THURSDAY, SEPTEMBER 7, 1876

SCIENTIFIC WORTHIES

IX.—Sir William Thomson.

SIR WILLIAM THOMSON was born in Belfast in June 1824. His father, Dr. James Thomson, was a very remarkable man. The family of Thomsons had for several generations occupied a farm near Ballynahinch, in County Down, Ireland; but James Thomson, when quite a boy, endeavouring all alone to understand the principles of dialling, was led into the study of mathematics, for which it soon appeared that he possessed an extraordinary capacity. His father then permitted him to go to a small classical and mathematical school at his native place, and there he was soon promoted to be an assistant teacher. Without ceasing to labour as a teacher for his own support, he became a student in the University of Glasgow, attending there during the winter months, and teaching at Ballynahinch during the summer.

When he had nearly completed his fifth year at Glasgow, he was appointed Headmaster of the School of Arithmetic and Geography at the Royal Belfast Academical Institution, and subsequently Professor of Mathematics in that Institution. In 1832 he was appointed Professor of Mathematics in the University of Glasgow, and removed thither with his family. He was the discoverer of many improvements in algebra and in the calculus, and in particular, he was the first to apply systematically Horner's method of solving algebraic equations to the arithmetical extraction of cube roots and higher roots of numbers. He was also the author of several important educational works which have come extensively into use; and besides excelling in science, he was highly accomplished in classical and in general literature. But he is perhaps better remembered in Scotland for his success as a teacher; and those who were his pupils speak with delight of his voluntary catechetical hours, in which *vivâ voce* questions were proposed and rapidly passed from bench to bench in a class of ready and enthusiastic pupils.

There are many who remember among the readiest and the most enthusiastic, a little lad of eleven or twelve years of age who could scarcely make himself seen among his older class fellows. This was William Thomson, who at that early age had entered the University; and who even then distinguished himself greatly for originality and high mathematical ability. Having passed through Glasgow University he entered St. Peter's College, Cambridge. In 1845 he graduated as second wrangler and first Smith's Prizeman, and was immediately elected a Fellow of his college. While he was at Cambridge he was remarkable for his scholarly attainments in science and in literature. He won the Colquhoun, and was for some time president of the Cambridge University Musical Society.

After completing his undergraduate course at Cambridge he went to Paris, and spent some time working in the laboratory of Regnault, who was then engaged in some of his most important researches. After the death of Dr. Meikleham he became a candidate for the Chair of Natural Philosophy in the University of Glasgow, and was elected. Thus in 1846, at the early age of twenty-two, he was appointed to the Chair which he has filled with such distinction, and still holds.

Sir W. Thomson's earliest contributions to physical science were a defence of Fourier in answer to a charge of erroneousness which had been brought against some of the fundamental formulas of his harmonic analysis, and a paper on "The Uniform Motion of Heat in Homogeneous Solid Bodies, and its Connection with the Mathematical Theory of Electricity." These were written at the age of seventeen. They were published in 1841 and 1842 in the *Cambridge and Dublin Mathematical Journal*. The latter paper is a very remarkable one: its spirit runs through much of Sir William Thomson's subsequent work. He points out in it the analogy between the theory of the conduction of heat in solid bodies and the theory of electric and magnetic attraction; and, aiding himself with this analogy, he makes use of known theorems as to the conduction of heat in order to establish some of the most important theorems in the mathematical theory of electricity. The method was thoroughly original; and later, taken in conjunction with Faraday's admirable researches on electrostatic induction, which led to the discovery of differences in the specific inductive capacity of various substances, and to the notion of *conduction of lines of force*, it proved of the highest value in the discussion of questions in electrostatics and also in magnetism. As to the results obtained, Thomson found, a few months after, that in some of the most important he had been anticipated by M. Chasles. Later he found that Gauss had given the same general theorems shortly before Chasles independently rediscovered them; and, three years after, having heard of a paper by Green, but long inquired for it in vain, he found, on obtaining a copy of that paper, that all these theorems had been discovered and published in the most complete and general manner, with rich applications to the theory of electricity and magnetism, as early as 1828. This memoir of George Green, of Nottingham, was printed privately, dedicated to his patron the Duke of Newcastle, and it lay unread and unknown till 1845, when Thomson obtained a copy, made known what a mine of wealth it contained, and had it republished in *Crelle's Mathematical Journal*.

Another very important paper written about the same time, and published in the *Cambridge and Dublin Mathematical Journal* for 1842, was on the "Linear Motion of Heat." It contained the foundation of the method of evaluating absolute geological dates from underground temperature, which he made the subject of his inaugural address on his institution to his professorship in the University, and which we believe forms a large part of the subject of his opening address to the Mathematical Section of the British Association.

The papers referred to were followed by a paper on the "Elementary Laws of Statical Electricity," which first appeared in *Liouville's Journal de Mathématiques*, in 1845, and was translated and published the same year in the *Cambridge and Dublin Mathematical Journal*. Sir W. Snow Harris had undertaken an experimental examination of the fundamental laws of electric attraction and repulsion; and his results, which received the Copley Medal of the Royal

Society, were at the time supposed to disprove the well-known laws first given by Coulomb. Thomson, however, at the age of twenty-one, undertook the examination of the results of Snow Harris, and showed that, instead of being out of harmony with the laws of Coulomb, they were, so far as they went, confirmatory of those laws. He pointed out clearly the precautions necessary in experiments on the elementary laws, and he showed that it was through a misunderstanding as to the conditions of the simple laws enunciated by Coulomb that Snow Harris was led into error. In this memoir we find also the first steps towards making Faraday's new theory of induction the basis of the mathematical theory of electricity. In subsequent papers this method of proceeding to the mathematical theory is completely worked out; and, reading together the memoirs of Faraday and of Thomson, we cannot help being struck with the way in which the notion of lines of force and lines of flow of heat fascinated the minds and guided the intuition of our two greatest investigators.

We cannot here follow Sir William Thomson in detail through his series of papers on electrostatics and on magnetism. They were collected and published in 1872, with notes and most important additions, in a volume of 600 pages. It is greatly to be desired that the same may be done for the very numerous memoirs on other physical subjects with which he has enriched the *Transactions* and *Proceedings* of a host of learned Societies.

In 1846 Mr. Thomson became editor of the *Cambridge and Dublin Mathematical Journal*, a post which he held for about seven years. Among the contributors to the journal during his editorship he could count Stokes, Cayley, De Morgan, Liouville, Salmon, Sir William Rowan Hamilton, and many other distinguished mathematicians; while from his own pen proceeded many memoirs of great importance. It was about this time, also, that he contributed to *Liouville's Journal de Mathématiques* the memoirs in which he unfolded his principle of "electric images." By means of this principle, which he in his first letter likens to Brewster's kaleidoscope, he shows how, by simple geometrical principles, to solve many problems of an apparently very complicated nature, as to the distribution of electricity on a system of conductors under the influence of a given electrified system. The veteran Liouville, concluding a note suggested by the letters of Mr. Thomson, writes of his own developments of the theory: "Mon but sera rempli, je le répète, s'ils peuvent aider à bien faire comprendre la haute importance du travail de ce jeune géomètre, et si M. Thomson lui-même veut bien y voir une preuve nouvelle de l'amitié que je lui porte et de l'estime que j'ai pour son talent."

His electrostatic researches led Thomson to the invention of very beautiful instruments for electrostatic measurement. The subject of electrostatic measurement occupied much of his attention from the very earliest, when he was obliged to call attention to the defects of the electrometers of Snow Harris. His labours in this direction have produced the quadrant electrometer, which is employed for all kinds of electric testing in telegraph construction, and for the registration of atmospheric electricity at Kew Observatory; the portable electrometer, for atmospheric electricity and for other purposes in which the extreme sensitiveness of the quadrant-electrometer is not required; and the *absolute* electrometer, which serves for reducing the scale readings of other instruments to absolute measure, and which was used by Thomson in his measurement of the electrostatic force producible by a Daniell's battery and in many other investigations. Those who have seen the collection of electrometers in the Loan Collection at South Kensington will not think it too much to say that to Sir W. Thomson is due our present system of practical electrometry.

But while thus engaged in investigations in electrostatics and magnetism, there were many other branches of science that were receiving from him advancement in a not less remarkable way. There is no part of his work of higher importance than his investigations on the Dynamical Theory of Heat. These were communicated in a series of papers to the Royal Society of Edinburgh, the first of which was given in 1849. It was a critical account of Carnot's memoir of 1824, "Réflexions sur la Puissance Motrice du Feu." Though Rumford and Davy had, in the beginning of this century, experimentally disproved the material theory of heat, their experiments and arguments were unheeded and nearly unknown; and it was only after 1843, when Joule actually determined the dynamical equivalent of heat, that the great truth that heat is a mode of motion was admitted and appreciated. Thus Carnot, although dissatisfied with it, was obliged to adopt the material theory of heat in 1824; and, regarding heat as indestructible, spoke of the letting down of the heat from a higher to a lower temperature, and looked on the production of work by the heat engine as a phenomenon analogous to that in which water, descending from a higher to a lower level, does work by means of a water-wheel. Thomson, among the first to appreciate the importance of Joule's results, set himself to alter the theory given by Carnot into agreement with the true theory; and in the series of papers referred to, placed the whole science of Thermodynamics on a thoroughly scientific basis. In 1846 he first suggested the reckoning of temperature on an absolute thermodynamic scale independent of the properties of any particular substance. Subsequently, in consequence of experimental investigations of the thermodynamic properties of air, and other gases, made in conjunction with Joule, he showed how to define a thermodynamic scale of temperature having the convenient property that air thermometers and other gas thermometers agree with it as closely as they agree with one another. This system of reckoning temperature gives great facility for the simple expression of thermodynamic principles and results.

Having here mentioned Joule and Thomson together, we cannot omit to remark that some of the most admirable researches in thermodynamics were those undertaken in conjunction by these two attached friends.

Among the many important results of Sir W. Thomson's investigations in thermodynamics, one of the most remarkable was his discovery of the principle of dissipation of energy, announced by him in 1852. During any transformation of energy of one form into energy of another form there is always a certain amount of energy rendered unavailable for further useful application. No known process in nature is exactly reversible, that is to say, there is no known process by which we can convert a given amount of energy of one form into energy of another form, and then, reversing the process, reconvert the energy of the second form thus obtained into the *original quantity* of energy of the first form. In fact, during any transformation of energy from one form into another, there is always a certain portion of the energy changed into heat in the process of conversion; and the heat thus produced becomes dissipated and diffused by radiation and conduction.

Consequently there is a tendency in nature for all the energy in the universe, of whatever kind it be, gradually to assume the form of heat, and, having done so, to become equally diffused. Now, were all the energy of the universe converted into uniformly diffused heat, it would cease to be available for producing mechanical effect, since for that purpose we must have a hot *source* and a cooler *condenser*. This gradual degradation of energy is perpetually going on; and sooner or later, unless there be some restorative power, of which we at present have no knowledge whatever, the present state of things must come to an end.

We must pass very briefly over a large number of Sir W. Thomson's contributions to science that, were our limits less circumscribed, we would gladly dwell upon. In 1855 his paper on "Electrodynamics of Qualities of Metals" was made the Bakerian Lecture for the year. This paper represents a marvellous amount of ingenuity and labour and contains, most valuable new results that, strange to say, are only now beginning to be known. In it was announced his discovery of the electric convection of heat, and a great number of most important new relations between thermal and electric properties of matter. It is interesting to remark that it was while engaged in these investigations that Thomson first called in the experimental aid of his students, and thus made a beginning of the Glasgow Physical Laboratory.

We can do no more than mention here Sir William Thomson's *proof* of electricity of contact, his calculation of the size of atoms, his memoir on the mechanical energies of the solar system, his determination of the rigidity of the earth, his researches on the tides in connection with a British Association Committee on that subject, and his recent splendid researches on vortex motion, as we have still to refer to his connection with submarine telegraphy.

In 1854 Faraday, with an experimental cable, investigated the cause of the *retardation of signals* first observed in the working of the cable between Harwich and the Hague. Thomson, taking up the question published an investigation of the nature of the phenomenon, one practical result of which was that with cables similar in lateral dimensions the retardations are proportional to the *squares of the lengths*. This law is now commonly referred to as the "law of squares." About this time it was proposed to construct a cable to connect England with America; and it became obvious that the discovery of the retardation of signals raised a question whether the transatlantic cable would not prove a commercial failure. Whitehouse, experimenting with 1,125 miles of cable, found the transmission of an instantaneous signal to the farther end of the cable to occupy one second and a half. The length of a cable required to connect Ireland with Newfoundland is twice that of the experimental cable of Whitehouse; and thus, according to the law of squares, the time taken to transmit an instantaneous signal through a cable similar in lateral dimensions to that of Whitehouse, and joining those two places, would be no less than *six seconds*. In 1856 Whitehouse read a paper before the British Association, in which he described experiments by which he hoped to disprove the law of squares. Thomson replied in the *Athenæum* (Nov. 1, 1856); and subsequent experiments have established the correctness of his law.

Fortunately a true understanding of the nature of the phenomenon of retardation led Prof. Thomson to the method of overcoming the difficulties presented. The disturbance produced at the extremity of a long submarine cable by the application for an instant of electromotive force at the other end is not, as in the case of a signal through an overhead land-line, a pulse, practically infinitely short, and received only a minute fraction of a second after it was communicated. Instead of this, a long wave is observed at the farther extremity, gradually swelling in intensity, and as gradually dying away. Its duration for such a cable as we have been speaking of would be the whole six seconds, calculated from the experiments of Whitehouse. Prof. Thomson perceived that an instrument was required which should give an indication of a signal received long before the wave has acquired its maximum intensity, and in which the subsequent rising to maximum intensity should not render unreadable a fresh signal sent quickly after the previous one. This was effected by his "mirror galvanometer"; and it was by means of it that the messages transmitted through the 1858 Atlantic cable were read.

The 1858 cable, submerged under difficulties that many times threatened to be insurmountable, soon failed. Several important messages were, however, transmitted through it; and it served to *prove* the feasibility of the project which many eminent engineers up till that time regarded as chimerical. Before another attempt was made the labours of Prof. Thomson and others, to all of whom the world owes a deep debt of gratitude, had so improved the construction of the cables and the mechanical arrangements for submersion, that though many difficulties presented themselves they were all, in 1866, triumphantly overcome. It was on his return from the submersion of the 1866 cable, and the raising and the completion of the 1865 cable, that the honour of knighthood was conferred on him along with others of his distinguished fellow-workers.

Recently Sir William Thomson has invented a new and very beautiful instrument, the "siphon recorder," for recording signals on long submarine lines. It is in use at all the telegraph stations along the submarine line connecting England with India. It is also used on the French Atlantic Cable, and on the direct United States line. Sir W. Thomson, Mr. Varley, and Prof. Jenkin, combining their inventions together, have given the only system by which submarine telegraphy on long lines has been carried on up to the present time.

Sir William Thomson is an enthusiastic yachtsman and a skilful navigator. His recently-published popular lecture on Navigation proves this; and, with that bright genius which enriches all with which it comes in contact, his improvements in navigation, as we had occasion to remark a fortnight ago, in noticing his newly published "Tables for Facilitating the Use of Sumner's Method at Sea," are of very high importance. The general adoption of Sumner's Method, now made simple for the navigator, would be a reform in navigation almost amounting to a revolution, and is one most highly to be desired. Sir William Thomson has also invented a new form of mariner's compass of exquisite construction. It possesses many advantages over the best of those in general use, not excluding the Standard Admiralty Compass; but its special feature is that it permits of the *practical* application of Sir George Airy's method of correcting compasses for the permanent and temporary magnetism of iron ships. He has also invented an apparatus for deep-sea sounding by pianoforte wire. This apparatus is so simple and easily managed that he has brought up "bottom" from a depth of nearly three nautical miles, sounding from his own yacht, without aid of steam or any of the ordinary requisites for such depths. His method was much employed in taking rapid soundings during the laying of telegraph cables along the Brazilian coast to the West Indies. It has also been used with great success on the United States Submarine Survey. Recently, while on his way to Philadelphia, Sir W. Thomson himself was able to take flying soundings, reaching the bottom in 68 fathoms, from a Cunard Line steamship going at full speed.

The treatise on "Natural Philosophy" written by Prof. Thomson, in conjunction with Prof. Tait, brings before us another branch of activity in which he has shown himself as eminent as in research.

Sir William Thomson is a Fellow of the Royal Society of London and of the Royal Society of Edinburgh. He has received the Royal Medal of the former and the Keith Medal of the latter. He is also an honorary member of several foreign societies. The Universities of Dublin, of Cambridge, and of Edinburgh have each conferred upon him the honorary degree of LL.D., and that of Oxford the honorary degree of D.C.L. On his marriage in 1852 he gave up his Fellowship at St. Peter's College, Cambridge; but in 1871 his college again elected him to a Fellowship, which he now holds.

Sir William Thomson's brother, Dr. James Thomson, is Professor of Civil Engineering in the University of Glasgow. He is well known as the discoverer of the lowering of the freezing-point of water by pressure; and is the author of many other important physical researches.

The following opinion of Sir William Thomson's merit as a worker in science has been sent us by Prof. Helmholtz:—"His peculiar merit, according to my own opinion, consists in his method of treating problems of mathematical physics. He has striven with great consistency to purify the mathematical theory from hypothetical assumptions which were not a pure expression of the facts. In this way he has done very much to destroy the old unnatural separation between experimental and mathematical physics, and to reduce the latter to a precise and pure expression of the laws of phenomena. He is an eminent mathematician, but the gift to translate real facts into mathematical equations, and *vice versâ*, is by far more rare than that to find the solution of a given mathematical problem, and in this direction Sir William Thomson is most eminent and original. His electrical instruments and methods of observation, by which he has rendered amongst other things electrostatical phenomena as precisely measureable as magnetic or galvanic forces, give the most striking illustration how much can be gained for practical purposes by a clear insight into theoretical questions; and the series of his papers on thermodynamics and the experimental confirmations of several most surprising theoretical conclusions deduced from Carnot's axiom, point in the same direction."

British science may be congratulated on the fact that in Sir William Thomson the most brilliant genius of the investigator is associated with the most lovable qualities of the man. His single-minded enthusiasm for the promotion of knowledge, his wealth of kindliness for younger men and fellow-workers, and his splendid modesty are among the qualities for which those who know him best admire him most.

METEOROLOGICAL RESEARCH

IN previous articles the necessity of dividing into two groups the subjects usually called meteorological has been strongly insisted on. The one of these may be termed climatic meteorology, and is intimately connected with physiology and those sciences which have reference to life. The other may be called physical meteorology, and recent researches have shown that this is intimately connected with other branches of physical inquiry, forming in its wider aspect a sort of meeting ground between molar and molecular physics—a region, in fact, where we find the largest bodies of the universe influencing the smallest.

It is a fortunate thing that we have no longer any need to enlarge upon the practical importance of the latter branch, since this is now recognised even by those who are furthest from considering science worthy of investigation for its own sake; while our present Government, who have shown themselves so willing to further the interests of abstract science, are, we believe, no less anxious to encourage amongst us a truly scientific meteorology. I think, therefore, that the present moment is an opportune one for discussing our subject from the point of view of pure science.

Nor is a feeling of national pride out of place even here. England is the greatest maritime nation on record, and her interests are represented in every quarter of the globe. If her offspring, America, is content to bestow a yearly subsidy of 50,000*l.* on meteorology, it is surely not too much to expect that the subject should receive the most liberal and enlightened treatment from the mother country.

One of the reasons why it is necessary to call attention to meteorology is because the science, being young, is in a very different position from that occupied by her sister sciences, chemistry and physics, so that we cannot be said to have a school of meteorologists at present existing. It would be an object of national importance to encourage the formation of such a school.

Again, while a want of clearness exists generally and everywhere regarding the scope of meteorology, there is also a large amount of widespread ignorance. When a leg of mutton dropped from Nadar's balloon into the *place* of a French town, the Prefect thought it his duty to report the circumstance along with the state of the barometer and thermometer to his official superiors. Doubtless both dry and wet bulbs were accurately recorded. But I shrewdly suspect there are other nations besides the French who attach inordinate importance to the reading of dry and wet bulbs.

This confusion of mind arises doubtless from the state in which the science has been for more than a century—since the time when the *ignis fatuus* and the fall of an aërolite were grouped together as allied phenomena.

Leaving these times of extreme ignorance—the meteorologically dark ages—we next come to a period when our whole duty to meteorology was considered to be fulfilled by attaching observers of the barometer and thermometer to Royal Societies and Astronomical Institutions. These produced results, which were reduced after a mechanical and strictly statistical method, and then—put aside in a drawer. But we begin to perceive things more clearly now; we see that the duty we owe to the phenomena is to form them into a science, and that the last-mentioned method might have been pursued to the end of the world without leading to anything like a true science of meteorology. To take an extreme case, it would have been just as useful to tabulate the number of leaves that fall in autumn or the number of swallows observable in a day of summer. What then, it may be asked, are we to deal with? We reply that if we are to regard this subject as a science at all, we have here to deal with the action of external bodies upon the earth's envelopes, along with certain reactions of these envelopes upon each other. It will next be asked, How are we to deal with the subject?

In the first place, there ought, of course, to be a well-considered system of observations, which should be internationalised (if we may use the expression) as much as possible, so that each observation should be current coin over the largest possible area.

In the next place there must, of course, be a method of testing the accuracy of the observations. Lastly (and this is a point of the greatest possible importance), the individual observations ought to be thrown open to men of science in general, who should be encouraged and aided to utilise them to the greatest possible extent. Such continuous observations would thus lead to what may be called *sporadic researches*—that is to say, to researches not of the nature of ordinary reductions, and originating with men of science having free access to the observations and generously aided in their inquiries. It is only by this means that the edifice of a true science of meteorology can ever be erected, and then only stone by stone on the foundation of accurate observation.

Taking our present knowledge, let us see what sporadic researches naturally suggest themselves. For this purpose we may divide the subject into three parts—one embracing pure meteorology, another terrestrial magnetism, while a third has reference to the influence of the sun and moon upon terrestrial conditions.

In meteorology we should endeavour to obtain a clear and complete knowledge of the physical motions of the earth's atmosphere and liquid envelope, as well as of the various physical states of aqueous vapour existing in the air. Secondly, we should investigate the cyclical changes of these motions, and inquire into the causes of such changes. Thirdly, we should endeavour to utilise our knowledge, once obtained, in improving our power of predicting weather. In magnetism we should endeavour, by the help of observations already accumulated, to ascertain the causes of the changes which take place in the magnetism of the earth; and also to ascertain what is the nature of the connection between magnetism and meteorology. We should also investigate into the probable cause of the earth's magnetic polarity; and, lastly, ascertain whether a method of predicting meteorological changes may not be furnished by magnetism.

Thirdly and lastly, with respect to solar and lunar researches, we must ascertain the various periods and subperiods of sun-spot frequency, and of the frequency of solar faculæ and prominences.

We have then to investigate the causes and concomitants of these solar phenomena. It is well known that disturbances of the magnetism and meteorology of the earth are their concomitants. Well—we must try to find out whether such disturbances are caused by the solar outbreaks, or whether both are effects due to some common but unknown cause. Then, with regard to the moon, it will be necessary to investigate fully the nature of her action on meteorology and magnetism, and to ascertain whether this action is independent, or has reference to the position of the sun and to the state of his surface.

It ought here to be mentioned that the above list embraces only those prominent researches that have occurred to the writer of these remarks, and that if observations be thrown open and research encouraged, the dimensions of such a list would be almost indefinitely increased. And I will here repeat that it is only by carrying out such researches as those suggested that we can ever hope to raise meteorology to the rank of a true science.

It is well understood that the carrying out of such researches has formed no essential part of the duties discharged by the existing Meteorological Committee, and that as a matter of fact (with few exceptions) such researches have not been undertaken by them in the past.

Thus, whether or not the importance of such researches was in the minds of those statesmen who subsidised the present system, these inquiries have not yet been carried out, nor do we conceive that they could well be carried out by the existing machinery.

The Committee have, as a preliminary measure, directed their strength, perhaps wisely, to the accumulation of good observations, in other words, to laying the foundation of a future science, rather than to erecting the superstructure.

It remains to be considered whether any change in the present method of administration is absolutely necessary before research can receive due attention. We assume that the present meteorological system of the country is known to our readers: we may briefly state that this system is controlled by a committee consisting of eight unpaid members of the Royal Society, all of whom are eminent in science, although not all eminent in meteorology. This is sufficiently accounted for by reason of our statement, that there is not yet a true school or science of meteorology.

Through the past labours of these men and of their chief officers, the business of the meteorological office has now probably been put into a satisfactory position that will render unnecessary for the future any very great expenditure of energy upon the details of administration.

But such an arrangement, however excellent in a business point of view, must nevertheless necessarily fall short in developing scientific research. For this the undivided attention of several men of science must be secured, and the question we would here wish to submit to the consideration of our readers is the following.

Would not the combination of a few such men devoting their whole time to the subject, together with other men who though well acquainted with the subject, and otherwise qualified, are yet unable to devote their whole time to it, constitute the best possible committee of the future? We need hardly say that the functions of such a board would not be limited to that of producing research within itself. It ought likewise to stimulate and aid outsiders by various means, including advice and perhaps pecuniary aid. It might attach to itself as occasional members the meteorologists of the provinces, inviting their co-operation, giving and receiving advice, and it might even associate with itself as corresponding members, the meteorologists of the colonies and of foreign countries. In fine, the subject is one which perhaps more than any other demands the united action of men of various nations.

From what has just been said, it will readily appear that the sources of information upon which such a committee will draw in their investigations will by no means be confined to those which are under their own immediate control. The stores accumulated by foreign and colonial observers will, of course, be greatly drawn upon, and not only so, but the committee will doubtless also avail themselves of the stores of information possessed by other Governmental departments, as, for instance, those under the control of the hydrographer, who would naturally be a prominent member of the meteorological board, lending them his valuable assistance and co-operation. Besides the hydrographer, it would probably be found necessary to have, at least, three members of the board representing the three divisions of the subject already alluded to, who should be content to devote their whole time to their respective inquiries. The remainder would be composed of distinguished men interested in the subject, but unable to devote their whole time to it, embracing amongst them one or more mathematical physicists of high reputation.

If it be asserted that there are difficulties in the way of such an arrangement, it may be replied that undoubtedly there are; but if the subject were not one of difficulty, the Government would probably not have consulted the Royal Society from the very commencement of their inquiries. Such a powerful engine as a distinguished scientific committee, some of whom are pledged to devote their whole time, and others a portion of it, to the progress of scientific meteorology, is not meant to be used for the mere chopping of straws. The appropriate function of such a committee is surely that of overcoming difficulties.

BALFOUR STEWART

THE "ENCYCLOPÆDIA BRITANNICA."

Encyclopædia Britannica. Vol. IV. (Edinburgh: Adam and Charles Black.)

THE most prominent scientific contribution to the fourth volume of the "Encylopædia Britannica" is Prof. Balfour's article on Botany. In fact, with two other articles, it occupies a fourth of the whole number of pages, and this, together with its very comprehensive title, leads the reader to expect a tolerably complete review of all the various fields of botanical science, an expectation which is confirmed by their enumeration in the opening paragraphs. A little further examination shows, however, that it only treats of a single branch; the "Structure and Morphology of Plants;" "Classification" and "Distribution in Time and Space" are deferred for separate articles, and "Vegetable Physiology" has apparently dropped out of sight altogether. Any division of the matter is, for many reasons, better in an Encyclopædia than to despatch a whole subject *en bloc* with what is substantially a textbook rather than an article. But it is impossible not to regret that the vegetable side of Biology has not had a carefully planned series of contributions by different hands devoted to it like those which from the volumes already published appear to have been arranged for the animal side. And it is at any rate not easy to see why, as it is, one of several contributions should arrogate to itself the general title belonging to the whole. What would be thought of an article headed Zoology which only dealt with the myology of mammals?

The "Encyclopædia Britannica" has become, in its present edition, in a sense a national undertaking. It is so well supported by the best men in different departments of knowledge that it will no doubt come to be regarded as having a kind of representative character. The utterances of the several contributors will be taken as a kind of measure of the state of opinion in this country in each subject. From this point of view it is impossible not to feel that Prof. Balfour's exposition is disappointing as coming from so eminent a teacher, and that the idea it gives of botanical science is unsuggestive to the last degree.

Passing over an historical sketch of which many of the details, such as the last illness of the elder De Candolle, are essentially biographical, we commence with the "Structural Elements of Plants," in other words, their "General Histology." This opens with an account of the cell, which, even in its youngest condition, is stated to contain a sap-cavity; this is by no means the case, and the adjoining illustration, to which reference is made, shows cells with unvacuolated protoplasm, unless the nucleus is made to do duty for a vacuole. On the next page we are told that protoplasm "consists of albuminous substance mixed with water, and some incombustible materials," and that "it also contains some organic compounds;" are we to infer from this that albuminous substance is inorganic? From the cell we pass to the consideration of tissues, which are divided into cellular and vascular. This distinction carries us back half a century to De Candolle's "Organographie Végétale" (1827). Vegetable histologists have, indeed, laboured in vain from

Von Mohl onwards, if they have not succeeded in showing that *all* plant tissues are cellular. Coming to details, it is a little surprising to find (p. 87) sieve tubes (*siebrohren*) confused with dotted ducts (*porengefässe*); the two structures are entirely distinct and are characteristic respectively of the "bast" and "wood" tracts of the fibro-vascular bundle. It is remarked that the "latticed cells of some authors are of a similar nature;" but the real fact is that they are the same thing. Hartig, who discovered the sieve-tubes in *Cucurbita*, found the pores open and occupied by threads of protoplasm which united that of adjoining cells; Von Mohl, doubting this, proposed for them the name of *gitterzellen*, so as to avoid the implication of perforation. Under the head of substances found in cells, we have, of course, an account of starch, sugar, gum, &c. The very important fact that starch grains are always formed in protoplasm, which is also the agent in their eventual solution, is overlooked. Gum, also, is not usually found in cells at all; it is the result of a mucilaginous change of the cell-*walls*.

