connection between these varied departments of study. Statistics prove that the consumption of coal is now advancing, not at the gradual pace which recent calculations allowed, but at a rapidly accelerating speed, and they make the householder anxious about rising prices, and the political economist about the duration of our coal-fields. It is well known that there is a great waste of fuel throughout the country, as the maximum of heat produced by the combustion is very far from being ever utilised; and it will be for the combined wisdom of the chemist, physicist, and mechanician to devise means for reducing this lavish expenditure, or to indicate other available sources of power.

While this correlation of the natural sciences renders it desirable that the votary of one should have some general acquaintance with the rest, the correlation of all knowledge shows that no education can be complete which ignores the study of nature. A mind fed only on one particular kind of lore, however excellent that kind may be, must fail of proper nourishment. I am not going to say a word against philological studies; I am too fond of them myself for that; and I could wish that the modern languages were taught more, and the classic languages were taught better than they are at present. What I do contend for is, that chemistry (or some cognate branch of science) should have an honoured place in the education of every English lady and gentleman. I say purposely "an honoured place," for at present where chemistry is introduced we too often find the idea latent which was expressed by one principal of a lady's college, who told a friend of mine that he was to give the girls a course of pretty experiments, but she did not expect him to teach them anything; and we know that when boys repeat chemical experiments at home it is looked upon as an amusement, a philosophical one no doubt, but rather objectionable, inasmuch as they spoil their mother's towels and singe their own eyebrows.

Of course some knowledge of chemistry is indispensable for a large number of our manufacturers, and for the medical profession, while it is extremely valuable to the farmer, the miner, and the engineer. It will also be readily granted that information about the air we breathe, the water we drink, the food we live upon, the fuel we burn, and the various common objects we handle, must be of service to every man. But we are met by the advocates of the old system of education with the remark that the value of school-teaching does not depend so much upon the information given as upon the mental training. This I admit; though it seems to me that if the same training can be secured by two studies, the one of which (like the making of Latin verses) gives no information at all, and the other (like chemical analysis imparts some useful knowledge, we should prefer the latter. But I hold that, as a means of educating the mental faculties, chemistry, faithfully taught, has in many respects the advantage over literary studies. There is superabundant scope for the exercise of the memory; the powers of observation are developed by it to a wonderful degree; the reasoning powers may be well disciplined on the philosophy of chemical change, or the application of the laws of Dalton, Mitscherlich, and Avogadro; while the imagination may be cultivated by the attempt to form a conception of the ultimate particles of matter, with their affinities and atomicities, as they act and react upon one another under the control of the physical forces.

Our Government insists on a certain standard of education for all who are allowed to teach in our elementary schools. In those schools which receive no State aid it is only public opinion which can insist that the teacher shall be duly qualified himself. Such bodies as the British Association form this public opinion, and will deserve well of their country if they demand that these masters and mistresses shall know something of the material universe in which they move, and be able to impart to every child such scientific knowledge as shall afford him an interesting subject for thought, give him useful information, and discipline his mental powers.

Among the many services rendered by the monthly reports of the progress of chemistry which the Chemical Society publishes, and the British Association helps to pay for, there is one which is rather salutary than pleasant. They bring prominently before our notice the fact that in the race of original research we are being distanced by foreign chemists. I refer not to the quality of our work, about which opinions will probably differ, but to the quantity, which can be determined by very simple arithmetic. This is a matter of no small importance, not only for the honour of England, but still more for the advancement of science, and the welfare of man. From the Physical Chair of this Association last year, a note of warning was uttered in the following words, after a reference to the sad fate of Newton's successors, who allowed mathematical science almost to die out of the country:—"If the successors of Davy and Faraday pause to ponder even on *their* achievements, we shall soon be again in the same state of ignominious inferiority." The President of the Chemical Society, also in the last Anniversary Address, drew attention to the diminished activity of Chemical discovery, and to the lamentable fewness of original papers communicated. He traces this chiefly to "the non-recognition of experimental research by our universities," and suggests that in granting of science-degrees every candidate should be required, as in Germany, to prove his ability for original investigation.

Concurring in this, I would remark that other causes have also been assigned, and other suggestions have been made. There is the small recognition of original research even by our learned societies—at least such recognition as will come home to the understanding of the general pubiic. It is true the Fellowship of the Royal Society is awarded mainly for original discoveries, and there are two or three medals to be disposed of annually; but these distinctions fall to the lot of the seniors in science, often men who are beyond the need of encouragement, and though they doubtless are serviceable as incentives, there is many a beginner in the honourable contest of discovery who is too modest even to hope for the blue ribbon of science. While the Victoria Cross is awarded to few, every soldier who has borne part in a victory expects his clasp, and so might every man who has won victories over the secrets of nature fairly look for some public recognition. It has been suggested, for instance, that the Royal Society, in addition to the F.R.S., might institute an Associateship, with the letters A.R.S., designed exclusively for those younger men who have shown zeal and ability in original research, but whose discoveries have not been sufficient to entitle them already to the Fellowship. It is suggested, too, that the Chemical Society might give some medal, or diploma, or some similar distinction, to those who contribute papers of sufficient merit.

