THURSDAY, AUGUST 15, 1878

THE COMING ECLIPSE

WHEN I wrote two articles in NATURE a little while ago, discussing the various methods which I thought might with advantage be employed next Monday, I little thought that it would fall to my lot to come to America to take part in the observations. The fates, however, have so ruled it, and here I am, in what was not long ago called the "Great American Desert," but by no means a martyr to science; for, although Rawlins—where I now am—is nearly 7,000 feet high, and near the Rocky Mountain divide; although elk and antelope may be shot within a mile of the town; yet the sluggard is roused at six by the voluminous steam whistle of the railway works: there is a thriving "city" and population.

The energy displayed by the American astronomers is, if possible, greater than I anticipated. There is scarcely a man of note among them who is not now along the totality line which runs from the Yellowstone Park to the Gulf of Mexico. Where the wonderful Union Pacific Railway cuts the line east and west there will be four stations—Rawlins, Separation, Fulmore, and Creton. Along this line will be gathered Professors Newcomb, Harkness, Draper, Watson, with their many assistants. In the middle region, including Denver Central City, and Pike's Peak, will be Professors Young, Holden, Langley, Cleveland Abbe, and General Myer, the chief of the Weather Signal Service. The parties under these are many of them numerous, Prof. Young's camp, for instance, including thirteen persons. In the southern region, at Pueblo and Los Animas, Prof. Hall heads a large party, including Prof. Wright, of Yale, and Prof. Thorpe and Dr. Schuster from the old country.

In all three groups of stations the various kinds of work have been divided in a most judicious manner. In all attempts will be made to obtain the spectrum of the chromosphere and coronal atmosphere in the way suggested in my previous articles; in all the structure of the coronal atmosphere will be carefully inquired into. So far as photographs of the corona are concerned, perhaps the strongest attack will be made by an impromptu party not referred to in the preceding enumeration. On my way here from Cheyenne it was my great good fortune to travel with Prof. Hayden, *facile princeps* among the great geological surveyors of this vast continent. He was on his way to the north, and, as usual, had with him a strong photographic equipment. As his march lies along the line of totality, he will obtain, or at all events endeavour to obtain, a large series of photographs.

It is agreed on all hands that never has such summer weather been known in this locality. Ordinarily the chances, as determined by the officers of the signal service from their registers, are—Northern stations, 80 per cent.; Denver, 60; Pike's Peak, 40; and Los Animas, 80; but here, for the last fortnight, fine mornings have been succeeded by a break-up in the weather in the afternoon, while at Denver matters have been much worse.

A most valuable second series of instructions, written by Prof. William Harkness, of the United States Navy, by direction of Admiral Rodgers, has been published. Of these Sections I., II., V., and VIII. describe such observations as can be made with ordinary apparatus, while the other sections relate mostly to observations which can only be carried out by persons who are able to command expensive apparatus, and who are skilled in astronomy and physics.

This is a most useful following up of the work of organisation undertaken in England for the first time in 1870 and carried out in 1871 and 1875.

Prof. Harkness has freely availed himself of the Instructions compiled for the English Expeditions of those years, and in his carefully written memorandum has given us an opportunity of seeing how the problems have been advanced of late years; he has also collected a valuable series of data which give permanent value to it. I do not think I can do better than refer to some of the more important points touched on in the Instructions.

All the most rapid varieties of lenses in the market suited for use as equatorial cameras are given in the following table, in which the corresponding intensity ratios have been taken from Dallmeyer's catalogue.

Reference No.	Description of photographic objective.	Intensity ratio.[1]	Focal distance of largest lens made.	Diameter of image of sun.	Exposure required for the corona.
			Inches.	Inches.	s. s.
1	Extra quick acting portrait	$\frac{1}{2}$	$5\frac{1}{2}$	0·051	0·3 to 1·6
2	Quick acting portrait ...	$\frac{1}{3}$	$13\frac{1}{2}$	·126	0·7 to 3·6
3	Ordinary portrait	$\frac{1}{4}$	24	·224	1·3 to 6·4
4	Portrait and Group (D) ...	$\frac{1}{6}$	33	·308	2·9 to 14·4
5	Rapid rectilinear	$\frac{1}{8}$	$33\frac{1}{2}$	0·313	5·1 to 25·6

Prof. Harkness points out that "the data from which to determine an approximate value of *C* for the corona are very limited." He considers that it is probably safe to conclude that, with a clear sky and a moderately high sun, exposures in which the value of *C* is about 0·002 will give only the prominences and the outline of the moon. When *C* becomes 0·08 the corona will begin to appear and will increase in extent as the exposure increases, at least up to the point where *C* becomes 0·40. Accordingly, the shortest exposure specified in the table above corresponds to $C = 0·08$, and the longest to $C = 0·40$.

If we adopt a lens of thirty-three inches focus an attempt can be made to use the lens for another purpose, "even more important than photographing the corona," that is in the search for intra-Mercurial bodies. Prof. Harkness points out that the magnitude of its intensity ratio enables it to depict faint objects rapidly, and the extent of its angle of view is such as to embrace a field of more than forty degrees. The lens will cover a plate measuring twenty by twenty-two inches, but as it is desirable to keep the apparatus light, plates measuring seventeen by twenty inches, which will suffice to cover a space of thirty-three and a half degrees along the ecliptic, are recommended.

[1] If *F* is the equivalent focal distance of a photographic objective, *d* its working aperture, *C* the exposure constant, whose value depends upon the intensity of the light and the sensitiveness of the chemicals employed, and the time of exposure required to produce a good negative, then the intensity ratio is $\frac{d}{F}$, and $t = C\left(\frac{F}{d}\right)^2$.

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"Assuming the adoption of an equatorial camera twenty inches square, provided with a lens whose intensity ratio is one-sixth and whose focal distance is about thirty-three inches, it yet remains to consider how this apparatus should be managed during a totality lasting only three minutes. As the illumination of different parts of the corona varies greatly, there can evidently be no certainty of geting all the details of the phenomenon unless a series of plates are taken, in which the exposures vary from the shortest possible up to the point where it is certain that an increase of time does not improve the picture. On this account it will be desirable to take as many as six plates, the exposures being, respectively—

3^s, 5^s, 10^s, 20^s, 40^s, and 60^s.

The first four of these plates will receive such short exposures that it is unlikely they will show anything but the corona, and therefore their size should be $4\frac{1}{4}$ by $5\frac{1}{2}$ inches. With the last two plates the case is different. Their size should be 17 by 20 inches, because their longer exposures will probably suffice to bring out upon them any bright points which may exist within their field. A lens such as is here under consideration should depict an eighth-magnitude star in about one minute, but of course the intensity of the sky-illumination during totality will determine the limit of brightness at which faint luminous points will cease to impress themselves upon the negatives, and what this limit may be it is impossible to predict. The necessity for at least two large plates is evident when it is remembered that the image of a small bright point could not be distinguished from an accidental blemish in the film, and it would only be by finding it upon both plates that its true character could be unmistakably recognised. It is exceedingly desirable to determine accurately the maximum exposure that the corona will bear with advantage, and it is hoped that on at least one of the large plates it will prove to be over-exposed."

It has been proposed to photograph the red prominences on a scale of ten seconds of arc to a millimetre. The optical apparatus for the production of such pictures must have an equivalent focal distance of 2062·7 centimetres, or 812·1 inches, and if we take *C* equal to 0·002, which is probably very near the truth, the value of *t* for lenses of various apertures are given as follows in the instructions:—

Aperture of objective.	$\frac{F}{d}$	$\left(\frac{F}{d}\right)^2$	Exposure required.	Motion of moon.
Inches.			s.	"
6	135·3	18306	36·6	20·1
8	101·5	10302	20·6	11·3
10	81·2	6593	13·2	7·2
12	67·7	4583	9·2	5·0
15	54·1	2927	5·8	3·2
20	40·6	1648	3·3	1·8
26	31·2	973	1·9	1·0

As a prominence one minute high could scarcely be photographed with a six-inch objective, because twenty seconds of its height would be covered by the advancing moon before the exposure was over, Prof. Harkness thinks it does not seem possible to photograph prominences during eclipses on the scale here contemplated with an aperture much less than ten inches.

The section relating to telescopic observation is very full and complete; full instructions concerning the structure of the corona are given, and the remark is made that "Since the spectroscope furnishes an efficient means of studying the red prominences at any time, it will be very undesirable to waste a single one of the precious moments of totality in examining them."

To facilitate the work of such astronomers as may desire to search for intra-Mercurial planets with considerable telescopic power, a chart is given showing every star so large as the seventh magnitude in that portion of the heavens which will be occupied by the sun on the 29th of July next. The black circle in R.A. 8h. 36m. Dec. + 18° 39′ indicates the position of the sun. Mercury, Regulus, and Mars will be pretty close together, and probably quite conspicuous during totality, but they are so far to the eastward that only the last-named comes within the limits of the chart. Venus may also be seen, but she will be low in the western sky. While looking for planets, the possibility of discovering a small comet, or a meteor stream, should be borne in mind.

"The corona forms a luminous background upon which the moon's limb is sometimes seen projecting beyond the sun; and a little before totality it is even possible that the complete outline of the moon may become visible. Look for these phenomena, and note the time of their occurrence. It is difficult to assign any reason for the existence of rays, or brushes, of light at the cusps of the sun, but it is said they have been seen. If any such appearances present themselves, they should be carefully scrutinised to ascertain if they change either their position or intensity; and the interior of the telescope should be examined to make sure that they do not originate in reflections, either from the tube or from the lenses."

The instructions as to the use of the spectroscope and polariscope are so full that they deserve reprinting *in extenso*. I shall therefore say nothing about them here except to express my belief that no stone has been left unturned to secure results, if results be possible. Spectroscopically, I suppose Dr. Draper and Prof. Young have the strongest outfit, while, so far as I know, Prof. Harkness is the only one who is equipped for photographing the polarisation of the corona.

For the first time thermo-electric observation forms part of eclipse work. One of the many points of interest here, to me, has been the observatory in which Mr. Edison has been experimenting on his tasimeter. It is truly a very wonderful instrument, and from the observations made last night on the heat of Arcturus, it is quite possible that he may succeed in his expectations. For its extreme delicacy I can personally vouch. The instrument, however, is so young, that doubtless there are many pitfalls to be discovered. Mr. Edison, however, is no unwary experimenter.

So much, then, for the present. The day after tomorrow will find us all busier than ever, and if the weather prove fine I hope I shall have, as in 1870 and 1871, another distinct advance in solar physics to chronicle.

J. NORMAN LOCKYER

Rawlins, Wyoming Territory, July 27

OUR NATURAL HISTORY COLLECTIONS

THE Bill to enable the trustees of the British Museum to remove the natural history collections to South Kensington, on which we commented in our issue of August 1, has passed both Houses and virtually become law. The measure having been introduced at a late period of the session, and hurried through all its stages, evidently for the express purpose of eluding observation, it could hardly have been expected that the result would have been otherwise. The assent of the Treasury was of course secured before the Bill was introduced, and it is by no means surprising that, what with Cyprus and Turkey, and the enormous pressure of other more interesting business, it was never discovered by the Government that the Bill was exactly contrary to the recommendations of the Royal Commission on Science. So far, therefore, the trustees have it all their own way, and are now authorised to continue at South Kensington the system of government that has made the state of our national natural history collections at Bloomsbury so long a byeword amongst naturalists. There remains, however, still one more chance of introducing some salutary reforms into the present system. Following on the authority to remove the collections, which the trustees have now obtained, money will be required to carry out the transfer, and to obtain the requisite funds a fresh application to Parliament will be necessary.

This application will, no doubt, be made in the ensuing session, but before it is complied with we trust that some sort of terms will be obtained from the trustees. In the first place they should be required to delegate the control of the New Natural History Museum to a small committee, in which should be included the two or three naturalists who now happen to be members of the trust. It is obvious that most of the great dignitaries of state and eminent noblemen who form the trustees of the British Museum, neither know nor care anything about natural history. Until recently, indeed, there has not been a single person who could fairly be called a naturalist on the trust. Lately, however, two excellent naturalists (Sir John Lubbock and Lord Walsingham) have become ordinary members of the trust, and Sir Joseph Hooker, as president of the Royal Society, is *ex officio* one of the trustees. A committee consisting of these gentlemen and of a few others, who might be presumed to have some general acquaintance with the requirements of modern science, would, we need hardly say, completely command the confidence of naturalists, and would answer to the board of visitors, which the Royal Commission on Science in their Report suggested should be constituted to look after the director. In the second place the chief executive officer of the new museum should be appointed secretary to the trustees *ad hoc*, and the estimates for the two buildings should be kept entirely separate. So long as there remains any sort of subordination of the natural history to the principal librarian, who is the sole executive officer of the trustees at Bloomsbury, the old policy will be continued. The natural history will be starved in order to feed the overgrown library, and the petty restrictions and regulations which have so long vexed the souls of the visitors to the British Museum will be continued at South Kensington. The best chance of obtaining the necessary reforms lies in an entire change of administration, and for this reason it is much to be lamented that the recommendation of the Royal Commission on Science to place the New Museum of Natural History directly under the control of the Government has not been attended to. Still if the Trustees can be induced to commit the management of the new institution to a well-selected delegacy and to appoint a director free from the evil influence of Bloomsbury, there is every hope that our New Museum of Natural History may be worthy of the nation, and take rank with the sister institutions of Paris, Leyden, and Berlin.

THE BRITISH ASSOCIATION

DUBLIN, *Monday*

TO-DAY the Reception Room was opened in Trinity College, the Examination Hall being used for this purpose; and the doors were scarcely opened before a rush for tickets took place, and 500 associate tickets were sold in the course of two or three hours. As usual, the local honorary secretaries are working vigorously, and so far the arrangements they have made seem to be excellent; in fact, it is generally admitted that at few places has the British Association had so excellent a reception nor so convenient and complete accommodation. One feature in this meeting is the shilling lunch tickets, which every member can procure, and that entitles him to an excellent lunch, costing at least double the price of his ticket. The numerous foreign visitors have been lodged at the chief hotels, and private hospitality has been offered to the leading members of the Association who have announced their intention of being present.