Next in order would come an account of the principal systems of tissues—epidermal, fundamental, and fibro-vascular. Prof. Balfour treats of the former alone, only recognising a classification of the tissues of the plant into an outer layer bounding an inner mass. This leads him into numerous difficulties. Thus he calls the velamen of the aerial roots of orchids (which is a development of the epidermis), hypoderma, although *that* is a modification of the fundamental *i.e.*, as the name indicates, sub-epidermal tissue. Again, he derives cork from the epidermal layer, whereas it is almost always developed from cortical tissue *under* the epidermis, which is usually destroyed at the time of its production; lenticels, also are connected with cork-formation, and not, as Prof. Balfour states, with the development of aerial roots. It may be noted, also, that the popular theory of the action of stomata still keeps its place here, although long ago shown to be as often as not untrue.

The remainder of the article deals with the morphology of flowering plants with brief and utterly insufficient references to other groups lumped together under the general head of Cryptogams. Prof. Balfour is equally fond of the classification of plants into Dicotyledons, Monocotyledons, and Acotyledons, of which the value at the present time may be estimated by the fact that it was proposed by Jussieu in 1789, and is about as significant now as a classification of religions would be into Unitarians, Christians, and Heathen. Neither here nor elsewhere is there any grasp of general principles to relieve the arid monotony of technicalities often unfortunately inaccurate. A few instances of this may be noted:—The rhizome of Solomon's seal (p. 98) is a typical instance of a definite (*sympodium*), not of an indefinite rhizome (*monopodium*); the stem of the Shola plant is not wholly composed of pith (p. 100), but of a peculiar kind of wood; the dark bands in the transverse section of the tree-fern (Fig. 89) are not woody fibres, they do not belong to the fibro-vascular, but to the fundamental system; on p. 92 we are told that the group of Thallogens comprise Algæ and Fungi, while on p. 107 we find added "and many Hepaticæ"—nothing more being really meant than that they have a thalloid habit; lastly, not to prolong the citations interminably, we are told on p. 119 that "the absorption of carbonic acid water and other fluids is carried on by the leaves, chiefly by the stomata," whereas it is pretty generally believed that the business of leaves is to get rid of water, not to take it in.

One feature of Prof. Balfour's article cannot fail to strike even the most casual reader. This is the extraordinary profusion of technical terms made still more repellent by catching the eye, in italics. Here are some specimens:—

"The names of *bothrenchyma* and *taphrenchyma* have been given to a tissue composed of such cells. Not unfrequently contractions are visible on the outside of the vessel, indicating its formation by coalescence of superposed cells. To vessels exhibiting contractions of this kind, whether spiral or pitted, the terms *moniliform* or *vermiform* have been applied; and the tissue composed of these *moniliform* vessels has been denominated *phleboidal*," p. 87.

"The *parenchyma* of the leaf is the cellular tissue surrounding the vessels, and inclosed within the epidermis. It has sometimes received the names of *diachyma*, *mesophyllum*, and *diploe*," p. 108.

Now it will seem almost incredible to say that all but one of the terms italicised in the passages just quoted are absolutely obsolete. Science is like nature; each stage in the progress of either is furnished with appropriate belongings and surroundings, which pass away for the most part, and give place to new. Literature preserves their remembrance in the one case as the safe keeping of the rocks does in the other. Science has its fossils as geology has, and if we delve after them into the *débris* of the past, it is to learn that the history of either has been continuous, and not to inform ourselves of the actualities of the present. But Prof. Balfour's article is like a breccia to which deposits of every age have sent ill-assorted contributions, and the waifs and strays of to-day have to settle down as best they can with the most ancient and singular remains. No botanist who is engaged in solving actual botanical problems—the existence of which readers of Prof. Balfour's article will hardly suspect—talks about *phleboidal bothrenchyma* or *diplöe* any more than he thinks of calling a walnut a *Tryma*, or a grape a *Nuculanium*. Students bent on passing examinations may perhaps persuade themselves, if not their examiners, that in committing such hard words to memory they are making solid additions to their mental furniture. But the real fact is that such things are of no use to anybody, and Mr. Bentham, than whom no living botanist has written more systematic works, has included all the terms—mostly extremely simple—with their definitions which are really needed for the purpose of describing plants, in a short pamphlet of some thirty pages. So that while Prof. Balfour has not succeeded in giving us—what it is the business of an Encyclopædia article to do—a comprehensive view of the broad facts of Vegetable Histology and Morphology in their present aspect, he has certainly not given an account of their terminology which does the subjects justice, but has produced a sort of terminological cemetery in which all kinds of decaying language have been affectionately embalmed.

The unfortunate result of this kind of treatment of the subject—and it is but fair to admit that Prof. Balfour is by no means the only culprit—is that, even amongst scientific men, botany has come to be regarded in this country as scarcely a serious branch of science at all, and as little more than a kind of biological equivalent of the "Use of the Globes," suitable, indeed, for ladies' schools, and useful, also, like "Materia Medica," for the purpose of occupying the attention of medical students so as to keep them out of mischief during their first summer session.

LETTERS TO THE EDITOR

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

Visual Phenomena

In support of Mr. Arnulph Mallock's conclusion (vol. xiv. p. 350) as to the cause of the star-shaped appearance presented to the naked eye by a bright point, perhaps the following epitome of some notes which I made last January may have interest:—

1. Looking at a distant lamp, with both eyes, I see a radiant corona round the lamp.

2. I find that this corona is composed of two coronæ superposed, one due to the right eye, the other to the left.

3. Each corona has distinctive features of its own, which are recognised in every observation and have remained the same for years.

4. The radiant beams (which are the conspicuous feature) are not exactly radial, but are forked once or twice.

5. The corona is bounded by a peripheral fringe of blue succeeding to red.

6. The diameter of the corona varies with the diameter of the pupil. The distal portions of the radiant beams are concealed or revealed by the contraction or dilatation of the pupil.

7. Any given part of the radiance may be cut off by advancing an opaque body in front of the eye, from the *same* side as the given part of the radiance.

An obstacle which cuts off only the central rays of the entering pencil only dims the central image, but does not affect the radiance.

The radiance, therefore, is due to the outside rays of the entering pencil becoming excessively refracted so as to be thrown across the visual line before reaching the retina.

8. There is nothing in the *cornea*, *aqueous humour*, or pupillary margin of the *iris*, that can cause such refraction.

9. It appears most likely that this excessive refraction of the outside rays of the entering pencil is caused by something in the crystalline lens—probably, in the first place, by undue convexity of the more marginal parts of the lens, which are uncovered by dilatation of the pupil.

10. The *radiant* appearance of the corona is probably due to the radiate structure of the crystalline lens.

The agreement between them is pretty close, extending to the furcate character of the rays.

11. On examining my own eyes by a pencil of diverging rays (admitted so as to throw a shadow of anything inside the eye upon the retina) I find that the main beams of the radiance correspond pretty well to the main radii of my lenses, in the opposite direction.

This confirms that the radiance is due to the radiate structure of the lens.

In one particular my observations differ from Mr. Mallock's. He finds that the length of the rays depends "on the brightness of the point" as well as on the size of the pupil. I find that points of different degrees of brightness have rays of the same length, that length being limited only by the size of the pupil, but that the *breadth* of each ray varies with the apparent size of the lamp-flame or other luminous point under observation. Indeed, if the lamp-flame is so bright as to stimulate the iris to contraction, the rays become shortened in like measure.

The radiate structure of the lens of the human eye is well shown in Kölliker's "Manual of Human Microscopic Anatomy" (ed. 1860, p. 568). The "non-fibrillated" or "central planes" there described are possibly of greater refractive power than the wedge-shaped fibrillated portions between them. This would cause the phenomenon of radiance by excessive refraction of the outside rays of the entering pencil. Hubert Airy

Edensor, Kidbrook Grove, Blackheath, Aug. 28

Species and Varieties

It may be taken, I presume, that the description and naming of "species" has now a great value as material for studying the laws of the evolution of species and of geographichal distribution and variation; and that the question is not so much to know what name to call a "species" as to account for its presence and form in the economy of nature. And it will, perhaps, also be granted that the study of geographical distribution does not consist alone in acquiring a knowledge of the fauna of a district, so much as in investigating the laws of the special differentiation of that fauna. It thus becomes evident that the slightest modification tending to persistency requires the most careful record, as it is only by the knowledge of first slight modifications that we can expect to understand the process of larger divergencies.

In faunistic catalogues, should the author be inclined to the "lumping method" (perhaps in one sense correctly), a form may be considered as only a variety of some well-known species, and recorded under that name. We thus learn nothing of its modification, and are led to think of it as agreeing with the typical form, thus losing one of the most important facts in our study of variation and distribution. Another element of error seems also co-existent. When a newly-discovered form is designated a variety of another species, it would lead us to suppose that that species is the original type from which the other form has branched. But it is quite possible to suppose on such reasoning that the newly-discovered form may have been the parent from which the previously described species may have been derived. The offspring may have been collected and described before the parent; or we may be filling a space with planets to which there is no sun. This I have frequently suspected in the study of exotic lepidoptera, a study which is principally confined to the perfect form of the insects and of whose larval changes we know in most cases nothing. It rests generally on the judgment of individual entomologists as to the amount of difference to be considered specific.

Without venturing on the vexed question as to species and varieties, would it not be furthering the cause of science that what are considered as merely local varieties should, *as such*, have as careful a description as though they were ranked as species. W. L. Distant

Buccleuch Road, West Dulwich.

Antedated Books

I cannot allow that our *Transactions* are antedated, as is asserted by "Another F.Z.S." As is the case with the *Philosophical Transactions* of the Royal Society and the Linnean *Transactions*, no date beyond the year of issue is given on the cover, but there is the additional advantage of the date at which each number goes through the press at the bottom of the sheets. There can, however, I think, be no objection to giving a still more exact date of publication on the cover, and this, in accordance with "Another F.Z.S.'s" suggestion, for which I am much obliged to him, I propose to do in future.

P. L. Sclater

I am sorry to see that "F.Z.S." still prefers to write under the signature of his first letter. I was in hopes that when he saw the gravity of the accusation which he was bringing against me, he would in his own person have disclaimed any intention of seriously laying such a charge to my door, but as no word of apology escapes him, I regret that in making such an injurious statement he has not added the weight of his name to the accusation. I am, therefore, at liberty to suppose that he is no stranger himself to the "evil practice" (*honi soit qui mal y pense*) which he deprecates, otherwise I cannot imagine such a thought occurring to anyone; but it is evident that on receiving the book his only impulse was to write a letter to this journal instead of charitably endeavouring to discover some feasible explanation of what he calls the "false" date. As most naturalists in England are in the habit of following the rules of the British Association in regard to nomenclature, "F.Z.S." can find more profitable employment for his pen in proposing such alterations or additions to these rules as would remedy the "grievance" (as I term it) or the "evil practice" (as he prefers to call it), and in that I can assure him he would meet with hearty co-operation from all English naturalists, and from none more heartily than from the undersigned. R. BOWDLER SHARPE

The Origin of Variations

THERE is a slight difference between all three of the answers to Mr. Murphy's queries on Protective Mimicry (vol. xiv. pp. 309, 329); but, I think, the authors of those replies are unanimously over-hasty to call in the aid of protective selection. I cannot but think that the perpetuation of nascent variations may more safely be attributed to causes identical with those in which the variations originate. If this be so, though the origin of variation is necessarily beyond the scope of any selection-theory, these causes continuing to act on the varying organism become of vast importance to the evolutionist. Too much importance cannot, I think, be given in this connection, to the principle of economy of nutrition, or balancement of growth, formulated by Aristotle, in the words "ἅμα δὲ τὴν αὐτὴν ὑπεροχὴν εἰς πολλοὺς τόπους ἀδυνατεῖ διανέμειν ἡ φύσις," and by Goethe, in the expression "Nature, in order to spend on one side, is forced to economise on the other." I gave one example of this law in a previous letter (NATURE, vol. xiii. p. 107) in speaking of the indirect uses of the waste, or secondary products of metastasis in plants; but as the subject is admirably sketched in Mr. Lowne's suggestive essay on "The Philosophy of Evolution," a work too little known or appreciated, I will give here an outline of the argument. Food may be divided into three parts—for nutrition, for the production of energy and waste, or excretion. In many lower organisms the excreted material forms a simple shell; in plants manna, nectar, and resins belong to this group. The chief form of energy in the organic kingdom is that resulting from the oxidation of carbon, chiefly characteristic of animals, while plants secrete the energy-producing material. In higher organisms it is physiologically advantageous that the parts of an organism should differ in the kind of nourishment they require and thus act, as Sir James Paget has shown, as excretory organs to one another. Thus all animals which feed on large quantities of comparatively slightly nutritious matter have a complicated digestive apparatus, and a strong tendency to the production of large skeletons or cutaneous organs, which relieve the special excretory organs. The stag applies a large portion of the calcareous salts derived from the herbage to the production of horns in the male and the bones of the young in the female. The thoracic appendages of the lamellicorns and the beaks of the toucan and hornbill are given as further examples, and "the dermal appendages of reptiles and the feathers of birds, rich in pigment and nitrogen are probably entirely excrementitious to the other tissues." Mr. Lowne makes an interesting final application of this hypothesis to the loss of the hairy covering of the human skin, it being the albuminous tissue most easily dispensed with to nourish the highly developed nervous system. "Phosphorus was likewise required in large quantities and the osseous system became reduced in size." The composition of the nutrient fluid of the organism remaining constant, the excreted matter will be so also, and thus, for example, a rudimentary horn or a pigment may be produced by a change of food and preserved, while the food remains the same, by a physiological selection, as preventing the overtaxing of the kidneys, before sexual selection or protective selection come into play. I have an instance in point before me. Two plants of variegated kale under the influence of the late drought have produced axial structures from the midribs and veins of all their leaves, and I have no doubt that were their seedlings grown under similar nutritive conditions, a race of plants thus substituting fibro-vascular tissue for the usually abnormal development of parenchyma in the kale would be produced. G. S. BOULGER

Agricultural College, Cirencester, Aug. 25

THE BRITISH ASSOCIATION

THE forty-sixth Annual Session of the British Association was formally opened last night by the address of the president, Prof. Andrews, of Belfast. From the reports of the preparations we have already given, it will have been seen that unusual efforts have been made to render this Glasgow meeting a success, and so far as can be judged at present this end has been gained. This is the third time the Association has met in Glasgow. The first occasion was in 1840, when the Marquis of Breadalbane was president, and the last time in 1855, when a similar honour was conferred on the Duke of Argyll. This is the eighth time the Association has held its meeting in Scotland, Edinburgh having been the first northern town visited, so far back as 1834, four years after the foundation of the Association; the Scottish capital was again visited in 1850 and 1871; Aberdeen in 1859, when Prince Albert was president; and Dundee in 1867.

The arrangements for the evening lectures, about which there was at one time some difficulty, have been happily completed. The first lecture will be delivered on Friday in the Kibble Palace, by Prof. Tait; the second, for working-men, in the City Hall, on Saturday evening, probably by Commander Cameron, R.N.; and the third on Monday, Sept. 11, in the Kibble Palace, by Prof. Sir C. Wyville Thomson.

Among the foreign visitors who are expected at the Glasgow meeting, are:—Dr. Janssen, Prof. Negri, of Florence, Prof. Braune, Leipsic, Dr. Edward Grubi, Breslau, Prof. Cohn, Breslau, Prof. Stoletow, Baron von Wrangell, St. Petersburg, and Prof. Ceruti, Rome.

The specially prepared Guide-book to Glasgow is in three volumes, some of the former guides of this description being considered too bulky. The volumes will be full of interesting information regarding such subjects as the geology of the Valley of the Clyde, fossils found in the West of Scotland; the archæology, zoology, and botany of the district; the rise and progress of the iron manufacture in Scotland, chemical industries, the engineering and ship-building industries of the Clyde, and the textile industries of Glasgow and neighbourhood. Mr. Graham, the Hon. Secretary of the Association, has prepared an excellent sketch map of the country surrounding Glasgow, with its general geological features, which has been lithographed, and will be inserted in one of the volumes. Each member of the Association will be presented with a copy of the handbook.

INAUGURAL ADDRESS OF THOMAS ANDREWS, M.D., LL.D., F.R.S., Hon.F.R.S.E., M.R.I.A., &c., PRESIDENT.

SIX and thirty years have passed over since the British Association for the Advancement of Science held its tenth meeting in this ancient city, and twenty-one years have elapsed since it last assembled here. The representatives of two great Scottish families presided on these occasions; and those who had the advantage of hearing the address of the Duke of Argyll in 1855 will recall the gratification they enjoyed while listening to the thoughtful sentiments which reflected a mind of rare cultivation and varied acquirements. On the present occasion I have undertaken, not without anxiety, the duty of filling an office at first accepted by one whom Scotland and the Association would alike have rejoiced to see in this Chair, not only as a tribute to his own scientific services, but also as recognising in him the worthy representative of that long line of able men who have upheld the pre-eminent position attained by the Scottish schools of medicine in the middle of the last century, when the mantle of Boerhaave fell upon Monro and Cullen.

The task of addressing this Association, always a difficult one, is not rendered easier when the meeting is held in a place which presents the rare combination of being at once an ancient seat of learning and a great centre of modern industry. Time will not permit me to refer to the distinguished men who in early days have left here their mark behind them; and I regret it the more, as there is a growing tendency to exaggerate the value of later discoveries, and to underrate the achievements of those who have lived before us. Confining our attention to a period reaching back to little more than a century, it appears that during that time three new sciences arose, at least as far as any science can be said to have a distinct origin, in this city of Glasgow—Experimental Chemistry, Political Economy, and Mechanical Engineering. It is now conceded that Black laid the foundation of modern chemistry; and no one has ever disputed the claims of Adam Smith and of Watt to have not only founded, but largely built up the two great branches of knowledge with which their names will always be inseparably connected. It was here that Dr. Thomas Thomson established the first school of Practical Chemistry in Great Britain, and that Sir W. Hooker gave to the chair of Botany a European celebrity; it was here that Graham discovered the law of gaseous diffusion and the properties of polybasic acids; it was here that Stenhouse and Anderson, Rankine and J. Thomson made some of their finest discoveries; and it was heie that Sir William Thomson conducted his physico-mathematical investigations, and invented those exquisite instruments, valuable alike for ocean telegraphy and for scientific use, which are among the finest trophies of recent science. Nor must the names ot Tennant, Mackintosh, Neilson, Walter Crum, Young, and Napier be omitted, who, with many others in this place, have made large and valuable additions to practical science.

The safe return of the *Challenger*, after an absence of three and a half years, is a subject of general congratulation. Our knowledge of the varied forms of animal life, and of the remains of animal life, which occur, it is now known, over large tracts of the bed of the ocean, is chiefly derived from the observations made in the *Challenger* and in the previous deep-sea expeditions which were organised by Sir Wyville Thomson and Dr. Carpenter. The physical observations, and especially those on the temperature of the ocean, which were systematically conducted throughout the whole voyage of the *Challenger*, have already supplied valuable data for the resolution of the great question of ocean-currents. Upon this question, which has been discussed with singular ability, but under different aspects, by Dr. Carpenter and Mr. Croll, I cannot attempt here to enter; nor will I venture to forestall, by any crude analysis of my own, the narrative which Sir W. Thomson has kindly undertaken to give of his own achievements and of those of his staff during their long scientific cruise.

Another expedition, which has more than fulfilled the expectations of the public, is Lieut. Cameron's remarkable journey across the continent of Africa. It is by such enterprises, happily conceived and ably executed, that we may hope at no distant day to see the Arab slave-dealer replaced by the legitimate trader, and the depressed populations of Africa gradually brought within the pale of civilised life.

From the North Polar Expedition no intelligence has been received; nor can we expect for some time to hear whether it has succeeded in the crowning object of Arctic enterprise. In the opinion of many, the results, scientific or other, to be gained by a full survey of the Arctic regions can never be of such value as to justify the risk and cost which must be incurred. But it is not by cold calculations of this kind that great discoveries are made or great enterprises achieved. There is an inward and irrepressible impulse—in individuals called a spirit of adventure, in nations a spirit of enterprise—which impels mankind forward to explore every part of the world we inhabit, however inhospitable or difficult of access; and if the country claiming the foremost place among maritime nations shrink from an undertaking because it is perilous, other countries will not be slow to seize the post of honour. If it be possible for man to reach the poles of the earth, whether north or south, the feat must sooner or later be accomplished; and the country of the successful adventurers will be thereby raised in the scale of nations.

The passage of Venus over the sun's disc is an event which cannot be passed over without notice, although many of the circumstances connected with it have already become historical. It was to observe this rare astronomical phenomenon, on the occasion of its former occurrence in 1769, that Capt. Cook's memorable voyage to the Pacific was undertaken, in the course of which he explored the coast of New South Wales, and added that great country to the possessions of the British Crown.

As the transit of Venus gives the most exact method of calculating the distance of the earth from the sun, extensive preparations were made on the last occasion for observing it at selected stations—from Siberia in northern, to Kerguelen's Land in southern latitudes. The great maritime powers vied with each other to turn the opportunity to the best account; and Lord Lindsay had the spirit to equip, at his own expense, the most complete expedition which left the shores of this country. Some of the most valuable stations in southern latitudes were desert islands, rarely free from mist or tempest, and without harbours or shelter of any kind. The landing of the instruments was in many cases attended with great difficulty and even personal risk. Photography lent its aid to record automatically the progress of the transit; and M. Janssen contrived a revolving plate, by means of which from fifty to sixty images of the edge of the sun could be taken at short intervals during the critical periods of the phenomenon.

The observations of M. Janssen at Nagasaki, in Japan, were of special interest. Looking through a violet-blue glass he saw Venus, two or three minutes before the transit began, having the appearance of a pale round spot near the edge of the sun. Immediately after contact the segment of the planet's disc, as seen on the face of the sun, formed with what remained of this spot a complete circle. The pale spot when first seen was, in short, a partial eclipse of the solar corona, which was thus proved beyond dispute to be a luminous atmosphere surrounding the sun. Indications were at the same time obtained of the existence of an atmosphere around Venus.

The mean distance of the earth from the sun was long supposed to have been fixed within a very small limit of error at about 95,000,000 miles. The accuracy of this number had already been called in question on theoretical grounds by Hansen and Leverrier, when Foucault, in 1862, decided the question by an experiment of extraordinary delicacy. Taking advantage of the revolving-mirror, with which Wheatstone had some time before enriched the physical sciences, Foucault succeeded in measuring the absolute velocity of light in space by experiments on a beam of light, reflected backwards and forwards, within a tube little more than thirteen feet in length. Combining the result thus obtained with what is called by astronomers the constant of aberration, Foucault calculated the distance of the earth from the sun, and found it to be one-thirtieth part, or about 3,000,000 miles, less than the commonly received number. This conclusion has lately been confirmed by M. Cornu, from a new determination he has made of the velocity of light according to the method of Fizeau; and in complete accordance with these results are the investigations of Leverrier, founded on a comparison with theory of the observed motions of the sun and of the planets Venus and Mars. It remains to be seen whether the recent observations of the transit of Venus, when reduced, will be sufficiently concordant to fix with even greater precision the true distance of the earth from the sun.

In this brief reference to one of the finest results of modern science, I have mentioned a great name whose loss England has recently had to deplore, and in connection with it the name of an illustrious physicist whose premature death deprived France, a few years ago, of one of her brightest ornaments—Wheatstone and Foucault, ever to be remembered for their marvellous power of eliciting, like Galileo and Newton, from familiar phenomena the highest truths of nature!

The discovery of Huggins that some of the fixed stars are moving towards and others receding from our system, has been fully confirmed by a careful series of observations lately made by Mr. Christie in the Observatory of Greenwich. Mr. Huggins has not been able to discover any indications of a proper motion in the nebulæ; but this may arise from the motion of translation being less than the method would discover. Few achievements in the history of science are more wonderful than the measurement of the proper motions of the fixed stars, from observing the relative position of two delicate lines of light in the field of the telescope.

The observation of the American astronomer Young, that bright lines, corresponding to the ordinary lines of Fraunhofer reversed, may be seen in the lower strata of the solar atmosphere for a few moments during a total eclipse, has been confirmed by Mr. Stone, on the occasion of the total eclipse of the sun which occurred some time ago in South Africa. In the outer corona, or higher regions of the sun's atmosphere, a single green line only was seen, the same which had been already described by Young.

I can here refer only in general terms to the observations of Roscoe and Schuster on the absorption-bands of potassium and sodium, and to the investigations of Lockyer on the absorptive powers of metallic and metalloidal vapours at different temperatures. From the vapour of calcium the latter has obtained two wholly distinct spectra, one belonging to a low, and the other to a high, temperature. Mr. Lockyer is also engaged on a new and greatly extended map of the solar spectrum.

Spectrum analysis has lately led to the discovery of a new metal —gallium—the fifth whose presence has been first indicated by that powerful agent. This discovery is due to M. Lecoq de Boisbaudran, already favourably known by a work on the application of the spectroscope to chemical analysis.

Our knowledge of aerolites has of late years been greatly increased; and I cannot occupy a few moments of your time more usefully than by briefly referring to the subject. So recently as 1860 the most remarkable meteoric fall on record, not even excepting that of L'Aigle, occurred near the village of New Concord in Ohio. On a day when no thunder-clouds were visible, loud sounds were heard resembling claps of thunder, followed by a large fall of meteoric stones, some of which were distinctly seen to strike the earth. One stone, above 50 pounds in weight, buried itself to the depth of 2 feet in the ground, and when dug out was found to be still warm. In 1872 another remarkable meteorite, at first seen as a brilliant star with a luminous train, burst near Orvinio in Italy, and six fragments of it were afterwards collected.

Isolated masses of metallic iron, or rather of an alloy of iron and nickel, similar in composition and properties to the iron usually diffused in meteoric stones, have been found here and there on the surface of the earth, some of large size, as one described by Pallas, which weighed about two-thirds of a ton. Of the meteoric origin of these masses of iron there is little room for doubt, although no record exists of their fall. Sir Edward Sabine, whose life has been devoted with rare fidelity to the pursuit of science, and to whose untiring efforts this Association largely owes the position it now occupies, was the pioneer of the newer discoveries in meteoric science. Eight and fifty years ago he visited, with Capt. Ross, the northern shores of Baffin's Bay, and made the interesting discovery that the knife-blades used by the Esquimaux in the vicinity of the Arctic highlands were formed of meteoric iron. This observation was afterwards fully confirmed; and scattered blocks of meteoric iron have been found from time to time around Baffin's Bay. But it was not till 1870 that the meteoric treasures of Baffin's Bay were truly discovered. In that year Nordenskiöld found, at a part of the shore difficult of approach even in moderate weather, enormous blocks of meteoric iron, the largest weighing nearly twenty tons, imbedded in a ridge of basaltic rock. The interest of this observation is greatly enhanced by the circumstance that these masses of meteoric iron, like the basalt with which they are associated, do not belong to the present geological epoch, but must have fallen long before the actual arrangement of land and sea existed—during, in short, the middle Tertiary, or Miocene period of Lyell. The meteoric origin of these iron masses from Ovifak has been called in question by Lawrence Smith; and it is no doubt possible that they may have been raised by upheaval from the interior of the earth. I have, indeed, myself shown by a magneto-chemical process, that metallic iron, in particles so fine that they have never yet been actually seen, is everywhere diffused through the Miocene basalt of Slieve Mish, in Antrim, and may likewise be discovered by careful search in almost all igneous and in many metamorphic rocks. These observations have since been verified by Reuss in the case of the Bohemian basalts. But, as regards the native iron of Ovifak, the weight of evidence appears to be in favour of the conclusion, at which M. Daubrée, after a careful discussion of the subject, has arrived —that it is really of meteoric origin. This Ovifak iron is also remarkable from containing a considerable amount of carbon partly combined with the iron, partly diffused through the metallic mass in a form resembling coke. In connection with this subject, I must refer to the able and exhaustive memoirs of Maskelyne on the Busti and other aerolites, to the discovery of vanadium by R. Apjohn in a meteoric iron, to the interesting observations of Sorby, and to the researches of Daubrée, Wöhler, Lawrence Smith, Tschermak, and others.

The important services which the Kew Observatory has rendered to meteorology and to solar physics have been fully recognised; and Mr. Gassiot has had the gratification of witnessing the final success of his long and noble efforts to place this observatory upon a permanent footing. A physical observatory for somewhat similar objects, but on a larger scale, is in course of erection, under the guidance of M. Janssen, at Fontenay, in France, and others are springing up or already exist in Germany and Italy. It is earnestly to be hoped that this country will not lag behind in providing physical observatories on a scale worthy of the nation and commensurate with the importance of the object. On this question I cannot do better than refer to the high authority of Dr. Balfour Stewart, and to the views he expressed in his able address last year to the Physical Section.

Weather telegraphy, or the reporting by telegraph the state of the weather at selected stations to a central office, so that notice of the probable approach of storms may be given to the seaports, has become in this country an organised system; and considering the little progress meteorology has made as a science, the results may be considered to be on the whole satisfactory. Of the warnings issued of late years, four out of five were justified by the occurrence of gales or strong winds. Few storms occurred for which no warnings had been given; but unfortunately among these were some of the heaviest gales of the period. The stations from which daily reports are sent to the meteorological office in London embrace the whole coast of Western Europe, including the Shetland Isles. It appears that atmospheric disturbances seldom cross the Atlantic without being greatly altered in character, and that the origin of most of our storms lies eastward of the longitude of Newfoundland.

As regards the velocity of the wind, the cup-anemometer of Dr. Robinson has fully realised the expectations of its discoverer; and the venerable astronomer of Armagh has been engaged during the past summer, with all the ardour of youth, in a course of laborious experiments to determine the constants of his instrument. From seven years' observations at the Observatory of Armagh, he has found that the mean velocity of the wind is greatest in the S.S.W. octant, and least in the opposite one, and that the amount of wind attains a maximum in January, after which it steadily decreases, with one slight exception, till July, augmenting again till the end of the year.