But beyond this is the non-recognition of scientific research by society in general. We can scarcely expect the average enlightened Englishman to be anything but scared by a graphic formula, or a doubly sesquipedalian word containing two or three compound radicals; but he need not continue to talk of the four elements, or of acids being neutralised by sugar. But, indeed, the so-called educated classes in England are not only supremely ignorant of science, they have scarcely yet arrived at the first stage of improvement—the knowledge of their own ignorance. Then, again, there is the excessive preference of practical over theoretical discoveries; or rather, perhaps, the inability to appreciate anything but tangible results. Thus a new aniline compound is nothing unless it will dye a pretty colour; if we speak of the discovery of a new metal by the spectroscope, they simply ask—What is it useful for? and the rigorous determination of an atomic weight has for them no meaning nor interest nor beauty. The general appreciation of science must be of gradual growth; yet there are wealthy men who know its value, and who might well become the endowers of research. There are, indeed, at present funds available for the purpose—such as the Government grant, and the surplus funds of this association; but the money is given simply to cover actual outlay, and this, though very useful, scarcely meets the case of those young philosophers who have no balance at their bankers, and yet must live. Will not some of these wealthy men endow experimental scholarships, or professorships, in connection with our colleges, institutions, or learned societies? As an instance of the good that may be effected in this way, may be cited the Fullerian professorships; and as a very recent example, worthy of all honour, may be mentioned the purpose of Mr. J. B. Lawes, not only to continue his elaborate experiments at Rothamsted throughout his lifetime, but to place his laboratory and experimental fields in trust, together with 100,000*l.*, so that investigations may be continued in the wider and more scientific questions which the progress of agriculture may suggest.

The Government of our country, through the Science and Art Department, renders good assistance to the teaching of science; and if the recommendations of the Royal Commission on Scientific Instruction and the Advancement of Science be adopted, the introduction of practical examinations for the obtaining of certificates for a superior grade of science master will certainly foster a spirit of research. It has been generally held that the promotion of research is within the legitimate scope of Government; and where, as in the case of Aristotle and Alexander, genius and industry have been sustained by princely munificence, the happiest results have ensued. Yet this question of Government aid is a delicate one: for genius, when put into swaddling clothes, is apt to be stifled by them; and were science to depend on political favour or imperial support, it would be a fatal calamity. Still I think it will be everywhere admitted that science might with propriety be subsidised from the public funds in cases where the results may be expected to confer a direct benefit upon the community, and where the inquiry, either from its expense, its tediousness, its uninteresting character, or the amonnt of co-operation required, is not likely to be carried out by voluntary effort. The astronomical work which is paid for by Government bears upon navigation, and answers both these requirements; and it is easy to conceive of inquiries in our own science that might equally deserve the assistance of the State. Some of these might also more than repay the outlay, though perhaps the profit would not fall into next year's budget.

I believe that this diminution of original research, which we deplore, is partly due to a cause in which we rejoice—the recent extension of science-teaching. The professorships of chemistry are scarcely more numerous now than they were twenty years ago, while the calls upon the professor's time in conducting classes or looking over examination papers have greatly augmented. Thus some of the most capable men have been drawn away from the investigation of nature; and in order to afford them sufficient leisure for the purpose, means must be found to multiply the number of the professorships in our various colleges.

While the rudiments of science are being infused into our primary education, now happily becoming national, while physical science is gradually gaining a footing in our secondary and our large public schools, and while it is winning for itself an honoured place at our universities, it is to be hoped that many new investigators will arise, and that British chemists will not fall behind in the upward march of discovery, but will continue hand in hand with their continental brethren thus to serve their own and future generations.

SECTION C

GEOLOGICAL SECTION

OPENING ADDRESS BY THE PRESIDENT, R. A. C. GODWIN-AUSTEN, F.R.S.

THE Geological Section is fortunate in respect of this year's place of meeting of the British Association. The county of Sussex presents a wide range to the geological observer; there is the great fresh water Wealden series, next the entire Cretaceous group, then portions of the Nummulitic group, including the unique fossiliferous beds of Bracklesham. At Selsey is to be seen a remnant of a definite Tertiary period, of which at no other place in England is there any record. Lastly, the evidence as to local conditions during the Glacial period is peculiarly interesting. This rich field has not wanted for competent labourers, foremost amongst whom must be named Dr. Gideon Mantell, who in his day did so much by his zeal and knowledge to diffuse a taste for his favourite pursuit. There must also be added the names of Mr. Martin, of Pulborough, and Mr. Dixon, of Bognor.

It might perhaps be a fitting preliminary to the local communications which we may expect in the course of this meeting, should I here give a summary of what has been already done with reference to the geology of this S.E. of England; but to many who meet now in this section, very much of such a survey would be familiar. Instead of this I propose to call attention to what is the peculiar feature of our local geology, namely, its great Wealden formation, the product of that vast lake or sound, which at a time before a particle of the chalk hills of Sussex had been formed, covered an area larger than the whole of the south-east of this island. What I shall endeavour to put before you, a point not generally understood, is with reference to the place of formations akin to our Wealden, in the records of past time, and enable you to realise what were then the geographical conditions of the northern hemisphere, what the distribution and extent of other areas of fresh water, the equivalents of our Wealden.

Place of the Fresh- and Brackish-water formations on the Geological Scale

If a general view be taken of the successive physiographical conditions of bygone geological periods, it is seen in respect of each, such as those of the Palæozoic period, or of the Mesozoic such as for the Jurassic, Cretaceous, and Nummulitic, which all represent distinct periods of past time, and are all the products of purely marine conditions, that what is at present terrestrial surface, was at those times to a great extent covered by water, and that the great geological formations are merely old sea-beds.

When on a projection of the northern hemisphere, the known extent of each of these old seas is represented, as on the accompanying maps, it is also seen to how great an extent at those times the area of water exceeded what it has at present; at each of these great periods the northern hemisphere must have presented just such a preponderance of water which the southern hemisphere does at present; and it is further to be remarked how closely the area of one period of northern geological submergence corresponds with the others, as the Nummulitic with the Cretaceous, and the Cretaceous with the Jurassic. Whatever the course, there is to be seen in this a recurrence of like conditions at enormously long intervals of time.

If next the internal evidence to be derived from these Mesozoic formations be taken, it is to be seen, as is familiar to most geologists, that each, when most complete, presents a like order of change, from its older to its newer portions.