With regard to a paragraph in a contemporary on the soreness which has been produced in Dublin by the cavalier treatment local scientific men have received from the Council of the Association, and stating that local papers would be withheld, we are able to contradict the latter part of this statement, although it cannot be denied that considerable umbrage has been given by the singular omission of some most distinguished local names from the first list of sectional officers; notably is this the case in Section G, Mechanical Science. But the list is stated to be incomplete, and doubtless the omissions will be repaired at the first meeting of the General Committee before these lines are published.

The guide-book which is issued to the Association has each year become a more and more important work, and that to Dublin is certainly the most complete and most carefully compiled of any that we have seen. Nine months ago a guide-book committee was formed, and this committee, after breaking up into different sections, has worked unremittingly to make their undertaking as accurate and complete as possible. No better editors could have been chosen than Professors MacAlister and McNab, who, besides the distinguished position they hold in their own departments of zoology and botany, possess an intimate acquaintance with the topography and biology of the neighbourhood, and who, since the beginning of the year, have unsparingly devoted their time to their arduous editorial labours—labours that have been largely augmented by the unexpected and unfortunate delays on the part of the firm of printers to whom the MSS. was first sent months ago. Thus it comes about that a somewhat awkward division is made into two unequal parts; but the division is one created by the printers and not by the nature of the contents. It is right to add that the thanks of the Association are due to Messrs. Dollard for the energy and skill which they have shown in the final printing of the guide-book. The book is illustrated with three maps; the first is a ten-mile to the inch map of the County Dublin; the second an admirable geological map of the county; and the third in a pocket is a six-inch map of Dublin and the neighbourhood. A concise and valuable topographical sketch of Dublin, by Dr. MacAlister, opens the series of papers, Dr. J. W. Moore having added to this paper an important set of Meteorological Tables, some hitherto unpublished. The Rev. Maxwell Close contributes an important paper on the Physical Geology of Dublin, Mr. W. H. Bailey a paper on the Palæontology of the Neighbourhood, Dr. Haughton on the Mineralogy of the County, J. T. Pim on the Textile Industries, Dr. Emerson Reynolds on the Places of Manufacturing and Industrial Interest in and around Dublin; then follows Historical Notes and Antiquities of Dublin, by Dr. MacAlister, after which an extended and valuable list of the Flora of the Counties of Dublin and Wicklow is contributed by Mr. W. Archer, F.R.S., Dr. Perceval Wright, the Rev. Eugene O'Meara, Dr. D. Moore, Mr. G. Pim, and Mr. A. G. More. The fauna of the county is next dealt with, being compiled by Mr. H. W. Mackintosh, Mr. W. F. Kirby, Prof. W. R. McNab, the Rev. B. W. Adams, Prof. MacAlister, Mr. A. G. More, and Mr. R. M. Barrington. This unique and invaluable guide-book ends with a notice of the more interesting of the corporation records compiled by the Rev. W. G. Carroll.

We have already noticed the extensive preparations made for the *soirée* to be given to the Association by the Royal Dublin Society on Thursday. At this *soirée*, among many other things, Mr. Johnstone Stoney, the honorary secretary of the Society, will show the remarkable absorption spectrum of chloro-chromic anhydride, Mr. Gordon his recent and beautiful experiments on electric induction, and Prof. Barrett the effect of inaudible vibrations on sensitive flames.

One of the special objects of interest to be seen in Dublin is the great telescope Mr. Howard Grubb is constructing for the Austrian Government. We may state that the mechanical part is almost complete; the object-glass, which is to be twenty-seven inches in clear aperture, is of course not ready, as the glass was only received from Paris in the spring of this year. Visitors, however, need not be disappointed, as there is plenty to see in the mechanical details of the instrument and the great forty-five-foot dome for the covering of the telescope, the largest dome ever constructed. We may add, in conclusion, that among other places of interest, a visit to the Royal College of Science on Stephen's Green, with its museum of Irish industries and its excellent equipment of physical and mechanical apparatus, should not be omitted.

Inaugural Address of William Spottiswoode, M.A., F.R.S., LL.D., D.C.L., President.

Introductory.

On looking back at the long array of distinguished men who both in this and in the sister countries have filled the chair of the British Association; on considering also the increased pains which have been bestowed upon, and the increased importance attaching to, the Presidential Address, it may well happen when, as on this occasion, your choice has fallen upon one outside the sphere of professional science, that your nominee should feel unusual diffidence in accepting the post. Two considerations have, however, in my own case outweighed all reasons for hesitation: first, the uniform kindness which I received at the hands of the Association throughout the eight years during which I had the honour of holding another office; and secondly, the conviction that the same goodwill which was accorded to your treasurer would be extended to your president.

These considerations have led me to arrange my observations under two heads, viz., I propose first to offer some remarks upon the purposes and prospects of the Association with which, through your suffrages, I have been so long and so agreeably connected; and secondly, to indulge in a few reflections, not indeed upon the details or technical progress, but upon the external aspects and tendencies of the science which on this occasion I have the honour to represent. The former of these subjects is, perhaps trite, but as an old man is allowed to become garrulous on his own hobby, so an old officer may be pardoned for lingering about a favourite theme. And although the latter may appear somewhat unpromising, I have decided to make it one of the topics of my discourse, from the consideration that the holder of this office will generally do better by giving utterance to what has already become part of his own thought, than by gathering matter outside of its habitual range for the special occasion. For, as it seems to me, the interest (if any), of an address consists, not so much in the multitude of things therein brought forward, as in the individuality of the mode in which they are treated.

British Association, past History.

The British Association has already entered its fifth decade. It has held its meetings, this the forty-eighth, in twenty-eight different towns. In six cities of note, viz., York, Bristol, Newcastle-on-Tyne, Plymouth, Manchester, and Belfast, its curve of progress may be said to have a node, or point through which it has twice passed; in the five Universities of Oxford, Cambridge, Dublin, Edinburgh, and Glasgow, and in the two great commercial centres, Liverpool and Birmingham, it may similarly be said to have a triple point, or one through which it has three times passed. Of our forty-six presidents, more than half (twenty-six, in fact), have passed away; while the remainder hold important posts in science, and in the public service, or in other avocations not less honourable in themselves, nor less useful to the commonwealth. And whether it be due to the salubrity of the climate, or to the calm and dispassionate spirit in which science is pursued by its votaries here, I do not pretend to say; but it is a fact that the earliest of our ex-presidents still living, himself one of the original members of the Association, is a native of and resident in this country.

At both of our former meetings held in Dublin, in 1835 and 1857 respectively, while greatly indebted to the liberal hospitality of the citizens at large, we were, as we now are, under especial obligations to the authorities of Trinity College for placing at our disposal buildings, not only unusually spacious and convenient in themselves, but full of reminiscences calculated to awake the scientific sympathies of all who may be gathered in them. At both of those former Dublin meetings the venerable name of Lloyd figured at our head; and if long-established custom had not seemed to preclude it, I could on many accounts have wished that we had met for a third time under the same name. And although other distinguished men, such as Dr. Robinson, Professors Stokes, Tyndall, and Andrews, are similarly disqualified by having already passed the presidential chair, while others again, such as Sir W. R. Hamilton, Dr. M'Cullagh, and Prof. Jukes, are permanently lost to our ranks; still we should not have had far to seek, had we looked for a president in this fertile island itself. But as every one connected with the place of meeting partakes of the character of host towards ourselves as guests, it has been thought by our oldest and most experienced members that we should better respond to an invitation by bringing with us a president to speak as our representative than by seeking one on the spot; and we may always hope on subsequent occasions that some of our present hosts may respond to a similar call.

But leaving our past history, which will form a theme more appropriate to our jubilee meeting in 1881 at the ancient city of York, I will ask your attention to a few particulars of our actual operations.

Its Relation to other Societies.

Time was when the Royal Societies of London and Edinburgh and the Royal Irish Academy were the only representative bodies of British science and the only receptacles of memoirs relating thereto. But latterly, the division of labour, so general in industrial life, has operated in giving rise to special societies, such as the Astronomical, the Linnæan, the Chemical, the Geological, the Geographical, the Statistical, the Mathematical, the Physical, and many others. To both the earlier or more general, and the later or more special societies alike, the British Association shows resemblance and affinity. We are general in our comprehensiveness; we are special in our sectional arrangement; and in this respect we offer not only a counterpart, but to some extent a counterpoise, to the general tendency to subdivision in science. Further still, while maintaining in their integrity all the elements of a strictly scientific body, we also include, in our character of a microcosm, and under our more social aspect, a certain freedom of treatment, and interaction of our various branches, which is scarcely possible among separate and independent societies.

The general business of our meetings consists first, in receiving and discussing communications upon scientific subjects at the various sections into which our body is divided, with discussions thereon; secondly, in distributing, under the advice of our Committee of Recommendations, the funds arising from the subscriptions of members and associates; and thirdly, in electing a council upon whom devolves the conduct of our affairs until the next meeting.

The communications to the sections are of two kinds, viz., papers from individuals, and reports from committees.

Papers for the Sections, &c.

As to the subject matter of the papers, nothing which falls within the range of natural knowledge as partitioned among our sections, can be considered foreign to the purposes of the Association; and even many applications of science, when viewed in reference to their scientific basis, may properly find a place in our proceedings. So numerous, however, are the topics herein comprised, so easy the transition beyond these limits, that it has been thought necessary to confine ourselves within this range, lest the introduction of other matters, however interesting to individual members, should lead to the sacrifice of more important subjects. As to the form of the communications, while it is quite true that every scientific conclusion should be based upon substantial evidence, every theory complete before being submitted for final adoption, it is not the less desirable that even tentative conclusions and hypothetical principles, when supported by sufficient *primâ facie* evidence, and enunciated in such a manner as to be clearly apprehended, should find room for discussion at our sectional meetings. Considering, however, our limitations of time and the varied nature of our audience, it would seem not inappropriate to suspend, mentally if not materially, over the doors of our section rooms, the Frenchman's dictum, that no scientific theory "can be considered complete until it is so clear that it can be explained to the first man you meet in the street."

Special Reports.

Among the communications to the sections undoubtedly the most important, as a rule, are the Reports; that is to say, documents issuing from specially appointed committees, some of which have been recipients of the grants mentioned above. These reports are in the main of two kinds, first, accounts of observations carried on for a series of years, and intended as records of information on the special subjects; such, for instance, have been those made by the Kew Committee, by the Committees on Luminous Meteors, on British Rainfall, on the Speed of Steamships, on Underground Temperature, on the Exploration of Certain Geological Caverns, &c. These investigations, frequently originating in the energy and special qualifications of an individual, but conducted under the control of a committee, have in many cases been continued from year to year, until either the object has been fully attained or the matter has passed into the hands of other bodies, which have thus been led to recognise an inquiry into these subjects as part and parcel of their appropriate functions. The second class is one which is perhaps even more peculiar to the Association; viz., the reports on the progress and present state of some main topics of science. Among these may be instanced the early Reports on Astronomy, on Optics, on the Progress of Analysis; and later, those on Electrical Resistance and on Tides; that of Prof. G. G. Stokes on Double Refraction; that of Prof. H. J. Smith on the Theory of Numbers; that of Mr. Russell on Hyperelliptic Transcendents, and others. On this head Prof. Carey Foster, in his address to the Mathematical and Physical Section at our meeting last year, made some excellent recommendations, to which, however, I need not at present more particularly refer, as the result of them will be duly laid before the section in the form of the report from a committee to whom they were referred. It will be sufficient here to add that the wide extension of the sciences in almost every branch, and the consequent specialisation of the studies of each individual, have rendered the need for such reports more than ever pressing, and if the course of true science should still run smooth it is probable that the need will increase rather than diminish.

If time and space had permitted, I should have further particularised the committees, occasionally appointed, on subjects connected with education. But I must leave this theme for some future president, and content myself with pointing out that the British Association alone among scientific societies concerns itself directly with these questions, and is open to appeals for counsel and support from the great teaching body of the country.

Grants.

One of the principal methods by which this Association materially promotes the advancement of science, and consequently one of its most important functions, consists in grants of money from its own income in aid of special scientific researches. The total amount so laid out during the forty-seven years of our existence has been no less than 44,000*l.*; and the average during the last ten years has been 1,450*l.* per annum. These sums have not only been in the main wisely voted and usefully expended, but they have been themselves productive of much additional voluntary expenditure of both time and money on the part of those to whom the grants have been entrusted. The results have come back to the Association in the form of papers and reports, many of which have been printed in our volumes. By this appropriation of a large portion of its funds the Association has to some extent anticipated, nay, even it may have partly inspired the ideas now so much discussed, of the endowment of research. And whether the aspirations of those who advocate such endowment be ever fully realised or not, there can, I think, be no doubt whatever that the Association in the mattter of these grants has afforded a most powerful stimulus to original research and discovery.

Regarded from another point of view these grants, together with others to be hereafter mentioned, present a strong similarity to that useful institution the Professoriate Extraordinary of Germany, to which there are no foundations exactly corresponding in this country. For beside their more direct educational purpose, these professorships are intended, like our own grants, to afford to special individuals an opportunity of following out the special work for which they have previously proved themselves competent. And in this respect the British Association may be regarded as supplying, to the extent of its means, an elasticity which is wanting in our own universities.

Other Funds.

Besides the funds which through your support are at the disposal of the British Association there are, as is well known to many here present, other funds of more or less similar character at the disposal, or subject to the recommendations, of the Royal Society. There is the Donation Fund, the property of the Society; the Government Grant of 1,000*l.* per annum, administered by the Society; and the Government Fund of 4,000*l.* per annum (an experiment for five years) to be distributed by the Science and Art Department, both for research itself and for the support of those engaged thereon, at the recommendation of a committee consisting mainly of Fellows of the Royal Society. To these might be added other funds in the hands of different scientific societies.