Passing to the subject of electricity, it is with pleasure that I have to announce the failure of a recent attempt to deprive Oerstedt of his great discovery. It is gratifying thus to find high reputations vindicated, and names which all men love to honour transmitted with undiminished lustre to posterity. At a former meeting of this Association, remarkable for an unusual attendance of distinguished foreigners, the central figure was Oerstedt. On that occasion Sir John Herschel in glowing language compared Oerstedt's discovery to the blessed dew of heaven which only the mastermind could draw down, but which it was for others to turn to account and use for the fertilisation of the earth. To Franklin, Volta, Coulomb, Oerstedt, Ampère, Faraday, Seebeck, and Ohm are due the fundamental discoveries of modern electricity—a science whose applications in Davy's hands led to grander results than alchemist ever dreamed of, and in the hands of others (among whom Wheatstone, Morse, and Thomson occupy the foremost place) to the marvels of the electric telegraph. When we proceed from the actual phenomena of electricity to the molecular conditions upon which those phenomena depend, we are confronted with questions as recondite as any with which the physicist has had to deal, but towards the solution of which the researches of Faraday have contributed the most precious materials. The theory of electrical and magnetic action occupied formerly the powerful minds of Poisson, Green, and Gauss; and among the living it will surely not be invidious to cite the names of Weber, Helmholtz, Thomson, and Clerk Maxwell. The work of the latter on electricity is an original essay worthy in every way of the great reputation and of the clear and far-seeing intellect of its author.

Among recent investigations I must refer to Prof. Tait's discovery of consecutive neutral points in certain thermo-electric junctions, for which he was lately awarded the Keith prize. This discovery has been the result of an elaborate investigation of the properties of thermo-electric currents, and is specially interesting in reference to the theory of dynamical electricity. Nor can I omit to mention the very interesting and original experiments of Dr. Kerr on the dielectric state, from which it appears that when electricity of high tension is passed through dielectrics, a change of molecular arrangement occurs, slowly in the case of solids, quickly in the case of liquids, and that the lines of electric force are in some cases lines of compression, in other cases lines of extension.

Of the many discoveries in physical science due to Sir William Grove, the earliest and not the least important is the battery which bears his name, and is to this day the most powerful of all voltaic arrangements; but with a Grove's battery of 50 or even 100 cells in vigorous action, the spark will not pass through an appreciable distance of cold air. By using a very large number of cells, carefully insulated and charged with water, Mr. Gassiot succeeded in obtaining a short spark through air; and lately De La Rue and Müller have constructed a large chloride of silver battery giving freely sparks through cold air, which, when a column of pure water is interposed in the circuit, accurately resemble those of the common electrical machine. The length of the spark increasing nearly as the square of the number of cells, it has been calculated that with 100,000 elements of this battery the discharge should take place through a distance of no less than eight feet in air.

In the solar beam we have an agent of surpassing power, the investigation of whose properties by Newton forms an epoch in the history of experimental science scarcely less important than the discovery of the law of gravitation in the history of physical astronomy. Three actions characterise the solar beam, or, indeed, more or less that of any luminous body—the heating, the physiological, and the chemical. In the ordinary solar beam we can modify the relative amount of these actions by passing it through different media, and we can thus have luminous rays with little heating or little chemical action. In the case of the moon's rays it required the highest skill on the part of Lord Rosse, even with all the resources of the observatory of Parsonstown, to investigate their heating properties, and to show that the surface of our satellite facing the earth passes, during every lunation, through a greater range of temperature than the difference between the freezing- and boiling-points of water.

But if, instead of taking an ordinary ray of light, we analyse it as Newton did by the prism, and isolate a very fine line of the spectrum (theoretically a line of infinite tenuity), that is to say, if we take a ray of definite refrangibility, it will be found impossible, by screens or otherwise, to alter its properties. It was his clear perception of the truth of this principle that led Stokes to his great discovery of the cause of epipolic dispersion, in which he showed that many bodies had the power of absorbing dark rays of high refrangibility and of emitting them as luminous rays of lower refrangibility—of absorbing, in short, darkness and of emitting it as light. It is not, indeed, an easy matter in all cases to say whether a given effect is due to the action of heat or light; and the question which of these forces is the efficient agent in causing the motion of the tiny discs in Crookes's radiometer has given rise to a good deal of discussion. The answer to this question involves the same principles as those by which the image traced on the daguerreotype plate, or the decomposition of carbonic acid by the leaves of plants, is referred to the action of light and not of heat; and applying these principles to the experiments made with the radiometer, the weight of evidence appears to be in favour of the view that the repulsion of the blackened surfaces of the discs is due to a thermal reaction occurring in a highly rarefied medium. I have myself had the pleasure of witnessing many of Mr. Crookes's experiments, and I cannot sufficiently express my admiration of the care and skill with which he has pursued this investigation. The remarkable repulsions he has observed in the most perfect vacua hitherto attained are interesting, not only as having led to the construction of a beautiful instrument, but as being likely, when the subject is fully investigated, to give valuable data for the theory of molecular actions.

A singular property of light, discovered a short time ago by Mr. Willoughby Smith, is its power of diminishing the electrical resistance of the element selenium. This property has been ascertained to belong chiefly to the luminous rays on the red side of the spectrum, being nearly absent in the violet or more refrangible rays and also in heat-rays of low refrangibility. The recent experiments of Prof. W. G. Adams have fully established the accuracy of the remarkable observation, first made by Lord Rosse, that the action appeared to vary inversely as the simple distance of the illuminating source.

Switzerland sent, some years ago, as its representative to this country, the celebrated De la Rive, whose scientific life formed lately the subject of an eloquent *éloge* from the pen of M. Dumas. On this occasion we have to welcome, in General Menabrea, a distinguished representative both of the kingdom of Italy and of Italian science. His great work on the determination of the pressures and tensions in an elastic system is of too abstruse a character to be discussed in this address; but the principle it contains may be briefly stated in the following words:—"When any elastic system places itself in equilibrium under the action of external forces, the work developed by the internal forces is a minimum." General Menabrea has, however, other and special claims upon us here, as the friend to whom Babbage entrusted the task of making known to the world the principles of his analytical machine—a gigantic conception—the effort to realise which it is known was one of the chief objects of Babbage's later life. The latest development of this conception is to be found in the mechanical integrator of Prof. J. Thomson, in which motion is transmitted, according to a new kinematic principle, from a disk or cone to a cylinder through the intervention of a loose ball, and in Sir W. Thomson's machine for the mechanical integration of differential equations of the second order. In the exquisite tidal machine of the latter we have an instrument by means of which the height of the tide at a given port can be accurately predicted for all times of the day and night.

The attraction-meter of Siemens is an instrument of great delicacy for measuring horizontal attractions, which it is proposed to use for recording the attractive influences of the sun and moon, upon which the tides depend. The bathometer of the same able physicist is another remarkable instrument, in which the constant force of a spring is opposed to the variable pressure of a column of mercury. By an easy observation of the bathometer on shipboard, the depth of the sea may be approximately ascertained without the use of a sounding-line.

The Loan Exhibition of Apparatus at Kensington has been a complete success, and cannot fail to be useful both in extending a knowledge of scientific subjects and in promoting scientific research throughout the country. Unique in character, but most interesting and instructive, this exhibition will, it is to be hoped, be the precursor of a permanent museum of scientific objects, which, like the present exhibition, shall be a record of old as well as a representation of new inventions.

It is often difficult to draw a distinct line of separation between the physical and chemical sciences; and it is perhaps doubtful whether the division is not really an artificial one. The chemist cannot, indeed, make any large advance without having to deal with physical principles; and it is to Boyle, Dalton, Gay-Lussac, and Graham that we owe the discovery of the mechanical laws which govern the properties of gases and vapours. Some of these laws have of late been made the subject of searching inquiry, which has fully confirmed their accuracy, when the body under examination approaches to what has not inaptly been designated the ideal gaseous state. But when gases are examined under varied conditions of pressure and temperature, it is found that these laws are only particular cases of more general laws, and that the laws of the gaseous state, as it exists in nature, although they may be enunciated in a precise and definite form, are very different from the simple expressions which apply to the ideal condition. The new laws become in the turn inapplicable when from the gaseous state proper we pass to those intermediate conditions which, it has been shown, link with unbroken continuity the gaseous and liquid states. As we approach the liquid state, or even when we reach it, the problem becomes more complicated; but its solution even in these cases will, it may confidently be expected, yield to the powerful means of investigation we now possess.

Among the more important researches made of late in physical chemistry, I may mention those of F. Weber on the specific heat of carbon and the allied elements, of Berthelot on thermo-chemistry, of Bunsen on spectrum analysis, of Wüllner on the band- and line-spectra of the gases, and of Guthrie on the chryohydrates.

Cosmical chemistry is a science of yesterday; and yet it already abounds in facts of the highest interest. Hydrogen, which, if the absolute zero of the physicist does not bar the way, we may hope yet to see in the metallic form, appears to be everywhere present in the universe. It exists in enormous quantity in the solar atmosphere, and it has been discovered in the atmospheres of the fixed stars. It is present, and is the only known element of whose presence we are certain, in those vast sheets of ignited gas of which the nebulæ proper are composed. Nitrogen is also widely diffused among the stellar bodies, and carbon has been discovered in more than one of the comets. On the other hand, a prominent line in the spectrum of the Aurora Borealis has not been identified with that of any known element; and the question may be asked:—Does a new element, in a highly rarefied state, exist in the upper regions of our atmosphere? or are we, with Angström, to attribute this line to a fluorescent or phosphorescent light produced by the electrical discharge to which the aurora is due? This question awaits further observations before it can be definitely settled, as does also that of the source of the remarkable green line which is everywhere conspicuous in the solar corona.

I must here pause for a moment to pay a passing tribute to the memory of Angström, whose great work on the solar spectrum will always remain as one of the finest monuments of the science of our period. The influence, indeed, which the labours of Angström and of Kirchhoff have exerted on the most interesting portion of later physics can scarcely be exaggerated; and it may be truly said that there are few men whose loss will be longer felt or more deeply deplored than that of the illustrious astronomer of Upsala.

I cannot pursue this subject further, nor refer to the other terrestrial elements which are present in the solar and stellar atmospheres. Among the many elements that make up the ordinary aërolite, not one has been discovered which does not occur upon this earth. On the whole, we arrive at the grand conclusion that this mighty universe is chiefly built up of the same materials as the globe we inhabit.

In the application of science to the useful purposes of life, chemistry and mechanics have run an honourable race. It was in the valley of the Clyde that the chief industry of this country received, within the memory of many here present, an extraordinary impulse from the application by Neilson of the hot blast to the smelting of iron. The Bessemer steel process and the regenerative furnace of Siemens are later applications of high scientific principles to the same industry. But there is ample work yet to be done. The fuel consumed in the manufacture of iron—as, indeed, in every furnace where coal is used—is greatly in excess of what theory indicates; and the clouds of smoke which darken the atmosphere of our manufacturing towns, and even of whole districts of country, are a clear indication of the waste, but only of a small portion of the waste, arising from imperfect combustion. The depressing effect of this atmosphere upon the working population can scarcely be overrated. Their pale—I had almost said etiolated—faces are a sure indication of the absence of the vivifying influence of the solar rays, so essential to the maintenance of vigorous health. The chemist can furnish a simple test of this state of the atmosphere in the absence of ozone—the active form of oxygen—from the air of our large towns. At some future day the efforts of science to isolate, by a cheap and available process, the oxygen of the air for industrial purposes may be rewarded with success. The effect of such a discovery would be to reduce the consumption of fuel to a fractional part of its present amount; and although the carbonic acid would remain, the smoke and carbonic oxide would disappear. But an abundant supply of pure oxygen is not now within our reach; and in the meantime may I venture to suggest that in many localities the waste products of the furnace might be carried off to a distance from the busy human hive by a few horizontal flues of large dimensions, terminating in lofty chimneys on a hill-side or distant plain? A system of this kind has long been employed at the mercurial mines of Idria, and in other smelting-works where noxious vapours are disengaged. With a little care in the arrangements the smoke would be wholly deposited, as flue-dust or soot, in the horizontal galleries, and would be available for the use of the agriculturist.

The future historian of organic chemistry will have to record a succession of beneficent triumphs, in which the efforts of science have led to results of the highest value to the well-being of man. The discovery of quinine has probably saved more human life, with the exception of that of vaccination, than any discovery of any age; and he who succeeds in devising an artificial method of preparing it will be truly a benefactor of the race. Not the least valuable, as it has been one of the most successful, of the works of our Government in India has been the planting of the cinchona-tree on the slopes of the Himalaya. As artificial methods are discovered, one by one, of preparing the proximate principles of the useful dyes, a temporary derangement of industry occurs, but in the end the waste materials of our manufactures set free large portions of the soil for the production of human food.

The ravages of insects have ever been the terror of the agriculturist, and the injury they inflict is often incalculable. An enemy of this class, carried over from America, threatened lately with ruin some of the finest vine districts in the south of France. The occasion has called forth a chemist of high renown; and in a classical memoir recently published, M. Dumas appears to have resolved the difficult problem. His method, although immediately applied to the *Phylloxera* of the vine, is a general one, and will no doubt be found serviceable in other cases. In the apterous state the *Phylloxera* attacks the roots of the plant; and the most efficacious method hitherto known of destroying it has been to inundate the vineyard. After a long and patient investigation, M. Dumas has discovered that the sulphocarbonate of potassium, in dilute solution, fulfils every condition required from an insecticide, destroying the insect without injuring the plant. The process requires time and patience; but the trials in the vineyard have fully confirmed the experiments of the laboratory.

The application of artificial cold to practical purposes is rapidly extending; and, with the improvement of the ice machine, the influence of this agent upon our supply of animal food from distant countries will undoubtedly be immense. The ice machine is already employed in paraffin-works and in large breweries; and the curing or salting of meat is now largely conducted in vast chambers, maintained throughout the summer at a constant temperature by a thick covering of ice.

I have now completed this brief review, rendered difficult by the abundance, not by the lack of materials. Even confining our attention to the few branches of science upon which I have ventured to touch, and omitting altogether the whole range of pure chemistry, it is with regret that I find myself constrained to make only a simple reference to the important work of Cayley on the Mathematical Theory of Isomers, and to elaborate memoirs which have recently appeared in Germany on the reflection of heat- and light-rays, and on the specific heat and conducting-power of gases for heat, by Knoblauch, E. Wiedemann, Winkelmann, and Buff.

The decline of science in England formed the theme, fifty years ago, of an elaborate essay by Babbage; but the brilliant discoveries of Faraday soon after wiped off the reproach. I will not venture to say the alarm which has lately arisen, here and elsewhere, on the same subject will prove to be equally groundless. The duration of every great outburst of human activity, whether in art, in literature, or in science, has always been short, and experimental science has made gigantic advances during the last three centuries. The evidence of any great failure is not, however, very manifest, at least in the physical sciences. The journal of Poggendorff, which has long been a faithful record of the progress of physical research throughout the world, shows no sign of flagging; and the Jubelband by which Germany celebrated the fiftieth year of Poggendorff's invaluable services was at the same time an ovation to a scientific veteran who has perhaps done more than any man living to encourage the highest forms of research, and a proof that in Northern Europe the physical sciences continue to be ably and actively cultivated. If in chemistry the case is somewhat weaker, the explanation, at least in this country, is chiefly to be found in the demand on the part of the public for professional aid from many of our ablest chemists.

But whatever view be taken of the actual condition of scientific research, there can be no doubt that it is both the duty and the interest of the country to encourage a pursuit so ennobling in itself, and fraught with such important consequences to the well-being of the community. Nor is there any question in which this Association, whose special aim is the advancement of science, can take a deeper interest. The public mind has also been awakened to its importance, and is prepared to aid in carrying out any proposal which offers a reasonable prospect of advantage.

In its recent phase the question of scientific research has been mixed up with contemplated changes in the great Universities of England, and particularly in the University of Oxford. The national interests involved on all sides are immense, and a false step once taken may be irretrievable. It is with diffidence that I now refer to the subject, even after having given to it the most anxious and careful consideration.

As regards the higher mathematics, their cultivation has hitherto been chiefly confined to the Universities of Cambridge and Dublin, and two great mathematical schools will probably be sufficient for the kingdom. The case of the physical and natural sciences is different, and they ought to be cultivated in the largest and widest sense at every complete university. Nor, in applying this remark to the English Universities, must we forget that if Cambridge was the *alma mater* of Newton and Cavendish, Oxford gave birth to the Royal Society. The ancient renown of Oxford will surely not suffer, while her material position cannot fail to be strengthened, by the expansion of scientific studies and the encouragement of scientific research within her walls. Nor ought such a proposal to be regarded as in any way hostile to the literary studies, and especially to the ancient classical studies, which have always been so carefully cherished at Oxford. If, indeed, there were any such risk, few would hesitate to exclaim —let science shift elsewhere for herself, and let literature and philosophy find shelter in Oxford! But there is no ground for any such anxiety. Literature and science, philosophy and art, when properly cultivated, far from opposing, will mutually aid one another. There will be ample room for all, and by judicious arrangements all may receive the attention they deserve.

A University, or Studium Generale, ought to embrace in its arrangements the whole circle of studies which involve the material interests of society, as well as those which cultivate intellectual refinement. The industries of the country should look to the universities for the development of the principles of applied as well as of abstract science; and in this respect no institutions have ever had so grand a possession within easy reach as have the universities of England at this conjuncture, if only they have the courage to seize it. With their historic reputation, their collegiate endowments, their commanding influence, Oxford and Cambridge should continue to be all that they now are; but they should, moreover, attract to their lecture-halls and working cabinets students in large numbers preparing for the higher industrial pursuits of the country. The great physical laboratory in Cambridge, founded and equipped by the noble representative of the House of Cavendish, has in this respect a peculiar significance, and is an important step in the direction I have indicated. But a small number only of those for whom this temple of science is designed are now to be found in Cambridge. It remains for the University to perform its part, and to widen its portals so that the nation at large may reap the advantage of this well-timed foundation.

If the Universities, in accordance with the spirit of their statutes, or at least of ancient usage, would demand from the candidates for some of the higher degrees proof of original powers of investigation, they would give an important stimulus to the cultivation of science. The example of many continental universities, and among others of the venerable University of Leyden, may here be mentioned. Two proof essays recently written for the degree of Doctor of Science in Leyden, one by Van der Waals, the other by Lorenz, are works of unusual merit; and another pupil of Professor Rijke is now engaged in an elaborate experimental research as a qualification for the same degree.

The endowment of a body of scientific men devoted exclusively to original research, without the duty of teaching or other occupation, has of late been strongly advocated in this country; and M. Fremy has given the weight of his high authority to a somewhat similar proposal for the encouragement of research in France. I will not attempt to discuss the subject as a national question, the more so as after having given the proposal the most careful consideration in my power, and turned it round on every side, I have failed to discover how it could be worked so as to secure the end in view.

But whatever may be said in favour of the endowment of pure research as a national question, the Universities ought surely never to be asked to give their aid to a measure which would separate the higher intellects of the country from the flower of its youth. It is only through the influence of original minds that any great or enduring impression can be produced on the hopeful student. Without original power, and the habit of exercising it, you may have an able instructor, but you cannot have a great teacher. No man can be expected to train others in habits of observation and thought he has never acquired himself. In every age of the world the great schools of learning have, as in Athens of old, gathered around great and original minds, and never more conspicuously than in the modern schools of chemistry, which reflected the genius of Liebig, Wöhler, Bunsen, and Hofmann. These schools have been nurseries of original research as well as models of scientific teaching: and students attracted to them from all countries became enthusiastically devoted to science, while they learned its methods from example even more than from precept. Will any one have the courage to assert that organic chemistry, with its many applications to the uses of mankind, would have made in a few short years the marvellous strides it has done, if Science, now as in mediæval times, had pursued her work in strict seclusion,

> Semota ab nostris rebus, seiunctaque longe
> Ipsa suis pollens opibus, nil indiga nostri?

But while the Universities ought not to apply their resources in support of a measure which would render their teaching ineffective, and would at the same time dry up the springs of intellectual growth, they ought to admit freely to university positions men of high repute from other universities, and even without academic qualifications. An honorary degree does not necessarily imply a university education; but if it have any meaning at all, it implies that he who has obtained it is at least on a level with the ordinary graduate, and should be eligible to university positions of the highest trust.

Not less important would it be for the encouragement of learning throughout the country that the English Universities, remembering that they were founded for the same objects, and derive their authority from a common source, should be prepared to recognise the ancient Universities of Scotland as freely as they have always recognised the Elizabethan University of Dublin. Such a measure would invigorate the whole university system of the country more than any other I can think of. It would lead to the strengthening of the literary element in the northern, and of the practical element in the southern Universities, and it would bring the highest teaching of the country everywhere more fully into harmony with the requirements of the times in which we live. As an indirect result, it could not fail to give a powerful impulse to literary pursuits as well as to scientific investigations. Professors would be promoted from smaller positions in one university to higher positions in another, after they had given proofs of industry and ability; and stagnation, hurtful alike to professorial and professional life, would be effectually prevented. If this union were established among the old Universities, and if at the same time a new University (as I myself ten years ago earnestly proposed) were founded on sound principles amidst the great populations of Lancashire and Yorkshire, the university system of the country would gradually receive a large and useful extension, and, without losing any of its present valuable characteristics, would become more intimately related than hitherto with those great industries upon which mainly depend the strength and wealth of the nation.

It may perhaps appear to many a paradoxical assertion to maintain that the industries of the country should look to the calm and serene regions of Oxford and Cambridge for help in the troublous times of which we have now a sharp and severe note of warning. But I have not spoken on light grounds, nor without due consideration. If Great Britain is to retain the commanding position she has so long occupied in skilled manufacture, the easy ways which (owing partly to the high qualities of her people, partly to the advantages of her insular position and mineral wealth) have sufficed for the past, will not be found to suffice for the future. The highest training which can be brought to bear on practical science will be imperatively required; and it will be a fatal policy if that training is to be sought for in foreign lands because it cannot be obtained at home. The country which depends unduly on the stranger for the education of its skilled men, or neglects in its highest places this primary duty, may expect to find the demand for such skill gradually pass away, and along with it the industry for which it was wanted. I do not claim for scientific education more than it will accomplish, nor can it ever replace the after-training of the workshop or factory. Rare and powerful minds have, it is true, often been independent of it; but high education always gives an enormous advantage to the country where it prevails. Let no one suppose I am now referring to elementary instruction, and much less to the active work which is going on everywhere around us, in preparing for examinations of all kinds. These things are all very useful in their way; but it is not by them alone that the practical arts are to be sustained in the country. It is by education in its highest sense, based on a broad scientific foundation, and leading to the application of science to practical purposes—in itself one of the noblest pursuits of the human mind—that this result is to be reached. That education of this kind can be most effectively given in a university, or in an institution like the Polytechnic School of Zürich, which differs from the scientific side of a university only in name, and to a large extent supplements the teaching of an actual university, I am firmly convinced; and for this reason, among others, I have always deemed the establishment in this country of Examining Boards with the power of granting degrees, but with none of the higher and more important functions of a university, to have been a measure of questionable utility. It is to Oxford and Cambridge, widely extended as they can readily be, that the country should chiefly look for the development of practical science; they have abundant resources for the task; and if they wish to secure and strengthen their lofty position, they can do it in no way so effectually as by showing that in a green old age they preserve the vigour and elasticity of youth.

If any are disposed to think that I have been carrying this meeting into dream-land, let them pause and listen to the result of similar efforts to those I have been advocating, undertaken by a neighbouring country when on the verge of ruin, and steadily pursued by the same country in the climax of its prosperity. "The University of Berlin," to use the words of Hofmann, "like her sister of Bonn, is a creation of our century. It was founded in the year 1810, at a period when the pressure of foreign domination weighed almost insupportably on Prussia; and it will ever remain significant of the direction of the German mind that the great men of that time should have hoped to develop, by high intellectual training, the forces necessary for the regeneration of their country." It is not for me, especially in this place, to dwell upon the great strides which Northern Germany has made of late years in some of the largest branches of industry, and particularly in those which give a free scope for the application of scientific skill. "Let us not suppose," says M. Wurtz in his recent report on Artificial Dyes, "that the distance is so great between theory and its industrial applications. This report would have been written in vain, if it had not brought clearly into view the immense influence of pure science upon the progress of industry. If unfortunately the sacred flame of science should burn dimly or be extinguished, the practical arts would soon fall into rapid decay. The outlay which is incurred by any country for the promotion of science and of high instruction will yield a certain return; and Germany has not had long to wait for the ingathering of the fruits of her far-sighted policy. Thirty or forty years ago, industry could scarcely be said to exist there; it is now widely spread and successful." As an illustration of the truth of these remarks, I may refer to the newest of European industries, but one which in a short space of time has attained considerable magnitude. It appears (and I make the statement on the authority of M. Wurtz) that the artificial dyes produced last year in Germany exceeded in value those of all the rest of Europe, including England and France. Yet Germany has no special advantage for this manufacture except the training of her practical chemists. We are not, it is true, to attach undue importance to a single case; but the rapid growth of other and larger industries points in the same direction, and will, I trust, secure some consideration for the suggestions I have ventured to make.

The intimate relations which exist between abstract science and its applications to the uses of life have always been kept steadily in view by this Association, and the valuable Reports, which are a monument to the industry and zeal of its members, embrace every part of the domain of science. It is with the greater confidence, therefore, that I have ventured to suggest from this chair that no partition wall should anywhere be raised up between pure and applied science.

The same sentiment animates our vigorous ally, the French Association for the Advancement of Science, which, rivalling, as it already does, this Association in the high scientific character of its proceedings, bids fair in a few years to call forth the same interest in science and its results, throughout the great provincial towns of France, which the British Association may justly claim to have already effected in this country. No better proof can be given of the wide base upon which the French Association rests than the fact that it was presided over last year by an able representative of commerce and industry, and this year by one who has long held an exalted position in the world of science, and has now the rare distinction of representing in her historic Academies the literature as well as the science of France.

Whatever be the result of our efforts to advance science and industry, it requires no gift of prophecy to declare that the boundless resources which the supreme Author and Upholder of the Universe has provided for the use of man will, as time rolls on, be more and more fully applied to the improvement of the physical, and, through the improvement of the physical, to the elevation of the moral condition of the human family. Unless, however, the history of the future of our race be wholly at variance with the history of the past, the progress of mankind will be marked by alternate periods of activity and repose; nor will it be the work of any one nation or of any one race. To the erection of the edifice of civilised life, as it now exists, all the higher races of the world have contributed; and if the balance were accurately struck, the claims of Asia for her portion of the work would be immense, and those of northern Africa not insignificant. Steam-power has of late years produced greater changes than probably ever occurred before in so short a time. But the resources of nature are not confined to steam, nor to the combustion of coal. The steady water-wheel and the rapid turbine are more perfect machines than the stationary steam-engine; and glacier-fed rivers with natural reservoirs, if fully turned to account, would supply an unlimited and nearly constant source of power, depending solely for its continuance upon solar heat. But no immediate dislocation of industry is to be feared, although the turbine is already at work on the Rhine and the Rhone. In the struggle to maintain their high position in science and its applications, the countrymen of Newton and Watt will have no ground for alarm so long as they hold fast to their old traditions, and remember that the greatest nations have fallen when they relaxed in those habits of intelligent and steady industry upon which all permanent success depends.

SECTION C.

GEOLOGY.

Opening Address by Prof. J. Young, M.D., F.G.S., President of the Section.

When the British Association met in Glasgow twenty-one years ago, Sir Roderick Murchison presided over Section C, and was surrounded by a brilliant company, whose names, now historical, were even then familiar for their accuracy of observation, for philosophic generalisation, and for the eloquence with which their science was clothed in words that charmed while they instructed. Lyell, Hugh Miller, Sedgwick, Jukes, Smith of Jordan Hill, Thomas Graham, Agassiz, Salter, Leonard Horner, John Phillips, Robert Chambers, H. D. Rogers, Charles Maclaren, Sir W. Logan. The list is a heavy one even for twenty-one years, and the changed circumstances will be fully realised by Nicol, Harkness, Egerton, Darwin, Ramsay, and others when they find Murchison's place occupied by one who holds it rather by the courtesy of the Council to the Institution in which we are assembled than by any claim he has to the honour.

It would be out of place for me to do more than refer to the geological advantages which have given to Glasgow its commercial greatness. In the Handbook prepared at the instance of the Local Committee will be found gathered together all the positive knowledge we possess regarding the mineralogy, stratigraphy, and palæontology of the west of Scotland. The specimens themselves are exhibited in the Hunterian Museum and in the Corporation Galleries; and I take it upon me to say the Glasgow geologists are as ready as ever to assist the investigations of students in special departments with all the material which richly fossiliferous strata yield and the careful skill of assiduous collectors can secure.

Thus relieved from entering into local details, I would ask your attention for a short while to some of the difficulties which a teacher experiences in summarising the principles of geology for his students.

I may be pardoned for reminding you that as yet there are in Scotland only two specially endowed teachers of geology. In the Universities, that science for which Scotsmen had done so much received only the odd hours spared from zoology. In 1867 the two courses were separated in Glasgow; in 1870 Sir R. J. Murchison founded the Chair of Geology in Edinburgh; in 1876 Mr. Honyman Gillespie endowed a Lectureship on Geology in Glasgow, not separating it from zoology, but rather desiring the two to remain associated, while means were provided for tutorial instruction in the elementary work of the class. When next the Association meets in Glasgow, I hope that the services which science has rendered to mining and metallurgy may have been recognised by those who have reaped the benefit. During the efforts of years to obtain provision for systematic teaching in mining and metallurgy, practical and scientific have always been set in opposition by those whom I addressed. In another twenty years it may have become apparent that it is possible for a man to be both practical and scientific, and that the combination is most conducive to economy.