Over the mid-European area shallow-water accumulations, such as shingle and sand-zones (infra-Liassic), preceded the deeper water Lias-shales and limestones. Jurassic oolites follow upon these, indicating somewhat decreased depths for the Middle Jurassic series. Oscillations of surface mark this period; and with respect to its physiography, Mr. Darwin has given his opinion that the Malay Archipelago, with its numerous large islands separated by wide and shallow seas, probably represents the former state of Europe, when the Middle Jurassic beds were accumulating. Next follow deep-water depositions, when the widely spread Kimmeridge series was formed, ending upwards with the Portland beds.

The Cretaceous group, as it is exhibited here in the South of England, where its vertical thickness is very great, presents in its lower beds (Neocomian) a marine fauna which indicated to Edward Forbes a limited sea, with depths not exceeding eighteen fathoms. Sand-zones hundreds of feet in thickness overlie these. The argillaceous Gault, in its composition and fauna, is a deep-water deposit, followed by shallower-water sands (Upper Green Sand) indicating oscillating conditions as to depth of water. To which succeeds the widely-spread oceanic depositions of the white chalk. Here recurring conditions come about in like order as in the Jurassic series; and a corresponding illustration might be derived from the physical changes indicated in the course of the Nummulitic period.

In respect to none of these marine geological formations is there any indication whatever that one passed into, or was in continuous sequence with, another, either stratigraphically or geologically; on the contrary, wherever there is apparent continuity, either upwards or downwards, it is by change or transition from one set of conditions to another wholly different. The purely Marine Upper Silurian beds of the Welsh border are followed conformably by the Old Red Sandstone, which last is now universally accepted as a lacustrine formation, the place of which, in time, was intermediate between the middle Palæozoic group, and the Upper or Carboniferous, which commenced with the so-called "Devonian." The positions and extent of the "Old Red" lacustrine beds in all parts of the British Islands indicate, even at this day, to what extent Silurian sea-bed had become terrestrial surface, to which the lacustrine basins were subordinate.

In the contrary direction, and in our own area, the next group indicating widely spread marine conditions, that represented by the Devonian and Mountain limestone formations, sets in, as in North Devon, with shallow-water sands, and a marine fauna (Lower Devonian) in sequence in "Old Red" depositions, with fresh-water fishes and crustaceans. There is no continuity from "Old Red" into the earliest Devonian beds, any more than from uppermost Silurian into Lower "Old Red" (Phillips's Geology of Oxford, pp. 77—79).

The later Palæozoic ocean-floor, now one mountain limestone, in turn become terrestrial surface on which the Coal-measures were accumulated, and over which the abundant vegetation of that period established itself. The Coal-measures represent so much of the surface of their time, as from position favoured expanses of fresh and brackish waters, and of alterations from one set of conditions to the other.

Geologists are familiar with the amount of physical change which took place over the European area after the coal-growth period. The subsequent condition of surface which resulted is still distinctly traceable. The Perm-Trias period presents true Aralo-Caspian conditions, physically defined, subordinate to the same Continental area.

The marine Jurassic series next in sequence was succeeded by that period of terrestrial conditions to the more detailed physiography of which I here propose to call your attention. It may suffice on this occasion to state that at the end of the great Cretaceous period, the area of those seas, in our hemisphere down to depths at which the great chalk-floor had been deposited, became part of a continental land, on which the fresh-water formations of the times which preceded the marine Nummulitic were accumulated.

These evidences of successive physical conditions over the northern hemisphere indicate an order of recurrence of corresponding conditions, and, as already noticed, of a progress of change which, in the course of each period, came about in a corresponding order. Great periods, during which wide marine conditions prevailed, alternated with others of wide terrestrial surfaces. The marine periods, as we measure them by the products of the agents which seas and oceans call into action, must have been of vast duration. In like manner we may feel assured that the great fresh-water formations are not, as some geologists have supposed them, mere subordinate parts of the great marine groups, as our "Wealden" of the "Cretaceous," but rather true intermediate groups, of equal geological value with them in the estimate of past time.

The Wealden Formation

Mr. Martin proposed this designation for the assemblage of fresh-water depositions exhibited in the counties of Kent, Surrey, and Sussex, and which may be described generally as consisting of thick accumulations of sands and sandstones, for a lower or earlier part, surmounted by a great argillaceous deposit (Weald clay). Mr. Webster suggested the propriety of uniting the Purbeck beds Hastings sands, and Weald clay into one group, the whole being mainly a consecutive fresh-water series. It must be understood, however, that there is not a definite line separating the Hastings sands from the Weald clay; all that is signified is that sands predominate for the lower, and clays for the upper portion of the Wealden depositions; but just as thick bands of clay occur in the lower series, so bands of sandstone occur in the upper.

The arrangement adopted by the Geological Survey, in descending order, is Weald clay, Tunbridge Wells sand, Wadhurst clay, Ashdown sands, Ashburnham beds, which in Sussex are the equivalent of the Purbeck beds of Dorsetshire.

The Lower Sands are well seen on the coast at Hastings, whence they took their name, and extend thence continuously to near Horsham, rising into the central ridge of the Wealden elevations of St. Leonard's, Tilgate, and Ashdown Forests. On every side this tract is bounded by the Weald Clay, which extends to the base of the escarpment of the Lower Green Sand, beneath which it passes.

This surface of fresh-water strata, so defined, extends for seventy miles from E. to W., and has a breadth from N. to S. of thirty-five miles. Over the whole of this area the fresh-water depositions attain a great thickness; the lower sandy group may be taken at 820 feet, and the Weald Clay at 450 feet at least.