But although it must be admitted that the purposes of these various funds are not to be distinguished by any very simple line of demarcation, and that they may therefore occasionally appear to overlap one another, it may still, I think, be fairly maintained that this fact does not furnish any sufficient reason against their co-existence. There are many topics of research too minute in their range, too tentative in their present condition, to come fairly within the scope of the funds administered by the Royal Society. There are others, ample enough in their extent, and long enough in their necessary duration, to claim for their support a national grant, but which need to be actually set on foot or tried before they can fairly expect the recognition either of the public or of the government. To these categories others might be added; but the above-mentioned instances will perhaps suffice to show that even if larger and more permanent funds were devoted to the promotion of research than is the case at present, there would still be a field of activity open to the British Association as well as to other scientific bodies which may have funds at their disposal.

On the general question it is not difficult to offer strong arguments in favour of permanent national scientific institutions; nor is it difficult to picture to the mind an ideal future when Science and Art shall walk hand in hand together, led by a willing minister into the green pastures of the Endowment of Research.[1] But while allowing this to be no impossible a

[1] It is worth while to compare the following passage from Plato's "Republic," Book vii. (Jowett's translation):—
After plane geometry, we took solids in revolution instead of taking solids

future, we must still admit that there are other and less promising possibilities, which under existing circumstances cannot be altogether left out of our calculations. I am therefore, on the whole, inclined to think that, while not losing sight of larger schemes, the wisest policy, for the present at all events, and pending the experiment of the Government Fund, will be to confine our efforts to a careful selection of definite persons to carry out definite pieces of work; leaving to them the honour (or the onus if they so think it) of justifying from time to time a continuation of the confidence which the government or other supporting body may have once placed in them.

Continuance of British Association.

Passing from the proceedings to other features and functions of our body, it should be remembered that the continued existence of the Association must depend largely upon the support which it receives from its members and associates. Stinted in the funds so arising, its scientific effectiveness would be materially impaired; and deprived of them, its existence would be precarious. The amount at our disposal in each year will naturally vary with the population, with the accessibility, and with other circumstances of the place of meeting; there will be financially, as well as scientifically, good years and bad years. But we have in our invested capital a sum sufficient to tide over all probable fluctuations, and even to carry us efficiently through several years of financial famine, if ever such should occur. This seems to me sufficient; and we have therefore, I think, no need to increase our reserve, beyond perhaps the moderate addition which a prudent treasurer will always try to secure, against expenditure which often increases and rarely diminishes.

But however important this material support may be to our existence and well being, it is by no means all that is required. There is another factor which enters into the product, namely, the personal scientific support of our best men. It is, I think, not too much to say, that without their presence our meetings would fail in their chief and most important element, and had best be discontinued altogether. We make, it must be admitted, a demand of sensible magnitude in calling upon men who have been actively engaged during a great portion of the year, at a season when they may fairly look for relaxation, to attend a busy meeting, and to contribute to its proceedings; but unless a fair quota at least of our veterans, and a good muster of our younger men, put in their appearance, our gatherings will be to little purpose. There was a period within my own recollection when it was uncertain whether the then younger members of our scientific growth would cast in their lot with us or not, and when the fate of the Association depended very much upon their decision. They decided in our favour; they have since become presidents, lecturers, and other functionaries of our body; with what result it is for you to judge.

Of the advantages which may possibly accrue to the locality in which our meetings are held, it is not for us to speak; but it is always a ground for sincere satisfaction to learn that our presence has been of any use in stimulating an interest, or in promoting local efforts, in the direction of science.

The Council.

The functions of the British Association do not, however, terminate with the meeting itself. Beside the special committees already mentioned, there remains a very important body, elected by the General Committee, viz., the Council, which assembles at the office in London from time to time as occasion requires. To this body belongs the duty of proposing a president, of preparing for the approval of the General Committee the list of vice-presidents and sectional officers, the selection of evening lecturers, and other arrangements for the coming meeting.

At the present time another class of questions occupies a good deal of the attention of the Council. In the first generation of the Association, and during the period of unwritten, but not yet traditional, law, questions relating to our own organisation or procedure either "settled themselves," or were wisely left to the discretionary powers of those who had taken part in our proceedings during the early years of our existence. These and other kindred subjects now require more careful formularisation and more deliberate sanction. And it is on the shoulders of the Council that the weight of these matters in general falls. These facts deserve especial mention on the present occasion, because one part of our business at the close of this meeting will be to bid farewell officially to one who has served us as assistant secretary so long and so assiduously that he has latterly become our main repertory of information, and our Mentor upon questions of precedent and procedure. The post hitherto held by Mr. Griffith (for it is to him that I allude) will doubtless be well filled by the able and energetic member who has been nominated in his place; but I doubt not that even he will be glad for some time to come to draw largely upon the knowledge and experience of his predecessor.

But, beside matters of internal arrangement and organisation, the duties of the Council comprise a variety of scientific subjects referred to them by the General Committee, at the instance of the Committee of Recommendations, for deliberation and occasionally for action. With the increasing activity of our body in general, and more particularly with that of our various officers, these duties have of late years become more varied and onerous than formerly; nor is it to be wished that they should diminish in either variety or extent.

Once more, questions beyond our own constitution, and even beyond the scope of our own immediate action, such as education, legislation affecting either [the promotion or the applications of science to industrial and social life, which have suggested themselves at our meetings, and received the preliminary sanction of our Committee of Recommendations, are frequently referred to our Council. These, and others which it is unnecessary to particularise, whether discussed in full council or in committees specially appointed by that body, render the duties of our councillors as onerous as they are important.

Its Relations with Government.

While the government has at all times, but in a more marked manner of late years, recognised the Royal Society of London, with representatives from the sister societies of Dublin and of Edinburgh, as the body to which it should look for counsel and advice upon scientific questions, it has still never shown itself indisposed to receive and entertain any well-considered recommendation from the British Association. Two special causes have in all probability contributed largely to this result. First, the variety of elements comprised by the Association, on account of which its recommendations imply a more general concurrence of scientific opinion than those of any other scientific body. Secondly, the peculiar fact that our period of maximum activity coincides with that of minimum activity of other scientific bodies is often of the highest importance. At the very time when the other bodies are least able, we are most able, to give deliberate consideration, and formal sanction, to recommendations whether in the form of applications to government or otherwise which may arise. In many of these time is an element so essential, that it is not too much to say that without the intervention of the British Association many opportunities for the advancement of science, especially at the seasons in question, might have been lost. The government has, moreover, formally recognised our scientific existence by appointing our president for the time being a member of the Government Fund Committee; and the public has added its testimony to our importance and utility by imposing upon our president and officers a variety of duties, among which are conspicuous those which arise out of its very liberal exercise of civic and other hospitality.

Presidential Addresses

Of the nature and functions of the presidential address this is perhaps neither the time nor the place to speak; but if I might for a moment forget the purpose for which we are now assembled, I would take the opportunity of reminding those who have not attended many of our former meetings, that our annual volumes contain a long series of addresses on the progress of science, from a number of our most eminent men, to which there is perhaps no parallel elsewhere. These addresses are perhaps as remarkable for their variety in mode of treatment as

in themselves; whereas after the second dimension the third, which is concerned with cubes and dimensions of depth, ought to have been followed.

It is true, Socrates; but these subjects seem to be as yet hardly explored.

Why, yes, I said, and for two reasons; in the first place, no government patronises them, which leads to a want of energy in the study of them, and they are difficult; in the second place, students cannot learn them unless they have a teacher. But then a teacher is hardly to be found, and even if one could be found, as matters now stand the students of these subjects, who are very conceited, would not mind him; that, however, would be otherwise if the whole state patronised and honoured them, then they would listen, and there would be continuous and earnest search, and discoveries would be made; since even now, disregarded as they are by the world, and maimed of their fair proportions, and although none of their votaries can tell the use of them, still these studies force their way by their natural charm, and very likely they may emerge into light.

for the value of their subject-matter. Some of our presidents, and especially those who officiated in the earlier days of our existence, have passed in review the various branches of science, and have noted the progress made in each during the current year. But, as the various sciences have demanded more and more special treatment on the part of those who seriously pursue them, so have the cases of individuals who can of their own knowledge give anything approaching to a general review become more and more rare. To this may be added the fact that although no year is so barren as to fail in affording sufficient crop for a strictly scientific budget, or for a detailed report of progress in research, yet one year is more fertile than another in growths of sufficient prominence to arrest the attention of the general public, and to supply topics suitable for the address. On these accounts, apparently, such a presidential survey has ceased to be annual, and has dropped into an intermittence of longer period. Some presidents have made a scientific principle, such as the time-element in natural phenomena, or continuity, or natural selection, the theme of their discourse, and have gathered illustrations from various branches of knowledge. Others again, taking their own special subject as a fundamental note, and thence modulating into other kindred keys, have borne testimony to the fact that no subject is so special as to be devoid of bearing or of influence on many others. Some have described the successive stages of even a single but important investigation; and while tracing the growth of that particular item, and of the ideas involved in it, have incidentally shown to the outer world what manner of business a serious investigation is. But there is happily no pattern or precedent which the president is bound to follow; both in range of subject-matter and in mode of treatment each has exercised his undoubted right of taking an independent line. And it can hardly be doubted that a judicious exercise of this freedom has contributed more than anything else to sustain the interest of a series of annual discourses extending now over nearly half a century.

The nature of the subjects which may fairly come within the scope of such a discourse has of late been much discussed; and the question is one upon which every one is of course entitled to form his own judgment; but lest there should be any misapprehension as to how far it concerns us in our corporate capacity, it will be well to remind my hearers that as, on the one hand, there is no discussion on the presidential address, and the members as a body express no opinion upon it, so, on the other, the Association cannot fairly be considered as in any way committed to its tenour or conclusions. Whether this immunity from comment and reply be really on the whole so advantageous to the president as might be supposed need not here be discussed, but suffice it to say that the case of an audience assembled to listen without discussion finds a parallel elsewhere, and in the parallel case it is not always considered that the result is altogether either advantageous to the speaker or conducive to excellence in the discourse.

Their Range of Subjects.

But, apart from this, the question of a limitation of range in the subject-matter for the presidential address is not quite so simple as may at first sight appear. It must, in fact, be borne in mind that, while on the one hand knowledge is distinct from opinion, from feeling, and from all other modes of subjective impression, still the limits of knowledge are at all times expanding, and the boundaries of the known and the unknown are never rigid or permanently fixed. That which in time past or present has belonged to one category, may in time future belong to the other. Our ignorance consists partly in ignorance of actual facts, and partly also in ignorance of the possible range of ascertainable fact. If we could lay down beforehand precise limits of possible knowledge, the problem of physical science would be already half solved. But the question to which the scientific explorer has often to address himself is, not merely whether he is able to solve this or that problem, but whether he can so far unravel the tangled threads of the matter with which he has had to deal as to weave them into a definite problem at all. He is not like a candidate at an examination with a precise set of questions placed before him; he must first himself act the part of the examiner and select questions from the repertory of nature, and upon them found others, which in some sense are capable of definite solution. If his eye seem dim, he must look steadfastly and with hope into the misty vision, until the very clouds wreath themselves into definite forms. If his ear seem dull, he must listen patiently and with sympathetic trust to the intricate whisperings of nature—the goddess, as she has been called, of a hundred voices—until here and there he can pick out a few simple notes to which his own powers can resound. If, then, at a moment when he finds himself placed on a pinnacle from which he is called upon to take a perspective survey of the range of science, and to tell us what he can see from his vantage ground; if, at such a moment, after straining his gaze to the very verge of the horizon, and after describing the most distant of well-defined objects, he should give utterance also to some of the subjective impressions which he is conscious of receiving from regions beyond; if he should depict possibilities which seem opening to his view; if he should explain why he thinks this a mere blind alley and that an open path; then the fault and the loss would be alike ours if we refused to listen calmly, and temperately to form our own judgment on what we hear; then assuredly it is we who would be committing the error of confounding matters of fact and matters of opinion, if we failed to discriminate between the various elements contained in such a discourse, and assumed that they had all been put on the same footing.

Presidential Difficulties.

But to whatever decision we may each come on these controverted points, one thing appears clear from a retrospect of past experience; viz., that first or last, either at the outset in his choice of subject or in the conclusions ultimately drawn therefrom, the president, according to his own account at least, finds himself on every occasion in a position of "exceptional, or more than usual difficulty." And your present representative, like his predecessors, feels himself this moment in a similar predicament. The reason which he now offers is that the branch of science which he represents is one whose lines of advance, viewed from a mathematician's own point of view, offer so few points of contact with the ordinary experiences of life or modes of thought, that any account of its actual progress which he might have attempted must have failed in the first requisite of an address, namely, that of being intelligible.

View of Mathematics here taken.

Now if this esoteric view had been the only aspect of the subject which he could present to his hearers, he might well have given up the attempt in despair. But although in its technical character mathematical science suffers the inconveniences, while it enjoys the dignity, of its Olympian position; still in a less formal garb, or in disguise, if you are pleased so to call it, it is found present at many an unexpected turn; and although some of us may never have learnt its special language, not a few have, all through our scientific life, and even in almost every accurate utterance, like Molière's well-known character, been talking mathematics without knowing it. It is, moreover, a fact not to be overlooked that the appearance of isolation, so conspicuous in mathematics, appertains in a greater or less degree to all other sciences, and perhaps also to all pursuits in life. In its highest flight each soars to a distance from its fellows. Each is pursued alone for its own sake, and without reference to its connection with, or its application to, any other subject. The pioneer and the advanced guard are of necessity separated from the main body, and in this respect mathematics does not materially differ from its neighbours. And therefore as the solitariness of mathematics has been a frequent theme of discourse, it may be not altogether unprofitable to dwell for a short time upon the other side of the question, and to inquire whether there be not points of contact in method or in subject-matter between mathematics and the outer world which have been frequently overlooked; whether its lines do not in some cases run parallel to those of other occupations and purposes of life; and lastly, whether we may not hope for some change in the attitude too often assumed towards it by the representatives of other branches of knowledge and of mental activity.

In his Preface to the "Principia," Newton gives expression to some general ideas which may well serve as the key-note for all future utterances on the relation of mathematics to natural, including also therein what are commonly called artificial, phenomena.

Newton's Preface.