Geology occupies the anomalous position of being a science without a special terminology—a position largely the result of its history, but to some extent inherent in its subject-matter. Treated of by Hutton and Playfair and their opponents in the ordinary language of conversation, current phrases were adopted into science not so much acquiring special meanings as adding new ambiguities to those already existing. Every one seemed to understand them at once; and thus, as no one was obliged to attach very precise meanings to them, the instruments of research became its impediments, and the phrases in common use at the beginning of the century have transmitted to the present day the erroneous ideas of those by whom they were first employed. When Lyell, in 1832, methodised the knowledge accumulated prior to that date, he had, in organising the science, to choose between inventing an appropriate terminology and adopting that in common use. By doing the latter he promoted the popularity of the science, though at the cost of some subsequent confusion; by attempting the former he would have set in arms against him those who would, according to the pedantry of the time, have denounced his neologisms and formed in them a decorous veil for the objections which they entertained on other grounds to his views. Lyell was not the man to face the latter difficulty, nor can it be charged against him that he was wittingly neglectful of the interests of science. But to the use of conversational language are traceable certain assumptions to which I desire to draw your attention. In venturing criticism of this kind, I am not unmindful of the Nemesis which has overtaken my colleague, Sir W. Thomson, for his comments on Lyell's language. Thomson took exception to language which implied a kind of perpetual motion—a circulation of energy at variance with the teaching of physics; and behold, two or three years after, Lockyer has published, as a physical astronomer, and Prestwich has approved, as a geologist, the opinion that the temperature of the sun may have fluctuated—that, in fact, changes of chemical combination may from time to time have refreshed the heat of the planet, whose uniform rate of cooling Sir William had assumed.

When stratigraphical geology first received due attention, the notion was prevalent that each formation terminated suddenly by cataclysm; it was therefore natural that the British succession—the earliest to be tabulated in detail—should be taken as a standard for other countries, and that the enumeration of the series should be a generalised section in which were incorporated those strata not present in Britain. The "intercalation" of beds thus practised to make an "incomplete" series "complete" still survives, as do the terms, though the notions which underlie them are formally denied by those who use them. A patriotic fellow-countryman once surprised us by his vehement denunciation of a treacherous Scot who called the Lanarkshire limestones meagre and incomplete as compared with the English. With knowledge he might have made his criticism useful; as it was he only gave a fresh example of the national peculiarity which, if it cannot prove Scotland to be better off than its neighbours, is content to make it out to be no worse. The abundant fossils of the Mesozoic strata of England and France rendered comparison easy, and created the impression that conchology was the A B C of geology, physical being subordinated to palæontological evidence. The balance has been somewhat restored by the Geological Survey, the precision of whose physical observations enables them to guide the palæontologist as often as guided by him. But one legacy from our predecessors we have not got rid of, nor indeed has its value been much called in question.

The process of intercalation had at first to do only with observed gaps into which obvious equivalents could be received. But as the needs of speculative biology rapidly increased, in the same ratio did belief in the imperfection of the geological record increase, till now we have that record described as a most fragmentary volume, nay as the remains of the last volume whose predecessors are lost to us.

Sir W. Thomson did good service by calling in question, on physical grounds, the indefinite extension backwards of geological time. The first fruits of his crusade were the definitions of Uniformitarianism and Evolution which Prof. Huxley gave. Henceforth no one will maintain the onesided notions regarding these two opposing views of the earth's history which were adopted in ignorant misconception or dictated by conceit and bigotry. But the service done was even greater, for while it became clear that a knowledge of physics was indispensable to him who would promulgate sound notions, it was further apparent that both biological and geological evolution had a limit in time; that in fact, on the assumption of the primitive incandescence of our globe, the date might be at least approximately fixed when the mechanical processes now at work commenced, and when the surface of the earth became habitable. Nothing more has yet been done than to point out the way; for, though Prof. Guthrie Tait indicates a limit of from fifteen to ten millions of years, that statement can only be regarded as in effect, though not perhaps in intention, as a protest against the liberality and vagueness of Sir W. Thomson's allowance, which gave geologists a range of one to two hundred millions of years.

The reconciliation of physicists and geologists is not likely to come through Mr. Lockyer's researches, even if the earth's history be shown to have been identical, unless the renewal of the earth's heat be shown to be compatible with continued life on the surface. If the reconciliation is looked for through the prolonged duration of the sun's life, that being the gauge of the earth's duration, the expectation is still based on the supposed need of very great time for geological processes, or rather on the supposed need of very great time for biological evolution, to which geological evolution has been squared. There is another direction in which these results may help us to meet the limitation assigned by the physicists; the intervals of variation of temperature may be shorter than those which separate the maxima of eccentricity of the earth's orbit, and thus the repeated cold periods of which we have suggestions in the stratified rocks, may have recurred within a shorter total period than is at present claimed.

It is scarcely within the compass of this address to enter into the questions involved, but it is permissible to indicate the reason for delaying meanwhile acceptance of any precise limit of time. There is as yet too much diversity of opinion as to the elements of the problem. Physicists are by no means at one as to the conditions which permit or prohibit shifting of the earth's axis. Calculations are based on the assumption of the regularity of the earth's form under a certain constant relation of the masses, albeit of diverse specific gravity, which compose it. It is moreover assumed that the ratio of land and water has been uniform, though the formation of the grand features of the land by contraction of the cooling mass has not yet been considered as affecting this assumption by altering the disposition of the water. On the one hand it has been shown that the existence of uniform temperatures over the earth's surface is a gratuitous hypothesis; on the other hand it is clear that the existing distribution of light and heat is incompatible with the flourishing of an abundant Carboniferous and Miocene flora within a short distance of the North Pole. One expects that astronomers will look to the shifting of the axis of rotation as the possible explanation of the difficulty, taking into account likewise the shifting of the centre of gravity necessarily following those displacements of matter which, on the contraction theory, have determined the positions of the main continents and oceans.

Mr. Evans, in his address to the Geological Society, referred to the deviation of the magnetic axis as perhaps due to such shifting of the materials composing the inner mass of our globe. May not the conjectures of M. Elie de Beaumont be after all in the right direction? May not the change of trend which led him to classify the mountain-chains by reference to the age at which they had been elevated, be associated with movements which did not in all cases result in shiftings of the earth's axis, so pronounced as those which permitted the Carboniferous and Miocene floras to invade successfully the Arctic Regions, or the phenomena of the glacial epoch or epochs, to manifest themselves in the low latitudes when their traces have been recognized?

Waiving, for the present, inquiry into the influence which the admission of a possible shifting of the earth's axis might have on our estimate of geological time, I shall return to the phraseology whose amendment seems advisable.

The confusion which exists is well illustrated in a remark by an eminent writer to the effect that the progress of geological research tends to prove the "continuity of geological time." The phrase in itself involves an absurdity; but what is meant is, that the successive so-called formations pass into each other by imperceptible gradation; and that, as time goes on, we shall be more and more able to intercalate strata so as to present a continuous scale of animal and vegetable forms. This is one out of many samples of the extreme length to which the thirst for strict correlation may go. We find in Murchison's writings and elsewhere pointed protests against the succession of strata in one district being held to rule that in other districts; but these are rather concessions wrung from their author by the pressure of particular instances than acknowledgments of a rule applicable to contiguous and to distinct localities alike. I could not perhaps take a better example than the strata which contain the remains of the fossil *Equidæ*. If we arrange the fossils in any series representing the modification of particular structures, or averaging the modifications of all the structures, we shall find that the terms of the series are met with, now in Europe, now in America; yet no one would venture to intercalate the European in the American Tertiary series so as to square the geological record with an assumed zoological standard. The notion of gradations, the extreme view of correlations has led to results which are, to put it mildly, of doubtful value. Yet it was a natural result of the work of Cuvier and other palæontologists among the Mesozoic and Eocene fossiliferous deposits. The statistical method invented by Lyell is simply a mode of gradations. Intercalation of strata is therefore a survival from an earlier stage of the science, and carries with it a distinct echo of the catastrophic notion that strata were formed simultaneously and generally over the earth's surface, if not universally.

The geological record has been compared to a volume of which pages have here and there disappeared; and the incompleteness of the record has been inferred from the frequency of pronounced gaps in the succession of strata. Of these gaps, these unconformities, Prof. Ramsay has shown the importance by demonstrating that they represent the lapse of unknown, but varying, and in all cases, considerable periods of time. The intercalation of strata, assumed to fill up the gap, and hereby to give symmetry to systematic classifications, can only be done by an appeal to the statistical method, a fauna containing forms characteristic of higher and lower beds being assumed to represent an intermediate point in time, whereas it might be equally well claimed as representing an intermediate area in space, and as being possibly representative of the whole gap and of some of the strata above and below it.

The definition of a formation as representing a certain period of time, still repeated with various modifications, is to blame for this and several other curiosities of procedure. But the climax of symmetrical adjustments is reached when we find "natural groups" established—when, in other words, an attempt is made to show a regular periodicity of phenomena in geology. Dawson proposed a quarternary, Hull a ternary classification, to neither of which should I now refer, but that the deserved estimation of these writers is apt to perpetuate what seems to be an unsafe view of geological succession.

Hull's arrangement has the merit, by force of its simplicity, of bringing the vainness of the attempt into prominence. Dawson has complicated his classification so as to render it impracticable. A natural group of strata, one in which elevation, deep depression, elevation, record themselves in rocks so as to establish geological cycles, implies several things for which we have no evidence. Most important of all does it imply, that the events above noted should recur in every area in the same order, that they should recur at equal intervals of time, and therefore yield equal masses of strata, and above all that the superior and inferior limits of each natural and coterminous group should consist of a mass of similar strata, one portion of which shall belong to the earlier, the other to the later group. Here then we have implied, not catastrophic simplicity as regards the strata, but something very like it as regards the subterranean forces.

Mr. Hull has not, however, been able to surrender himself wholly to his speculation. He has admitted "gaps," breaks, that is to say, for which he finds no equivalents in the British series; the strata that should occupy these gaps having been either removed by denudation or never deposited, the British area being at these times above water. The concession is fatal to the scheme. But the very use of the word gap recalls the phrases "complete and incomplete," and their nearest of kin "base of a formation." Prof. Ramsay used the word "break" to mark his unconformities, but no term has been proposed for "the base of a formation." The term was in constant use when such base was always claimed to be a conglomerate. That notion is now exploded, but no distinction is drawn between the lowest bed of a group of conformable strata, and the bed or beds which repose unconformably on those below them. Thus, the London basin has the Thanet beds, the Reading beds, and the London clay successively resting on the chalk, and each of these is the base for its proper locality, unless it be asserted that in this and similar cases the lowest beds once covered a wider area, and were then removed. But a more important case is presented by the great calcareous accumulations of the Carboniferous and Chalk series. The Lower Greensand is to the latter series in England what the lowest stratum of the Chalk would be if we could get at it. The Carboniferous Limestone rests directly on the Red Sandstone in central England, farther north it rests on the Calciferous Sandstones. Thus the base of the formation varies according to locality, or rather according to the circumstances of deposition, and we need a term which would indicate a difference between the conformable and unconformable succession. Mr. Judd has lamented the equivocal use by English writers, of the term formation, which etymologically is as well applied to the Chalk without flints as to the whole Cretaceous series. He advocates "system" as applicable to the larger groups, the Cretaceous system for example. But it seems as if the time were come for still further restrictions of either or both terms.

The analogy of the geological record to an incomplete volume is, like most analogies, at once imperfect and misleading. Rather might the record be compared to the fragments of two volumes which have come to be bound together, so that it is not possible to recognise the sequence. Or perhaps it might be better compared to a universal history in which, by omission of dates, the chronology is thoroughly obscured, and the necessary treatment of each nation by itself conceals the contemporaneity of events. We have the aquatic record and the terrestrial record, and these two are going on simultaneously. It is as yet, and probably always will be impossible to recognise the marine deposits which correspond to the terrestrial remains, save perhaps in the most recent geological times. We now know that the life of the Cretaceous seas is not wholly extinct in the existing Atlantic Ocean, but exists there to an extent which would entitle the deposits of that area to rank by the statistical method as intermediate between the Cretaceous and the Tertiary. It is obviously impossible to include under one term deposits which are associated with geographical changes so important as those commonly accepted as having prevailed during the Tertiary epoch. The Meosozoic forms pass gradually into the Tertiary, how gradually we cannot say, since the deep sea equivalents of the European Tertiaries are not certainly known to us. But as a portion survives to the present day, and as, presumably, the extinction was not rapid (for it is only in the case of land animals that sudden disappearances are as yet probable), it is obvious that the successor, the heir of the Chalk, was not the Eocene, nor necessarily the Miocene known to us, but probably deposits still buried under the Atlantic.

My object is to show that, even the limitation of time which Prof. Tait prescribes for us, may not after all be too narrow for the processes which have resulted in our known stratigraphy. Mr. Darwin speaks of the geologic record being the imperfect record of the last series of changes, the indefinite extension of time anterior to the earliest fossiliferous rocks being necessary for the full evolution of organic forms. But is there any ground for the assumption? True that the Laurentians contain fragments of antecedent rock, but were these fossiliferous? Are they the remains of land surfaces on which living beings flourished, or are they only the *débris* of the first consolidated portion of the earth's crust on which if organisms existed they may have been the most primitive of our organic series? Mr. Jukes refers to the possibility of such earlier strata having existed, but he wrote when geologists were dominated with the belief in the indefiniteness of geological time. Now we are brought by physicists, like Sir W. Thomson and Captain Dutton, to face the question—is there evidence of such earlier masses of stratified deposits? If we allow to the physical argument all the weight to which its advocates deem it entitled, if we accept fifteen millions of years, nay, even if we admit one hundred millions of years as our limit, it follows that we may still regard the earth as in its first stage of cooling. But when we turn to the geological evidence, all that can be advanced is that the Laurentian strata contain fragments presumably derived from earlier strata; but metamorphosed fragments among metamorphic rocks are not the most reliable guides, and there is the positive evidence that the Laurentian area has not been covered to any extent, if at all, by later deposits. So far as direct proof goes, therefore, we have none that the earliest known stratified rocks are not also the earliest deposited after cooling. Even if we disregard the limits imposed by the philosophers, liberal though they are in Sir W. Thomson's hands, the absence of proof that later deposits covered Laurentian areas seems entitled to greater weight than is usually allowed to negative evidence. At best the assertion of antecedent strata is an arbitrary one, which any of us is at liberty to contradict, and in favour of which no physical evidence, and only zoological prejudices can be adduced. The earliest stratified deposits known are the Laurentian, and they are, so far as we know, the earliest to have been deposited.

But apart from these possible though improbable earlier deposits, geological time is said to be lengthened by the missing strata of later periods. Mr. Croll has given great prominence to this, which is another of the things taken for granted in geology, commenting on Mr. Huxley's remark that if deposit went on at the rate of 1 foot for 1,000 years, the 100,000 feet of strata assumed by him to form the earth's crust, would be laid down in the 100 millions of years which Sir W. Thomson had given as the limit. But, says Mr. Croll, what of the missing strata? It is commonly said that we have only a part of the deposits of any period, that the last have been denuded away, and that thus the time needed for their deposit and for their subsequent removal are out of our knowledge. This is based on what we see on the shore when the tide rises and falls and washes off at each turn a part of the sand and mud laid down in the interval. But the older deposits were laid down in deeper water than that between tide-marks, and were for the most part laid down during subsidence. Even admitting removal of part of the strata to have taken place during re-emergence, the quantity so withdrawn cannot be proved to represent more than a small fraction of the total. To provide the needed elongation of geological time by an appeal to arbitrary speculations is not admissible. Belief on belief is, as Butler says, bad heraldry. The denudation to which importance is justly ascribed is that represented by unconformity. Re-elevation has been accompanied by disturbance of the area from a different centre than that around which subsidence took place. The strata are worn obliquely, and thus thickness of the mass at one place is greatly diminished, though it does not follow in all cases that the maximum thickness of the strata has been effected.

The importance, as I deem it, the excessive importance which is attached to the missing strata is asserted by biologists who, apparently unconsciously, seek to gain, by prolonging the interval between successive groups, the time which ought rather to be sought for in tracing, were that possible, the migrations of the species which seem to have suddenly died out. In other words there is a reversion to the older ideas regarding the succession of strata which are embodied in such phrases as the Age of Fishes, the Age of Reptiles, and the like.

But the inequality of surface which unconformity involves, entails that other consequence that the maximum thicknesses of the two masses of deposits do not coincide in position. Hence the thickness of the strata in the area will be exaggerated, the time spent in deposit also exaggerated, if the two thicknesses are put together. This has been done by Mr. Darwin in drawing inferences from the measurements given him by Prof. Ramsay, measurements which, on the face of them, do not represent a continuous pile of rock. Mr. Darwin assumes either that the Welsh Hills (not to speak of the Hebrides) were covered by all the later strata now denuded or that if we sink a bore, say on the east coast, we should go through the whole series as tabulated. When Prof. Huxley took 100,000 feet as the thickness of the sedimentary series, the same notion was unconsciously present, the same survival of catastrophism, the onion-coat theory as Herbert Spencer named it.

The Geological Survey has corrected its tables in one important direction; it has shown the contemporaneity of unlike groups in different parts of Britain, the distinct types of the old red sandstone, carboniferous, permian, and purbecks being placed in parallel columns. To some extent this is a curtailment of the thickness of the rock series, the dissimilar strata are not piled on each other. But the curtailment might be carried still farther. The marine and terrestrial conditions are simultaneous; if we could identify the dry land for each deep sea we should have possibly the overlap of periods producing extraordinary combinations, though not perhaps of Mesozoic and Palæozoic faunas contemporaneous. But the British series may be tabulated as follows:—

Land Surfaces.	*Lacustrine and Fluviatile.*	*Marine.*
		Laurentian?
	Cambrian.	Silurian.
	Old Red Sandstone.	
	Calciferous Sandstones.	Carboniferous Limestone.
Coal Measures.		
Permian.		Jurassic.
	Trias.	
	Purbeck.	Neocomian.
Wealden.		
	Miocene.	Cretaceous.
	Pleistocene.	

In the case of the Cretaceous series, Mr. Ramsay has given illustration of the ingenious views of De La Beche regarding the contemporaneity of deposits superposed one on the other. The Lower Greensand is contemporaneous with part of the Chalk, so were parts of the Wealden: nay, even of the Purbecks a portion must have been forming while the Cretaceous sea was gradually deepening southward and eastward.

It may be said that the recognition of the parallelism would not make very much difference after all; that it would not one whit lessen the time spent in forming 500 feet of rock to know that there was elsewhere another 500 feet formed at the same time. But the shortening of the geological list by striking out the overlaps of the formations and thus counting them only once is of itself a matter of some consequence, since the maximum thickners of the Cretaceous being nearly 3,000 feet and that of the Weald 1,500 feet, even the partial coincidence, in time, of these masses, would, on Mr. Croll's calculation of 1 foot of deposit per 1,000 years, make a considerable difference in the chronology, still more if the Carboniferous Limestone be set against its probable contemporaries the Upper Old Red Sandstone and Coal-measures. Mr. Jukes' bold erasure of the Devonians was of itself a very important change on the chronological table, and I doubt not others may yet be achieved. But, it may be said, the Cretaceous still rests on the Wealden; the vertical thickness still remains. But is the ordinary method of estimating the thickness quite reliable? In some cases, as in the productive coal-measures, there is tolerable uniformity; but among the lower coals and the Mesozoic strata, where the strata or groups of strata are not regular, the maximum thicknesses of all are, as has been already shown, apt to be taken, and thus an aggregate more or less in excess of the real thickness results.

But recurring to an objection already referred to, arrange it as you like, you get, say in Wales, a known thickness of 50,000 feet. But the rocks there are tilted, and the absolute depth which they attain in this position is unknown. In North America the Laurentians are estimated at 30,000 feet; but though there is every reason to believe that they have not been covered to any extent with later deposits, the total thickness of sedimentary crust is, for the same reason as in Wales, unknown. Bigsby has shown how varied are the surfaces on which the later deposits are laid down; how great, therefore, must be the deductions from the same total of maximum or even average thickness of all formations before we approximate to the actual thickness of sedimentary deposits at any one point. But take the actual thickness in Wales as given in Jukes's Manual from the Survey data: for the Cambrians we have from 23-28,000 feet; Silurians, Upper and Lower, not counting breaks by unconformities, 20,000. If denudation takes place at the rate of 1 foot in 6,000 years, and deposit at the same rate, we should have for the Silurians alone 120,000,000 of years needed. If, however, deposit takes place at the rate of 1 foot in 14,400 years, 288,000,000 millions of years would be needed for the accumulation of the surviving strata. It is obvious that the rate of deposit or denudation, or both is misunderstood. The stratified rocks equal in amount the material denuded; if we knew the total amount of denudation we should know, not merely the residuum of rock open to our inspection, but the total amount of stratified deposits which had been formed, or at least approximately, for the deposit of materials removed is not synchronous with their removal. Obviously these elements are not known, and cannot be known to us. Mr. Croll, who has investigated the question theoretically, assumes that deposit and denudation take place in equal times, and assumes further a uniform distribution over the whole or over a part of the sea-bottom. But Prof. Geikie's table shows that, if we are to take averages as a safe guide, the land is lowered at the rate of two in 6,000 years. Moreover, if, as Mr. Croll points out, deposit was less during the glacial epoch, the process must have been more rapid since, and thus an irregularity is introduced which impairs the value of the calculations. Prof. Hughes, in the brief abstract of his Royal Institution address, which alone I have had the opportunity of seeing, contests the validity of any estimates of time on the basis of our existing knowledge. I do not mean to enter into this question, but I may be allowed to remark that any conclusions, founded on mean thickness of sedimentary formations are of no value. It is not the time necessary for the building up of a mean thickness, but that necessary for the formation of the maximum thickness in particular regions which we have to consider.

If the Laurentian rocks and their equivalents are to be regarded as the earliest stratified deposits, or rather, if there is no reason for believing that they were preceded by other stratified rocks, the relation of Huxley's homotaxis to any classification of strata having the Laurentians as a fixed point is worth investigating. The universal diffusion of species in the earlier strata was first the accepted creed of geologists. Then it was denied, though the language of the earlier faith continued current. Again, we return towards the doctrine of extensive simultaneous diffusion, but under a very much modified form. The *Challenger* reports bear testimony to the wide distribution of forms in the deepest oceans, and when we turn from these and compare the lists of fossil species so found widely distributed, it appears that here again we have oceanic forms, or at any rate those found in such limestones as are safely assigned to a deep water origin. Ramsay has shown that the continental epochs in Western Europe overlasted considerable periods of time. The antiquity of the Atlantic and Pacific is certain; even their primitive character is possible. Thus there are two conditions—land and deep sea—reasoning regarding which must be quite different from that applicable to the intermediate conditions. It is exactly these intermediate states which present practical and speculative difficulty. Theories which account for mountains and oceans fail to explain the "oscillations" which were wont to be appealed to when terrestrial and marine surfaces succeeded each other. But the assumed movement of the land is by no means a certainty, and as in the kindred case of faults, we need terms which shall be neutral, whether the land has moved upwards or the sea shrunk downwards. The terms Palæozoic, Mesozoic, and Cainzoic have long held their places from the reluctance to disturb established nomenclature, as well as from the difficulty of inventing appropriate substitutes; but if retained at all, we know now that the relations they represent are not the same for the terrestrial, the deep oceanic, and the intermediate areas, any more than the life is the same under those three conditions.

I have once before called attention to a grave difficulty in the physical geography of Scotland; and as Mr. Seeley has since then raised the same question without obtaining an answer, I would again state the case as one which seems to involve the revisal of some definitions.

The Silurian hills of South Scotland are commonly said to have been covered by Old Red Sandstone and even by Carboniferous strata, patches of these rocks being met with on the south side of the fault which defines these hills with their abrupt, coast-like margin seen from Edinburgh, or from Symington station on the Caledonian line. But the surface of these Silurians was denuded before the Old Red times, as Mr. Geikie has showed. Nay, valleys existed as now, and in the same positions as now. At the present time the rivers flow in identically the same valleys, in at least the cases of the Nith, the Annan, the Lauder, and the Liddell; and the boundaries of the areas are so well known that we can safely assert no buried channel to exist such as we find on the tributaries of the Clyde. That the channels were occluded in glacial times we may take for certain; that the obstruction has been washed away and the courses cleared is equally certain. The surface contours were not materially altered, so that the retreating ice left hollows in the position of the old valleys. But the case is quite different when we deal with the older rocks. Their succession is marked by unconformities and overlaps, which it is impossible to picture as associated with full preservation of the surface features on which they were laid down; and when the thickness comes to be as much as 1,000 feet or more, and of that thickness a part at least made up of marine strata, the relapse of all the streams to their old causes is an event of the highest improbability. Mr. Topley has pointed out how the dip of strata may, under certain circumstances, coincide with their thinning out to the margins of their area of deposit, changes of angle in highly inclined strata pointing in the same direction. The ordinary rule of protracting strata, and thus restoring their thickness over the adjacent high ground, is, in the case, at least, of South Scotland, a method which imposes on atmospheric denudation, even if aided by the sea, a most complicated task.

Had time permitted, it might have been interesting to note the changing phraseology regarding faults, and the pertinacity with which phrases involving the most unsatisfactory and improbable causation continue to be used. Upcast and downcast, upthrow and downthrow, displacement upwards or downwards—these it may be said are of small importance; they are only symbols. But in the first place they are mischievous so far as they give students confused ideas with which to contend, and in the second place the continued acceptance of loose phraseology is peculiar to geology; even in metaphysics, where the subject matter is much more conveniently discussed in ordinary language, new terms are employed to a great extent. But important as I therefore regard these terms from the teacher's point of view, the greater importance attaches to the accuracy of the notions which underlie our language regarding the processes and rates of deposit and denudation.

So far as our present knowledge goes, we must accept it as certain that there is some limit to the duration of the earth in the past. Neither philosophers nor astronomers are agreed on the essential points of the problem, nor have they considered all the possible changes in the position of the earth's axis, and in the rate at which the earth loses heat. The limits hitherto prescribed are so discrepant that we cannot as yet accept any as fixed. Neither have geologists so accurate a knowledge of geological processes that they can speak with confidence either of the absolute or relative rates at which rock formation has advanced. The geologist has hitherto asked for more time, not because he himself was aware of his need, but from a generous regard for the difficulties in which his zoological brother found himself when he attempted to explain the diversity of the animal series as the result of slowly-operating causes. The geologist asked for more time simply because he could form no just estimate of what was needed for the physical processes with whose results he was familiar. But palæontological domination is now at an end; and the increasing number of geologists, who are also competent physicists and mathematicians, seems to mark a new school, which will strive to interpret more precisely the accumulated facts. Such at least seems the history of the past fifteen or twenty years. Such seems the direction in which speculation now tends, and in the foregoing remarks I have endeavoured faithfully to represent the drift of our science. To many here present much of what I have said is already familiar; I therefore give place to the more legitimate business of the Section, looking to receive elsewhere "such censures as may be my lot."

SECTION D.

Biology.

Opening Address by the President, Alfred Russel Wallace.

Introduction.

The range of subjects comprehended within this Section is so wide, and my own acquaintance with them so imperfect and fragmentary, that it is not in my power to lay before you any general outline of the recent progress of the biological sciences. Neither do I feel competent to give you a summary of the present status of any one of the great divisions of our science—such as Anatomy, Physiology, Embryology, Histology, Classification, or Evolution—Philology, Ethnology, or Prehistoric Archæology; but there are fortunately several outlying and more or less neglected subjects to which I have for some time had my attention directed, and which I hope will furnish matter for a few observations, of some interest to biologists, and at the same time not unintelligible to the less scientific members of the Association who may honour us with their presence.

The subjects I first propose to consider have no general name, and are not easily grouped under a single descriptive heading; but they may be compared with that recent development of a sister-science, which has been termed Surface-geology or Earth-sculpture. In the older geological works we learnt much about strata, and rocks, and fossils, their superposition, contortions, chemical constitution, and affinities, with some general notions of how they were formed in the remote past; but we often came to the end of the volume no whit the wiser as to how and why the surface of the earth came to be so wonderfully and beautifully diversified; we were not told why some mountains are rounded and others precipitous; why some valleys are wide and open, others narrow and rocky; why rivers so often pierce through mountain-chains; why mountain lakes are often so enormously deep; whence came the gravel, and drift, and erratic blocks, so strangely spread over wide areas while totally absent from other areas equally extensive. So long as these questions were almost ignored, geology could hardly claim to be a complete science, because, while professing to explain how the crust of the earth came to be what it is, it gave no intelligible account of the varied phenomena presented by its surface. But of late years these surface-phenomena have been assiduously studied; the marvellous effects of denudation and glacial action in giving the final touches to the actual contour of the earth's surface, and their relation to climatic changes and the antiquity of man, have been clearly traced, thus investing geology with a new and popular interest, and at the same time elucidating many of the phenomena presented in the older formations.

Now, just as a surface-geology was required to complete that science, so a surface-biology was wanted to make the science of living things more complete and more generally interesting, by applying the results arrived at by special workers, to the interpretation of those external and prominent features whose endless variety and beauty constitute the charm which attracts us to the contemplation or to the study of nature. We have the descriptive zoologist, for example, who gives us the external characters of animals; the anatomist studies their internal structure; the histologist makes known the nature of their component tissues; the embryologist patiently watches the progress of their development; the systematist groups them into classes and orders, families, genera, and species; while the field-naturalist studies for us their food and habits and general economy. But till quite recently, none of these earnest students, nor all of them combined, could answer satisfactorily, or even attempted to answer, many of the simplest questions concerning the external characters and general relations of animals and plants. Why are flowers so wonderfully varied in form and colour? what causes the Arctic fox and the ptarmigan to turn white in winter? why are there no elephants in America and no deer in Australia? why are closely allied species rarely found together? why are male animals so frequently bright coloured? why are extinct animals so often larger than those which are now living? what has led to the production of the gorgeous train of the peacock and of the two kinds of flower in the primrose? The solution of these and a hundred other problems of like nature, was rarely approached by the old method of study, or if approached was only the subject of vague speculation. It is to the illustrious author of the "Origin of Species" that we are indebted, for teaching us how to study nature as one great, compact, and beautifully adjusted system. Under the touch of his magic wand the countless isolated facts of internal and external structure of living things—their habits, their colours, their development, their distribution, their geological history,—all fell into their approximate places; and although from the intricacy of the subject and our very imperfect knowledge of the facts themselves, much still remains uncertain; yet we can no longer doubt that even the minutest and most superficial peculiarities of animals and plants either, on the one hand, are or have been useful to them, or, on the other hand, have been developed under the influence of general laws, which we may one day understand to a much greater extent than we do at present. So great is the alteration effected in our comprehension of nature by the study of variation, inheritance, cross-breeding, competition, distribution, protection, and selection—showing, as they often do, the meaning of the most obscure phenomena, and the mutual dependence of the most widely-separated organisms, that it can only be fitly compared with the analogous alteration produced in our conception of the universe by Newton's grand discovery of the law of gravitation.