To realise the conditions under which these accumulations were formed, the now upraised central Sandstone ranges must be put back to their original horizontal position, and the whole series must be regarded as the infilling by fresh-water rivers of what was an area of depression, with reference to the terrestrial surface of the time. This Wealden formation can be traced far beyond the limits of the denudation of the S.E. counties. In a southerly direction it occurs in the Isle of Wight, with its two divisions of Weald clay and Lower sands. In this quarter the Weald clay is reduced to a thickness of 68 feet. In a westerly direction (Swanage Bay) the Wealden sands have a great thickness, and are surmounted by only a thinnish band of Weald clay or deep-water deposit, and both divisions decrease rapidly, in the extension of the formation across the Isle of Purbeck, and have not been recognised in the Isle of Portland, from which, if they even extended there, they must be denuded off.

In a northerly direction, several sections about Oxford, as from Shotover Hill to Great Hazeley, from Wheatley to Tetsworth, from Brill through Long Crenden to Thame, from Whitchurch to Aylesbury, extending from S.W. to N.E. for a breadth of thirty miles, show Purbeck beds, and fresh-water ferruginous sands passing beneath Cretaceous beds. It is obvious that the Wealden formation has been cut back in this quarter, and that originally it had a much greater extension. In this quarter, too, the ferruginous sands overlap the Purbeck beds, showing that the lake had here widened its area beyond the dimensions of the Purbeck lake.

From Oxford* to the Vale of Wardour is an interval of seventy miles, from over which the Portland Oolite has been removed, except at Swindon, at which place there are beds which are unmistakeably referable to the Purbeck group; and it is a fair inference that it is to this denudation that the absence of the lacustrine depositions is to be attributed, which everywhere on our area, and on much of that of Continental Europe which was adjacent, follow next upon the Portland stage. Such being the case, the smallest possible dimensions which can be assigned to the great Wealden lake, are that it extended from beyond Aylesbury to Portland for 120 miles, and from Portland to the Boulonnais for 200 miles.

From Rye to Portland the Wealden beds pass out of sight beneath the level of the English Channel. The valley of the Channel is the result of the disturbance which produced the E. and W. lines of the South of England, and was produced subsequently to the Nummulitic period.

Dr. Fitton remarks that the subdivisions of the Wealden formation, especially at its upper part, being in some measure arbitrary, it is difficult to determine to which of the three groups any outlying depositions ought to be referred. (Geol. Trans. vi., p. 323.)

Such a difficulty existed when corresponding portions of a formation were supposed to require an agreement in mineral character and composition; but it happened at all times, as now, with respect to the depositions within areas of water, whether of lakes or seas, that the beds which were strictly equivalent in respect of time, varied from place to place, from marginal shingle to submarginal sand-zones, and deeper and most distant argillaceous ar calcareous mud-beds. Considered in this way, the distant Oxford and Buckingham portions of the Wealden formation, are referable to the submarginal accumulations of the great lake, and may be synchronous with "Wealden clays." For the threefold division of the Wealden series into Purbeck beds, Hastings sands, and Weald clay, must therefore be substituted the more natural divisions of Lower Wealden for the Purbeck series, and Upper Wealden for the series as exhibited in the south-east of England may be of sand and sandstone or Weald clay, according to local conditions of depth.

There are indications that changes in the area surrounding the Wealden formation took place in the progress of that series; the lower and earlier sandy deposits indicate only inconsiderable depths of water. Yet the vertical thickness of the series may be estimated at nearly 2,000 feet; for that area at least progressive depression must have been going on, but not uninterruptedly. As regards the upper and lower divisions of the formation, the difference consists in the greater coarseness of the detritus of the upper, and in the evidence of strong currents settling in definite directions in an extension of the area and of an increased depth, so that at the later stage a central area of deep-water depositions may be defined as well as the directions in which such conditions thinned away. Great changes took place in the depth of the water of the lake, as indicated by the alternations of the drift-sand beds with deeper-water mud deposits, and in places by the conversion of lake-bed into land-surface, upon which plant-growths established themselves for considerable periods of time, and which were again submerged.

Such changes as these seem to imply change in the physical geography of the land region to which this great fresh-water area was subordinate—such, for instance, as would give rise to larger rivers, great influx of fresh waters, and stronger currents.

The successive conditions indicated by the great Wealden group, as a whole, are, for the first stage, that of an extensive shallow lake, or sound, at the sea-level of the time, the inflowing waters to which were largely charged with lime derived from the surface of Portland Oolite, from which they came. This is the Purbeck stage, which commenced with a long period of purely fresh-water conditions. Brackish-water conditions followed with a change of fauna. Mollusca, such as *Corbula*, *Cardium*, *Modiola*, *Rissoa*, appear, presenting—as was observed by the late Edward Forbes—the change of character which the Caspian-sea Molluscs have at present in adapting themselves to brackish-water.

During the Middle Purbeck series the alternations from fresh to brackish water conditions were frequent, and apparently of short duration, till finally it was closed as it commenced, by a thick set of purely fresh-water depositions.

The changes in the Purbeck series are readily accounted for by reference to areas of water such as occur on the American coast at present, and which may be salt or brackish, according to the extent to which the sea-waters are excluded by sand-bars from mixing with the fresh waters flowing from the land.

The S. and E. coast-line of our Wealden lake must be looked for beyond the area of our island.

Wealden Formations of the European Surface

The elliptical form of the Wealden elevation and denudation has its completion on the east in Picardy, across the English Channel. In the Boulonnais there occur ferruginous sands like those of Shotover, full of fresh-water shells (*Unio*) overlying Purbeck limestone, and passing beneath the Cretaceous formation, just as happens in this country. These Wealden beds are not now of any considerable thickness, having been reduced by the denudation of the district. They are so mixed up with pebble-beds in places as clearly to indicate a marginal line, which may safely be placed to the north of the Boulonnais denudations; for the Wealden depositions proper hardly rise to the level of the Palæozoic rocks of Marquise. The great fissures and pot-holes in the limestones there, which have been produced under subærial conditions, and filled with sand, mould, and much vegetable matter, had been produced antecedently to the deposition of the Gault over that area.