"The ancients divided mechanics into two parts, rational and practical; and since artisans often work inaccurately, it came to pass that mechanics and geometry were distinguished in this way—that everything accurate was referred to geometry, and everything inaccurate to mechanics. But the inaccuracies appertain to the artisan and not to the art, and geometry itself has its foundation in mechanical practice, and is in fact nothing else than that part of universal mechanics which accurately lays down and demostrates the art of measuring."[1] He next explains that rational mechanics is the science of motion resulting from forces, and adds, "The whole difficulty of philosophy seems to me to lie in investigating the forces of nature from the phenomena of motion; and in demonstrating that from these forces other phenomena will ensue." Then, after stating the problems of which he has treated in the work itself, he says: "I would that all other natural phenomena might similarly be deduced from mechanical principles. For many things move me to suspect that everything depends upon certain forces in virtue of which the particles of bodies, through forces not yet understood, are either impelled together so as to cohere in regular figures, or are repelled and recede from one another."

Burrowes' Remarks.

Newton's views, then, are clear; he regards mathematics not as a method independent of, though applicable to, various subjects, but as itself the higher side or aspect of the subjects themselves; and it would be little more than a translation of his notions into other language, little more than a paraphrase of his own words, if we were to describe the mathematical as one aspect of the material world itself, apart from which all other aspects are but incomplete sketches, and however accurate after their own kind, are still liable to the imperfections of the inaccurate artificer. Mr. Burrowes, in his Preface to the first volume of the *Transactions* of the Royal Irish Academy, has carried out the same argument, approaching it from the other side; "No one science," he says, "is so little connected with the rest as not to afford many principles whose use may extend considerably beyond the science to which they primarily belong, and no proposition is so purely theoretical as to be incapable of being applied to practical purposes. There is no apparent connection between duration and the cycloidal arch, the properties of which have furnished us with the best method of measuring time; and he who has made himself master of the nature and affections of the logarithmic curve has advanced considerably towards ascertaining the proportionable density of the air at various distances from the earth. The researches of the mathematician are the only sure ground on which we can reason from experiments; and how far experimental science may assist commercial interests is evinced by the success of manufactures in countries where the hand of the artificer has taken its direction from the philosopher. Every manufacture is in reality but a chemical process, and the machinery requisite for carrying it on but the right application of certain propositions in rational mechanics." So far your academician. Every subject, therefore, whether in its usual acceptation scientific or otherwise, may have a mathematical aspect; as soon, in fact, as it becomes a matter of strict measurement, or of numerical statement, so soon does it enter upon a mathematical phase. This phase may, or it may not, be a prelude to another in which the laws of the subject are expressed in algebraical formulæ or represented by geometrical figures. But the real gist of the business does not always lie in the mode of expression; and the fascination of the formulæ or other mathematical paraphernalia may after all be little more than that of a theatrical transformation scene. The process of reducing to formulæ is really one of abstraction, the results of which are not always wholly on the side of gain; in fact, through the process itself the subject may lose in one respect even more than it gains in another. But long before such abstraction is completely attained, and even in cases where it is never attained at all, a subject may to all intents and purposes become mathematical. It is not so much elaborate calculations or abstruse processes which characterise this phase, as the principles of precision, of exactness, and of proportion. But these are principles with which no true knowledge can entirely dispense. If it be the general scientific spirit which at the outset moves upon the face of the waters, and out of the unknown depth brings forth light and living forms; it is no less the mathematical spirit which breathes the breath of life into what would otherwise have ever remained mere dry bones of fact, which re-unites the scattered limbs and re-creates from them a new and organic whole.

And as a matter of fact, in the words used by Prof. Jellett at our meeting at Belfast, viz., "Not only are we applying our methods to many sciences already recognised as belonging to the legitimate province of mathematics, but we are learning to apply the same instrument to sciences hitherto wholly or partially independent of its authority. Physical science is learning more and more every day to see in the phenomena of nature modifications of that one phenomenon (namely, motion) which is peculiarly under the power of mathematics." Echoes are these, far off and faint perhaps, but still true echoes, in answer to Newton's wish that all these phenomena may some day "be deduced from mechanical principles."

Mathematics, Literature, and Art.

If, turning from this aspect of the subject, it were my purpose to enumerate how the same tendency has evinced itself in the arts, unconsciously it may be to the artists themselves, I might call as witnesses each one in turn with full reliance on the testimony which they would bear. And, having more special reference to mathematics, I might confidently point to the accuracy of measurement, to the truth of curve, which, according to modern investigation, is the key to the perfection of classic art. I might triumphantly cite not only the architects of all ages, whose art so manifestly rests upon mathematical principles, but I might cite also the literary as well as the artistic remains of the great artists of the Cinquecento, both painters and sculptors, in evidence of the geometry and the mechanics which, having been laid at the foundation, appear to have found their way upwards through the superstructure of their works.[1] And in a less ambitious sphere, but nearer to ourselves in both time and place, I might point with satisfaction to the great school of English constructors of the eighteenth century in the domestic arts; and remind you that not only the engineer and the architect, but even the cabinet-makers, devoted half the space of their books to perspective and to the principles whereby solid figures may be delineated on paper, or what is now termed descriptive geometry.[2]

Nor perhaps would the sciences which concern themselves with reasoning and speech, nor the kindred art of music, nor even literature itself, if thoroughly probed, offer fewer points of dependence upon the science of which I am speaking. What, in fact, is logic but that part of universal reasoning; grammar but that part of universal speech: harmony and counterpoint but that part of universal music, "which accurately lays down," and demonstrates (so far as demonstration is possible) precise methods appertaining to each of these arts? And I might even appeal to the common consent which speaks of the mathematical as the pattern form of reasoning and model of a precise style.

Taking, then, precision and exactness as the characteristics which distinguish the mathematical phase of a subject, we are naturally led to expect that the approach to such a phase will be indicated by increasing application of the principle of measurement, and by the importance which is attached to numerical results. And this very necessary condition for progress may, I think, be fairly described as one of the main features of scientific advance in the present day.

Measurements in Physics.

If it were my purpose, by descending into the arena of special sciences, to show how the most various investigations alike tend to issue in measurement, and to that extent to assume a mathematical phase, I should be embarrassed by the abundance of instances which might be adduced. I will therefore confine myself to a passing notice of a very few, selecting those which exemplify not only the general tendency, but also the special character of the measurements now particularly required, viz., that of minuteness, and the indirect method by which alone we can at present hope to approach them. An object having a diameter of an 80,000th of an inch is perhaps the smallest of which the microscope could give any well-defined representation; and it is improbable that one of 120,000th of an inch could be singly discerned with the highest powers at our command.[3] But the solar beams and the electric light reveal to us the presence of bodies far smaller than these. And, in the absence of any means of observing them singly, Prof. Tyndall has suggested a scale of these minute objects in terms of the lengths of luminiferous waves. To this he was led, not by any attempt at individual measurement, but by taking account of them in the

[1] Compare with this the latter part of Plato's "Philebus" on knowledge and the handicraft arts; also Prof. Jowett's Introduction thereto.

[1] See "Trattato della Pittura," by Leonardo da Vinci; also the Memoir on the MSS. of L. d. V., by Venturi, 1797.

[2] "The Gentleman and Cabinet Maker's Director," by Thomas Chippendale. London, 1754. "The Cabinet Maker and Upholsterer's Drawing Book," by Thomas Sheraton. London, 1793.

[3] See Sorby's Address to the Microscopical Society, 1876.

aggregate, and observing the tints which they scatter laterally when clustered in the form of actinic clouds.[1] The small bodies with which experimental science has recently come into contact are not confined to gaseous molecules, but comprise also complete organisms; and the same philosopher has made a profound study of the momentous influence exerted by these minute organisms in the economy of life.[2] And if, in view of their specific effects, whether deleterious or other, on human life, any qualitative classification, or quantitative estimate be ever possible, it seems that it must be effected by some such method as that indicated above.

Again, to enumerate a few more instances of the measurement of minute quantities, there are the average distances of molecules from one another in various gases and at various pressures; the length of their free path, or range open for their motion without coming into collision; there are movements causing the pressures and differences of pressure under which Mr. Crookes' radiometers execute their wonderful revolutions.[3] There are the excursions of the air while transmitting notes of high pitch, which through the researches of Lord Rayleigh appear to be of a diminutiveness altogether unexpected.[4] There are the molecular actions brought into play in the remarkable experiments by Dr. Ker,[5] who has succeeded, where even Faraday failed, in effecting a visible rotation of the plane of polarisation of light in its passage through electrified dielectrics, and on its reflection at the surface of a magnet. To take one more instance, which must be present to the minds of us all, there are the infinitesimal ripples of the vibrating plate in Mr. Graham Bell's most marvellous invention. Of the nodes and ventral segments in the plate of the telephone which actually converts sound into electricity and electricity into sound, we can at present form no conception. All that can now be said is that the most perfect specimens of Chladni's sand figures on a vibrating plate, or of Kundt's lycopodium heaps in a musical tube,[6] or even Mr. Sedley Taylor's more delicate vortices in the films of the phoneidoscope,[7] are rough and sketchy compared with these. For notwithstanding the fact that in the movements of the telephone-plate we have actually in our hand the solution of that old world-problem the construction of a speaking-machine, yet the characters in which that solution is expressed are too small for our powers of decipherment. In movements such as these we seem to lose sight of the distinction, or perhaps we have unconsciously passed the boundary between massive and molecular motion.[8]

Through the phonograph[9] we have not only a transformation, but a permanent and tangible record of the mechanism of speech. But the differences upon which articulation (apart from loudness, pitch, and quality) depends, appear from the experiments of Fleeming Jenkin and of others, to be of microscopic size. The microphone affords another instance[10] of the unexpected value of minute variations,—in this case of electric currents; and it is remarkable that the gist of the instrument seems to lie in obtaining and perfecting that which electricians have hitherto most scrupulously avoided, viz., loose contact.

Once more, Mr. De La Rue has brought forward as one of the results[11] derived from his stupendous battery of 10,000 cells, strong evidence for supposing that a voltaic discharge, even when apparently continuous, may still be an intermittent phenomenon; but all that is known of the period of such intermittence is, that it must recur at exceedingly short intervals. And in connection with this subject, it may be added that, whatever be the ultimate explanation of the strange stratification which the voltaic discharge undergoes in rarefied gases, it is clear that the alternate disposition of light and darkness must be dependent on some periodic distribution in space or sequence in time which can at present be dealt with only in a very general way. In the exhausted column we have a vehicle for electricity not constant like an ordinary conductor, but itself modified by the passage of the discharge, and perhaps subject to laws differing materially from those which it obeys at atmospheric pressure. It may also be that some of the features accompanying stratification form a magnified image of phenomena belonging to disruptive discharges in general; and that consequently so far from expecting among the known facts of the latter any clue to an explanation of the former, we must hope ultimately to find in the former an elucidation of what is at present obscure in the latter. A prudent philosopher usually avoids hazarding any forecast of the practical application of a purely scientific research. But it would seem that the configuration of these striæ might some day prove a very delicate means of estimating low pressures, and perhaps also for effecting some electrical measurements.

Now, it is a curious fact that almost the only small quantities of which we have as yet any actual measurements are the wave-lengths of light; and that all others, excepting so far as they can be deduced from these, await future determination. In the mean time, when unable to approach these small quantities individually, the method to which we are obliged to have recourse is, as indicated above, that of averages, whereby, disregarding the circumstances of each particular case, we calculate the average size, the average velocity, the average direction, &c., of a large number of instances.[1] But although this method is based upon experience, and leads to results which may be accepted as substantially true; although it may be applicable to any finite interval of time, or over any finite area of space (that is, for all practical purposes of life) there is no evidence to show that it is so when the dimensions of interval or of area are indefinitely diminished. The truth is that the simplicity of nature which we at present grasp is really the result of infinite complexity; and that below the uniformity there underlies a diversity whose depths we have not yet probed, and whose secret places are still beyond our reach.

The present is not an occasion for multiplying illustrations, but I can hardly omit a passing allusion to one all-important instance of the application of the statistical method. Without its aid social life, or the History of Life and Death, could not be conceived at all, or only in the most superficial manner. Without it we could never attain to any clear ideas of the condition of the poor, we could never hope for any solid amelioration of their condition or prospects. Without its aid, sanitary measures, and even medicine, would be powerless. Without it, the politician and the philanthropist would alike be wandering over a trackless desert.

Mathematical Methods and other Subjects.

It is, however, not so much from the side of science at large as from that of mathematics itself, that I desire to speak. I wish from the latter point of view to indicate connections between mathematics and other subjects, to prove that hers is not after all such a far-off region, nor so undecipherable an alphabet, and to show that even at unlikely spots we may trace under-currents of thought which, having issued from a common source, fertilise alike the mathematical and the non-mathematical world.

Having this in view, I propose to make the subject of special remark some processes peculiar to modern mathematics; and, partly with the object of incidentally removing some current misapprehensions, I have selected for examination three methods in respect of which mathematicians are often thought to have exceeded all reasonable limits of speculation, and to have adopted for unknown purposes an unknown tongue.

And it will be my endeavour to show not only that in these very cases our science has not outstepped its own legitimate range, but that even art and literature have unconsciously employed methods similar in principle. The three methods in question are first, that of imaginary quantities; secondly that of manifold space, and thirdly, that of geometry not according to Euclid.

Imaginaries.

First it is objected that, abandoning the more cautious methods of ancient mathematicians, we have admitted into our formulæ quantities which by our own showing, and even in our own nomenclature, are imaginary or impossible; nay, more, that out of them we have formed a variety of new algebras to which there is no counterpart whatever in reality, but from which we claim to arrive at possible and certain results.

On this head it is in Dublin, if anywhere, that I may be per-

1 *Phil. Trans.* of the Royal Society, 1870, p. 333; and 1876, p. 27.
2 *Phil. Trans.* 1877, p. 149.
3 "On Attraction and Repulsion Resulting from Radiation," *Phil. Trans.* 1874, p. 501; 1875, p. 519; 1876, p. 325.
4 *Philosophical Magazine*, April, 1878.
5 *Philosophical Magazine*, 1875, vol. ii. pp. 337, 446; 1877, vol. i. p. 321; 1878, vol. i. p. 161.
6 *Poggendorff's Annalen*, tom. xxxv. p. 337.
7 Royal Society's *Proceedings*, 1878.
8 The papers on the telephone are too numerous to specify.
9 See various papers in NATURE, and elsewhere, during the last twelve months.
10 Royal Society's *Proceedings*, May 9, 1878.
11 *Phil. Trans.* vol. clxix. pp. 55 and 155, and other papers catalogued in the Appendix to Part II. of the Memoir.