I know it will be said (and is said), that Darwin is too highly rated; that some of his theories are wholly and others partially erroneous, and that he often builds a vast superstructure on a very uncertain basis of doubtfully interpreted facts. Now, even admitting this criticism to be well founded—and I myself believe that to a limited extent it is so—I nevertheless maintain that Darwin is not and cannot be too highly rated. For his greatness does not at all depend upon his being infallible, but on his having developed, with rare patience and judgment, a new system of observation and study, guided by certain general principles which are almost as simple as gravitation, and as wide-reaching in their effects. And if other principles should hereafter be discovered, or if it be proved that some of his subsidiary theories are wholly or partially erroneous, this very discovery can only be made by following in Darwin's steps, by adopting the method of research which he has taught us, and by largely using the rich stores of material which he has collected. The "Origin of Species," and the grand series of works which have succeeded it, have revolutionised the study of biology. They have given us new ideas and fertile principles. They have infused life and vigour into our science, and have opened up hitherto unthought of lines of research on which hundreds of eager students are now labouring. Whatever modifications some of his theories may require, Darwin must none the less be looked up to as the founder of philosophical biology.

As a small contribution to this great subject, I propose now to call your attention to some curious relations of organisms to their environment, which seem to me worthy of more systematic study than has hitherto been given them. The points I shall more especially deal with are—the influence of locality, or of some unknown local causes, in determining the colours of insects and, to a less extent, of birds; and the way in which certain peculiarities in the distribution of plants may have been brought about by their dependence on insects. The latter part of my address will deal with the present state of our knowledge as to the antiquity and early history of mankind.

On some Relations of Living Things to their Environment.

Of all the external characters of animals, the most beautiful, the most varied, and the most generally attractive, are the brilliant colours and strange yet often elegant markings with which so many of them are adorned. Yet, of all characters, this is the most difficult to bring under the laws of utility or of physical connection. Mr. Darwin—as you are well aware—has shown how wide is the influence of sex on the intensity of colouration; and he has been led to the conclusion that active or voluntary sexual selection is one of the chief causes, if not the chief cause, of all the variety and beauty of colour we see among the higher animals. This is one of the points on which there is much divergence of opinion even among the supporters of Mr. Darwin, and one as to which I myself differ from him. I have argued, and still believe, that the need of protection is a far more efficient cause of variation of colour than is generally suspected; but there are evidently other causes at work, and one of these seems to be an influence depending strictly on locality, whose nature we cannot yet understand, but whose effects are everywhere to be seen when carefully searched for.

Although the careful experiments of Sir John Lubbock have shown that insects can distinguish colours—as might have been inferred from the brilliant colours of the flowers which are such an attraction to them—yet we can hardly believe that their appreciation and love of distinctive colours is so refined as to guide and regulate their most powerful instinct—that of reproduction. We are therefore led to seek some other cause for the varied colours that prevail among insects; and as this variety is most conspicuous among butterflies,—a group perhaps better known than any other—it offers the best means of studying the subject. The variety of colour and marking among these insects is something marvellous. There are probably about ten thousand different kinds of butterflies now known, and about half of these are so distinct in colour and marking that they can be readily distinguished by this means alone. Almost every conceivable tint and pattern is represented, and the hues are often of such intense brilliance and purity as can be equalled by neither birds nor flowers.

Any help to a comprehension of the causes which may have concurred in bringing about so much diversity and beauty must be of value, and this is my excuse for laying before you the more important cases I have met with of a connection between colour and locality.

Our first example is from tropical Africa, where we find two unrelated groups of butterflies belonging to two very distinct families (Nymphalidæ and Papilionidæ) characterised by a prevailing blue green colour not found in any other continent.[1] Again, we have a group of African Pieridæ which are white or pale yellow with a marginal row of bead-like black spots, and in the same country one of the Lycænidæ (*Liptena erastus*) is coloured so exactly like these that it was at first described as a species of *Pieris*. None of these four groups are known to be in any way specially protected so that the resemblance cannot be due to protective mimicry.

In South America we have far more striking cases. For in the three sub-families—Danainæ, Acræinæ, and Heliconiinæ—all of which are specially protected, we find identical tints and patterns reproduced, often in the greatest detail, each peculiar type of coloration being characteristic of distinct geographical subdivisions of the continent. Nine very distinct genera are implicated in these parallel changes—*Lycorea*, *Ceratinia*, *Mechanitis*, *Ithomia*, *Melinæa*, *Tithorea*, *Acræa*, *Heliconius*, and *Eueides*—groups of three or four (or even of five) of them appearing together in the same livery in one district, while in an adjoining district most or all of them undergo a simultaneous change of coloration or of marking. Thus in the genera *Ithomia*, *Mechanitis*, and *Heliconius*, we have species with yellow apical spots in Guiana, all represented by allied species with white apical spots in South Brazil. In *Mechanitis*, *Melinæa*, and *Heliconius*, and sometimes in *Tithorea*, the species of the Southern Andes (Bolivia and Peru) are characterised by an orange and black livery, while those of the Northern Andes (New Grenada) are almost always orange-yellow and black. Other changes of a like nature, which it would be tedious to enumerate, but which are very striking when specimens are examined, occur in species of the same groups inhabiting these same localities, as well as Central America and the Antilles. The resemblance thus produced between widely different insects is sometimes general, but often so close and minute that only a critical examination of structure can detect the difference between them. Yet this can hardly be true mimicry, because all are alike protected by the nauseous secretion which renders them unpalatable to birds.

In another series of genera (*Catagramma*, *Callithea*, and *Agrias*), all belonging to the Nymphalidæ, we have the most vivid blue ground, with broad bands of orange-crimson or a different tint of blue or purple, exactly reproduced in corresponding, yet unrelated species, occurring in the same locality; yet, as none of these groups are protected, this can hardly be true mimicry. A few species of two other genera in the same country (*Eunica* and *Siderone*) also reproduce the same colours, but with only a general resemblance in the marking. Yet again, in Tropical America we have species of *Apatura* which, sometimes in both sexes, sometimes in the female only, exactly imitate the peculiar markings of another genus (*Heterochroa*) confined to America. Here, again, neither genus is protected, and the similarity must be due to unknown local causes.

[1] *Romaleosoma* and *Euryphene* (Nymphalidæ), *Papilio zalmoxis*, and several species of the *Nireus* group (Papilionidæ).

But it is among islands that we find some of the most striking examples of the influence of locality on colour, generally in the direction of paler, but sometimes of darker and more brilliant hues, and often accompanied by an unusual increase of size. Thus, in the Moluccas and New Guinea we have several Papilios (*P. euchenor*, *P. ormenus*, and *P. tydeus*), distinguished from their alli s by a much paler colour, especially in the females, which are almost white. Many species of *Danais* (forming the sub-genus *Ideopsis*) are also very pale. But the most curious are the Euplæas, which, in the larger islands, are usually of rich dark colours, while in the small islands of Banda, Ké, and Matabello at least three species not nearly related to each other (*E. hoppferi*, *E. euripon*, and *E. assimilata*) are all broadly banded or suffused with white, their allies in the larger islands being all very much darker. Again, in the genus *Diadema*, belonging to a distinct family, three species from the small Aru and Ké islands (*D. deois*, *D. hewitsonii*, and *D. polymena*) are all more conspicuously white-marked than their representatives in the larger islands. In the beautiful genus *Cethosia*, a species from the small island of Waigiou (*C. cyrene*), is the whitest of the genus. *Prothoe* is represented by a blue species in the continental island of Java, while those inhabiting the ancient insular groups of the Moluccas and New Guinea are all pale yellow or white. The genus *Drusilla*, almost confined to these islands, comprises many species which are all very pale; while in the small island of Waigiou is found a very distinct genus, *Hyantis*, which, though differing completely in the neuration of the wings, has exactly the same pale colours and large ocellated spots as *Drusilla*. Equally remarkable is the fact that the small island of Amboina produces larger-sized butterflies than any of the larger islands which surround it. This is the case with at least a dozen butterflies belonging to many distinct genera,[1] so that it is impossible to attribute it to other than some local influence. In Celebes, as I have elsewhere pointed out,[2] we have a peculiar form of wing and much larger size running through a whole series of distinct butterflies, and this seems to take the place of any specialty in colour.

From the Fiji Islands we have comparatively few butterflies, but there are several species of *Diadema* of unusually pale colours, some almost white.

The Philippine Islands seem to have the peculiarity of developing metallic colours. We find there at least three species of *Euplæa*[3] not closely related, and all of more intense metallic lustre than their allies in other islands. Here also we have one of the large yellow Ornithopteræ (*O. magellanus*), whose hind wings glow with an intense opaline lustre not found in any other species of the entire group; and an *Adolias*[4] is larger and of more brilliant metallic colouring than any other species in the Archipelago. In these islands also we find the extensive and wonderful genus of weevils, *Pachyrhynchus*, which in their brilliant metallic colouring surpass anything found in the whole eastern hemisphere, if not in the whole world.

In the Andaman Islands, in the Bay of Bengal, there are a considerable number of peculiar species of butterflies differing slightly from those on the continent, and generally in the direction of paler or more conspicuous colouring. Thus, two species of *Papilio*, which on the continent have the tails black, in their Andaman representatives have them either red- or white-tipped.[5] Another species[6] is richly blue-banded where its allies are black; while three species of distinct genera of Nymphalidæ[7] all differ from their allies on the continent in being of excessively pale colours, as well as of somewhat larger size.

In Madagascar we have the very large and singularly white-spotted *Papilio antenor*, while species of three other genera[8] are very white or conspicuous, compared with their continental allies.

Passing to the West Indian Islands and Central America (which latter country has formed a group of islands in very recent times), we have similar indications. One of the largest of the Papilios inhabits Jamaica,[9] while another, the largest of its group, is found in Mexico.[1] Cuba has two of the same genus whose colours are of surpassing brilliancy;[2] while the fine genus *Clothilda*—confined to the Antilles and Central America—is remarkable for its rich and showy colouring.

Persons who are not acquainted with the important structural differences that distinguish these various genera of butterflies, can hardly realise the importance and the significance of such facts as I have now detailed. It may be well, therefore, to illustrate them by supposing parallel cases to occur among the mammalia. We might have, for example, in Africa, the gnus, the elands, and the buffaloes all coloured and marked like zebras, stripe for stripe over the whole body exactly corresponding. So the hares, marmots, and squirrels of Europe might be all red, with black feet, while the corresponding species of Central Asia were all yellow, with black heads. In North America we might have raccoons, squirrels, and opossums in parti-coloured livery of white and black, so as exactly to resemble the skunk of the same country; while in South America they might be black, with a yellow throat patch, so as to resemble with equal closeness the tayra of the Brazilian forests. Were such resemblances to occur in anything like the number, and with the wonderful accuracy of imitation met with among the Lepidoptera, they would certainly attract universal attention among naturalists, and would lead to the exhaustive study of the influence of local causes in producing such startling results.

One somewhat similar case does indeed occur among the Mammalia, two singular African animals, the Aard-wolf (*Proteles*) and the Hyæna-dog (*Lycaon*), both strikingly resembling hyænas in their general form as well as in their spotted markings. Belonging as they all do to the Carnivora, though to three distinct families, it seems quite an analogous case to those we have imagined; but as the Aard-wolf and the hyæna-dog are both weak animals compared with the hyæna, the resemblance may be useful, and in that case would come under the head of mimicry. This seems the more probable because, as a rule, the colours of the Mammalia are protective, and are too little varied to allow of the influence of local causes producing any well-marked effects.

When we come to birds, however, the case is different; for although they do not exhibit such distinct marks of the influence of locality as do butterflies—probably because the causes which determine colour are in their case more complex—yet there are distinct indications of some effect of the kind, and we must devote some little time to their consideration.

One of the most curious cases is that of the parrots of the West Indian Islands and Central America, several of which have white heads or foreheads, occurring in two distinct genera,[3] while none of the more numerous parrots of South America are so coloured. In the small island of Dominica we have a very large and richly-coloured parrot (*Chrysotis augusta*) corresponding to the large and richly-coloured *Papilio homerus* of Jamaica.

The Andaman Islands are equally remarkable, at least six of the peculiar birds differing from their continental allies in being much lighter, and sometimes with a large quantity of pure white in the plumage,[4] exactly corresponding to what occurs among the butterflies.

In the Philippines this is not so marked a feature,—yet we have here the only known white-breasted Kingcrow (*Dicrurus mirabilis*),—the newly discovered *Eurylæmus Steerii*, wholly white beneath,—three species of *Diceum*, all white beneath,—several species of *Parus*, largely white-spotted,—while many of the pigeons have light ashy tints. The birds generally, however, have rich dark colours, similar to those which prevail among the butterflies.

In Celebes we have a swallow-shrike and a peculiar small crow allied to the jackdaw,[5] whiter than any of their allies in the surrounding islands, but otherwise the colours of the birds call for no special remark.

In Timor and Flores we have white-headed pigeons,[6] and a long-tailed flycatcher almost entirely white.[7]

In the small Lord Howe's Island we have the recently extinct white rail (*Notornis alba*), remarkably contrasting with its allies in the larger islands of New Zealand.

We cannot, however, lay any stress on isolated examples of white colour, since these occur in most of the great continents,

[1] *Ornithoptera priamus*, *O. helena*, *Papilio deiphobus*, *P. ulysses*, *P. gambrisuis*, *P. codrus*, *Iphias leucippe*, *Euplæa prothöe*, *Hestia idea*, *Athyma jocaste*, *Diadema pandarus*, *Nymphalis pyrrhus*, *N. euryalus*, *Drusilla jairus*.

[2] "Contributions to the Theory of Natural Selection," pp. 168–173.

[3] *Euplæa hewitsonii*, *E. diocletiana*, *E. lætifica*, *E. dupesnii*.

[4] *Adolias calliphorus*.

[5] *Papilio rhodifer* (near *P. doubledayi*) and *Papilio charicles* (near *P. memnon*).

[6] *Papilio mayo*.

[7] *Euplæa andamanensis*, *Cethozia biblis*, *Cyrestis cocles*.

[8] *Danais nossima*, *Melanitis massoura*, *Diadema dexithea*.

[9] *Papilio homerus*.

[1] *P. daunus*.

[2] *P. gundlachianus*, *P. villiersi*.

[3] *Pionus albifrons* and *Chrysotis senilis* (C. America), *Chrysotis sallæi* (Hayti).

[4] *Kittacincla albiventris*, *Geocichla albigularis*, *Sturnia andamanensis*, *Hyloterpe grisola*, *var.*, *Janthænas palumboides*, *Osmotreron chloroptera*.

[5] *Artamus monachus*, *Corvus advena*.

[6] *Ptilopus cinctus*, *P. albocinctus*.

[7] *Tchitrea affinis*, *var.*

but where we find a series of species of distinct genera, all differing from their continental allies in a whiter colouration, as in the Andaman Islands and the West Indies; and among butterflies, in the smaller Moluccas, the Andamans, and Madagascar, we cannot avoid the conclusion that in these insular localities some general cause is at work.

There are other cases, however, in which local influences seem to favour the production or preservation of intense crimson or a very dark colouration. Thus in the Moluccas and New Guinea alone we have bright red parrots belonging to two distinct families,[1] and which, therefore, most probably have been independently produced or preserved by some common cause. Here too and in Australia we have black parrots and pigeons;[2] and it is a most curious and suggestive fact that in another insular subregion—that of Madagascar and the Mascarene Islands—these same colours reappear in the same two groups.[3]

Some very curious physiological facts bearing upon the presence or absence of white colours in the higher animals have lately been adduced by Dr. Ogle.[4] It has been found that a coloured or dark pigment in the olfactory region of the nostrils is essential to perfect smell, and this pigment is rarely deficient except when the whole animal is pure white. In these cases the creature is almost without smell or taste. This, Dr. Ogle believes, explains the curious case of the pigs in Virginia adduced by Mr. Darwin, white pigs being poisoned by a poisonous root which does not affect black pigs. Mr. Darwin imputed this to a constitutional difference accompanying the dark colour, which rendered what was poisonous to the white-coloured animals quite innocuous to the black. Dr. Ogle however observes, that there is no proof that the black pigs eat the root, and he believes the more probable explanation to be that it is distasteful to them, while the white pigs, being deficient in smell and taste, eat it and are killed. Analogous facts occur in several distinct families. White sheep are killed in the Tarentino by eating *Hypericum criscum*, while black sheep escape; white rhinoceroses are said to perish from eating *Euphorbia candelabrum*; and white horses are said to suffer from poisonous food where coloured ones escape. Now it is very improbable that a constitutional immunity from poisoning by so many distinct plants should in the case of such widely different animals be always correlated with the same difference of colour; but the facts are readily understood if the senses of smell and taste are dependent on the presence of a pigment which is deficient in wholly white animals. The explanation has, however, been carried a step further, by experiments showing that the absorption of odours by dead matter, such as clothing, is greatly affected by colour, black being the most powerful absorbent, then blue, red, yellow, and lastly white. We have here a physical cause for the sense-inferiority of totally white animals which may account for their rarity in nature. For few, if any, wild animals are wholly white. The head, the face, or at least the muzzle or the nose, are generally black. The ears and eyes are also often black; and there is reason to believe that dark pigment is essential to good hearing, as it certainly is to perfect vision. We can therefore understand why white cats with blue eyes are so often deaf—a peculiarity we notice more readily than their deficiency of smell or taste.

If then the prevalence of white colouration is generally accompanied with some deficiency in the acuteness of the most important senses, this colour becomes doubly dangerous, for it not only renders its possessor more conspicuous to its enemies, but at the same time makes it less ready in detecting the presence of danger. Hence, perhaps, the reason why white appears more frequently in islands where competition is less severe and enemies less numerous and varied. Hence, also, a reason why *albinoism*, although freely occurring in captivity never maintains itself in a wild state, while *melanism* does. The peculiarity of some islands in having all their inhabitants of dusky colours—as the Galapagos—may also perhaps be explained on the same principles, for poisonous fruits or seeds may there abound which weed out all white or light-coloured varieties, owing to their deficiency of smell and taste. We can hardly believe, however, that this would apply to white-coloured butterflies, and this may be a reason why the effect of an insular habitat is more marked in these insects than in birds or mammals. But though inapplicable to the lower animals, this curious relation of sense-acuteness with colours may have had some influence on the development of the higher human races. If light tints of the skin were generally accompanied by some deficiency in the senses of smell, hearing, and vision, the white could never compete with the darker races, so long as man was in a very low or savage condition, and wholly dependent for existence on the acuteness of his senses. But as the mental faculties became more fully developed and more important to his welfare than mere sense-acuteness, the lighter tints of skin, and hair, and eyes, would cease to be disadvantageous whenever they were accompanied by superior brain-power. Such variations would then be preserved; and thus may have arisen the Xanthochroic race of mankind, in which we find a high development of intellect accompanied by a slight deficiency in the acuteness of the senses as compared with the darker forms.

I have now to ask your attention to a few remarks on the peculiar relations of plants and insects as exhibited in islands.

Ever since Mr. Darwin showed the immense importance of insects in the fertilization of flowers, great attention has been paid to the subject, and the relation of these two very different classes of natural objects has been found to be more universal and more complex than could have been anticipated. Whole genera and families of plants have been so modified, as first to attract and then to be fertilized by, certain groups of insects, and this special adaptation seems in many cases to have determined the more or less wide range of the plants in question. It is also known that some species of plants can be fertilized only by particular species of insects, and the absence of these from any locality would necessarily prevent the continued existence of the plant in that area. Here, I believe, will be found the clue to much of the peculiarity of the floras of oceanic islands, since the methods by which these have been stocked with plants and insects will be often quite different. Many seeds are, no doubt, carried by oceanic currents, others probably by aquatic birds. Mr. H. N. Moseley informs me that the albatrosses, gulls, puffins, tropic birds, and many others, nest inland, often amidst dense vegetation, and he believes they often carry seeds, attached to their feathers, from island to island for great distances. In the tropics they often nest on the mountains far inland, and may thus aid in the distribution even of mountain plants. Insects, on the other hand, are mostly conveyed by aërial currents, especially by violent gales; and it may thus often happen that totally unrelated plants and insects may be brought together, in which case the former must often perish for want of suitable insects to fertilise them. This will, I think, account for the strangely fragmentary nature of these insular floras, and the great differences that often exist between those which are situated in the same ocean, as well as for the preponderance of certain orders and genera. In Mr. Pickering's valuable work on the Geographical Distribution of Animals and Plants, he gives a list of no less than sixty-six natural orders of plants *unexpectedly* absent from Tahiti, or which occur in many of the surrounding lands, some being abundant in other islands—as the Labiatæ at the Sandwich Islands. In these latter islands the flora is much richer, yet a large number of families which abound in other parts of Polynesia are totally wanting. Now much of the poverty and exceptional distribution of the plants of these islands is probably due to the great scarcity of flower-frequenting insects. Lepidoptera and Hymenoptera are exceedingly scarce in the eastern islands of the Pacific, and it is almost certain that many plants which require these insects for their fertilization have been thereby prevented from establishing themselves. In the Western islands, such as the Fijis, several species of butterflies occur in tolerable abundance, and no doubt some flower-haunting Hymenoptera accompany them, and in these islands the flora appears to be much more varied, and especially to be characterized by a much greater variety of showy flowers, as may be seen by examining the plates of Dr. Seeman's "Flora Vitiensis."

Darwin and Pickering both speak of the great preponderance of ferns at Tahiti, and Mr. Moseley, who spent several days in the interior of the island, informs me that "at an elevation of from 2,000 to 3,000 feet the dense vegetation is composed almost entirely of ferns. A tree-fern (*Alsophila tahitensis*) forms a sort of forest, to the exclusion of almost every other tree, and, with huge plants of two other ferns (*Angiopteris evecta* and *Aspelenium nidus*), forms the main mass of the vegetation." And he adds, "I have nowhere seen ferns in so great proportionate abundance." This unusual proportion of ferns is a general feature of insular as compared with continental floras; but it has, I believe, been generally attributed to favourable conditions, especially to equable

[1] *Lorius, Eos* (Trichoglossidæ), *Eclectus* (Palæornithidæ).
[2] *Microglossus, Calyptorhynchus, Turacæna.*
[3] *Coracopsis, Alectrænas.*
[4] Medico-Chirurgical Transactions, vol. liii. (1870).

climate and perennial moisture. In this respect, however, Tahiti can hardly differ greatly from many other islands, which yet have no such vast preponderance of ferns. This is a question that cannot be decided by mere lists of species, since it is probable that in Tahiti they are less numerous than in some other islands where they form a far less conspicuous feature in the vegetation. The island most comparable with Tahiti in that respect is Juan Fernandez. Mr. Moseley writes to me—"In a general view of any wide stretch of the densely-clothed mountainous surface of the island, the ferns, both tree-ferns and the unstemmed forms, are seen at once to compose a very large proportion of the mass of foliage." As to the insects of Juan Fernandez, Mr. Edwyn C. Reed, who made two visits and spent several weeks there, has kindly furnished me with some exact information. Of butterflies there is only one (*Pyrameis carie*), and that rare—a Chilian species, and probably an accidental straggler. Four species of moths of moderate size were observed—all Chilian, and a few larvæ and pupæ. Of bees there were none, except one very minute species (allied to *Chilicola*), and of other Hymenoptera, a single specimen of *Ophion luteus*—a cosmopolitan ichneumon. About twenty species of flies were observed, and these formed the most prominent feature of the entomology of the island.

Now, as far as we know, this extreme entomological poverty agrees closely with that of Tahiti; and there are probably no other portions of the globe equally favoured in soil and climate and with an equally luxuriant vegetation, where insect-life is so scantily developed. It is curious therefore to find that these two islands also agree in the wonderful predominance of ferns over the flowering plants—in individuals even more than in species, and there is no difficulty in connecting the two facts. The excessive minuteness and great abundance of fern-spores causes them to be far more easily distributed by winds than the seeds of flowering plants, and they are thus always ready to occupy any vacant places in suitable localities, and to compete with the less vigorous flowering plants. But where insects are so scarce, all plants which require insect fertilisation, whether constantly to enable them to produce seed at all, or occasionally to keep up their constitutional vigour by crossing, must be at a great disadvantage; and thus the scanty flora which oceanic islands must always possess, peopled as they usually are by waifs and strays from other lands, is rendered still more scanty by the weeding out of all such as depend largely on insect fertilisation for their full development. It seems probable, therefore, that the preponderance of ferns in islands (considered in mass of individuals rather than in number of species) is largely due to the absence of competing phenogamous plants; and that this is in great part due to the scarcity of insects. In other oceanic islands, such as New Zealand and the Galapagos, where ferns, although tolerably abundant, form no such predominant feature in the vegetation, but where the scarcity of flower-haunting insects is almost equally marked, we find a great preponderance of small, green, or otherwise inconspicuous flowers, indicating that only such plants have been enabled to flourish there as are independent of insect fertilisation. In the Galapagos—which are perhaps even more deficient in flying insects than Juan Fernandez—this is so striking a feature that Mr. Darwin speaks of the vegetation as consisting in great part of "wretched-looking weeds," and states that "it was some time before he discovered that almost every plant was in flower at the time of his visit." He also says that he "did not see one beautiful flower" in the islands. It appears, however, that Compositæ, Leguminosæ, Rubiaceæ, and Solanaceæ, form a large proportion of the flowering plants, and as these are orders which usually require insect fertilisation, we must suppose either that they have become modified so as to be self-fertilised, or that they are fertilised by the visits of the minute Diptera and Hymenoptera, which are the only insects recorded from these islands.

In Juan Fernandez, on the other hand, there is no such total deficiency of showy flowers. I am informed by Mr. Moseley that a variety of the Magnoliaceous winter's bark abounds, and has showy white flowers, and that a Bignoniaceous shrub with abundance of dark blue flowers, was also plentiful; while a white-flowered liliaceous plant formed large patches on the hill-sides. Besides these there were two species of woody Compositæ with conspicuous heads of yellow blossoms, and a species of white-flowered myrtle also abundant; so that, on the whole, flowers formed a rather conspicuous feature in the aspect of the vegetation of Juan Fernandez.

But this fact—which at first sight seems entirely at variance with the view we are upholding of the important relation between the distribution of insects and plants—is well explained by the existence of two species of humming-birds in Juan Fernandez, which, in their visits to these large and showy flowers fertilise them as effectually as bees, moths, or butterflies. Mr. Moseley informs me that "these humming-birds are *extraordinarily abundant*, every tree or bush having one or two darting about it." He also observed that "nearly all the specimens killed had the feathers round the base of the bill and front of the head clogged and coloured yellow with pollen." Here, then, we have the clue to the perpetuation of large and showy flowers in Juan Fernandez; while the total absence of humming-birds in the Galapagos may explain why no such large-flowered plants have been able to establish themselves in those equatorial islands.

This leads to the observation that many other groups of birds also, no doubt, aid in the fertilization of flowers. I have often observed the beaks and faces of the brush-tongued lories of the Moluccas covered with pollen; and Mr. Moseley noted the same fact in a species of *Artamus*, or swallow-shrike, shot at Cape York, showing that this genus also frequents flowers and aids in their fertilisation. In the Australian region we have the immense group of the Meliphagidæ, which all frequent flowers, and as these range over all the islands of the Pacific, their presence will account for a certain proportion of showy flowers being found there, such as the scarlet *Metrosideros*, one of the few conspicuous flowers in Tahiti. In the Sandwich Islands, too, there are forests of *Metrosideros*; and Mr. Charles Pickering writes me, that they are visited by honey-sucking birds, one of which is captured by sweetened bird-lime, against which it thrusts its extensile tongue. I am also informed that a considerable number of flowers are occasionally fertilised by humming-birds in North America; so that there can, I think, be little doubt that birds play a much more important part in this respect than has hitherto been imagined. It is not improbable that in Tropical America, where this family is so enormously developed, many flowers will be found to be expressly adapted to fertilisation by them, just as so many in our own country are specially adapted to the visits of certain families or genera of insects.

It must also be remembered, as Mr. Moseley has suggested to me, that a flower which had acquired a brilliant colour to attract insects might, on transference to another country, and becoming so modified as to be capable of self-fertilisation, retain the coloured petals for an indefinite period. Such is probably the explanation of the *Pelargonium* of Kerguelen's land, which forms masses of bright colour near the shore during the flowering season; while most of the other plants of the island have colourless flowers in accordance with the almost total absence of winged insects. The presence of many large and showy flowers among the indigenous flora of St. Helena must be an example of a similar persistence. Mr. Melliss indeed states it to be "a remarkable peculiarity that the indigenous flowers are, with very slight exceptions, all perfectly colourless;"[1] but although this may apply to the general aspect of the remains of the indigenous flora, it is evidently not the case as regards the *species*, since the interesting plates of Mr. Melliss's volume show that about one-third of the indigenous flowering plants have more or less coloured or conspicuous flowers, while several of them are exceedingly showy and beautiful. Among these are a *Lobelia*, three *Wahlenbergias*, several *Compositæ*, and especially the handsome red flowers of the now almost extinct forest-trees, the ebony and redwood (species of *Melhania*, Byttneriaceæ). We have every reason to believe, however, that when St. Helena was covered with luxuriant forests, and especially at that remote period when it was much more extensive than it is now, it must have supported a certain number of indigenous birds and insects, which would have aided in the fertilisation of these gaily-coloured flowers. The researches of Dr. Hermann Müller have shown us by what minute modifications of structure or of function many flowers are adapted for partial insect- and self-fertilisation in varying degrees, so that we have no difficulty in understanding how, as the insects diminished and finally disappeared, self-fertilisation may have become the rule, while the large and showy corollas remain to tell us plainly of a once different state of things.