The Wealden beds of the Boulonnais were formed beneath the waters of the same lake as our own. This fresh-water area had an extension southwards; thus M. D'Archiac refers the mottled clays beneath the iron sands and sandstones at Havre to the Wealden series of this country, so that the limits of our lake in that direction, or in the south, lay somewhere along the line of the English Channel.

Sixty miles to the south of the Boulonnais is a district known as the Pays de Bray, which is an elliptical valley of elevation and denudation, like our own Wealden on a small scale, extending from Beauvais to Neufchâtel, a distance of forty-five miles. In this denudation the lowest beds exposed belong to the marine Jurassic series (Portland Kimmeridge). Next above the Portland stone is a Wealden formation. "Les dépots regardés comme fluviatiles sont les plus voisins de létage Portlandien et forment le groupe inférieur du terrain Néocomien" (Graves, Oise, p. 55). The remains of the fishes, *Cyrene*, *Cyprides*, and ferns are such as occur in our Wealden.

* *Vide* evidence as to range of Wealden deposits, Phillips' "Geology of Oxford."

The thickness of this fresh-water formation is inconsiderable compared with our Wealden. The separation of the fresh-water formation from the marine Portland is well defined; not so that betwixt the Wealden and Neocomian. Here, as in the Punfield section, the fresh-water and marine conditions seem to have alternated, and the manner in which this takes place suggests the supposition that the influx of a considerable body of fresh water from the land of the time took place not far from this place.

Neufchâtel is seventy miles south of Boulogne; the Wealden beds, as we have seen, indicate that the series extended southwards from Marquise; and it is no unreasonable supposition that the deposits of this Pays de Bray were formed under the waters of the same lake as were those of our own Wealden.

Such, then, were the dimensions of the Wealden Lake or Sound; it extended from parts of Buckingham, on the north, half across the English Channel on the south, a breadth of 160 miles. In the contrary direction it reached from Wiltshire into France, beyond Beauvais for 250 miles.

In another part of France, Depart. de l'Aube, Mr. Cornuel has described a fluvio-lacustrine formation between the Jurassic and Cretaceous formations at Vassy, containing *Iguanodon*, several species of *Unio* and *Planorbes*. The lacustrine formation at Cimey is in a corresponding geological position.

In the Jura, Villers, Forcine-le-bas, the Portland beds are followed by hard bluish marls, calcareous marls, and gypsum, the whole very like our Purbeck series. These lacustrine formations are interesting, as they seem to show the existence of a chain of lakes stretching across France into Switzerland for 260 miles, with a general direction parallel to the axis of Artois, and thus connected as part of one great lake system with our Wealden.

In France, Depart. des Deux Charentes, some 350 miles due south of our Sussex coast, there occurs a great fresh-water formation in intermediate position between the Portland Oolite and what were then the lowest beds of the Cretaceous series. Like our own Wealden, this also is exhibited over a surface from which the Cretaceous strata have been denuded. This formation has engaged the attention of many French geologists, more particularly of M. Coquand, who has determined its age and purely lacustrine character, and who puts it as the equivalent of the Purbeck beds of England; in this he seems to be guided by the general likeness as to composition and the presence of *Physa Bristowi*, a well-known Purbeck species.

The sequence of events at this place was as follows:—Subsequently to the formation of the Portland Oolite the sea-bed became terrestrial surface, and subsequently again to that, a depression extending from Chateauneuf, near Angoulême, to beyond the Island of Oléron, became the site of a great fresh-water lake. From St. Jean d'Angely to Chateauneuf is a distance of thirty-five miles, and from Chateauneuf to Oléron, S.E. to N.W., is upwards of 100 miles; but then figures do not give the full dimensions of this fresh-water area, as its deposits have been reduced by denudation on the north, and passes beneath the Cretaceous series on the south. The original lake must have had a westerly extension seawards, and its area must have equalled that of Lake Ladoga.

The feeders of this lake are more easily accounted for than in the case of our own Wealden. Such a lake would necessarily have received all the streams descending from the western slopes of a terrestrial surface of very ancient date, namely, the granitic district of Central France.

In North Germany there is a well-exhibited Wealden formation, extending from Bentheim by Rheine, with a breadth from north to south of twelve miles. From Ibbenbüsen it reaches on the south side of the Triassic and Palæozoic axis of Osnaburg for many miles. It is everywhere in an intermediate position betwixt the Upper Jurassic and Lower Cretaceous formations. On the north of the axis it spreads for seventy miles to Minden, certainly as far as north as the Steinhuder Meer to near Hanover, and as far south as the Hils district. From west to east the ascertained extent of this lake is upwards of 120 miles.

At Bentheim the dark Wealden clays, with bands of limestone and spathic iron ore, with *Cyrenæ*, *Melaniæ*, &c., like those of Sussex here, are 400 metres thick, so that the real dimensions of this northern lake were very much greater than has been here given.

These large lacustrine areas imply that there was at that time a corresponding extent of terrestrial surface. And it may fairly be asked, what is the geological evidence of such a condition? There occur over parts of Belgium the remains of such a terrestrial condition of surface beneath the lower Cretaceous beds there (Tourtia) consisting of variegated sands and clays, with much diffused vegetable matter, and occasionally with beds of lignite; such surfaces can be traced along the line of the Belgian coal-field (Mons), and overlying parts of the Palæozoic series. These beds are not of sufficient dimensions to be termed lacustrine, but have all the characters of the deposits of ponds and marshes; and M. Dumont has properly referred them to the Wealden period. Such like evidence of terrestrial conditions recur over a wide European area; such are the subcretaceous beds of pisiform iron ore, of subaerial origin, and the wide area over which fresh-water sands with *Pterophyllum*, *Pecopteris*, *Cycadises*, &c., of our Wealden are met.