1 See Maxwell "On Heat," chap. xxii.

mitted to speak. For to the fertile imagination of the late Astronomer-Royal for Ireland we are indebted for that marvellous calculus of quaternions, which is only now beginning to be fully understood, and which has not yet received all the applications of which it is doubtless capable. And even although this calculus be not coextensive with another (the Ausdehnungslehre of Grassmann)[1] which almost simultaneously germinated on the Continent, nor with ideas more recently developed in America (Pierce's "Linear Associative Algebras");[2] yet it must always hold its position as an original discovery and as a representative of one of the two great groups of generalised algebras (viz., those the squares of whose units are respectively negative unity and zero) the common origin of which must still be marked on our intellectual map as an unknown region. Well do I recollect how in its early days we used to handle the method as a magician's page might try to wield his master's wand, trembling as it were between hope and fear, and hardly knowing whether to trust our own results until they had been submitted to the present and ever ready counsel of Sir W. R. Hamilton himself.

To fix our ideas, consider the measurement of a line, or the reckoning of time, or the performance of any mathematical operation. A line may be measured in one direction or in the opposite; time may be reckoned forward or backward; an operation may be performed or be reversed, it may be done or may be undone; and if having once reversed any of these processes we reverse it a second time, we shall find that we have come back to the original direction of measurement or reckoning, or to the original kind of operation.

Suppose, however, that at some stage of a calculation our formulæ indicate an alteration in the mode of measuremen such that if the alteration be repeated, a condition of things ntot the same as, but the reverse of the original, will be produced. Or suppose that, at a certain stage, our transformations indicate that time is to be reckoned in some manner different from future or past, but still in a way having definite algebraical connection with time which is gone and time which is to come.[3] It is clear that in actual experience there is no process to which such measurements correspond. Time has no meaning except as future or past; and the present is but the meeting point of the two. Or, once more, suppose that we are gravely told that all circles pass through the same two imaginary points at an infinite distance, and that every line drawn through one of these points is perpendicular to itself. On hearing the statement we shall probably whisper, with a smile or a sigh, that we hope it is not true, but that in any case it is a long way off, and perhaps, after all, it does not very much signify. If, however, we are not satisfied to dismiss the question on these terms, the mathematician himself must admit that we have here reached a definite point of issue. Our science must either give a rational account of the dilemma, or yield the position as no longer tenable.

Explanation of them.

Special modes of explaining this anomalous state of things have occurred to mathematicians. But, omitting details as unsuited to the present occasion, it will, I think, be sufficient to point out in general terms that a solution of the difficulty is to be found in the fact that the formulæ which give rise to these results are more comprehensive than the signification which has been given to them; and when we pass out of the condition of things first contemplated they cannot (as it is obvious they ought not) give us any results intelligible on that basis. But it does not therefore by any means follow that upon a more enlarged basis the formulæ are incapable of interpretation; on the contrary, the difficulty at which we have arrived indicates that there must be some more comprehensive statement of the problem which will include cases impossible in the more limited, but possible in the wider view of the subject.

A very simple instance will illustrate the matter. If from a point outside a circle we draw a straight line to touch the curve, the distance between the starting point and the point of contact has certain geometrical properties. If the starting point be shifted nearer and nearer to the circle the distance in question becomes shorter, and ultimately vanishes. But as soon as the point passes to the interior of the circle the notion of a tangent and distance to the point of contact cease to have any meaning; and the same anomalous condition of things prevails as long as the point remains in the interior. But if the point be shifted still further until it emerges on the other side, the tangent and its properties resume their reality; and are as intelligible as before. Now the process whereby we have passed from the possible to the impossible, and again repassed to the possible (namely the shifting of the starting point) is a perfectly continuous one, while the conditions of the problem as stated above have abruptly changed. If, however, we replace the idea of a line touching by that of a line cutting the circle, and the distance of the point of contact by the distances at which the line is intercepted by the curve, it will easily be seen that the latter includes the former as a limiting case, when the cutting line is turned about the starting point until it coincides with the tangent itself. And further, that the two intercepts have a perfectly distinct and intelligible meaning whether the point be outside or inside the area. The only difference is that in the first case the intercepts are measured in the same direction; in the latter in opposite directions.

The foregoing instance has shown one purpose which these imaginaries may serve, viz., as marks indicating a limit to a particular condition of things, to the application of a particular law, or pointing out a stage where a more comprehensive law is required. To attain to such a law we must, as in the instance of the circle and tangent, reconsider our statement of the problem; we must go back to the principle from which we set out, and ascertain whether it may not be modified or enlarged. And even if in any particular investigation, wherein imaginaries have occurred, the most comprehensive statement of the problem of which we are at present capable fails to give an actual representation of these quantities; if they must for the present be relegated to the category of imaginaries; it still does not follow that we may not at some future time find a law which will endow them with reality, nor that in the meantime we need hesitate to employ them, in accordance with the great principle of continuity, for bringing out correct results.

Illustration from Art and Literature.

If, moreover, both in geometry and in algebra we occasionally make use of points or of quantities which from our present outlook have no real existence, which can neither be delineated in space of which we have experience, nor measured by scale as we count measurement; if these imaginaries, as they are termed, are called up by legitimate processes of our science; if they serve the purpose not merely of suggesting ideas, but of actually conducting us to practical conclusions; if all this be true in abstract science, I may perhaps be allowed to point out, at all events in illustration, that in art unreal forms are frequently used for suggesting ideas, for conveying a meaning for which no others seem to be suitable or adequate. Are not forms unknown to biology, situations incompatible with gravitation, positions which challenge not merely the stability but the very possibility of equilibrium—are not these the very means to which the artist often has recourse in order to convey his meaning and to fulfil his mission? Who that has ever revelled in the ornamentation of the Renaissance, in the extraordinary transitions from the animal to the vegetable, from faunic to floral forms, and from these again to almost purely geometric curves, who has not felt that these imaginaries have a claim to recognition very similar to that of their congeners in mathematics? How is it that the grotesque paintings of the middle ages, the fantastic sculpture of remote nations, and even the rude art of the prehistoric past, still impress us, and have an interest over and above their antiquarian value; unless it be that they are symbols which, although hard of interpretation when taken alone, are yet capable from a more comprehensive point of view of leading us mentally to something beyond themselves, and to truths which, although reached through them, have a reality scarcely to be attributed to their outward forms?

Again, if we turn from art to letters, truth to nature and to fact is undoubtedly a characteristic of sterling literature; and yet in the delineation of outward nature itself, still more in that

[1] Grunert's Archiv., vol. vi. p. 337; also separate work, Berlin, 1862.

[2] "Linear Associative Algebra," by Benjamin Pierce, Washington City, 1870.

[3] Sir W. Thomson, *Cambridge Mathematical Journal*, vol. iii. p. 174. Jevons' "Principles of Science," vol. ii. p. 438. But an explanation of the difficulty seems to me to be found in the fact that the problem, as stated, is one of the conduction of heat, and that the "impossibility" which attaches itself to the expression for the "time" merely means that previous to a certain epoch the conditions which gave rise to the phenomena were not those of conduction, but those of some other action of heat. If, therefore, we desire to comprise the phenomena of the earlier as well as of the later period in one problem, we must find some more general statement; viz., that of physical conditions which at the critical epoch will issue in a case of conduction. I think that Prof. Clifford has somewhere given a similar explanation.

of feelings and affections, of the secret springs of character and motives of conduct, it frequently happens that the writer is driven to imagery, to an analogy, or even to a paradox, in order to give utterance to that of which there is no direct counterpart in recognised speech. And yet which of us cannot find a meaning for these literary figures, an inward response, to imaginative poetry, to social fiction, or even to those tales of giant and fairyland written, it is supposed, only for the nursery or schoolroom? But in order thus to reanimate these things with a meaning beyond that of the mere words, have we not to reconsider our first position, to enlarge the ideas with which we started; have we not to cast about for something which is common to the idea conveyed and to the subject actually described, and to seek for the sympathetic spring which underlies both; have we not, like the mathematician, to go back as it were to some first principles, or as it is pleasanter to describe it, to become again as a little child?

Manifold Space.

Passing to the second of the three methods, viz., that of manifold space, it may first be remarked that our whole experience of space is in three dimensions, viz., of that which has length, breadth, and thickness; and if for certain purposes we restrict our ideas to two dimensions as in plane geometry, or to one dimension as in the division of a straight line, we do this only by consciously and of deliberate purpose setting aside, but not annihilating, the remaining one or two dimensions. Negation, as Hegel has justly remarked, implies that which is negatived, or, as he expresses it, affirms the opposite. It is by abstraction from previous experience, by a limitation of its results, and not by any independent process, that we arrive at the idea of space whose dimensions are less than three.

It is doubtless on this account that problems in plane geometry which, although capable of solution on their own account, become much more intelligible, more easy of extension, if viewed in connection with solid space, and as special cases of corresponding problems in solid geometry. So eminently is this the case that the very language of the more general method often leads us almost intuitively to conclusions which from the more restricted point of view require long and laborious proof. Such a change in the base of operations has, in fact, been successfully made in geometry of two dimensions, and although we have not the same experimental data for the further steps, yet neither the modes of reasoning, nor the validity of its conclusions, are in any way affected by applying an analogous mental process to geometry of three dimensions, and by regarding figures in space of three dimensions as sections of figures in space of four in the same way that figures in plano are sometimes considered as sections of figures in solid space. The addition of a fourth dimension to space not only extends the actual properties of geometrical figures, but it also adds new properties which are often useful for the purposes of transformation or of proof. Thus it has recently been shown that in four dimensions a closed material shell could be turned inside out[1] by simple flexure, without either stretching or tearing,[2] and that in such a space it is impossible to tie a knot.

Again, the solution of problems in geometry is often effected by means of algebra: and as three measurements, or co-ordinates as they are called, determine the position of a point in space, so do three letters or measurable quantities serve for the same purpose in the language of algebra. Now many algebraical problems involving three unknown or variable quantities admit of being generalised so as to give problems involving many such quantities. And as, on the one hand, to every algebraical problem involving unknown quantities or variables by ones, or by twos, or by threes, there corresponds a problem in geometry of one or of two or of three dimensions, so on the other it may be said that to every algebraical problem involving many variables there corresponds a problem in geometry of many dimensions.

There is, however, another aspect under which even ordinary space presents to us a four-fold, or indeed a manifold character. In modern physics space is not regarded as a vacuum in which bodies are placed and forces have play, but rather as a plenum with which matter is co-extensive. And from a physical point of view the properties of space are the properties of matter, or of the medium which fills it. Similarly from a mathematical point of view, space may be regarded as a *locus in quo*, as a plenum, filled with those elements of geometrical magnitude which we take as fundamental. These elements need not always be the same. For different purposes different elements may be chosen; and upon the degree of complexity of the subject of our choice will depend the internal structure or manifoldness of space.

Thus, beginning with the simplest case, a point may have any singly infinite multitude of positions in a line, which gives a one-fold system of points in a line. The line may revolve in a plane about any one of its points, giving a two-fold system of points in a plane; and the plane may revolve about any one of the lines, giving a three-fold system of points in space.

Suppose, however, that we take a straight line as our element, and conceive space as filled with such lines. This will be the case if we take two planes, *e.g.*, two parallel planes, and join every point in one with every point in the other. Now the points in a plane form a two-fold system, and it therefore follows that the system of lines is four-fold; in other words, space regarded as a plenum of lines is four-fold. The same result follows from the consideration that the lines in a plane, and the planes through a point, are each two-fold.

Again, if we take a sphere as our element we can through any point as a centre draw a singly infinite number of spheres, but the number of such centres is triply infinite; hence space as a plenum of spheres is four-fold. And generally, space as a plenum of surfaces has a manifoldness equal to the number of constants required to determine the surface. Although it would be beyond our present purpose to attempt to pursue the subject further, it should not pass unnoticed that the identity in the four-fold character of space, as derived on the one hand from a system of straight lines, and on the other from a system of spheres, is intimately connected with the principles established by Sophus Lie in his researches on the correlation of these figures.

If we take a circle as our element we can around any point in a plane as a centre draw a singly infinite system of circles; but the number of such centres in a plane is doubly infinite; hence the circles in a plane form a three-fold system, and as the planes in space form a three-fold system, it follows that space as a plenum of circles is six-fold.

Again, if we take a circle as our element, we may regard it as a section either of a sphere, or of a right cone (given except in position) by a plane perpendicular to the axis. In the former case the position of the centre is three-fold; the directions of the plane, like that of a pencil of lines perpendicular thereto, two-fold; and the radius of the sphere one-fold; six-fold in all. In the latter case, the position of the vertex is three-fold; the direction of the axis two-fold; and the distance of the plane of section one-fold; six-fold in all, as before. Hence space as a plenum of circles is six-fold.

Similarly, if we take a conic as our element we may regard it as a section of a right cone (given except in position) by a plane. If the nature of the conic be defined, the plane of section will be inclined at a fixed angle to the axis; otherwise it will be free to take any inclination whatever. This being so, the position of the vertex will be three-fold, the direction of the axis two-fold, the distance of the plane of section from the vertex one-fold, and the direction of that plane one-fold if the conic be defined, two-fold if it be not defined. Hence, space as a plenum of definite conics will be seven-fold, as a plenum of conics in general, eight-fold. And so on for curves of higher degrees.

This is, in fact, the whole story and mystery of manifold space. If not seriously regarded as a reality in the same sense as ordinary space, it is a mode of representation, or a method which, having served its purpose, vanishes from the scene. Like a rainbow, if we try to grasp it, it eludes our very touch; but, like a rainbow, it arises out of real conditions of known and tangible quantities, and if rightly apprehended it is a true and valuable expression of natural laws, and serves a definite purpose in the science of which it forms part.