Another interesting fact in connection with this subject is the presence of arborescent forms of Compositæ in so many of the remotest oceanic islands. They occur in the Galapagos, in Juan Fernandez, in St. Helena, in the Sandwich Islands, and in New Zealand; but they are not directly related to each other, representatives of totally different tribes of this extensive order becoming arborescent in each group of islands. The immense range and almost universal distribution of the Compositæ is due to the combination of a great facility of distribution (by their seeds),

[1] Melliss's *St. Helena*, p. 226, note.

with a great attractiveness to insects, and the capacity of being fertilised by a variety of species of all orders, and especially by flies and small beetles. Thus they would be among the earliest of flowering plants to establish themselves on oceanic islands; but where insects of all kinds were very scarce it would be an advantage to gain increased size and longevity, so that fertilization at an interval of several years might suffice for the continuance of the species. The arborescent form would combine with increased longevity the advantage of increased size in the struggle for existence with the ferns and other early colonists, and these advantages have led to its being independently produced in so many distant localities, whose chief feature in common is their remoteness from continents and the extreme poverty of their insect life.

As the sweet odours of flowers are known to act in combination with their colours, as an attraction to insects, it might be anticipated that where colour was deficient scent would be so also. On applying to my friend Dr. Hooker for information as to New Zealand plants, he informed me that this was certainly the case, and that the New Zealand flora is, speaking generally, as strikingly deficient in sweet odours as in conspicuous colours. Whether this peculiarity occurs in other islands I have not been able to obtain information, but we may certainly expect it to be so in such a marked instance as that of the Galapagos flora.

Another question which here comes before us is the origin and meaning of the odoriferous glands of leaves. Dr. Hooker informed me that not only are New Zealand plants deficient in scented flowers, but equally so in scented leaves. This led me to think that perhaps such leaves were in some way an additional attraction to insects, though it is not easy to understand how this could be, except by adding a general attraction to the special attraction of the flowers, or by supporting the larvæ which as perfect insects aid in fertilisation. Mr. Darwin, however, informs me that he considers that leaf-glands bearing essential oils are a protection against the attacks of insects where these abound, and would thus not be required in countries where insects were very scarce. But it seems opposed to this view that highly aromatic plants are characteristic of deserts all over the world, and in such places insects are not abundant. Mr. Stainton informs me that the aromatic Labiatæ enjoy no immunity from insect attacks. The bitter leaves of the cherry-laurel are often eaten by the larvæ of moths that abound on our fruit-trees; while in the Tropics the leaves of the orange tribe are favourites with a large number of lepidopterous larvæ; and our northern firs and pines, although abounding in a highly aromatic resin, are very subject to the attacks of beetles. My friend Dr. Richard Spruce—who while travelling in South America allowed nothing connected with plant-life to escape his observation—informs me that trees whose leaves have aromatic and often resinous secretions in immersed glands abound in the plains of tropical America, and that such are in great part, if not wholly, free from the attacks of leaf-eating ants, except where the secretion is only slightly bitter, as in the orange tribe, orange-trees being sometimes entirely denuded of their leaves in a single night. Aromatic plants abound in the Andes up to about 13,000 feet, as well as in the plains, but hardly more so than in Central and Southern Europe. They are perhaps most plentiful in the dry mountainous parts of Southern Europe; and as neither here nor in the Andes do leaf-eating ants exist, Dr. Spruce infers that, although in the hot American forests where such ants swarm the oil-bearing glands serve as a protection, yet they were not originally acquired for that purpose. Near the limits of perpetual snow on the Andes such plants as occur are not, so far as Dr. Spruce has observed, aromatic: and as plants in such situations can hardly depend on insect visits for their fertilisation, the fact is comparable with that of the flora of New Zealand, and would seem to imply some relation between the two phenomena, though what it exactly is cannot yet be determined.

I trust I have now been able to show you that there are a number of curious problems lying as it were on the outskirts of biological inquiry which well merit attention, and which may lead to valuable results. But these problems are, as you see, for the most part connected with questions of locality, and require full and accurate knowledge of the productions of a number of small islands and other limited areas, and the means of comparing them the one with the other. To make such comparisons is, however, now quite impossible. No museum contains any fair representation of the productions of these localities, and such specimens as do exist, being scattered through the general collection, are almost useless for this special purpose. If, then, we are to make any progress in this inquiry, it is absolutely essential that some collectors should begin to arrange their cabinets primarily on a geographical basis, keeping together the productions of every island or group of islands, and of such divisions of each continent as are found to possess any special or characteristic fauna or flora. We shall then be sure to detect many unsuspected relations between the animals and plants of certain localities, and we shall become much better acquainted with those complex reactions between the vegetable and animal kingdoms, and between the organic world and the inorganic, which have almost certainly played an important part in determining many of the most conspicuous features of living things.

Rise and Progress of Modern Views as to the Antiquity and Origin of Man.

I now come to a branch of our subject which I would gladly have avoided touching on, but as the higher powers of this Association have decreed that I should preside over the Anthropological Department, it seems proper that I should devote some portion of my address to matters more immediately connected with the special study to which that Department is devoted.

As my own knowledge of, and interest in, Anthropology, is confined to the great outlines, rather than to the special details of the science, I propose to give a very brief and general sketch of the modern doctrine as to the Antiquity and Origin of Man, and to suggest certain points of difficulty which have not, I think, yet received sufficient attention.

Many now present remember the time (for it is little more than twenty years ago) when the antiquity of man, as now understood, was universally discredited. Not only theologians, but even geologists, then taught us that man belonged altogether to the existing state of things; that the extinct animals of the Tertiary period had finally disappeared, and that the earth's surface had assumed its present condition, before the human race first came into existence. So prepossessed were even scientific men with this idea—which yet rested on purely negative evidence, and could not be supported by any arguments of scientific value—that numerous facts which had been presented at intervals for half a century, all tending to prove the existence of man at very remote epochs, were silently ignored; and, more than this, the detailed statements of three distinct and careful observers were rejected by a great scientific Society as too improbable for publication, only because they proved (if they were true) the coexistence of man with extinct animals![1]

But this state of belief in opposition to facts could not long continue. In 1859 a few of our most eminent geologists examined for themselves into the alleged occurrence of flint implements in the gravels of the North of France, which had been made public fourteen years before, and found them strictly correct. The caverns of Devonshire were about the same time carefully examined by equally eminent observers, and were found fully to bear out the statements of those who had published their results eighteen years before. Flint implements began to be found in all suitable localities in the South of England, when carefully searched for, often in gravels of equal antiquity with those of France. Caverns, giving evidence of human occupation at various remote periods, were explored in Belgium and the South of France,—lake dwellings were examined in Switzerland—refuse heaps in Denmark—and thus a whole series of remains have been discovered carrying back the history of mankind from the earliest historic periods to a long distant past. The antiquity of the races thus discovered can only be generally determined by the successively earlier and earlier stages through which we can trace them. As we go back, metals soon disappear and we find only tools and weapons of stone and of bone. The stone weapons get ruder and ruder; pottery, and then the bone implements, cease to occur; and in the earliest stage we find only chipped flints, of rude design though still of unmistakably human workmanship. In like manner domestic animals disappear as we go backward; and though the dog seems to have been the earliest, it is doubtful whether the makers of the ruder flint implements of the gravels possessed even this. Still more important as a measure of time are the changes of the earth's surface—of the distribution of animals—and of climate—which have occurred during the human period. At a comparatively recent epoch in the record of prehistoric times we find that the Baltic was far salter than it is now, and produced abundance of oysters; and that Denmark

[1] In 1854 (?) a communication from the Torquay Natural History Society confirming previous accounts by Mr. Godwin-Austen, Mr. Vivian, and the Rev. Mr. McEnery, that worked flints occurred in Kent's Hole with remains of extinct species, was rejected as too improbable for publication.

was covered with pine forests inhabited by Capercailzies, such as now only occur further north in Norway. A little earlier we find that reindeer were common even in the South of France, and still earlier this animal was accompanied by the mammoth and woolly rhinoceros, by the arctic glutton, and by huge bears and lions of extinct species. The presence of such animals implies a change of climate, and both in the caves and gravels we find proofs of a much colder climate than now prevails in Western Europe. Still more remarkable are the changes of the earth's surface which have been effected during man's occupation of it. Many extensive valleys in England and France are believed by the best observers to have been deepened at least a hundred feet;—caverns now far out of the reach of any stream must for a long succession of years have had streams flowing through them, at least in times of floods—and this often implies that vast masses of solid rock have since been worn away. In Sardinia land has risen at least 300 feet since men lived there who made pottery and probably used fishing-nets;[1] while in Kent's Cavern remains of man are found buried beneath two separate beds of stalagmite, each having a distinct texture, and each covering a deposit of cave-earth having well-marked differential characters, while each contains a distinct assemblage of extinct animals.

Such, briefly, are the results of the evidence that has been rapidly accumulating for about fifteen years as to the antiquity of man; and it has been confirmed by so many discoveries of a like nature in all parts of the globe, and especially by the comparison of the tools and weapons of prehistoric man with those of modern savages, so that the use of even the rudest flint implements has become quite intelligible,—that we can hardly wonder at the vast revolution effected in public opinion. Not only is the belief in man's vast and still unknown antiquity universal among men of science, but it is hardly disputed by any well-informed theologian; and the present generation of science-students must, we should think, be somewhat puzzled to understand, what there was in the earliest discoveries that should have aroused such general opposition and been met with such universal incredulity.

But the question of the mere "Antiquity of Man" almost sank into insignificance at a very early period of the inquiry, in comparison with the far more momentous and more exciting problem of the development of man from some lower animal form, which the theories of Mr. Darwin and of Mr. Herbert Spencer soon showed to be inseparably bound up with it. This has been, and to some extent still is, the subject of fierce conflict; but the controversy as to the fact of such development is now almost at an end, since one of the most talented representatives of Catholic theology, and an anatomist of high standing —Professor Mivart—fully adopts it as regards physical structure, reserving his opposition for those parts of the theory, which would deduce man's whole intellectual and moral nature from the same source, and by a similar mode of development.

Never, perhaps, in the whole history of science or philosophy has so great a revolution in thought and opinion been effected as in the twelve years from 1859 to 1871, the respective dates of publication of Mr. Darwin's "Origin of Species" and "Descent of Man." Up to the commencement of this period the belief in the independent creation or origin of the species of animals and plants, and the very recent appearance of man upon the earth, were, practically, universal. Long before the end of it these two beliefs had utterly disappeared, not only in the scientific world, but almost equally so among the literary and educated classes generally. The belief in the independent origin of man held its ground somewhat longer, but the publication of Mr. Darwin's great work gave even that its death-blow, for hardly anyone capable of judging of the evidence now doubts the derivative nature of man's bodily structure as a whole, although many believe that his mind and even some of his physical characteristics may be due to the action of other forces than have acted in the case of the lower animals.

We need hardly be surprised, under these circumstances, if there has been a tendency among men of science to pass from one extreme to the other, from a profession (so few years ago) of total ignorance as to the mode of origin of all living things, to a claim to almost complete knowledge, of the whole progress of the universe, from the first speck of living protoplasm up to the highest development of the human intellect. Yet this is really what we have seen in the last sixteen years. Formerly difficulties were exaggerated, and it was asserted that we had not sufficient knowledge to venture on any generalizations on the subject. Now difficulties are set aside, and it is held that our theories are so well established and so far-reaching, that they explain and comprehend all nature. It is not long ago (as I have already reminded you) since *facts* were contemptuously ignored, because they favoured our now popular views; at the present day it seems to me that facts which oppose them hardly receive due consideration. And as opposition is the best incentive to progress, and it is not well even for the best theories to have it all their own way, I propose to direct your attention to a few such facts, and to the conclusions that seem fairly deducible from them.

It is a curious circumstance, that notwithstanding the attention that has been directed to the subject in every part of the world, and the numerous excavations connected with railways and mines which have offered such facilities for geological discovery, no advance whatever has been made for a considerable number of years, in detecting the time or the mode of man's origin. The Palæolithic flint weapons first discovered in the North of France more than thirty years ago, are still the oldest undisputed proofs of man's existence; and amid the countless relics of a former world that have been brought to light, no evidence of any one of the links that must have connected man with the lower animals has yet appeared.

It is, indeed, well known that negative evidence in geology is of very slender value, and this is, no doubt, generally the case. The circumstances here are, however, peculiar, for many converging lines of evidence show that on the theory of development by the same laws which have determined the development of the lower animals, man must be immensely older than any traces of him yet discovered. As this is a point of great interest we must devote a few moments to its consideration.

1. The most important difference between man and such of the lower animals as most nearly approach him, is undoubtedly in the bulk and development of his brain, as indicated by the form and capacity of the cranium. We should therefore anticipate that these earliest races, who were contemporary with the extinct animals and used rude stone weapons, would show a marked deficiency in this respect. Yet the oldest known crania—those of the Engis and Cro-Magnon caves— show no marks of degradation. The former does not present so low a type as that of most existing savages, but is—to use the words of Prof. Huxley —"a fair average human skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." The latter are still more remarkable, being unusually large and well formed. Dr. Pruner-Bey states that they surpass the average of modern European skulls in capacity, while their symmetrical forms, without any trace of prognathism, compares favourably not only with the foremost savage races, but with many civilised nations of modern times.

One or two other crania of much lower type, but of less antiquity than this, have been discovered; but they in no way invalidate the conclusion which so highly developed a form at so early a period implies, viz., that we have as yet made a hardly perceptible step towards the discovery of any earlier stage in the development of man.

2. This conclusion is supported and enforced by the nature of many of the works of art found even in the oldest cave-dwellings. The flints are of the old chipped type, but they are formed into a large variety of tools and weapons—such as scrapers, awls, hammers, saws, lances, &c., implying a variety of purposes for which these were used, and a corresponding degree of mental activity and civilisation. Numerous articles of bone have also been found, including well-formed needles, implying that skins were sewn together, and perhaps even textile materials woven into cloth. Still more important are the numerous carvings and drawings representing a variety of animals, including horses, reindeer, and even a mammoth, executed with considerable skill on bone, reindeer-horns, and mammoth-tusks. These, taken together, indicate a state of civilisation much higher than that of the lowest of our modern savages, while it is quite compatible with a considerable degree of mental advancement, and leads us to believe that the crania of Engis and Cro-Magnon are not exceptional, but fairly represent the characters of the race. If we further remember that these people lived in Europe under the unfavourable conditions of a sub-Arctic climate, we shall be inclined to agree with Dr. Daniel Wilson, that it is far easier to produce evidences of deterioration than of progress in instituting a comparison between the contemporaries of the mammoth and later prehistoric races of Europe or savage nations of modern times.[2]

3. Yet another important line of evidence as to the extreme

[1] Lyell's Antiquity of Man, fourth edition, p. 115.

[2] "Prehistoric Man," 3rd ed. vol. i. p. 117.

antiquity of the human type has been brought prominently forward by Prof. Mivart.[1] He shows by a careful comparison of all parts of the structure of the body, that man is related, not to any one, but almost equally to many of the existing apes—to the orang, the chimpanzee, the gorilla, and even to the gibbons—in a variety of ways; and these relations and differences are so numerous and so diverse that on the theory of evolution the ancestral form which ultimately developed into man must have diverged from the common stock whence all these various forms and their extinct allies originated. But so far back as the Miocene deposits of Europe, we find the remains of apes allied to these various forms, and especially to the gibbons, so that in all probability the special line of variation which led up to man branched off at a still earlier period. And these early forms, being the initiation of a far higher type, and having to develop by natural selection into so specialised and altogether distinct a creature as man, must have risen at a very early period into the position of a dominant race, and spread in dense waves of population over all suitable portions of the great continent—for this, on Mr. Darwin's hypothesis, is essential to rapid developmental progress through the agency of natural selection.

Under these circumstances we might certainly expect to find some relics of these earlier forms of man along with those of animals which were presumably less abundant. Negative evidence of this kind is not very weighty, but still it has *some* value. It has been suggested that as apes are mostly tropical, and anthropoid apes are now confined almost exclusively to the vicinity of the equator, we should expect the ancestral forms also to have inhabited these same localities—West Africa and the Malay Islands. But this objection is hardly valid, because existing anthropoid apes are wholly dependent on a perennial supply of easily accessible fruits, which is only found near the equator, while not only had the south of Europe an almost tropical climate in Miocene times, but we must suppose even the earliest ancestors of man to have been terrestrial and omnivorous, since it must have taken ages of slow modification to have produced the perfectly erect form, the short arms, and the wholly non-prehensile foot, which so strongly differentiate man from the apes.

The conclusion which I think we must arrive at is, that if man has been developed from a common ancestor, with all existing apes, *and by no other agencies than such as have affected their development*, then he must have existed in something approaching his present form, during the tertiary period—and not merely existed, but predominated in numbers, wherever suitable conditions prevailed. If then, continued researches in all parts of Europe and Asia fail to bring to light any proofs of his presence, it will be at least a presumption that he came into existence at a much later date, and by a much more rapid process of development. In that case it will be a fair argument, that, just as he is in his mental and moral nature, his capacities and aspirations, so infinitely raised above the brutes, so his origin is due to distinct and higher agencies than such as have affected their development.

There is yet another line of inquiry bearing upon this subject to which I wish to call your attention. It is a somewhat curious fact, that, while all modern writers admit the great antiquity of man, most of them maintain the very recent development of his intellect, and will hardly contemplate the possibility of men equal in mental capacity to ourselves, having existed in prehistoric times. This question is generally assumed to be settled, by such relics as have been preserved of the manufactures of the older races showing a lower and lower state of the arts; by the successive disappearance in early times of iron, bronze, and pottery; and by the ruder forms of the older flint implements. The weakness of this argument has been well shown by Mr. Albert Mott in his very original, but little known presidential address to the Literary and Philosophical Society of Liverpool in 1873. He maintains that "our most distant glimpses of the past are still of a world peopled as now with men both civilised and savage"—and, "that we have often entirely misread the past by supposing that the outward signs of civilisation must always be the same, and must be such as are found among ourselves." In support of this view he adduces a variety of striking facts and ingenious arguments, a few of which I will briefly summarise.

On one of the most remote islands of the Pacific—Easter Island—2,000 miles from South America, 2,000 from the Marquesas, and more than 1,000 from the Gambier Islands, are found hundreds of gigantic stone images, now mostly in ruins, often thirty or forty feet high, while some seem to have been much larger, the crowns on their heads cut out of a red stone being sometimes ten feet in diameter, while even the head and neck of one is said to have been twenty feet high.[2] These once stood erect on extensive stone platforms, yet the island has only an area of about thirty square miles, or considerably less than Jersey. Now as one of the smallest images eight feet high weighs four tons, the largest must weigh over a hundred tons, if not much more; and the existence of such vast works implies a large population, abundance of food, and an established government. Yet how could these coexist in a mere speck of land wholly cut off from the rest of the world? Mr. Mott maintains that this necessarily implies the power of regular communication with larger islands or a continent, the arts of navigation, and a civilisation much higher than now exists in any part of the Pacific. Very similar remains in other islands scattered widely over the Pacific add weight to this argument.

The next example is that of the ancient mounds and earthworks of the North American continent, the bearing of which is even more significant. Over the greater part of the extensive Mississippi valley four well-marked classes of these earthworks occur. Some are camps, or works of defence, situated on bluffs, promontories, or isolated hills; others are vast inclosures in the plains and lowlands, often of geometric forms, and having attached to them roadways or avenues often miles in length; a third are mounds corresponding to our tumuli, often seventy to ninety feet high, and some of them covering acres of ground; while a fourth group consist of representations of various animals modelled in relief on a gigantic scale, and occurring chiefly in an area somewhat to the north-west of the other classes, in the plains of Wisconsin.

The first class—the camps or fortified inclosures—resemble in general features the ancient camps of our own islands, but far surpass them in extent. Fort Hill, in Ohio, is surrounded by a wall and ditch a mile and a half in length, part of the way cut through solid rock. Artificial reservoirs for water were made within it, while at one extremity, on a more elevated point, a keep is constructed with its separate defences and water-reservoirs. Another, called Clark's Work, in the Scioto valley, which seems to have been a fortified town, incloses an area of 127 acres, the embankments measuring three miles in length, and containing not less than three million cubic feet of earth. This area incloses numerous sacrificial mounds and symmetrical earthworks in which many interesting relics and works of art have been found.

The second class—the sacred inclosures—may be compared for extent and arrangement with Avebury or Carnak—but are in some respects even more remarkable. One of these, at Newark, Ohio, covers an area of several miles with its connected groups of circles, octagons, squares, ellipses, and avenues, on a grand scale, and formed by embankments from twenty to thirty feet in height. Other similar works occur in different parts of Ohio, and by accurate survey it is found not only that the circles are true, though some of them are one-third of a mile in diameter, but that other figures are truly square, each side being over 1,000 feet long, and what is still more important, the dimensions of some of these geometrical figures in different parts of the country and seventy miles apart, are identical. Now this proves the use, by the builders of these works, of some standard measures of length, while the accuracy of the squares, circles, and, in a less degree, of the octagonal figures—shows a considerable knowledge of rudimentary geometry, and some means of measuring angles. The difficulty of drawing such figures on a large scale is much greater than any one would imagine who has not tried it, and the accuracy of these is far beyond what is necessary to satisfy the eye. We must therefore impute to these people the wish to make these figures as accurate as possible, and this wish is a greater proof of habitual skill and intellectual advancement than even the ability to draw such figures. If, then, we take into account this ability and this love of geometric truth, and further consider the dense population and civil organisation implied by the construction of such extensive systematic works, we must allow that these people had reached the earlier stages of a civilisation of which no traces existed among the savage tribes who alone occupied the country when first visited by Europeans.

The animal mounds are of comparatively less importance for our present purpose, as they imply a somewhat lower grade of advancement; but the sepulchral and sacrificial mounds exist in vast numbers, and their partial exploration has yielded a quantity of articles and works of art, which throw some further light on the peculiarities of this mysterious people. Most of these mounds

[1] "Man and Apes," pp. 171–193.

[2] Journ. of Roy. Geog. Soc., 1870, pp. 177, 178.

contain a large concave hearth or basin of burnt clay, of perfectly symmetrical form, on which are found deposited more or less abundant relics, all bearing traces of the action of fire. We are, therefore, only acquainted with such articles as are practically fire-proof. These consist of bone and copper implements and ornaments, discs, and tubes—pearl, shell, and silver beads, more or less injured by the fire—ornaments cut in mica, ornamental pottery, and numbers of elaborate carvings in stone, mostly forming pipes for smoking. The metallic articles are all formed by hammering, but the execution is very good; plates of mica are found cut into scrolls and circles; the pottery, of which very few remains have been found, is far superior to that of any of the Indian tribes, since Dr. Wilson is of opinion that they must have been formed on a wheel, as they are often of uniform thickness throughout (sometimes not more than one-sixth of an inch) polished, and ornamented with scrolls and figures of birds and flowers in delicate relief. But the most instructive objects are the sculptured stone pipes, representing not only various easily recognisable animals, but also human heads, so well executed that they appear to be portraits. Among the animals, not only are such native forms as the panther, bear, otter, wolf, beaver, raccoon, heron, crow, turtle, frog, rattlesnake, and many others, well represented, but also the manatee, which perhaps then ascended the Mississippi as it now does the Amazon, and the toucan, which could hardly have been obtained nearer than Mexico. The sculptured heads are especially remarkable, because they present to us the features of an intellectual and civilised people. The nose in some is perfectly straight, and neither prominent nor dilated, the mouth is small, and the lips thin, the chin and upper lip are short, contrasting with the ponderous jaw of the modern Indian, while the cheek-bones present no marked prominence. Other examples have the nose somewhat projecting at the apex in a manner quite unlike the features of any American indigenes, and, although there are some which show a much coarser face, it is very difficult to see in any of them that close resemblance to the Indian type which these sculptures have been said to exhibit. The few authentic crania from the mounds present corresponding features, being far more symmetrical and better developed in the frontal region than those of any American tribes, although somewhat resembling them in the occipital outline;[1] while one was described by its discoverer (Mr. W. Marshall Anderson) as "a beautiful skull worthy of a Greek."

The antiquity of this remarkable race may perhaps not be very great, as compared with the prehistoric man of Europe, although the opinions of some writers on the subject seem affected by that "parsimony of time" on which the late Sir Charles Lyell so often dilated. The mounds are all overgrown with dense forest, and one of the large trees was estimated to be eight hundred years old, while other observers consider the forest growth to indicate an age of at least 1,000 years. But it is well known that it requires several generations of trees to pass away before the growth on a deserted clearing comes to correspond with that of the surrounding virgin forest, while this forest, once established, may go on growing for an unknown number of thousands of years. The 800 or 1,000 years estimate from the growth of existing vegetation is a minimum which has no bearing whatever on the actual age of these mounds, and we might almost as well attempt to determine the time of the glacial epoch from the age of the pines or oaks which now grow on the moraines.

The important thing for us, however, is that when North America was first settled by Europeans, the Indian tribes inhabiting it had no knowledge or tradition of any preceding race of higher civilisation than themselves. Yet we find that such a race existed; that they must have been populous and have lived under some established government; while there are signs that they practised agriculture largely, as indeed they must have done to have supported a population capable of executing such gigantic works in such vast profusion—for it is stated that the mounds and earthworks of various kinds in the state of Ohio alone amounts to between eleven and twelve thousand. In their habits, customs, religion, and arts, they differed strikingly from all the Indian tribes; while their love of art and of geometric forms, and their capacity for executing the latter upon so gigantic a scale, render it probable that they were a really civilised people, although the form their civilisation took may have been very different from that of later people subject to very different influences, and the inheritors of a longer series of ancestral civilisations. We have here, at all events, a striking example of the transition, over an

[1] Wilson's "Prehistoric Man," 3rd ed. vol. ii. pp. 123-130.

extensive country, from comparative civilisation to comparative barbarism, the former having left no tradition, and hardly any trace of influence on the latter.

As Mr. Mott well remarks:—Nothing can be more striking than the fact that Easter Island and North America both give the same testimony as to the origin of the savage life found in them, although in all circumstances and surroundings the two cases are so different. If no stone monuments had been constructed in Easter Island, or mounds, containing a few relics saved from fire, in the United States, we might never have suspected the existence of these ancient peoples. He argues, therefore, that it is very easy for the records of an ancient nation's life entirely to perish, or to be hidden from observation. Even the arts of Nineveh and Babylon were unknown only a generation ago, and we have only just discovered the facts about the mound-builders of North America.

But other parts of the American continent exhibit parallel phenomena. Recent investigations show that in Mexico, Central America, and Peru, the existing race of Indians has been preceded by a distinct and more civilised race. This is proved by the sculptures of the ruined cities of Central America, by the more ancient terra-cottas and paintings of Mexico, and by the oldest portrait-pottery of Peru. All alike show markedly non-Indian features, while they often closely resemble modern European types. Ancient crania, too, have been found in all these countries, presenting very different characters from those of any of the modern indigenous races of America.[1]

There is one other striking example of a higher being succeeded by a lower degree of knowledge, which is in danger of being forgotten because it has been made the foundation of theories which seem wild and fantastic, and are probably in great part erroneous. I allude to the Great Pyramid of Egypt, whose form, dimensions, structure, and uses have recently been the subject of elaborate works by Prof. Piazzi Smyth. Now, the admitted facts about this pyramid are so interesting and so apposite to the subject we are considering, that I beg to recall them to your attention. Most of you are aware that this pyramid has been carefully explored and measured by successive Egyptologists, and that the dimensions have lately become capable of more accurate determination owing to the discovery of some of the original casing-stones and the clearing away of the earth from the corners of the foundation, showing the sockets in which the corner-stones fitted. Prof. Smyth devoted many months of work with the best instruments in order to fix the dimensions and angles of all accessible parts of the structure; and he has carefully determined these by a comparison of his own and all previous measures, the best of which agree pretty closely with each other. The results arrived at are—

1. That the pyramid is truly square, the sides being equal and the angles right angles.

2. That the four sockets on which the four first stones of the corners rested are truly on the same level.

3. That the direction of the sides are accurately to the four cardinal points.

4. That the vertical height of the pyramid bears the same proportion to its circumference at the base, as the radius of a circle does to its circumference.

Now all these measures, angles, and levels are accurate, not as an ordinary surveyor or builder could make them, but to such a degree as requires the very best modern instruments and all the refinements of geodetical science to discover any error at all. In addition to this we have the wonderful perfection of the workmanship in the interior of the pyramid, the passages and chambers being lined with huge blocks of stones fitted with the utmost accuracy, while every part of the building exhibits the highest structural science.

In all these respects this largest pyramid surpasses every other in Egypt. Yet it is universally admitted to be the oldest, and also the oldest historical building in the world.

Now these admitted facts about the Great Pyramid are surely remarkable, and worthy of the deepest consideration. They are facts which, in the pregnant words of the late Sir John Herschel, "according to received theories ought not to happen," and which, he tells us, should therefore be kept ever present to our minds, since "they belong to the class of facts which serve as the clue to new discoveries." According to modern theories, the higher civilisation is ever a growth and an outcome from a preceding lower state; and it is inferred that this progress is visible to us throughout all history and in all the material records of human intellect. But here we have a building which marks the

[1] Wilson's "Prehistoric Man," 3rd ed. vol. ii. pp. 125, 144.

very dawn of history—which is the oldest authentic monument of man's genius and skill, and which, instead of being far inferior, is very much superior to all which followed it. Great men are the products of their age and country, and the designer and constructors of this wonderful monument could never have arisen among an unintellectual and half-barbarous people. So perfect a work implies many preceding less perfect works which have disappeared. It marks the culminating point of an ancient civilisation, of the early stages of which we have no record whatever.

The three cases to which I have now adverted (and there are many others) seem to require for their satisfactory interpretation a somewhat different view of human progress from that which is now generally accepted. Taken in connection with the great intellectual power of the ancient Greeks—which Mr. Galton believes to have been far above that of the average of any modern nation—and the elevation, at once intellectual and moral, displayed in the writings of Confucius, Zoroaster, and the Vedas, they point to the conclusion, that, while in material progress there has been a tolerably steady advance, man's intellectual and moral development reached almost its highest level in a very remote past. The lower, the more animal, but often the more energetic types, have however always been far the more numerous; hence such established societies as have here and there arisen under the guidance of higher minds, have always been liable to be swept away by the incursions of barbarians. Thus in almost every part of the globe there may have been a long succession of partial civilisation, each in turn succeeded by a period of barbarism; and this view seems supported by the occurrence of degraded types of skull along with such "as might have belonged to a philosopher"—at a time when the mammoth and the reindeer inhabited southern France.