The break betwixt the marine Jurassic and Cretaceous formations is very distinct, physically and zoologically; and it may be fairly asked, in what way do the forms entombed in the products of the intercalated period of terrestrial surface conditions serve to throw any light on what took place during that long interval of time?

That the earliest Purbeck-Wealden fauna should have Jurassic relations, that is to say, that it must have synchronised with such, wherever that formation was being continued, is only what might be expected; for the whole of the bed of the Jurassic seas in the northern hemisphere was not converted into subærial surface at once. Midway in the course of the Purbeck-Wealden series there is evidence of the recurrence of marine conditions with Portlandian forms, such as *Ostra distorta* and *Hemicidaris purbeckensis*. It was on this ground that Prof. E. Forbes suggested the propriety of placing the Purbeck series with the Jurassic in systematic grouping; for it showed that up to the time of the Middle Purbeck beds the marine fauna of the nearest seas was still Jurassic.

The considerable extent of land surface in the northern hemisphere during the whole of the marine Jurassic period, and the local conversion of any portions of such sea-bed into land, whether in the course of the deposition of the Lower Jurassic series (Stonesfield), or between the Lower and Middle (Brora, Staffin), or at the uppermost stage (Portland), would be merely the addition of so much more to the existing land.

The forms of life which would colonise such new surfaces would be such as migrated from the older adjacent lands; if any change should have taken place in the fauna or flora of such old land-surface, in the course of the production of the marine Jurassic series, it would be recorded in the forms entombed in the lacustrine formations of the several stages here alluded to.

The fossil plants and fresh-water shells from Brora, Lock Staffin, and the Wealden seemed at first to certain well-known and competent naturalists to show that an identical set of forms ranged throughout. A minutely critical examination has since indicated shades of difference; yet it may be questioned whether such are greater than different localities in the same zoological province now present, allowance being made for differences of these old estuarine and lacustrine areas.

The relations of the land-surface forms of the Wealden formations of the European surface has been recognised by all naturalists as being Jurassic rather than Cretaceous. In this the Purbeck-Wealden group offers an exact counterpart zoologically and geologically, of the Permian-Trias group; just as the marine zoological relations of the Permian are Palæozoic, so those of the Purbeck are Jurassic; and when next after each of these, and after the wide spread of purely marine conditions over the northern hemisphere at each period, the marine fauna is seen to have undergone a complete change, in the one case Palæozoic forms go out, and for ever, to be succeeded by Mesozoic or Jurassic; in the other Jurassic forms go out and the Lower Cretaceous come in, and are those which interchange with the uppermost Wealden fauna at Punfield and the Pays de Bray.

Did time allow, I might call attention to the results of the labours of the distinguished palæontologists who have described the forms of life of the Wealden period, both of animals and plants. From them we know that crocodiles and chelonians, referable to many genera, abounded in the Wealden waters. These, with the cycadeæ of the land, sufficiently mark the temperature of that time as being much higher than it is here at present. With respect to the numerous large terrestrial Dinosaurs, it is observable that as yet they are nearly all peculiar to our Wealden lake. The relative level of this lake seems throughout to have been such as to have admitted of easy communication and interchange with the waters of the sea; and this condition may serve to account for some of the peculiarities which its fauna presents.

SECTION D

SUB-SECTION ANTHROPOLOGY

Opening Address by the President, Colonel A. Lane Fox

After some opening remarks, the author said :—"As one of those who for some years past have taken part in those practical measures which have been as yet only partially and feebly instrumental in promoting the union of the anthropological sciences, it occurs to me that the present occasion may be a fitting one for expressing some of the views which have suggested themselves to me in the course of my experience whilst so engaged. I propose, therefore, after considering briefly the existing phases of one or two of the more important questions with which anthropology has to deal, and saying a few words on the relative value of certain classes of evidence, to speak of the anomalies and misadjustments in what may be called the machinery of anthropological science, defects in the existing constitution of some of the societies which either are or ought to be included amongst the branches of our great subject. In the remarks which I shall offer upon this subject it is not my wish that any undue weight should attach to the particular suggestions which I may be called upon to make as in any way emanating from this chair. My object is rather to draw the attention of anthropologists to the urgent necessity which exists for better organisation than to propound any particular schemes of my own; indeed, so rapidly do our views change in the infancy of a science that I should be sorry to bind myself over to accept many of my own opinions a couple of years hence, for there is, perhaps, no branch of study to which we may more truly apply the dictum of Faraday that "the only man who ought really to be looked upon as contemptible is the man whose ideas are not in a constant state of transition."

Amongst the questions which anthropology has to deal with, that of the descent of man has been so elaborately treated, and at the same time popularised by Mr. Darwin, that it would be serving no useful purpose were I to allude to any of the arguments on which he has based his belief in the unbroken continuity of man's development from the lower forms of life. Nor is it necessary for one to discuss the question of the *monogenesis* or *polygenesis* of man. On this subject also Mr. Darwin has shown how unlikely it is that races so closely resembling each other, both physically and mentally, and interbreeding as they invariably do, should on the theory of development have originated independently in different localities. Neither are we now, I think, in a position to doubt that civilisation has been gradually and progressively developed, and that a very extended, though not by any means uniform, period of growth must have elapsed before we could arrive at the high state of culture which we now enjoy. The arguments of our sectional president, Sir John Lubbock, on this subject may, I think, be accepted generally as those of the best exponent of these views in our own time; such was the opinion, as we learn from various authorities, that was held by most of the ancient authors, and it tallies in all respects with the phenomena of progress now observable in the world around us, or which have been recorded in history. . . .