Illustrations.

Again, if we seek a counterpart of this in common life, I might remind you that perspective in drawing is itself a method not altogether dissimilar to that of which I have been speaking; and that the third dimension of space, as represented in a picture, has its origin in the painter's mind, and is due to his skill, but has no real existence upon the canvas which is the groundwork of his art. Or again, turning to literature, when in legendary tales, or in works of fiction, things past and future are pictured as present, has not the poetic fancy brought time into correlation with the three dimensions of space, and brought all alike to a

[1] S. Newcomb "On Certain Transformations of Surfaces," *American Journal of Mathematics*, vol. i. p. 1.

[2] Tait "On Knots," *Transactions* of the Royal Society of Edinburgh, vol. xxviii. p. 145. Klein, *Mathematische Annalen*, ix. p. 478.

common focus? Or once more, when space already filled with material substances is mentally peopled with immaterial beings, may not the imagination be regarded as having added a new element to the capacity of space, a fourth dimension of which there is no evidence in experimental fact?

Non-Euclid Geometry.

The third method proposed for special remark is that which has been termed non-Euclidean geometry, and the train of reasoning which has led to it may be described in general terms as follows: some of the properties of space which on account of their simplicity, theoretical as well as practical, have, in constructing the ordinary system of geometry, been considered as fundamental, are now seen to be particular cases of more general properties. Thus a plane surface and a straight line may be regarded as special instances of surfaces and lines whose curvature is everywhere uniform or constant. And it is, perhaps, not difficult to see that, when the special notions of flatness and straightness are abandoned, many properties of geometrical figures which we are in the habit of regarding as fundamental, will undergo profound modification. Thus a plane may be considered as a special case of the sphere, viz., the limit to which a sphere approaches when its radius is increased without limit. But even this consideration trenches upon an elementary proposition relating to one of the simplest of geometrical figures. In plane triangles the interior angles are together equal to two right angles; but in triangles traced on the surface of a sphere, this proposition does not hold good. To this, other instances might be added.

Further, these modifications may affect not only our ideas of particular geometrical figures, but the very axioms of the science itself. Thus, the idea which, in fact, lies at the foundation of Euclid's method that a geometrical figure may be moved in space without change of size or alteration of form, entirely falls away, or becomes only approximate in a space wherein dimension and form are dependent upon position. For instance, if we consider merely the case of figures traced on a flattened globe like the earth's surface, or upon an egg-shell, such figures cannot be made to slide upon the surface without change of form, as is the case with figures traced upon a plane or even upon a sphere. But, further still, these generalisations are not restricted to the case of figures traced upon a surface; they may apply also to solid figures in a space whose very configuration varies from point to point. We may, for instance, imagine a space in which our rule or scale of measurement varies as it extends, or as it moves about, in one direction or another; a space, in fact, whose geometric density is not uniformly distributed. Thus we might picture to ourselves such a space as a field having a more or less complicated distribution of temperature, and our scale as a rod instantaneously susceptible of expansion or contraction under the influence of heat; or we might suppose space to be even crystalline in its geometric formation, and our scale and measuring instruments to accept the structure of the locality in which they are applied. These ideas are doubtless difficult of apprehension, at all events at the outset; but Helmholtz has pointed out a very familiar phenomenon which may be regarded as a diagram of such a kind of space. The picture formed by reflection from a plane mirror may be taken as a correct representation of ordinary space, in which, subject to the usual laws of perspective, every object appears in the same form and of the same dimensions, whatever be its position. In like manner the picture formed by reflexion from a curved mirror may be regarded as the representation of a space wherein dimension and form are dependent upon position. Thus in an ordinary convex mirror objects appear smaller as they recede laterally from the centre of the picture; straight lines become curved; objects infinitely distant in front of the mirror appear at a distance only equal to the focal length behind. And by suitable modifications in the curvature of the mirror, representations could similarly be obtained of space of various configurations.

Its Meaning and Use.

The diversity in kind of these spaces is of course infinite; they vary with the mode in which we generalise our conceptions of ordinary space; but upon each as a basis it is possible to construct a consistent system of geometry, whose laws, as a matter of strict reasoning, have a validity and truth not inferior to those with which we are habitually familiar. Such systems having been actually constructed, the question has not unnaturally been asked, whether there is anything in nature or in the outer world to which they correspond; whether, admitting that for our limited experience ordinary geometry amply suffices, we may understand that for powers more extensive in range or more minute in definition some more general scheme would be requisite? Thus, for example, although the one may serve for the solar system, is it legitimate to suppose that it may fail to apply at distances reaching to the fixed stars, or to regions beyond? Or again, if our vision could discern the minute configuration of portions of space, which to our ordinary powers appear infinitesimally small, should we expect to find that all our usual geometry is but a special case, sufficient indeed for daily use, but after all only a rough approximation to a truer although perhaps more complicated scheme? Traces of these questions are in fact to be found in the writings of some of our greatest and most original mathematicians. Gauss, Riemann, and Helmholtz have thrown out suggestions radiating, as it were, in these various directions from a common centre; while Cayley, Sylvester, and Clifford in this country, Klein in Germany, Lobatscheffsky in Russia, Bolyai in Hungary, and Beltrami in Italy, with many others, have reflected similar ideas with all the modifications due to the chromatic dispersion or their individual minds. But to the main question the answer must be in the negative. And, to use the words of Newton, since "geometry has its foundation in mechanical practice,' the same must be the answer until our experience is different from what it now is. And yet, all this notwithstanding, the generalised conceptions of space are not without their practical utility. The principle of representing space of one kind by that of another, and figures belonging to one by their analogues in the other, is not only recognised as legitimate in pure mathematics, but has long ago found its application in cartography. In maps or charts, geographical positions, the contour of coasts, and other features, belonging in reality to the earth's surface, are represented on the flat; and to each mode of representation, or projection as it is called, there corresponds a special correlation between the spheroid and the plane. To this might perhaps be added the method of descriptive geometry, and all similar processes in use by engineers, both military and civil.

It has often been asked whether modern research in the field of pure mathematics has not so completely outstripped its physical applications as to be practically useless; whether the analyst and the geometer might not now, and for a long time to come, fairly say, "hic artem remumque repono," and turn his attention to mechanics and to physics. That the Pure has outstripped the Applied is largely true; but that the former is on that account useless is far from true. Its utility often crops up at unexpected points: witness the aids to classification of physical quantities, furnished by the ideas (of Scalar and Vector) involved in the "Calculus of Quaternions;" or the advantages which have accrued to physical astronomy from Lagrange's "Equations," and from Hamilton's "Principle of Varying Action;" on the value of complex quantities, and the properties of general integrals, and of general theorems on integration for the theories of electricity and magnetism. The utility of such researches can in no case be discounted, or even imagined beforehand; who, for instance, would have supposed that the calculus of forms or the theory of substitutions would have thrown much light upon ordinary equations; or that Abolian functions and hyperelliptic transcendents would have told us anything about the properties of curves; or that the calculus of operations would have helped us in any way towards the figure of the earth? But upon such technical points I must not now dwell. If, however, as I hope, it has been sufficiently shown that any of these more extended ideas enable us to combine together, and to deal with as one, properties and processes which from the ordinary point of view present marked distinctions, then they will have justified their own existence; and in using them we shall not have been walking in a vain shadow, nor disquieting our brains in vain.

Mathematical Symmetry.

These extensions of mathematical ideas would, however, be overwhelming, if they were not compensated by some simplifications in the processes actually employed. Of these aids to calculation I will mention only two, viz., symmetry of form and mechanical appliances; or, say, mathematics as a fine art, and mathematics as a handicraft. And first, as to symmetry of form. There are many passages of algebra in which long processes of calculation at the outset seem unavoidable. Results are often obtained in the first instance through a tangled maze of formulæ, where at best we can just make sure of their process step by step, without any general survey of the path which we have traversed, and still less of that which we have to pursue. But almost within our own generation a new method has been devised to clear this entanglement. More correctly speaking, the method is not new, for it is inherent in the processes of algebra itself, and instances of it, unnoticed perhaps or disregarded, are to be found cropping up throughout nearly all mathematical treatises. By Lagrange, and to some extent also by Gauss, among the older writers, the method of which I am speaking was recognised as a principle; but beside these perhaps no others can be named until a period within our own recollection. The method consists in symmetry of expression. In algebraical formulæ combinations of the quantities entering therein occur and recur; and by a suitable choice of these quantities the various combinations may be rendered symmetrical and reduced to a few well-known types. This having been done, and one such combination having been calculated, the remainder, together with many of their results, can often be written down at once, without further calculations, by simple permutations of the letters. Symmetrical expressions, moreover, save as much time and trouble in reading as in writing. Instead of wading laboriously through a series of expressions which, although successively dependent, bear no outward resemblance to one another, we may read off symmetrical formulæ, of almost any length, at a glance. A page of such formulæ becomes a picture: known forms are seen in definite groupings; their relative positions, or perspective as it may be called, their very light and shadow, convey their meaning almost as much through the artistic faculty as through any conscious ratiocinative process. Few principles have been more suggestive of extended ideas or of new views and relation than that of which I am now speaking. In order to pass from questions concerning plane figures to those which appertain to space, from conditions having few degrees of freedom to others which have many—in a word, from more restricted to less restricted problems—we have in many cases merely to add lines and columns to our array of letters or symbols already formed, and then read off pictorially the extended theorems.

Mechanical Methods.

Next as to mechanical appliances. Mr. Babbage, when speaking of the difficulty of insuring accuracy in the long numerical calculations of theoretical astronomy, remarked that the science which in itself is the most accurate and certain of all had, through these difficulties, become inaccurate and uncertain in some of its results. And it was doubtless some such consideration as this, coupled with his dislike of employing skilled labour where unskilled labour would suffice, which led him to the invention of his calculating machines. The idea of substituting mechanical for intellectual power has not lain dormant; for beside the arithmetical machines, whose name is legion (from Napier's Bones, Earl Stanhope's calculator, to Schultz's and Thomas's machines now in actual use), an invention has lately been designed for even a more difficult task.[1] Prof. James Thomson has in fact recently constructed a machine which, by means of the mere friction of a disc, a cylinder, and a ball, is capable of effecting a variety of the complicated calculations which occur in the highest application of mathematics to physical problems. By its aid it seems that an unskilled labourer may, in a given time, perform the work of ten skilled arithmeticians. The machine is applicable alike to the calculation of tidal, of magnetic, of meteorological, and perhaps also of all other periodic phenomena. It will solve differential equations of the second and perhaps of even higher orders. And through the same invention the problem of finding the free motions of any number of mutually attracting particles, unrestricted by any of the approximate suppositions required in the treatment of the lunar and planetary theories, is reduced to the simple process of turning a handle.

When Faraday had completed the experimental part of a physical problem, and desired that it should thenceforward be treated mathematically, he used irreverently to say, "Hand it over to the calculators." But truth is ever stranger than fiction; and if he had lived until our day, he might with perfect propriety have said, "Hand it over to the machine."

Mathematics and Observation.

Had time permitted, the foregoing topics would have led me to point out that the mathematician, although concerned only with abstractions, uses many of the same methods of research as are employed in other sciences, and in the arts, such as observation, experiment, induction, imagination. But this is the less necessary because the subject has been already handled very ably, although with greater brevity than might have been wished, by Prof. Sylvester in his address to Section A at our meeting at Exeter.

[1] Royal Society's *Proceedings*, February 3, 1876, and May 9, 1878.

Origin of Mathematical Ideas.

In an exhaustive treatment of my subject there would still remain a question which in one sense lies at the bottom of all others, and which through almost all time has had an attraction for reflective minds, viz., what was the origin of mathematical ideas? Are they to be regarded as independent of, or dependent upon, experience? The question has been answered sometimes in one way and sometimes in another. But the absence of any satisfactory conclusion may after all be understood as implying that no answer is possible in the sense in which the question is put; or rather that there is no question at all in the matter, except as to the history of actual facts. And, even if we distinguish, as we certainly should, between the origin of ideas in the individual and their origin in a nation or mankind, we should still come to the same conclusion. If we take the case of the individual, all we can do is to give an account of our own experience; how we played with marbles and apples; how we learnt the multiplication table, fractions, and proportion; how we were afterwards amused to find that common things conformed to the rules of number; and later still how we came to see that the same laws applied to music and to mechanism, to astronomy, to chemistry, and to many other subjects. And then, on trying to analyse our own mental processes, we find that mathematical ideas have been imbibed in precisely the same way as all other ideas, viz., by learning, by experience, and by reflection. The apparent difference in the mode of first apprehending them and in their ultimate cogency arises from the difference of the ideas themselves, from the preponderance of quantitative over qualitative considerations in mathematics, from the notions of absolute equality and identity which they imply.

If we turn to the other question, How did the world at large acquire and improve its idea of number and of figures? How can we span the interval between the savage who counted only by the help of outward objects, to whom fifteen was "half the hands and both the feet," and Newton or Laplace? The answer is the history of mathematics and its successive developments, arithmetic, geometry, algebra, &c. The first and greatest step in all this was the transition from number in the concrete to number in the abstract. This was the beginning not only of mathematics but of all abstract thought. The reason and mode of it was the same as in the individual. There was the same general influx of evidence, the same unsought for experimental proof, the same recognition of general laws running through all manner of purposes and relations of life. No wonder then if, under such circumstances, mathematics, like some other subjects, and perhaps with better excuse, came after a time to be clothed with mysticism; nor that, even in modern times, they should have been placed upon an *à priori* basis as in the philosophy of Kant.

Their Survival and Transition.

Number was so soon found to be a principle common to many branches of knowledge that it was readily assumed to be the key to all. It gave distinctness of expression, if not clearness of thought, to ideas which were floating in the untutored mind, and even suggested to it new conceptions. In "the one" "the all," "the many in one" (terms of purely arithmetic origin), it gave the earliest utterance to men's first crude notions about God and the world. In "the equal," "the solid," "the straight," and "the crooked," which still survive as figures of speech among ourselves, it supplied a vocabulary for the moral notions of mankind, and quickened them by giving them the power of expression. In this lies the great and enduring interest in the fragments which remain to us of the Pythagorean philosophy.