Nor need we fear that there is not time enough for the rise and decay of so many successive civilisations as this view would imply; for the opinion is now gaining ground among geologists that palæolithic man was really preglacial, and that the great gap—marked alike by a change of physical conditions, and of animal life—which in Europe always separates him from his neolithic successor, was caused by the coming on and passing away of the great ice age.

If the views now advanced are correct, many, perhaps most, of our existing savages, are the successors of higher races; and their arts, often showing a wonderful similarity in distant continents, may have been derived from a common source among more civilised peoples.

I must now conclude this very imperfect sketch of a few of the offshoots from the great tree of Biological study. It will, perhaps, be thought by some that my remarks have tended to the depreciation of our science, by hinting at imperfections in our knowledge and errors in our theories, where more enthusiastic students see nothing but established truths. But I trust that I may have conveyed to many of my hearers a different impression. I have endeavoured to show that even in what are usually considered the more trivial and superficial characters presented by natural objects, a whole field of new inquiry is opened up to us by the study of distribution and local conditions. And as regards man, I have endeavoured to fix your attention on a class of facts which indicate that the course of his development has been far less direct and simple than has hitherto been supposed; and that, instead of resembling a single tide with its advancing and receding ripples, it must rather be compared to the progress from neap to spring tides, both the rise and the depression being comparatively greater as the waters of true civilisation slowly advance towards the highest level they can reach.

And if we are thus led to believe that our present knowledge of nature is somewhat less complete than we have been accustomed to consider it, this is only what we might expect; for however great may have been the intellectual triumphs of the nineteenth century, we can hardly think so highly of its achievements as to imagine that, in somewhat less than twenty years, we have passed from complete ignorance to almost perfect knowledge on two such vast and complex subjects as the origin of species and the antiquity of man.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY F. J. EVANS, C.B., F.R.S., CAPTAIN R.N., PRESIDENT.

Two events, notable in the annals of Geographical Science have to be recorded since the last meeting of the British Association; and these events as bearing materially on the advancement of our knowledge of geography are deserving the special commendation of this Section. I refer to the successful issue of Cameron's land journey across the tropical regions of Southern Africa and to the successful completion of the sea voyage of the *Challenger*; a voyage which in its scope included the circumnavigation of the globe, the traversing the several oceans between the 50th parallel of North latitude and the Antarctic circle, and the exploration throughout, by the medium of the sounding line and dredge, of the contour features, the formation, and the animal life of the great oceanic bed.

The general results of the notable African land journey have already, through our parent society in London, been brought largely under public review; and at our present meeting many details of interest will be placed before you by the intrepid traveller himself. The courage, perseverance and patient attention to the records of this long travel have been dwelt on by our highest geographical authorities, and so far it might appear superfluous to join in praise from this chair; nevertheless, it is to that part of the proceedings of Cameron, the unvarying attention and care he bestowed on instrumental observations, in order to give those proceedings a secure scientific basis, to which I would direct your attention as being of a high order of merit.

With this example before us, remembering the country and climate in which such unremitting labours were carried out, distinction to the future explorer cannot rest on the mere rendering of estimated topographical details, but can alone be fully merited when those details are verified by instrumental observations of an order sufficient to place numerically before geographers the physical features and characteristics of the explored region.

Turning now from the results of the land journey of Cameron to those of the sea voyage of the *Challenger* we are again reminded of the value of repeated and methodically arranged instrumental observations in geographical research. With our present knowledge of the sea-board regions of the globe, little remains, except in Polar areas, for the navigator to do in the field of discovery, or even of exploration, otherwise than in those details rendered necessary by the requirements of trade or special industries. It is to the development of the scientific features of geography that the attention of voyagers requires to be now mainly directed; and in this there is an illimitable field. The great advance in this direction resulting from the two leading events of the past year, to which I have referred, foreshadows geographical research of the future.

Communications of special value from some of those voyagers whose good fortune it was to leave and return to their native land in the ship *Challenger* will doubtless be made to this and other Sections. I trust nevertheless, as one officially interested in the expedition from its inception, and as having in early days been engaged in kindred work, and also as I hope without being considered to have trespassed on the scientific territories of these gentlemen—ground indeed so well earned,—this meeting will view with indulgence my having selected as the leading theme of my address to it, a review of that branch of our science now commonly known as the "Physical Geography of the Sea;" combined with such suggestive matter as has presented itself to me whilst engaged in following up the proceedings of this remarkable voyage.

It has been well observed that "contact with the ocean has unquestionally exercised a beneficial influence on the cultivation of the intellect and formation of the character of many nations, on the multiplication of those bonds which should unite the whole human race, on the first knowledge of the true form of the earth and on the pursuit of astronomy and of all the mathematical and physical sciences." The subject is thus not an ignoble one, and further, it appears to me appropriate, assembled as we are in the commercial metropolis of Scotland, from among whose citizens some of the most valuable scientific investigations bearing on the art of navigation have proceeded.

As a prefatory remark, I would observe that the distinctive appellation "Physical Geography of the Sea" is due to the accomplished geographer Humboldt; it is somewhat indefinite though comprehensive, and implies that branches of science not strictly pertaining to geography, as commonly understood, are invaded; but this intrusion or overlapping of scientific boundaries is inevitable with the expansion of knowledge: and it is difficult to see how the term can be wisely amended, or how the several included branches of physics can be separated from pure geographical science.

We are indebted in our generation to the genius and untiring energy of Maury, aided originally by the liberal support of his Government, for placing before us in the two-fold interests of science and commerce an abundant store of observed facts in this field; accompanied, too, by those broad generalisations, which, written with a ready pen and the fervour of an enthusiast gifted with a poetic temperament, have charmed so many readers, and in their practical bearings have undoubtedly advanced navigation in practice.

In our admiration, however, of modern progress we must not in justice pass by without recognition the labours of earlier workers in the same field. So early as the middle of the seventeenth century we find in Holland, Barnard Vanerius describing with commendable accuracy the direction of the greater currents of the Atlantic Ocean and their dependence on prevailing winds; the unequal saltness of the sea, the diversity of temperature as the causes of the direction of the winds, and also speculating on the depths of the sea. Vanerius's geographical writings were highly appreciated by Newton, and editions were prepared at Cambridge under the supervision of that great man in 1672 and 1681.

To Dampier the seaman, and Halley the philosopher, we owe graphic descriptions of the trade winds as derived from personal experience; while the investigations by Hadley of their causes, and the conclusions he arrived at, that they were due to the combined effects of the diurnal revolution of the earth on its axis, and the unequal distribution of heat over different parts of the earth's surface, in substance still remain unchallenged.

To Rennell we owe a masterly investigation of the currents of the Atlantic Ocean, an investigation, which for precision and a thorough conception of the conditions affecting the subject will long serve as a model for imitation. His period covered some thirty or forty years during the end of the last and the beginning of the present century. At that epoch, chronometers—though very efficient—had scarcely passed the stage of trial, but had nevertheless commended themselves to the first navigators of the day, whose aim it was to narrowly watch and test this, to them, marvellous acquisition. Rennell thus commanded nautical observations of a high order of merit; these he individually verified, both for determining the ship's position absolutely and relatively to the course pursued; and our knowledge of surface-currents was established on the secure basis of differential results obtained at short intervals, such as a day or parts of a day, instead of the previous rude estimation from a ship's reckoning extending over a whole voyage, or its greater part.

At a later date we have by Redfield, Reed, Thom, and others, solidly practical investigations of the gyratory and at the same time bodily progressive movements of those fierce and violent storms which, generated in tropical zones, traverse extensive districts of the ocean, not unfrequently devastating the narrow belt of land comprised in their track; and on the sea baffling all the care and skill of the seaman to preserve his ship scathless; while the clear and elegant exposition by Dove of their law and its application as one common general principle to the ordinary movements of the atmosphere must commend itself as one of the achievements of modern science.

While for the moment in the aerial regions, we must not forget the industry and scientific penetration of the present excellent secretary of the Scottish Meteorological Society. His more recent development of the several areas of barometric pressure, both oceanic and continental, bids fair to amend and enlarge our conceptions of the circulation of both the aerial and liquid coverings of our planet.

Looking then from our immediate stand-point on the extent of our knowledge, as confirmed by observational facts of the several branches of physics pertaining to the geography of the sea, just rapidly reviewed, we find that, resulting from the methodical gathering up of "ocean statistics" by our own and other maritime nations, in the manner shadowed forth by Maury and stamped by the Brussels Conference of 1853, we are in possession of a goodly array of broad but nevertheless sound results. The average seasonal limits of the trade winds and monsoons, with the areas traversed by circular storms are known; also the general linear direction and varying rates of motion of the several ocean currents and streams; together with the diffused values of air and sea surface temperatures, the areas of uniform barometric pressure, and the prevalent winds, over the navigable parts of the globe.

Thus far the practical advantages that have accrued to the art of navigation—and so directly aiding commerce—by the gradual diffusion of this knowledge through the medium of graphical rendering on charts, and concise textual descriptions, cannot be over-rated; still much is wanting in fulness and precision of detail, especially in those distant but limited regions more recently opened out by expanding trade. Science views, too, with increasing interest these advances in our knowledge of ocean physics, as bearing materially on the grand economy of nature: essays brilliant and almost exhaustive on some of its subjects, have been given to us by eminent men of our own day; but here one is reminded, by the diversity in the rendering of facts, how much remains to be done in their correlation, and what an extensive and still expanding field is before us.

The dawning efforts of science to pass beyond the immediate practical requirements of the navigator are worthy of note. We find—from an admirable paper on the "Temperatures of the Sea at different Depths," by Mr. Prestwich, just published in the *Philosophical Transactions*—that in the middle of last century the subject of deep-sea temperatures first began to attract attention, and thermometers for the purpose were devised; but it was not till the early part of the present century that the curiosity of seamen appears to have been generally awakened to know more of the ocean than could be gleaned on its surface. John Ross, when in the Arctic seas in 1818, caught glimpses of animal life at the depth of 6,000 feet; other navigators succeeded in obtaining the temperature of successive layers of water to depths exceeding 6,000 feet, but, so far as I can ascertain, James Ross was, in 1840, the first to record beyond doubt that bottom had been reached, "deeper than did ever plummet sound," at 16,060 feet, westward of the Cape of Good Hope.

The impetus to deep sea exploration was, however, given by the demand for electric telegraphic communication between countries severed by the ocean, or by impracticable land routes, and the past twenty years marks its steady growth. Appliances for reaching the bottom with celerity, for bringing up its water, for bringing up its formation, for registering its thermal condition *in situ*, have steadily improved, and thus the several oceans were examined both over present and prospective telegraph routes. Science, aroused by the consideration that vast fields for biological research were opening up—as proved by the returns, prolific with living and dead animal matter, rendered by the comparatively puny appliances originally used for bringing up the sea bottom—invoked, as beyond the reach of private enterprise, the aid of Government. Wisely, earnestly, and munificently was the appeal responded to, and thus the *Challenger* Expedition has become the culminating effort of our own day.

We have now reached, in all probability, a new starting-point in reference to many of our conceptions of the physics of the globe, and our own special branch may not be the least affected. There is opened up to us, for example, as fair a general knowledge of the depression of the bed of large oceanic areas below the sea level, as of the elevation of the lands of adjacent continents above that universal zero line. We learn for the first time by the *Challenger's* results—ably supplemented as they have recently been by the action of the U.S. Government in the Pacific, and by an admirable series of soundings made in the exploratory German ship of war *Gazelle*—that the unbroken range of ocean in the southern hemisphere is much shallower than the northern seas, that it has no features approaching in character those grand abyssal depths of 27,000 and 23,500 feet found respectively in the North Pacific and North Atlantic Oceans, as the greatest reliable depths recorded do not exceed 17,000 or 17,500 feet.

The general surface of the sea bed presents in general to the eye, when graphically rendered on charts by contour lines of equal soundings, extensive plateaux varied with the gentlest of undulations. There is diversity of feature in the western Pacific Ocean, where, in the large area occupied by the many groups of coral islands, their intervening seas are cut up into deep basins or hollows, some 15,000, some 20,000 feet deep. In the Northern Oceans one is struck with the fact that the profounder depths in the Pacific occupy a relative place in that ocean with those found in the Atlantic; both abyssal areas have this, too, in common, the maximum depths are near the land, the sea surface temperature has the maximum degree of heat in either ocean, and two of the most remarkable ocean streams—Florida Gulf and Japan—partially encompass them.

In the Atlantic Ocean, from a high Southern latitude, a broad channel with not less than some 12,000 to 15,000 feet can be traced, as extending nearly to the entrance of Davis Strait: a dividing undulating ridge of far less depression, on which stand the islands of Tristan d'Acunha, St. Helena, and Ascension, separates this which may be named the Western Channel from a similar one running parallel to the South African Continent, and which extends to the parallel of the British Islands. It is possible that certain tidal and, indeed, climatic conditions, peculiar to the shores of the North Atlantic, may be traced to this bottom conformation, which carries its deep, canal-like character into Davis Strait, and between Greenland, Iceland, and Spitzbergen, certainly to the 80th parallel.

There is, however, one great feature common to all oceans, and which may have some significance in the consideration of ocean circulation, and as affecting the genesis and translation of the great tidal wave and other tidal phenomena, of which we know so little; namely, that the fringe of the seaboard of the great continents and islands, from the depth of a few hundred feet below the sea-level, is, as a rule, abruptly precipitous to depths of 10,000 and 12,000 feet. This grand escarpment is typically illustrated at the entrance of the British Channel, where the distance between a depth of 600 feet and 12,000 feet is in places only ten miles. Imagination can scarcely realise the stupendous marginal features of this common surface depression.

Vast in extent as are these depressed regions—for we must recollect that they occupy an area three times greater than the dry land of the globe, and that a temperature just above the freezing-point of Fahrenheit prevails in the dense liquid layers covering them—life is sustained even in the most depressed and coldest parts; while in those areas equivalent in depression below the sea-level to that of European Alpine regions above it, animal life abundantly prevails: structural forms complicated in arrangement, elegant in appearance, and often lively in colour, clothe extensive districts; other regions apparently form the sepulchral resting-place of organisms which when living existed near the surface; their skeletons, as it has been graphically put, thus, "raining down in one continuous shower through the intervening miles of sea-water." Geological formations, stamped with the permanency of ages, common to us denizens of the dry land, appear, in these regions, to be in course of evolution; forces involving the formation of mineral concretions on a grand scale are at work; life is abundant everywhere in the surface and sub-surface waters of the oceans; in fine, life and death, reproduction and decay, are active, in whatever depths have been attained.

As a question of surpassing interest in the great scheme of nature, the economy of ocean circulation, affecting as it does the climatic conditions of countries, has of late attracted attention. The general facts of this circulation in relation to climate have been thus tersely summarised: "Cold climates follow polar waters towards the equator, warm climates follow warm equatorial streams towards the poles." We can all appreciate the geniality of our own climate, especially on the western shores of the kingdom, as compared with the Arctic climate of the shores of Labrador, situated on the same parallels of latitude; or indeed, with the vigorous winter climate of the adjacent North American seaboard, even ten degrees farther to the south. These, and kindred features in other parts of the globe, have led to the summarised generalisation I have just referred to, but the *rationale* of these movements of the waters is by no means assured to us.

That ocean currents were due primarily to the trade and other prevailing winds, was the received opinion from the earliest investigation made by navigators of the constant surface movement of the sea. Rennell's views are thus clearly stated—"The winds are to be regarded as the prime movers of the currents of the ocean, and of this agency the *trade winds* and *monsoons* have by far the greatest share, not only in operating on the *larger half* of the whole extent of the circumambient ocean, but as possessing greater power by their constancy and elevation to generate and perpetuate currents" . . . "next to these, in degrees, are the *most prevalent* winds, such as the westerly wind beyond, or to the north and south, of the region of trade winds."

Maury, so far as I am aware, was the first to record his dissent from these generally received views of surface currents being due to the impulse of the winds, and assigned to differences of specific gravity, combined with the earth's rotation on its axis, the movement of the Gulf Stream, and other well defined ocean currents.

A writer of the present time, gifted with high inductive reasoning powers and with observed facts before him in wide extension of those investigated by Rennell, regards the various ocean currents as members of one grand system of circulation; not produced by the trade winds alone, nor by the prevailing winds proper alone, but by the continued action of all the prevailing winds of the globe regarded as one system of circulation; and that without exception, he finds the direction of the main currents of the globe to agree exactly with the direction of the prevailing winds.

Another writer of the present day, distinguished for intellectual power, and who personally has devoted much time to the acquisition of exact physical facts bearing on the question both in the ocean near our own shores and in the Mediterranean sea, without denying the agency of the winds, so far as surface drifts are concerned, considers that general ocean circulation is dependent on thermal agency alone; resulting in the movement of a deep stratum of polar waters to the equator, and the movement of an upper stratum from the equator towards the poles: the "disturbance of hydrostatic equilibrium" being produced by the increase of density occasioned by polar cold and the reduction of density occasioned by equatorial heat; and that polar cold rather than equatorial heat is the *primum mobile* of the circulation. Analogous views had also been entertained by Continental physicists from sea temperature results obtained in Russian and French voyages of research in the early part of this century.

We have here presented to us two distinct conceptions of ocean circulation—the one to a great extent confined to the surface and horizontal in its movements, the other vertical extending from the ocean surface to its bed, and involving, as a consequence, "that every drop of water will thus [except in confined seas] be brought up from its greatest depths to the surface."

With these several hypotheses before us, it may be fairly considered that the problem of "ocean circulation" is still unsolved. Possibly, the real solution may require the consideration of physical causes beyond those which have been hitherto accepted. In attempting the solution, it appears to me impossible to deny that the agency of the winds is most active in bringing about great movements on the surface waters: the effects of the opposite monsoons in the India and China seas furnishing corroborative proof. Again, the remarkable thermal condition of the lower stratum of the water in enclosed seas, as the Mediterranean, and in those basin-like areas of the Western Pacific cut off by encircling submarine ridges from the sources of polar supplies, combined with the equally remarkable conditions of cold water from a polar source flowing side by side or interlacing with warm water from equatorial regions—as in the action of the Labrador and Gulf Streams—points to the hypothesis of a vertical circulation as also commanding respect.

The time may be considered, however, to have now arrived for gathering up the many threads of information at our disposal; and by fresh combinations to enlarge at least our conceptions, even if we fail in satisfying all the conditions of solution. To this task I will briefly address myself.

A grand feature in terrestrial physics, and one which I apprehend bears directly on the subject before us, is that producing ice movement in Antarctic seas. We know from the experience gained in ships—which, to shorten the passages to and from this country, Australia and New Zealand, have followed the great circle route, and thus attained high southern latitudes—that vast tracts of ice from time to time become disrupted from the fringe of southern lands. Reliable accounts have reached us of vessels frequently running down several degrees of longitude, sadly hampered by meeting islands of ice; and especially of one ship being constantly surrounded with icebergs in the corresponding latitudes to those of London and Liverpool, extending nearly the whole distance between the meridians of New Zealand and Cape Horn. Indeed, accumulated records point to the conclusion that on the whole circumference of the globe south of the 50th parallel, icebergs, scattered more or less, may be constantly fallen in with during the southern summer.

The Antarctic voyages of D'Urville, Wilkes, and James Ross assure us of the origin and character of these ice masses which dot the Southern seas. Each of these voyagers were opposed in their progress southward—D'Urville and Wilkes on the 65th parallel, Ross on the 77th, by barrier cliffs of ice. Ross traced this barrier 250 miles in one unbroken line; he describes it as one continuous perpendicular wall of ice, 200 to 100 feet high above the sea, with an unvarying level outline, and probably more than 1,000 feet thick—"a mighty and wonderful object." Ross did not consider this ice barrier as resting on the ground, for there were soundings in 2,500 feet a few miles from the cliffs; Wilkes also sounded in over 5,000 feet, only a short distance from the barrier.

There is singular accord in the descriptive accounts by Wilkes and Ross of this ice region; they both dwell on the difference in character of Antarctic from Arctic ice formation, on the tabular form of the upper surface of the floating icebergs, and their striated appearance; on the extreme severity of the climate in midsummer; of the low barometric pressure experienced—and express equal wonderment at the stupendous forces necessary to break away the face of these vast ice barriers, and the atmospheric causes necessary for their reproduction.

From the drift of this disrupted ice we have fair evidence of a great bodily movement of the waters northward; for it must be remembered that icebergs have been fallen in with in the entire circumference of the southern seas, and that they are pushed in the South Atlantic Ocean as far as the 40th parallel of latitude; in the South Indian to the 45th parallel; and in the South Pacific to the 50th parallel.

In the discussion of ocean circulation, it has been assumed that water flows from Equatorial into Antarctic areas; there is no evidence, so far as I am aware, that warm surface water in the sense implied is found south of the 55th parallel. Surface stream movement northward and eastward appears to be that generally experienced in the zone between the Antarctic circle and that parallel. With, then, this great bodily movement northward of Antarctic waters included certainly between the surface and the base, or nearly so, of these tabular icebergs (and thus representing a stratum certainly some thousand feet in thickness), the question arises, How and from whence does the supply come to fill the created void? Sir Wyville Thomson, the leader of the *Challenger* scientific staff, in one of the later of the many able reports he has forwarded to the Admiralty, furnishes, I think, a reasonable answer. Stating first his views as derived from study of the bottom temperature of the Pacific Ocean generally, he writes:—"We can scarcely doubt that, like the similar mass of cold bottom-water in the Atlantic, the bottom-water of the Pacific is an extremely slow indraught from the Southern Sea." He then gives the reason. "I am every day more fully satisfied that this influx of cold water into the Pacific and Atlantic Oceans from the southward is to be referred to the simplest and most obvious of all causes, the excess of evaporation over precipitation of the land-hemisphere; and the excess of precipitation over evaporation in the middle and southern parts of the water-hemisphere."

Before following up the great northward movement of Antarctic waters, I would draw attention to a physical feature in connection with tidal movements, which possibly may be one of the many links in the chain of causes affecting ocean circulation. The mean tide level (or that imaginary point equidistant from the high and low water-marks as observed throughout a whole lunation) has been assumed as an invariable quantity; our Ordnance Survey adopts it as the zero from whence all elevations are given: the *datum level* for Great Britain being the level of mean tide at Liverpool. For practical purposes, at least on our own shores, this mean sea-level may be considered invariable, although recent investigations of the tides at Liverpool and Ramsgate indicate changes in it to the extent of a few inches, and which changes are embraced in an annual period, attaining the maximum height in the later months of the year; these have been assumed as possibly due to meteorological rather than to the astronomical causes involved by tidal theory.

From an examination of some tidal observations recently made near the mouth of Swan River, in Western Australia, during the progress of the Admiralty survey of that coast, there appears to me evidence that in this locality—open, it will be remembered, to the wide southern seas—the sea-level varies appreciably during the year: thus, the greatest daily tidal range in any month very rarely exceeds 3 feet, but the high and low water-marks range during the year 5 feet. The higher level is attained in June, and exceeds the lower level, which is reached in November, by one foot or more. At Esquimalt in Vancouver Island, fairly open to the North Pacific Ocean, there are indications of the sea-level being higher in January than it is in June; and a distinct excess of the mean level of the tide by several inches in December and January, as compared with the summer months, was traced by the late Captain Beechy, R.N., at Holyhead (see *Phil. Trans.* 1848). If this surface oscillation is a general oceanic feature, and some further proofs indirectly appear in the Reports of the Tidal Committee to this Association for 1868, '70, '72, to which I have just referred—for mention is also made of a large annual tide of over three inches, reaching its maximum in August, having been observed at Cat Island, in the Gulf of Mexico;—we may have to recognise this physical condition, that the waters of the southern hemisphere attain a high level at the period of the year when the sun is to the north of the equator, and that the northern waters are highest at the period when the sun is to the south of the equator. This is a question of so much interest that I propose again to revert to it.

Variations in the sea level have been observed, notably in the central parts of the Red Sea, where the surface water, as shown by the exposure of coral reefs, is said to be fully two feet lower in the summer months than in the opposite season; these differences of level are commonly assigned to the action of the winds.

Rennell, in his "Investigation of the Currents of the Atlantic Ocean," states, on what would appear reliable authority, that on the African Guinea coast the level of the sea is higher by at least six feet perpendicular in the season of the strong S.W. and southerly winds—which winds blow obliquely into the Bay of Benin between April and September, the rainy season also—than during the more serene weather of the opposite season; the proof being that the tides ebb and flow regularly in the several rivers during the period of strong S.W. winds, but that in the other season the same rivers run ebb constantly, the level of the sea being then too low to allow the tide waters to enter the mouths of the rivers. It is possible the cause, here and elsewhere, may, in part be cosmical, and neither meteorological nor astronomical in a tidal sense.

These several facts in relation to the variations in levels of the surface of the ocean are interesting, and point to new fields of observation and research.

Another physical feature connected with the ocean level is deserving consideration; I refer to the effect of the pressure of the atmosphere. On good authority we know that the height of high water in the English Channel varies inversely as the height of the barometer; the late Sir John Lubbock laid it down as a rule that a rise of one inch in the barometer causes a depression in the height of high water amounting to seven inches at London and to eleven inches at Liverpool. Sir James Ross when at Port Leopold, in the Arctic seas, found that a difference of pressure of ·668 of an inch in the barometer produced a difference of 9 inches in the mean level of the sea, the greatest pressure corresponding to the lowest level. These results appeared to him to indicate "that the ocean is a water-barometer on a vast scale of magnificence, and that the level of its surface is disturbed by every variation of atmospheric pressure inversely as the mercury in the barometer, and exactly in the ratio of the relative specific gravities of the water and the mercury." When we consider the exceptionally low barometric pressure prevailing in the southern seas, and the comparatively low pressure of the Equatorial Ocean zones as compared with the areas of high pressure in the oceans north and south of the Equator—the latter features a late development by Mr. Buchan—these characteristic conditions of atmospheric pressures cannot exist without presumably affecting the surface conditions of adjacent waters.

There is yet one more point in connection with the ocean circulation which I venture to think has not received the attention it demands; this is the economy of those currents known as "counter equatorial." Their limits are now fairly ascertained, and are found to be confined to a narrow zone; they run in a direction directly opposite to, and yet side by side with, the equatorial streams of both the Atlantic and Pacific Oceans. We know that they run at times with great velocity (the *Challenger* experienced fifty miles in a day in the Pacific Ocean), and occasionally in the face of the trade wind; and that they are not merely local, stretching as they do across the wide extent of the Pacific; and in the Atlantic, during the summer months of our hemisphere, extending nearly across from the Guinea Coast to the West India Islands. They have too this significant feature that their narrow zone is confined to the *northern side alone* of the great west-going equatorial currents; this zone is approximately between the parallels of 7° and 10° N., and thus corresponds with the belt of greatest atmospherical heat on the earth's surface.

That the functions of the counter currents in the physics of the ocean are important must, I think, be conceded. They appear to act on their eastern limits as feeders to the equatorial currents; and from the seasonal expansion, which has been well traced in the Atlantic, are probably more immediately associated with some oscillatory movement of the waters following, though perhaps only remotely connected with, the sun's movements in declination.

A brief summary of the thermal conditions of the oceanic basins will now enable us to review the salient features of ocean circulation, and the more immediate scientific position the question has assumed.

In all seas within the torrid and temperate zones, provided any given area is not cut off by submarine barriers from a supply of polar or glacial water, the sea bed is covered by a thick stratum of water, the temperature of which is confined between 32° and 35° F. In the Pacific Ocean this cold stratum must be derived from Antarctic sources, for the opening of Behring Strait is too small to admit of an appreciable efflux of Arctic waters. In this ocean the cold stratum obtains generally at depths below 9,000 feet from the surface, with an almost invariable isothermal line of 40° F., at from 2,500 to 3,000 feet from the surface. Similarly, in the Indian Ocean basin, the cold stratum at the bottom is derived from Antarctic sources, for the temperature of 33°·5 F. underlies the hot surface waters of the Arabian Gulf.

In the South Atlantic, Antarctic waters, with a bottom temperature of 31° to 33°·5 F., certainly cross the equator; the bed of the North Atlantic basin then warms up to 35°—marked diversities in both the temperatures and thickness of the successive layers of water from the surface downwards are found—and in the central parts of the basin it is not until the vicinity of the Faroe Islands is reached that Arctic waters of an equivalent emperature to those from Antarctic sources are experienced.

Turning now to the scientific aspect of the question:—

The doctrine of a general oceanic thermal circulation assumes two general propositions—1, the existence of a deep under-flow of glacial water from each pole to the equator; and 2, the movement of the upper stratum of oceanic water from the equatorial region towards each pole, as the necessary complement of the deep polar under-flow—this double movement being dependent "upon the disturbance of hydrostatic equilibrium constantly maintained by polar cold and equatorial heat."

Proposition 2, in its general application as to the movement of surface waters, is unquestionable; but that of a deep under-flow from the poles, as a necessary complement, remains open to doubt. Proposition 1, in its wide generality, must, from what we know of the Pacific, be confined to the Atlantic Ocean; and it appears to me that it is on the interpretation of the movement of the waters in its northern basin that the hypothesis of a vertical circulation and the potency of thermal agency in bringing it about must be judged.

We have followed the movements of Antarctic waters in the Atlantic to the 40th parallel, as illustrated by the progress of icebergs; we know that the movement deflects the strong Agulhas current, and that the cold waters well up on the western shore of the South African continent, cooling the equatorial current near its presumed source; the thrusting power of this body of water is therefore great. About the equator it rises comparatively near to the surface. But we now come to another and distinct movement—the equatorial current—and on this, I apprehend, the material agency of the winds cannot be denied, in forcing an enormous mass of surface-water from east to west across the ocean. The Gulf Stream results, and the comparative powers of this stream, as especially influencing the climate of our own and neighbouring countries, together with the forces at work to propel its warm waters across the Atlantic, has become the controversial field for the upholders of horizontal and vertical circulation. The one hypothesis assigns to the Gulf Stream all the beneficent powers of its genial warmth—extending even beyond the North Cape of Europe—which has been conceded to it from the time of Franklin. The other hypothesis reduces its capacity and power, considers that it is disintegrated in mid-Atlantic, and that the modified climate we enjoy is brought by prevailing winds from the warm area surrounding the stream; and to this has been more recently added, "by the heating power of a warm sub-surface stratum, whose slow northward movement arises from a constantly renewed disturbance of thermal equilibrium between the polar and equatorial portions of the oceanic area."