How far the first beings worthy of being called men may have possessed superior organic psychical powers to their predecessors, and whether the superior functions of the human mind were developed slowly or rapidly is a point on which it is more difficult to form an opinion. In contrasting the psychical differences between man and the lower animals, it is so invariably the practice, and indeed so impossible to avoid including in our estimate of the human intellect all that conscious education and unconscious infantile culture has added to the powers of the mind, that unless we were able to try the experiment of the Egyptian king, and send children to be brought up with animals apart from all intercourse with the human race, we could not place ourselves in a position to compare truly the innate capacities of the two, or to form any just estimate of the difficulties which primæval man, even supposing him to have possessed mental powers equal to our own, must have encountered in the first stages of human culture. It has been shown by Prof. Huxley and others that there is really no cerebral barrier between men and animals, nor does it appear beyond the pale of possibility that a slight increase in the vividness or permanence of the impressions of external objects upon the mind over that possessed by the brutes, might, by marking more clearly the sequence of events, be sufficient to imitate that faculty for improvement which is the special characteristic of man.

Be that as it may, there is, I believe, nothing in the constitution of our own minds which can lead us to doubt that the progress of our first parents must have been extremely slow, or that the slight improvement observable in the implements of the neolithic over those of the palæolithic age, did actually correspond to the continuous progression of human culture during enormous periods of time.

Now, if it is true that during the countless ages included in the palæolithic and neolithic periods, which we know to have been marked by great geological changes, by the union and separation of great continents, by great changes of climates, and by the migration of various classes of fauna into distant parts of the earth, the progress of mankind was as slow and gradual as we are warranted in supposing it to have been by the relics which have been left us, considering how short the period of history during which the rapid development of civilisation has taken place is, in comparison with the long periods of time of which we have been speaking, and that progress is always advancing at a rapidly increasing ratio, we need find no difficulty in supposing that where savages are now found in the employment of implements corresponding to those of the neolithic age, they present us with fairly correct pictures of neolithic culture, being really in point of time only a little behind us in the race of improvement. It is reasonable also to suppose that the use of such tools by savages, and the culture associated with them, was also like that of our neolithic parents inherited from lower conditions of life, and that being slow and continuous, it was sufficiently stable to enable us to trace connections between people in the same stage now widely separated, and between them and our own neolithic ancestors.

The most remarkable analogies are in reality found to exist between races in the same condition of progress, and it is to the study of these analogies, with the view of ascertaining their causes and histories, that the attention of anthropologists has of late been especially drawn, and on this subject I propose to make a few observations.

There are two ways in which it has been attempted to account for those analogous coincidences, one by the hypothesis of inheritance to which I have already referred, the other by the view of the independent origin of culture in distant centres, assimilated in consequence of the similitude of the condition under which it arose. It is said that the wants of man being identical, and the means of supplying those wants by external nature being alike, like causes would produce like effects in many cases. There can be little doubt that many remarkable analogies have arisen in this manner, especially amongst the very variable myths, customs, religions, and even languages of savage races, and that it would be dangerous to assume connection to have existed except in cases where a continuous distribution of like arts can be traced. On the other hand, we should commit a grave error if we were to assume the hypothesis of independent origin, because no connection is found to exist at the present time, for we are as yet almost entirely ignorant of the archæology of savage and barbarous races. It is but fifteen years since we began to study the prehistoric archæology of our own race, which has already carried us so far on the road towards connecting us with savages; and can we say what further connections may be brought to light when the river drifts of such rivers as the Niger or the Amazons come to be studied. Nor can it fairly be said that the wants of mankind are alike in all cases; for if we adopt the principle of evolution, it is evident that the wants of man must have varied in each successive stage of progress, diminished culture being associated with reduced wants, thus carrying us back to a condition of man, in which, being analogous to the brutes, he could scarcely be said to have any wants at all of an intellectual or progressive character.

It would be an error to apply either of these principles exclusively to the interpretation of the phenomena of civilisation. In considering the origin of species, we are under the necessity of allying ourselves either on the side of the *monogenists* or that of the *polygenists*, but in speaking of the origin of culture, both principles may be, and undoubtedly are, applicable; there is in fact no royal road to knowledge on this subject by the application of general principles; the history of each art, custom, or institution, must be diligently worked out by itself, availing ourselves of the clue afforded by race as only the most probable channel of communication and development. We may be certain however that in all cases culture was continuously and slowly developed. . . .

There is but one existing race the habits of which are sufficiently well known, which can be said to present in any great degree the characteristics of a primæval people, and that is the Australians. As I have elsewhere noticed, all the weapons and tools of the Australians, whatever the uses to which they are applied, are closely allied to each other in form. The spear, the club, the malga, the boomerang, and the heileman, or rudimentary shield, all pass into each other by sub-varieties and connecting links, and all consist of the but slightly modified natural forms of the stems of trees, and other natural productions. The Australian in his arts corresponds the most closely of any people now living to those of the palæolithic age. His stone axe is sometimes held in the hand when used, and like the palæolithic man, he has not yet conceived the idea of boring a hole through it for the insertion of a handle. In some cases he cannot without instruction even understand the use of such a hole when he sees it in the axes of European manufacture. A most remarkable instance of this was brought to my notice not long ago by Mr. Grimaldi, who found on the site of a deserted native camping-ground, a European axe having a hole for the handle, which the natives, unable to conceive the use of this part, had filled up with gum, and hafted by means of a withy bent round the outsides of the hole, in accordance with their traditional custom. In the temporary museum established here during the meeting of the Association, you will see a case containing knives of stone, glass, and iron, all of exactly the same form, and hafted, if one may use such a term for the attempt to form a handle, precisely in the same manner; showing with what tenacity these people retain their ancient forms, even after they have been supplied with European materials.