The consecutive processes of mathematics led to the consecutive processes of logic, but it was not until long after mankind had attained to abstract ideas that they attained to any clear notion of their connection with one another. The leading ideas of mathematics became the leading ideas of logic. The "one" and the "many" passed into the "whole" and its "parts;" and thence into the "universal" and the "particular." The fallacies of logic, such as the well-known puzzle of Achilles and the tortoise, partake of the nature of both sciences. And perhaps the conception of the infinite and the infinitesimal, as well as of negation, may have been in early times transferred from logic to mathematics. But the connection of our ideas of number is probably anterior to the connection of any of our other ideas. And as a matter of fact, geometry and arithmetic had already made considerable progress when Aristotle invented the syllogism.

General Ideas.

General ideas there were beside those of mathematics—true flashes of genius which saw that there must be general laws to which the universe conforms, but which saw them only by occasional glimpses, and through the distortion of imperfect knowledge; and although the only records of them now remaining are the inadequate representations of later writers, yet we must still remember that to the existence of such ideas is due not only the conception but even the possibility of physical science. But these general ideas were too wide in their grasp, and in early days at least were connected to their subjects of application by links too shadowy to be thoroughly apprehended by most minds, and so it came to pass that one form of such an idea was taken as its only form, one application of it as the idea itself; and philosophy, unable to maintain itself at the level of ideas, fell back upon the abstractions of sense, and, by preference, upon those which were most ready to hand, namely, those of mathematics. Plato's ideas relapsed into a doctrine of numbers; mathematics into mysticism, into Neo-Platonism, and the like. And so, through many long ages, through good report and evil report, mathematics have always held an unsought-for sway. It has happened to this science, as to many other subjects, that its warmest adherents have not always been its best friends. Mathematics have often been brought in to matters where their presence has been of doubtful utility. If they have given precision to literary style, that precision has sometimes been carried to excess, as in Spinoza and perhaps Descartes; if they have tended to clearness of expression in philosophy, that very clearness has sometimes given an appearance of finality not always true;[1] if they have contributed to definition in theology, that definitiveness has often been fictitious, and has been attained at the cost of spiritual meaning.[2] And, coming to recent times, although we may admire the ingenuity displayed in the logical machines of Earl Stanhope and of Stanley Jevons, in the formal logic of De Morgan, and in the calculus of Boole; although as mathematicians we may feel satisfaction that these feats (the possibility of which was clear *à priori*) have been actually accomplished; yet we must bear in mind that their application is really confined to cases where the subject matter is perfectly uniform in character, and that beyond this range they are liable to encumber rather than to assist thought.

Not unconnected with this intimate association of ideas and their expression is the fact that, which ever may have been cause, which ever effect, or whether both may not in turn have acted as cause and effect, the culminating age of classic art was contemporaneous with the first great development of mathematical science.[3] In an earlier part of this discourse I have alluded to the importance of mathematical precision recognised in the *technique* of art during the Cinquecento; and I have now time only to add that, on looking still further back, it would seem that sculpture and painting, architecture and music, nay even poetry itself, received a new, if not their first true, impulse at the period when geometric form appeared fresh chiselled by the hand of the mathematician, and when the first ideas of harmony and proportion rang joyously together in the morning tide of art.

Relations of Science to Literature and Art.

Whether the views on which I have here insisted be in any way novel, or whether they be merely such as from habit or from inclination are usually kept out of sight, matters little. But whichever be the case, they may still furnish a solvent of that rigid aversion which both literature and art are too often inclined to maintain towards science of all kinds. It is a very old story that, to know one another better, to dwell upon similarities rather than upon diversities, are the first stages towards a better understanding between two parties; but in few cases has it a truer application than in that here discussed. To recognise the common growth of scientific and other instincts until the time of harvest is not only conducive to a rich crop, but it is also a matter of prudence, lest, in trying to root up weeds from among the wheat, we should at the same time root up that which is as valuable as wheat. When Pascal's father had shut the door of his son's study to mathematics, and closeted him with Latin and Greek, he found on his return that the walls were teeming with formulæ and figures, the more congenial product of the boy's mind. Fortunately for the boy, and fortunately also for science, the mathematics were not torn up, but were suffered to grow together with other subjects. And all said and done, the lad was not the worse scholar or man of letters in the end. But, truth to tell, considering the severance which still subsists in education and during our early years between literature and science, we can hardly wonder if, when thrown together in the afterwork of life, they should meet as strangers; or if the severe garb, the curious implements, and the strange wares of the latter, should seem little attractive when contrasted with the light companionship of the former. The day is yet young, and in the early dawn many things look weird and fantastic which in fuller light prove to be familiar and useful. The outcomings of science, which at one time have been deemed to be but stumbling-blocks scattered in the way, may ultimately prove stepping-stones which have been carefully laid to form a pathway over difficult places for the children of "sweetness and of light."

Concluding Remarks.

The instances on which we have dwelt are only a few out of many in which mathematics may be found ruling and governing a variety of subjects. It is as the supreme result of all experience, the framework in which all the varied manifestations of nature have been set, that our science has laid claim to be the arbiter of all knowledge. She does not indeed contribute elements of fact, which must be sought elsewhere; but she sifts and regulates them; she proclaims the laws to which they must conform if those elements are to issue in precise results. From the data of a problem she can infallibly extract all possible consequences, whether they be those first sought, or others not anticipated; but she can introduce nothing which was not latent in the original statement. Mathematics cannot tell us whether there be or be not limits to time or space; but to her they are both of indefinite extent, and this in a sense which neither affirms nor denies that they are either infinite or finite. Mathematics cannot tell us whether matter be continuous or discrete in its structure; but to her it is indifferent whether it be one or the other, and her conclusions are independent of either particular hypothesis. Mathematics can tell us nothing of the origin of matter, of its creation or its annihilation; she deals only with it in a state of existence; but within that state its modes of existence may vary from our most elementary conception to our most complex experience. Mathematics can tell us nothing beyond the problems which she specifically undertakes; she will carry them to their limit, but there she stops, and upon the great region beyond which she is imperturbably silent.

Conterminous with space and coeval with time is the kingdom of mathematics; within this range her dominion is supreme; otherwise than according to her order nothing can exist; in contradiction to her laws nothing takes place. On her mysterious scroll is to be found written for those who can read it that which has been, that which is, and that which is to come. Everything material which is the subject of knowledge has number, order, or position; and these are her first outlines for a sketch of the universe. If our more feeble hands cannot follow out the details, still her part has been drawn with an unerring pen, and her work cannot be gainsaid. So wide is the

[1] For example, in Herbart's "Psychologie."

[2] A specimen will be found in the Moralia of Gregory the Great, Lib. I., c. xiv., of which I quote only the arithmetical part:—

"Quid in septenario numero, nisi summa perfectionis accipitur? Ut enim humanæ rationis causas de septenario numero taceamus, quæ afferunt, quòd idcirco perfectus sit, quia exprimo pari constat, et primo impari; ex primo, qui dividi potest, et primo, qui dividi non potest; certissimè scimus, quòd septenarium numerum Scriptura Sacra pro perfectione ponere consuevit. . . . . A septenario quippe numero in duodenarium surgitur. Nam septenarius suis in se partibus multiplicatus, ad duodenarium tenditur. Sive enim quatuor per tria, sive per quatuor tria ducantur, septem in duodecim vertuntur. . . . . Jam superiùs dictum est, quòd in quinquagenario numero, qui septem hebdomadibus ac monade additâ impletur, requies designatur; denario autem numero summa perfectionis exprimetur."

[3] Approximate dates B.C. of—

Sculptors, Painters, and Poets.		Mathematicians.	
Stesichorus,	600.	Thales,	600.
Pindar,	522-442.	Pythagoras,	550.
Æschylus,	500-450.	Anaxagoras,	500-450.
Sophocles,	495-400.	Hippocrates,	460.
Euripides,	480-400.		
Phidias,	468-432.		
Praxiteles,	450-400.	Theætetus,	440.
Zeuxis,	400.	Archytas,	400.
Apelles,	350.		
Scopas,	350.	Euclid,	323-283.

range of mathematical science, so indefinitely may it extend beyond our actual powers of manipulation, that at some moments we are inclined to fall down with even more than reverence before her majestic presence. But so strictly limited are her promises and powers, about so much that we might wish to know does she offer no information whatever, that at other moments we are fain to call her results but a vain thing, and to reject them as a stone when we had asked for bread. If one aspect of the subject encourages our hopes, so does the other tend to chasten our desires; and he is perhaps the wisest, and in the long run the happiest among his fellows, who has learnt not only mathematics, but also the larger lesson which they indirectly teach, namely, to temper our aspirations to that which is possible, to moderate our desires to that which is attainable, to restrict our hopes to that of which accomplishment, if not immediately practicable, is at least distinctly within the range of conception. That which is at present beyond our ken may, at some period and in some manner as yet unknown to us, fall within our grasp; but our science teaches us, while ever yearning with Goethe for "Light, more light," to concentrate our attention upon that of which our powers are capable, and contentedly to leave for future experience the solution of problems to which we can at present say neither yea nor nay.

It is within the region thus indicated that knowledge in the true sense of the word is to be sought. Other modes of influence there are in society and in individual life, other forms of energy beside that of intellect. There is the potential energy of sympathy, the actual energy of work; there are the vicissitudes of life, the diversity of circumstance, health, and disease, and all the perplexing issues, whether for good or for evil, of impulse and of passion. But although the book of life cannot at present be read by the light of science alone, nor the wayfarers be satisfied by the few loaves of knowledge now in our hands, yet it would be difficult to overstate the almost miraculous increase which may be produced by a liberal distribution of what we already have, and by a restriction of our cravings within the limits of possibility.

In proportion as method is better than impulse, deliberate purpose than erratic action, the clear glow of sunshine than irregular reflection, and definite utterances than an uncertain sound; in proportion as knowledge is better than surmise, proof than opinion; in that proportion will the mathematician value a discrimination between the certain and the uncertain, and a just estimate of the issues which depend upon one motive power or the other. While on the one hand he accords to his neighbours full liberty to regard the unknown in whatever way they are led by the noblest powers that they possess, so on the other he claims an equal right to draw a clear line of demarcation between that which is a matter of knowledge, and that which is at all events something else, and to treat the one category as fairly claiming our assent, the other as open to further evidence. And yet, when he sees around him those whose aspirations are so fair, whose impulses so strong, whose receptive faculties so sensitive, as to give objective reality to what is often but a reflex from themselves, or a projected image of their own experience, he will be willing to admit that there are influences which he cannot as yet either fathom or measure, but whose operation he must recognise among the facts of our existence.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY THE PRESIDENT, JOHN EVANS, D.C.L., F.R.S., F.G.S., &c.

IN opening the proceedings of this Section, I cannot but call attention to the fact that the present is the third occasion on which the British Association has met in this city, its first meeting here having taken place in the year 1835, or forty-three years ago. On that occasion, as indeed for many years afterwards, the two distinct, though to some extent cognate branches of study, geology and geography, were classed in the same section, and its president was a man of whom Irish science may well be proud, and who, I am thankful to say, is still living to enjoy his well-deserved honours—the veteran geologist, Sir Richard John Griffith, the author of the first geological map of Ireland. It seems hardly credible that the construction of this map was commenced in the summer of 1812, or sixty-six years ago; but the records of the Geological Society of London testify to the still more remarkable fact that Sir Richard Griffith was elected a fellow of that Society in 1808—seventy years ago. Indeed, in 1854, when the Wollaston medal was awarded to the then Dr. Griffith, the president, the late Prof. Edward Forbes, spoke as he said reverentially to one of the earliest members of the Society, and to a geologist who appeared in print before he, the president, was born. It was well said on that occasion that the map lately mentioned was one of the most remarkable geological maps ever produced by a single geologist; and I make no doubt that those who are at present engaged on the Geological Survey of this island will testify, as did their predecessors, to the value of this "surprising monument of observation and skill." When speaking of the Geological Survey of Ireland, it will not, I am sure, be thought out of place if I offer here a tribute of respect to the memory of one who was originally a student in the college within whose walls we are assembled, and who subsequently occupied posts of the highest importance in connection with the Geological Society of Dublin and the Geological Survey of Ireland, besides filling the professorial Chair of Geology in this University: I mean Dr. Thomas Oldham, the late Director of the Geological Survey of India. With the marvellous amount of work which he was enabled to accomplish in that country you are all acquainted, and you will all share in the regret that the period of his well-earned retirement—that "requies optimorum meritorum"—should have been so quickly cut short by death. His name will, however, long survive, and future students of geology will have no difficulty in recognising the distinguished labourer in their science after whom the Cambrian *Oldhamia* of the Wicklow hills so worthily received its name.

But to return to this Association.

On the next occasion of its meeting in Dublin, in 1857, Section C had become devoted to geology alone, and geography was excluded, the president being Lord Talbot de Malahide, a nobleman whom also we still have among us, and who is alike well known to archæologists and geologists.

As the last meeting of the Association in this city took place twenty-one years ago, it would at first sight appear that in opening our proceedings I might with propriety dwell on the progress which has been made within that period in the development of the geology of Ireland. I must, however, remind you that it is only four years since the Association held its meeting in what I may almost call the neighbouring town of Belfast, when the accomplished chief of the Geological Survey in Ireland presided over this section and delivered an address, in which some of the more interesting features of the country, especially those of the volcanic district of the north-east of this island, were discussed. During the present year, moreover, he has published his comprehensive work on the "Physical Geology and Geography of Ireland," which I commend to you as far more likely to call your attention to the characteristic features of the country and the latest discoveries with regard to its geology than anything I could compile.

In addition to this, there has appeared during the present year another interesting volume, which records the impressions of a highly intelligent foreign geologist on visiting this country. I mean the "Aus Irland" of Dr. Arnold von Lassaulx, Professor of Mineralogy in the University of Breslau. For this volume, in which shrewd remarks on the country and its inhabitants are mingled with geological observations and valuable comparisons of the Irish formations with those of other countries, we are indebted to the meeting of the British Association having been held two years ago at Glasgow, which attracted the author to visit the British Islands.