Without denying the active powers of this disturbed thermal equilibrium—although in this special case it is an abstraction difficult to follow—and giving due weight to the many cogent facts which have been brought forward in support of both views, there appears to be still a connecting link or links wanting to account for the southern movements of Arctic waters; which movements to me are even more remarkable as physical phenomena than the translation of the warm waters from the Gulf Stream area to a high northern latitude.

This movement of Arctic waters is forcibly illustrated by the winter drifts down Davis Strait of the ships *Resolute*, *Fox*, *Advance*, and part of the crew of the *Polaris*, when enclosed in pack ice, exceeding in some cases a thousand miles; similarly of the winter drift of a part of the German expedition of 1870 down the east side of Greenland, from the latitude of 72° to Cape Farewell. If to these examples we add the experience of Parry in his memorable attempt to reach the North Pole from Spitzbergen in the summer of 1827, it must be inferred that a perennial flow of surface water from the polar area into the Atlantic obtains; and, judging from the strength of the winter northerly winds, that the outflow is probably at its maximum strength in the early months of the year.

When we further know that the northern movement of warm waters gives *in winter* a large accession of temperature to the west coast of Scotland, to the Faroe Islands, and extending to the coasts of Norway as far as the North Cape; the consideration arises whether this onward movement of waters from southern sources is not the immediate cause of displacement of th water in the polar area, and its forced return along the channels indicated by those winter drifts to which I have referred.

That some hitherto unlooked-for and unsuspected cause is the great agent in forcing southern waters into the Atlantic polar basin has long forced itself on my conviction, and I now suspect it is to the cause producing the annual variations in the sea level—for, as I have mentioned, indications exist of the seas of the northern hemisphere having a higher level in winter than in summer,—that we must direct our attention before the full solution of ocean circulation is accepted.

The facts of the annual changes of sea level, whatever they may ultimately prove, have hitherto ranged themselves as a part of tidal action, and so escaped general attention. Physicists well know the complication of tidal phenomena, and if one may be permitted to say, the imperfection of our tidal theory; certain it is that the tides on the European coasts of the Atlantic are so far abnormal that one of our best authorities on the subject (Sir William Thomson) describes them, in relation, I assume, to tidal theory, as "irregularly simple," while the tides in all other seas are comparatively complicated, but "regular and explicable." However this may be, specialists should direct their attention to the disentanglement of the variations in the sea level from tidal action simple; and our colonies, especially those in the southern hemisphere, would be excellent fields for the gathering in of reliable observations.

I am unwilling to leave the subject without tracing some of the consequences that might be fairly considered to follow this assumed change of level in the North Atlantic basin. We can by it conceive the gradual working up of the warmed water from southern sources as the winter season approaches, including the expansion of the Gulf Stream in the autumn months; the consequent welling up of a head of water in the enclosed and comparatively limited area northward of Spitzbergen, Greenland, and the broken land westward of Smith Sound; the forced return of these glacial waters, their greatest volume seeking the most direct course, and thus working down the Labrador coast, charged with ice, and passing the American coast inside the Gult Stream; while the smaller volume, reaching the higher latitudes in mid-Atlantic, interlaces with the warm barrier waters, causing those alternating bands of cold and warm areas familiar to us from the *Lightning* and *Porcupine* observations, and which are now being worked out by the Norwegian exploring expedition in the government ship *Vöringen*.

We can further conceive that the larger function of the "counter currents" on the north margin of the great equatorial streams is to act as conduits for the surcharged waters of the Northern Oceans consequent on the gradual changes of level. The Atlantic counter-current we know expands markedly in the autumnal season, and there may be some connection between this expansion and the high level of the waters said to exist in the Gold Coast and Guinea bights at the same season.

We are thus, as it appears to me, now only on the threshold of a large field of inquiry bearing on the physical geography of the sea; but we have this advantage,—the admirable discussions which have taken place in the past few years, productive as they have been of the marshalling hosts of valuable facts, will lighten the labours of those who engage in its prosecution. Science is deeply indebted to, and I am sure honours, those who have so earnestly worked on the opening pages of the coming chapter on Ocean Circulation.

Unwillingly I turn from this interesting subject; but the demands on my time and your patience are imperative: as, following precedent, it is incumbent on me briefly to bring under the review of the Association the latest unrecorded incidents in geographical progress or research.

There is one absorbing topic which, in the course of a few weeks, or even days, may attract general interest. I refer to accounts of our Arctic expedition. It is possible that while I am now addressing you, the ships *Alert* and *Discovery*, favoured by fine seasons, may have, in their endeavours to reach high northern latitudes, accomplished all that human skill and energy can do, and by fortuitous circumstances secured their return southward through Smith Sound, with the same facilities, as we have reason to hope, they entered what we suppose to be that notable gateway to the Pole. If so, they are now fairly in Davis Strait, homeward bound. We must not regard this estimate of progress as visionary, for, the conditions being favourable, the time at the disposal of the voyagers is ample. It is the varying conditions of Arctic seasons, we must remember, that baffle the forecasts of the most experienced Arctic experts.

Should unfavourable conditions, or the decision of the chief, detain the ships another year in their icy quarters, we have reason to hope that advices will reach us of their whereabouts in the spring of next year. The spirited enterprise of the well-trained Arctic navigator, Allan Young, supported as he has been by the Government, offers a sure guarantee that the leaders, Nares and Stephenson, will be ably seconded in their efforts to keep up communication with their countrymen. Here, again, we must not forget that baffling conditions may defeat the intentions of the commanders to communicate in time with the depôts at the portals of Smith Sound.

This prolonged banishment from intercourse with the outer world was, however, a contingency anticipated and provided for by that able Committee of Arctic Officers who, with a full sense of their responsibility, so fully advised the Government in every phase of this national undertaking. A parliamentary paper, published during this session, gives the fullest particulars relating to the progress of the expedition and the steps which have been taken to communicate with their depôts. There is a long chain of contingencies to be attended to, as will be seen on reference to the interesting details therein given, but I venture to think that not a link is missing, either in the conception, or in the means provided to bring the undertaking to a successful issue.

There is one feature to be kept in view, which from the exceptional conditions of ship navigation in the icy regions of the far north is rarely realized, unless by those who have had actual experience in polar service, and it is this, that between the time of the disruption of the old ice in August and the formation of the new in September, there exists a very short period when ships are free to move. This period of open or partially open water may be shortened by unfavourable circumstances, and *vice versâ*; it may be assumed, however, that in a straight fairway channel such as Smith Sound it almost always does occur, and as the return southward, on account of the drift, is always more easily accomplished than the advance north, the great probability is that, if the ships remain out another year, it will be the result of design rather than accident.

By the parliamentary papers relating to the expedition it will be seen that, in the event of the non-arrival of the *Alert* and *Discovery* during the autumn of this year, a relief ship will be despatched to a rendezvous in Smith Sound during the summer of 1877.

With regard to Africa, exploration and discovery have proceeded with accelerated strides during the past few years. Even since the recent date of Cameron's remarkable journey across the continent, important additions have been made to the rapidly filling-up map of the interior. Most of these additions relate to the great lakes, regarding which our knowledge was previously very incomplete and unsatisfactory. Thus, Mr. Young, the experienced Zambesi traveller, who undertook last year to lead the Scotch Missionary party to Lake Nyassa, has succeeded, after establishing the missionary settlement "Livingstonia," at the southern end of the lake, in reaching in a steam-launch the northern end of this great fresh-water sea, finding it to be fully one hundred miles longer than was previously believed. His journey was made in February of the present year, and in the following month the still more imperfectly known lake, Albert Nyanza, was successfully navigated by two boats under Signor Gessi, who was despatched for this purpose by Colonel Gordon, the present Governor of the new Equatorial Province of the Khedive's dominions. The details of Signor Gessi's interesting exploration, communicated by himself to the President of the Royal Geographical Society, have only recently reached England, and it is proposed to read them in the course of the present meeting.

A third, and equally important exploration of the same class is that performed during the same early months of the present year by that energetic traveller Mr. Stanley. After circumnavigating the much larger neighbouring lake, Victoria, and proving Speke's much disputed estimate of its dimensions to be approximately correct, he pushed his way across the difficult tract of country separating the Victoria and the Albert lakes, reaching the shores of the latter in the middle of January. Less fortunately situated than Signor Gessi, who embarked on the lake two months later, Stanley was unable to launch his boat on the then unexplored southern portions of its waters. A comparison of the accounts of the two travellers shows that we are yet far from knowing the true dimensions of this great sheet of water. Signor Gessi in fact did not reach its southern extremity; and as Mr. Stanley appears to have struck its shores at a point about thirty miles further south than the limits marked by the Italian traveller, the lake must be considerably longer than 140 miles, as estimated by the latter. Stanley subsequently proceeded south and explored the Kitangulé river of Speke; thence striking for Lake Tanganyika, the examination of which he intended to complete.

New Guinea has of late attracted some attention both at home and in the Australian colonies; rather, however, from political than geographical considerations. Our interest is of course in the latter, and I am glad the meeting will have the advantage of the presence of a gentleman, Mr. Octavius Stone, recently arrived in England, who has distinguished himself in the exploration of the south-eastern shores of this distant, little known, and barbarous region; to him we must refer for the latest geographical facts.

OUR ASTRONOMICAL COLUMN

THE CORDOBA "URANOMETRIA."—Dr. Gould has informed us, during his flying visit to this country in the last week, on his return to Cordoba from the United States, that he intends to give his *Uranometria* first and undivided attention, with the view to its early publication. It contains 8,000 stars to seventh magnitude inclusive, the whole estimated by not less than two observers, and often by more, each observer making his determination on not less than two nights, and often more, all cases of discordance between different observers being subsequently examined. The greater number of the stars have been observed with the meridian circle, and always in cases of doubt as to identification. The magnitudes are intended to be given to 0·1m by comparisons with previously established standards, on a most carefully-considered system. The manuscript charts are drawn to the scale of a globe of one *metre* radius, and the magnitudes of the stars are represented by dots of size proportional to the brilliancy to nearest two-tenths of a magnitude. Though this part of the work appears to have been completed to Dr. Gould's entire satisfaction, he expresses himself much disturbed as to the means of reproducing these manuscript charts with the necessary accuracy and delicacy; his hopes of success from the use of photography having been thus far disappointed. The *Uranometria* will include every star to the seventh magnitude inclusive, from the south pole of the heavens to ten degrees of north declination. Great care has been taken to secure accurate delineation of the course of the Milky Way and of the Magellanic Clouds.

The Zones, another most important work to which attention has been directed at Cordoba, are complete; they are 754 in number, and contain 105,000 stars.

In addition, materials have been obtained for the formation of a numerous catalogue of the brighter stars, each one observed several times with the meridian circle.

Dr. Gould is to be congratulated on the extraordinary energy he has displayed in his management of the new Observatory of the Argentine Republic, and the discriminating skill with which he has selected and worked his subjects of observation, which must undoubtedly result in his leaving a name lastingly associated with the astronomy of the southern hemisphere; and not less is the Government of that comparatively new country to be honoured for the constant and unstinted support they have afforded to their national Observatory, and its distinguished and indefatigable director.

AN INTRA-MERCURIAL PLANET (?).—At the sitting of the Paris Academy of Sciences on the 28th ultimo, M. Leverrier announced that he had received a letter from Prof. Rudolf Wolf, of Zurich, in which it was stated that three observers situated in three different places had witnessed, on April 4, the passage of a round spot over the sun's disc. The three localities were—in Germany (near Münster), Greece (Athens), and Switzerland (Zurich). The date is subsequent to the observation of Dr. Lescarbault by 6,219 days, which figure is the product of 148 into 42·02 (printed 40·02 in *L'Institut*, whence this notice is taken), and it may be conjectured that, if the object were a planet, it had made this number of revolutions of 42·02 days.

Such a body would have a mean distance from the sun equal to 0·2365 of the earth's mean distance, with a maximum elongation in a nearly circular orbit of about $13\frac{1}{2}$ degrees, the period of revolution being almost precisely half that of Mercury.

We await details of the observations before examining how far the date 1876, April 4, can be made to agree with similar ones already upon record, supposing all to refer to a single body revolving under the conditions named.

NEW MINOR PLANET.—The *Bulletin International* of the Paris Observatory notifies the discovery of No. 167, by Prof. Peters, at Clinton, U.S., on August 28. R.A. $21^h. 57^m.$, N.P.D., 101° 30′, motion south, twelfth magnitude.

NOTES

WE notice, with extreme regret, the announcement of the death of Mr. George Smith, of the British Museum, the accomplished Assyriologist. A telegram received on Monday at the British Museum from Constantinople stated that Mr. Smith died at Aleppo, on the 19th ult., and that further particulars would be ultimately sent. Faint hopes are entertained that the sad announcement will be contradicted. The Turkish Government and officials had thrown so many difficulties in his way that Mr. Smith was on his road home in disgust. It will be remembered that he started in February last on his third archæological expedition to the East. The high value of Mr. Smith's work in a department of research of great importance has been universally acknowledged, and it will be difficult to over-estimate his loss to science and to the British Museum. He has earned an enduring place in the important domain of Eastern archæology.

MR. HOWARD GRUBB, of Dublin, has presented to the Scientific Committee appointed to superintend the work, his Report on the Progress of the Great Equatorial for the Vienna Observatory, the contract for which was concluded in June last year with the Austro-Hungarian Government. The work, we are glad to say, has gone on smoothly and successfully. To enable him to carry on his important undertaking Mr. Grubb has constructed a spacious dodecagon chamber, forty-two feet in internal diameter, the roof of which is so constructed as to allow the great steel dome to be erected over it. Mr. Grubb had contracted with Feil of Paris for the supply of the discs of glass for the great objective, and the flint disc is already in Dublin, where it is now undergoing a rigid examination. The crown disc M. Feil expects to have ready in a few weeks; meanwhile active preparations are being made for the grinding and polishing of the objective. Parts of the general framing have been cast; the polar pillar is completely finished; the polar axis has had most of its parts adjusted. The cross-head and declination axis are completely finished, and the declination circle and adapter nearly so. The clockwork and many of the other parts of the elaborate apparatus necessary for the working of the telescope are also complete, and Mr. Grubb is preparing a travelling gantry across the observatory, and proposes commencing shortly to put together the general ramework and erect the larger portions of the mounting. A communication from Prof. Newcomb has induced Mr. Grubb to take means to obviate the temporary spherical aberration in the objective produced by the difference of temperature outside and inside the tube. Altogether Mr. Grubb is to be congratulated on the progress he is making in his great undertaking. From the *Deutsche Zeitung* we learn that the new observatory itself is making rapid progress towards completion, and may be ready by the beginning of winter, though it will take two or three years to complete the internal arrangements. The telescope, a refractor with a 26-inch objective and 30 feet focal distance, is expected to be ready by the autumn of 1878.

ALGOLOGISTS will be glad to hear that Prof. Agardh of Lund, Sweden, has just published a new volume (vol. iii.) of his work entitled "Species, Genera, et Ordines Algarum." (Epicrisis Systematis Floridearum. Auctore, J. G. Agardh. Lipsiæ: apud T. O. Weigel, 1876.) In it he treats of the Florideæ only; the whole of which, with the exception of the orders *Corallineæ* and *Rhodomeleæ*, are included in it. The Florideæ, it will be remembered, formed the subject of the second volume of "Species Algarum." Since it was published immense numbers of Algæ, in excellent condition, have been submitted to scientific observation; many new species and genera have been added to the list of marine plants; old observations have been verified or corrected; unexpected affinities between plants supposed to be far apart in the system of classification; or discrepancies, equally unexpected, between plants supposed to be closely allied, have been perceived. Improved methods of study have led to the discovery of former errors of classification and description; and the necessity has long been felt by algologists of a work, the arrangement of which should be more in accordance with the present state of knowledge, and in which old errors should be corrected, and new forms described. Such a work Prof. Agardh has now given us, and we are sure it will meet with a welcome reception. The present classification is based on a thorough examination of the internal structure of the frond and of the fruit; and the Professor tells us that no species has been admitted into the text which he had not previously examined. Species, which in the former work had been accurately described, are merely referred to in the present, which must therefore be considered supplementary, and as in no wise superseding the former volume. The present work contains upwards of 700 pages 8vo.

AMONG the questions down for discussion at the Social Science Congress to be held in October, 11th to 18th, at Liverpool, are the following:—In the Education Section—What methods are best adapted to secure the efficient Training of Teachers of all grades, especially in the art of teaching? How can the due connection between Secondary (Grammar) Schools, Elementary Schools, and the Universities, by means of exhibitions, scholarships, or otherwise, be most effectually maintained? How can Professional and Technical Instruction be best incorporated with a sound system of general education? In the Health Section—What is the best mode of making provision for the Supply and Storage of Water—(*a*) in large towns such as Liverpool and Manchester; (*b*) in groups of urban communities of lesser size, such as exist in the manufacturing districts of Lancashire and Yorkshire? What amendments are required in the legislation necessary to prevent the evils arising from Noxious Vapours and Smoke?

AT Pesth, on Monday, the International Prehistoric Congress was opened in presence of the Archduke Joseph, by Herr Trefort, the Minister of Public Instruction, who welcomed the guests on behalf of the Hungarian Government. The President of the Congress, Herr Pulszky, then gave an address, in which he enlarged on the prehistoric periods of Hungary. The secretary also read an address treating on the development of prehistoric studies in Hungary, and commenting on the fine collection of prehistoric articles now exhibited. There are over one hundred foreign guests of all nations. Among them are Mr. Franks, of the British Museum; M. Broca, delegated by the French Government; Signor Pigorini, by the Italian Government; and Herr Virchow, of Berlin.

MR. WILLETT has published his fourth and final Report to the British Association on the Sub-Wealden Exploration. After giving a brief history of the enterprise, he states that he resigned the hon. secretaryship on May 1, when Major Beaumont, M.P., chairman of the Diamond Boring Company, offered to take his place and raise funds to continue the work, which had been carried to 1,894 feet. Mr. Willett then says:—"Four months have elapsed. No committee have been summoned. No fresh funds have been raised, and, in my opinion, it is quite time that the whole affair be wound up, and that the exploration be finally abandoned in this locality." His reasons for this conclusion we shall give when his Report comes up at the British Association meeting.

LETTERS received from Baron A. von Hügel announce his arrival in Fiji, where he has already made considerable collections of birds. A full account of his work in New Zealand, with details of his future plans, has unfortunately been lost in transmission to England, but it would appear that he still intends to visit some more of the Pacific Islands, and perhaps New Guinea, before commencing his work in Western Australia. The investigation of the natural history of the latter country was his principal object on leaving England.

THE Iron and Steel Institute commences its autumn meeting at Leeds on the 18th inst.

IT has been observed by M. Jeannel that certain sonorous vibrations cause rotatory movement in the radiometer. In half obscurity, three radiometers were placed on the interior tablet of a chamber organ. The bass notes, those of the three first octaves, produced rotation, the most bass acting most, but *fa* and *fa* sharp of the lower octave (especially with the bourdon stop) produced more rapid rotation than *ut*, *re*, and *mi*, though these are more grave. Radiometers do not all act in the same manner, as to rapidity and direction of their rotation. Thus, to the low *fa* or *fa* sharp radiometer A, the less sensitive to light, made about one turn per second. The black faces first (*i.e.* a direction opposite to that produced by light), whilst radiometers B and C, which were more sensitive to light, turned more slowly and in the direction of the movement produced by light. M. Jeannel explains these effects by circular or angular vibrations of the supporting needle transmitted from the tablet of the organ. By applying the finger to the top of the radiometer, one may prevent the vibration and also the rotation. The board of a piano produces similar effects, but in less degree. If the experiments indicated be made where the diffuse light is nearly sufficient to drive the radiometer, grave sounds, even the weakest, cause rotation in the ordinary direction (bright surfaces first); the rumble of a vehicle will suffice. Here the light is at first insufficient to overcome the friction, but when the vibrations intervene, friction is lessened during certain intervals, and the apparatus is thus rendered more sensitive to light.

M. FRON has given, in the *Bulletin International* of Aug. 12, a short note of the thunderstorms in France on June 9, 1875, on which day they occurred in forty-three departments. The barometric depression accompanying this remarkable development of thunderstorms amounted to 0·630 inch at Valentia, 0·472 inch in Brittany, and 0·276 inch at Paris. An illustration is given showing that the barometer fell to its lowest point at Paris at the time the thunderstorm broke over the city, and that at the same time in the centre of this depression the barometer suddenly rose and as suddenly fell through about 0·033 inch, the whole of this brief-continued oscillation occupying less than an hour. It would be a valuable piece of work if the French meteorologists could, from an examination of the changes in the direction and force of the wind, the aqueous precipitation, the electrical and other meteorological phenomena which occurred at the time, trace this singular barometric fluctuation to its physical causes.

THE number of visitors to the Loan Collection of Scientific Apparatus during the week ending Sept. 2 was as follows:—Monday, 3,200; Tuesday, 2,977; Wednesday, 468; Thursday, 355; Friday, 332; Saturday, 3,925; total, 11,257.

À propos of the meeting of the British Association, *Science Gossip* for September contains an interesting article on the Geology of Glasgow and the neighbourhood, by Mr. R. L. Jack, F.G.S., of the Geological Survey.

THE first number of *The Mineralogical Magazine and Journal of the Mineralogical Society of Great Britain and Ireland* has just been issued. It contains eight papers on subjects of mineralogical interest. Lake and Lake of Truro are the publishers.

THE General Meteorological Council of the Gironde have passed a resolution asking the French Government to establish the Meteorological Service on the basis adopted in the United States; other general councils will do the same, and the result will very likely be an increase in the sums voted for the meteorological service.

M. WADDINGTON has published a circular organising an improved system for obtaining school statistics in France. The number of pupils admitted into primary schools has been, up to the present time, determined merely by the names of children inscribed on the school register, though the attendance of many is merely nominal. The roll will be called henceforth twice a-day, morning and afternoon, so that the real state of things may be known, and no compliment paid to national pride.

THE Municipal Council of Perpignan voted, at its last sitting, a sum of 15,000 francs for the purpose of erecting a statue to François Arago, who was born in the department of Pyrenées Orientales, of which Perpignan is the chief town. His native place was Estagel, a small village, where a monument has already been erected to him.

THE City of Grenoble inaugurated, on August 14, a statue in honour of Vaulanson, a celebrated mechanician born there in the beginning of the eighteenth century.

THE programmes for admission to the newly-created French National School of Agriculture have been officially published. The examination will take place very shortly, and the first promotions will be announced in the beginning of next year. The ex-imperial Vincennes farm has been devoted to the new establishment, which, besides those who have passed examinations, will admit a number of pupils free. No charge will be made for education.

AFTER repeated efforts an agricultural experimental station in Connecticut was successfully established, under the charge of the trustees of the Wesleyan University. The preliminary report of less than half a year's labours has just been published, and shows the enterprise to have been a legitimate one in view of the amount and character of the work accomplished. The establishment is in charge of Prof. W. O. Atwater, an agricultural chemist of eminence, under whose direction a considerable number of analyses of fertilisers have been made. The result of the labours of this experimental station has already been to define with precision the percentage of nitrogen to the ton in the various fertilisers offered for sale, and the withdrawal from the market of several worthless articles.

M. DURUOF the aëronaut made an ascent at Cherbourg on the occasion of a recent launch. He ascended to 12,000 feet, and came down in the bay at twenty miles from Cherbourg. A number of steamers had been sent out to help if needed, and M. Duruof was taken on board one of them unhurt. The manœuvre was most cleverly executed with the help of an apparatus which M. Duruof had immersed in the sea to diminish the velocity of the balloon, and permit the boat to board the car.

A CORRESPONDENT, writing from Waterloo, near Liverpool, asks if there are any works published on the Æolian Drift or Wind Driftage, its cause and cure.

THE Institution of Civil Engineers has published its list of subjects for papers and prizes for session 1876-77. A copy may be obtained by applying at 25, Great George Street, Westminster, S.W.

THE third number of the *Bothkamper Beobachtungen*, recently published, contains exclusively M. Lohse's researches in the years 1872 and 1873. The volume is in three parts:—1. Researches on the physical nature of the sun's surface. 2. Photographic registration of the sun-spots. 3. Meteorological observations in the year 1873. The promised fourth volume will contain M. Vogel's researches during the same period.

WE have received the second part of the second volume of the "Proceedings of the Natural History Society of Glasgow," which contains numerous papers by Prof. John Young, Messrs. Harvie Brown, James Lumsden, Robert Gray, D. Robertson, P. Cameron, jun., and others. The most important of the communications are ornithological and entomological; some are peculiarly briefly noticed.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, Aug. 2.—Sir Sidney Smith Saunders, vice-presdent, in the chair.—Messrs. Harold Swale and T. S. Hillman were elected ordinary members.—Mr. Stevens exhibited *Tillus unifasciatus* and *Xylotrogus brunneus* taken on an oak-fence at Upper Norwood; and Mr. Champion exhibited *Harpulus 4-punctatus*, *Dendrophagus crenatus*, and other rare Coleoptera from Aviemore, Inverness-shire.—Mr. Forbes exhibited a specimen of *Quedius dilatatus* taken by him with sugar in the New Forest.—From a despatch from H. M. Chargé d'Affaires at Madrid, a copy of which was forwarded to the secretary through the Foreign Office, it appeared that the damage done this year by the locusts was considerably less than that of last year, owing to the number of soldiers which the Government had been able to employ since the war was over, in assisting the inhabitants of the districts where the plague existed, in destroying the insects. Specimens of the locust, as well as a number of earthen tubes containing the eggs, were forwarded to the society, and on examination they were found to be the *Locusta albifrons*, Fab. (*Decticus albifrons*, Savigny).—Mr. M'Lachlan exhibited a series of thirteen examples of a dragon-fly (*Diplax meridionalis*, Selys), recently taken by him in the Alpes Dauphinés, remarkable for the extent to which they were infested by the red parasite described by De Geer as *Acarus libellulæ*. They were firmly fixed on the nervures at the base of the wing, almost invariably on the underside, and being arranged nearly symmetrically, had a very pretty appearance, the wings looking as if they were spotted with blood-red. He considered that the *Acari* must have attained their position by climbing up the legs of the dragon-fly when at rest—Mr. F. Smith read a note on *Nematus gallicola*, Steph., the Gall-maker, so common on the leaves of species of *Salix*, but of which the male had, apparently, not previously been observed. From 500 or 600 galls collected by him in 1875, he had obtained a multitude of females, but only *two* males; and he thought that by perseverance in this way it would be possible to obtain the males of this and other allied species, of which the males were practically unknown, the female being capable of continuing the species without immediate male influence; and he argued from this that the long-sought males of *Cynips* might some day be found by collecting the galls early in the year. He expressed his belief that Mr. Walsh had proved, beyond question, the breeding of a male *Cynips* in America, although the precise generic rank of the supposed *Cynips* was disputed by some of the members present.—The president (Prof. Westwood), who was unable to be at the meeting, forwarded some notes of the habits of a Lepidopterous insect, parasitic on *Fulgora candelaria*, by J. C. Bowring, with a description of the species and drawings of the insect in its different stages, by himself. It appeared that the Coccus-like larvæ were found attached to the dorsal surface of the *Fulgora*, feeding upon the waxy secretion of the latter, and covering itself with a cottony substance. From its general appearance the Professor was disposed to place the insect among the *Arctiidæ*. It was discovered many years ago by Mr. Bowring, and he (Mr. Westwood) had noticed it at the meeting of the British Association at Oxford, in 1860, under the name of *Epipyrobs anomala*.—The Rev. R. P. Murray forwarded a paper by Mr. W. H. Miskin, of Brisbane, containing descriptions of new species of Australian Diurnal Lepidoptera in his own collection.—Mr. Edward Saunders communicated the third and concluding portion of his synopsis of British Hemiptera-Heteroptera.

VIENNA

Imperial Academy of Sciences, June 16.—The following, among other papers, were read:—On an earthquake in Canea, Crete, on April 25, by M. Micksche. The waves of impulse came from the north. The sea was quiet, and there was no sound. The last earthquake was in January last year, and was more violent.—Communications from the laboratory of Prague University, by M. Linnemann. These relate to reactions with propylene.—On a gas battery of convenient form, by M. Mach. It consists of sixteen jars connected in pairs, by their like coatings. By a simple commutator the pairs can be connected together to an ordinary jar battery, or successively to a Franklin jar battery, and this combination can be quickly changed. Long and powerful sparks are had (16 ctm. *e.g.*).—Body-measurements of various peoples, made during the Austro-Hungarian expedition to Eastern Asia, by Dr. Janka, and extended, by personal observations, by Dr. Weisbach. Dr. Weisbach distinguishes—I. Short heads; II. Medium heads; III. Long heads. Each of these divisions fall into *a*, prognathous, and *b*, orthognathous, and each of these sub-divisions into 1, Long-armed; 2, Equal-limbed; 3, Short-armed. In this system the short-headed prognathous human races, whose arms are longer than the legs, stand lowest, *i.e.*, nearest to the apes; and the long-headed orthognathous and short-armed, stand highest.—A contribution to knowledge of Mediterranean fauna, by M. Hörnes.—On a constant winding in the human brain, observed by M. Heschl.—On the development-history of the ganglia, and the *Lobus electricus*, by M. Schenk.

June 22.—On an earthquake in Canea, Crete, on May 23, by M. Micksche. The disturbance may have come from Cyprus or Syria, or may be an awakening of the old volcanic action of Crete itself.—On the occurrence of the foraminiferal species, *Nubecularia*, in the Sarmatian sand of Kischenew, in Bessarabia, by MM. Karrer and Sinzow.

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