Now it has been shown that in some cases—and here I especially refer to the account lately published by Mrs. Millett, of the Native School, established under conditions only partially favourable to its success, in the interior of Western Australia *—The Australians are found to be not only capable, but even quick in receiving instruction. It is evident, therefore, that we should be wrong if we were to attribute the extraordinary retardation of culture on the Australian continent to racial incapacity *alone*; racial incapacity is one item, but not the only item to be considered in studying the development of culture.

The earliest inhabitants of the globe as they spread themselves over the earth, would carry with them the rudiments of culture which they possessed, and we should naturally expect to find that the most primitive arts were, in the first instance, the most widely disseminated. Amongst the primæval weapons of the Australians I have traced the boomerang, and the rudimentary parrying shield—which latter is especially a primitive implement—to the Dravidian races of the Indian peninsular and to the ancient Egyptians, and although this is not a circumstance to be relied upon by itself, it is worthy of careful attention in connection with the circumstance that these races have all been traced by Prof. Huxley to the Australoid stock, and that a connection between the Australian and Dravidian languages has been stated to exist by Mr. Morris, the Rev. R. Caldwell, Dr. Bleek, and others.† And here I must ask for one moment to repeat the reply which I have elsewhere given to the objection which has been made to my including these weapons under the same class, "that the Dravidian boomerang does not return like the Australian weapon." The return flight is not a matter of such primary importance as to constitute a generic difference, if I may use the expression, the utility of the return flight has been greatly exaggerated; it is owing simply to the comparative thinness and lightness of the Australian weapon. All who have witnessed its employment by the natives, concur in saying that it has a random range in its return flight. Any one who will take the trouble to practise with the different forms of this weapon, will perceive that the essential principle of the boomerang, call it by whatever name you please, consists in its bent and flat form, by means of which it can be thrown with a rotatory movement, thereby increasing the *range and flatness of the trajectory*. I have practised with the boomerangs of different nations. I made a *fac simile* of the Egyptian boomerang in the British Museum, and practised with it for some time upon Wormwood Scrubs, and I found that in time I could increase the range from fifty to one hundred paces, which is much farther than I could throw an ordinary stick of the same size with accuracy. I also succeeded in at last obtaining a slight return of flight; in fact it flies better than many Australian boomerangs, for they vary considerably in size, weight, and form, and many will not return when thrown. The efficacy of the boomerang consists entirely in the *rotation*, by means of which it sails up to a bird upon the wing and knocks it down with its rotating arms; very few of them have any twist in their construction. The stories about hitting an object with accuracy behind the thrower are nursery tales; but a boomerang, when thrown over a river or swamp will return and be saved. . . . . To deny the affinity of the Australian and Dravidian or Egyptian boomerang on account of the absence of a return flight would be the same as denying the affinity of two languages whose grammatical construction was the same because of their differing materially in their vocabularies.

(*To be continued.*)

* "An Australian Parsonage, or the Settler and the Savage," by Mrs. E. Millet, chap. vii.

† Journal of the Anthropological Institute, No. 1., vol. i., July 1871.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Kinetic Energy

PROFESSOR EVERETT asks from what source is the gain of Kinetic energy in water which has flowed from higher to lower latitudes derived? I answer, undoubtedly from the earth's rotation. If so, it will be asked, what becomes of the Kinetic Energy which disappears when water flows from a lower to a higher latitude? Mr. Ferrel, a physicist of high authority in all questions relating to the earth's rotation, says that it is all consumed in friction. "If a free body on the earth's surface," says Mr. Ferrel, "should be moved from a lower to a higher latitude without friction by a force in the direction of the meridian, it would acquire a certain amount of relative eastward velocity, which would be the same whether the body moved toward the pole with a very slow uniform velocity arising from a single impulse, or whether it moved with a continual accelerated velocity down a gradient by the force of gravity. If a particle of atmosphere or of the ocean is moved in the same way by a similar force, and does not acquire the same amount of relative eastward velocity, *the difference between the velocities in the two cases is the true measure of the effect of friction.*" (NATURE, June 13).

In my last two letters on the subject, I have inadvertently made a similar statement. But as regards the amount of energy lost being the measure of the effect of the friction, we are, I fear, evidently both wrong. A considerable amount of the 9,025 foot-pounds of energy would be consumed, not in friction, but in work of rotation. But let it be observed that so far as the argument under discusssion is concerned, it is a matter of perfect indifference in what way the energy is consumed. The point which Prof. Everett, Mr. Ferrel, and all those who defend Dr. Carpenter's theory has to explain is this, *viz.*, How is it that six foot-pounds of energy can carry a pound of water from the equator to latitude 60°, while during the passage of the pound of water not less than 9,925 foot-pounds of energy is consumed in overcoming the resistance to its eastward motion? How is it that in a fluid, in which the molecular resistance to motion is equal in all directions, a body manages to lose 1,500 times more energy in moving in one direction than it does in another, and yet the velocity of motion is the same in both directions? Then if this cannot be explained, how is the gravitation theory of oceanic circulation to be maintained?

JAMES CROLL

Edinburgh, August 9

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ERRATA.—Vol. vi. p. 273, col. 1, line 31, for "Lenou's" read "Lesson's"; line 45, for "special" read "spiral"; line 57, for "fold" read "folds"; col. 2, line 6, for "Edentata" read "Edentate."

THE BRITISH ASSOCIATION.—*Authors of papers are requested to favour the Editor of* NATURE *with copies or abstracts of their communications as soon as possible, addressed to him at the Post Office in the Reception Room, as by these means alone can an accurate and early notice be insured. The Editor appeals to men of science to aid him in his attempt to give an account of the results of their investigations to their brethren throughout the world.*