So much having lately been published upon the geology of this country, I shall content myself with making a very few general observations with regard to it, and propose subsequently to touch briefly on some of those questions which, within the last twelve months, have occupied the attention of those who are engaged in the advancement of our science.

As to the geology of this country, I may observe that we are here assembled just on the edge of that great central plain which forms so important a feature in the map of Ireland, and which stretches from Dublin Bay on the east coast, to Galway Bay on the west, with hardly a portion of it attaining to an elevation of 300 feet above the sea, over a tract of country nearly 150 miles in extent in almost every direction.

The boundaries of this great plain and those of the carboniferous limestone almost coincide, so that we have here the somewhat remarkable feature of a formation which in England is of such a character as to have received the name of the mountain limestone, constituting in the neighbouring island nearly the whole of the plain country. In some of the north-western counties, however, as for instance Fermanagh and Sligo, it assumes its more mountainous character. Nearly the whole of this central plain is overlain with boulder clay, limestone gravel, or middle drift, and extensive bogs, so that the subjacent rock is but occasionally seen. In several places detached bosses of old red sandstone rise through the limestone, and there is also good reason for believing, with Prof. Hull, that the whole of the area was at one time covered with the upper members of the carboniferous group, including the true coal measures, of which unfortunately but small patches remain, and those upon the margin of the plain. From the absence of the upper palæozoic, mesozoic, and cainozoic formations over the area, Prof. Hull has arrived at the conclusion that the surface remained in the condition of dry land, while that of England was being submerged beneath the waters of the sea, over the bed of which nearly all these formations were deposited. To a certain extent, however, he leaves it an open question whether some of the mesozoic strata which occur over the north-east of Ireland may not have been deposited over the centre and south. The amount of denudation over this central area has, no doubt, been such that the chances of even Prof. Judd finding traces of these later deposits appear at first sight to be but small; but whether the whole of this vast amount of denudation is due to the wasting influence of rain, rivers, and other sub-aërial agents of erosion, is a question which I venture to regard as at all events open to discussion. It appears to be the case that in some parts of the north of Ireland the whole of the upper carboniferous beds had been denuded before the deposition of any permian strata, as these are deposited immediately on the carboniferous limestone; and if this amount of denudation had taken place in pre-permian times in the north, there seems a possibility of the same having been the case in central Ireland. If so, it is possible that some traces of the later deposits may yet be found on the central plain. Certainly, if we are still to regard the white chalk as a deep-sea deposit, the cretaceous rocks of the north-east of Ireland must have at one time extended farther south than they do at present, and somewhere or other there must have been shore deposits of that period formed farther south than the Upper Greensand of Antrim. The careful investigations of Prof. Judd have largely extended our knowledge of the secondary rocks of the western coast and islands of Scotland, and he has been able to show that the Jurassic series of the Western Highlands could not have had a thickness of less than three thousand feet. It is therefore hard to believe that with such a development in so closely neighbouring a district, the deposits of the same age in Ireland can have been restricted to their present area.

Prof. Judd considers that the amount of denudation in the Scottish Highlands since the mesozoic, and even the miocene period, has been enormous, and that the great surface features of the Highlands were produced in pliocene times. It seems therefore possible, if not probable, that so long a period of exposure to sub-aërial influence as that assigned to the central plain of Ireland by Prof. Hull would have resulted in a more uneven land surface than that which we now find. At all events, the history of this remarkable physical feature is one which is of high interest, and can hardly as yet be considered as closed.

With regard to the mountainous districts surrounding the central plain, we shall, I believe, have the opportunity of visiting some parts of the Wicklow Mountains, a district from which a portion, at all events, of the native gold of Ireland was procured in ancient times, as indeed it continues to be. Of the abundance of gold in this country in early times, a glance at the magnificent collection of ancient ornaments preserved in the museum of the Royal Irish Academy will serve to give an idea. Even in times more recent than those in which the bulk of these ornaments were made, gold was an important product of this country, and I am tempted to quote a few lines from an early English poem, "The Libell of Englishe Policye," written in the year 1436. In treating of the commodities of Ireland, the author says that the country is

> "So large, so gode, and so commodious,
> That to declare is straunge and merveilous.
> For of silver and gold there is the ore
> Among the wilde Irish, though they be pore;
> For they ar rude and can thereon no skille,
> So that, if we hadde ther pese and good wille,
> To mine and fine and metal for to pure
> In wilde Irishe mighte we find the cure;
> As in Londone saith a jewellere
> Which broughte from thennes gold oor to us here,
> Wherof was fined metal gode and clene,
> That at the touch no better coud be sene."

Sir William Wilde has observed that the south-western half of Ireland has yielded a greater amount of gold antiquities than the north-western, and probably this would hold good with regard to the production of the metal itself, though it has been found in the counties of Antrim, Tyrone, and Derry, as well as in those of Dublin, Wicklow, Wexford, and Kildare.

The north-east of Ireland possesses, however, another geological feature peculiar to itself in that great expanse of volcanic beds which formed the subject of Professor Hull's address to this section at the Belfast meeting. My only object in now mentioning them is again to call attention to their containing the only remains of a miocene flora which are to be found in this island. Analogous beds were detected in the corresponding basalts in the Island of Mull by the Duke of Argyll in 1851. With the exception of the Hempstead beds of the Isle of Wight, which should probably be classed as oligocene, and the Bovey Tracey beds of Devonshire, these are almost the only deposits of miocene age in the British Isles. The contrast presented by the scarcity of deposits of this period in Britain with their abundance in the north-west, centre, and south of France, Switzerland, and generally in the south of Europe, is striking. Instead of thick deposits covering hundreds of square miles of country, like the miocene beds bordering the Pyrenees or those of the great system of the Auvergne, we have small patches owing their preservation either to volcanic outbursts having covered them up, or to some favourable circumstance having preserved them from total denudation. Whether we are to assume with the late Prof. Edward Forbes, that the general dearth of these strata in the British Isles arose from the extent of dry land which prevailed during the long interval between the eocene and pliocene periods, or whether we assume the former existence of widespread marine deposits which have since been entirely removed, the case is not one without difficulty. At all events, the absence of representatives of this period within the British area has a tendency to prevent a due appreciation of the enormous extent of the miocene period being generally felt in this country. Nor, generally speaking, do we, I think, take a fair estimate of the remoteness in time to which we must date back the commencement of that lengthened period. Prof. Haughton, judging from the maximum observed thickness of each successive deposit, has calculated that a greater interval of time now separates us from the miocene period than that which was occupied in producing all the secondary and tertiary strata from the triassic to the miocene epoch, and, without endorsing the whole of my accomplished friend's conclusions, I incline to concur in such an estimate. When it is considered that the Ballypalidy beds of Antrim and the Lough Neagh clays are the sole representatives in Ireland of two periods of such length and importance as the miocene and pliocene, their high interest will be more apparent, and I trust that no opportunity of minutely studying them will be neglected.

There is one other point with regard to Irish geology on which it will be well to say a few words, though it is of a negative rather than a positive character. I mean the absence, so far as at present known, of palæolithic implements in this country. It is true that Prof. Hull, in the book to which I am so much indebted, speaks of a raised beach on the Antrim coast as containing worked flints of that rude form and finish known as palæolithic; but this is a slip of the pen, by which the author has fallen into the not uncommon error of applying a term which is merely significant of the age of the implements to their external character. However rude may be the workmanship of the flint implements found at Kilroot, they belong to the neolithic, and not to the palæolithic period. So far as I am aware no example of any implement belonging to the age of the mammoth, rhinoceros, and other members of the quaternary fauna has as yet been found in Ireland. Indeed, the remains of *Elephas primigenius* and its associates are of exceedingly rare occurrence in this country, though they have been found with those of bear and reindeer in the Shandon Cave near Dungarvan. It is, of course, impossible to foretell what future researches may bring to light; but judging from analogy it seems hardly probable that until ancient river-gravels containing the remains of the quaternary group of mammals are found in this island, veritable palæolithic instruments will be discovered. The association of the two classes of remains is so constant that we may fairly assume that the animals formed the principal food of the palæolithic hunters, and that any causes which lead to the absence of the one class will lead to the absence of the other also.

There, is, however, one member of that old quaternary group which is far more abundant in Ireland than it is in England or on the continent of Europe—the *Megaceros*—which has rightly received the appellation of *Hibernicus*.

I hope that we may have an opportunity, under the guidance of Mr. Richard Moss, of seeing some of the remains of this "antlered monarch of the waste" in the position in which they were originally interred, and it will be an interesting question for consideration whether these remains can be regarded as of the same geological age as those of the English caves and river-gravels, or whether they do not for the most part belong to what Prof. Boyd Dawkins has termed the pre-historic period. It seems by no means improbable that this gigantic stag survived in this country for ages after he had become extinct in other lands, and that the view held by Prof. Hull of his extinction being due to persecution by man is correct. If this be so it would seem to follow that the human occupation of Ireland is of far more recent date than that of the sister country.

And this brings me to one of those questions which have of late been occupying the attention of geologists. I mean the date which is to be assigned to the implement-bearing beds of palæolithic age in England. Dr. James Geikie has held that for the most part they belong to an interglacial episode towards the close of the glacial period, and regards it as certain that no palæolithic bed can be shown to belong to a more recent date than the mild era that preceded the last great submergence.

His follower, Mr. Skertchley, records the finding of palæolithic implements in no less than three interglacial beds, each underlying boulder clays of different ages and somewhat different characters, the Hessle, the purple, and the chalky boulder clay. This raises two main questions, first, as to how far Dr. Croll's theory of the great alternations of climate during the glacial period can be safely maintained; and secondly, how far the observations as to the discovery of implements in the so-called Brandon beds underlying the chalky boulder clay can be substantiated. Another question is how far the palæolithic deposits can be divided into those of modern and ancient valleys, separated from each other by the purple boulder clay, and the later of the two older than the Hessle beds. It would be out of place here to discuss these questions at length. I will only observe, that in a considerable number of cases the gravels containing the implements can be distinctly shown to be of much later date than the chalky boulder clay, and that if the implements occur in successive beds in the same district, each separated from the other by an enormous lapse of time, during which the whole country was buried beneath incredibly large masses of invading ice, and the whole mammalian fauna was driven away, it is a very remarkable circumstance. It is not the less remarkable because this succession of different palæolithic ages seems to be observable in one small district only, and there is as close a resemblance between the instruments of the presumably different ages as there is between those of admittedly the same date. I have always maintained the probability of evidence being found of the existence of Man at an earlier period than that of the post-glacial or quaternary river gravels, but, as in all other cases, it appears to me desirable that the evidence brought forward should be thoroughly sifted and all probability of misapprehension removed before it is finally accepted. In the present state of our knowledge, I do not feel confident that the evidence as to these three successive palæolithic deposits has arrived at this satisfactory stage. At the same time is must be borne in mind that if we make the palæolithic period to embrace not only the river gravels but the cave deposits of which the south of France furnishes such typical examples, its duration must have been of vast extent.

In connection with the question of glacial and interglacial periods, I may mention that of climatal changes in general, which has formed another subject to which much attention has of late been given. The return of the Arctic Expedition, and the reports of the geological observations made during its progress, which have been published by Captain Feilden, one of the naturalists to the expedition, in conjunction with Mr. De Rance and Prof. Heer, have conferred additional interest on the question of possible changes in the position of the poles of the earth, and on other kindred speculations. Near Discovery Harbour, about latitude 81° 40', miocene beds were found containing a flora somewhat differing from that which was already known to exist within the Arctic regions. "The Grinnell Land lignite," say the authors of the report, "indicates a thick peat moss, with probably a small lake, with water-lilies on the surface of the water, and reeds on the edges, with birches, poplars, and taxodiums on the banks, and with pines, firs, spruce, elms, and hazel-bushes on the neighbouring hills." When we consider that all of the genera here represented have their present limits at least from twelve to fifteen degrees further south, while the taxodium is now confined to Mexico and the south of the United States, such a sylvan landscape as that described seems entirely out of place in a district within six hundred miles of the pole, to which indeed, if land then extended so far, these Arctic forests must have also extended in miocene times. Making all allowance for the possibility of the habits of such plants being so changed that they could subsist without sunlight during six months of a winter of even longer duration, I cannot see how so high a temperature as that which appears necessary, especially for the evergreen varieties, could have been maintained, assuming that Grinnell Land was then as close to the North Pole as it is at the present day. Nor is this difficulty decreased when we look back to formations earlier than the miocene, for the flora of the secondary and palæozoic rocks of the Arctic regions is identical in character with that of the same rocks when occurring twenty or thirty degrees farther south, while the corals, encrinites, and cephalopods of the carboniferous limestone are such as, from all analogy, might be supposed to indicate a warm climate.

The general opinion of physicists as to the possibility of a change in the position of the earth's axis has recently undergone modifications somewhat analogous in character to those which, in the opinion of some geologists, the position of the axis has itself undergone. Instead of a fixed dogma as to the impossibility of change, we find a divergence of mathematical opinion and variations of the pole differing in extent, allowed by different mathematicians who have of late gone into the question, as, for instance, the Rev. J. F. Twisden,[1] Mr. George Darwin,[2] Prof. Haughton,[3] the Rev. E. Hill,[4] and Sir William Thomson.[5] All agree in the theoretical possibility of a change in the geographical position of the earth's axis of rotation being effected by a redistribution of matter on the surface, but they do not appear to be all in accord as to the extent of such changes. Mr. Twisden, for instance, arrives at the conclusion that the elevation of a belt twenty degrees in width, such as that which I suggested in my presidential address to the Geological Society in 1876, would displace the axis by about ten miles only, while Prof. Haughton maintains that the elevation of two such continents as Europe and Asia would displace it by about sixty-nine miles, and Sir W. Thomson has not only admitted, but asserted as highly probable, that the poles may have been in ancient times "very far from their present geographical position, and may have gradually shifted through ten, twenty, thirty, forty, or more degrees without at any time any perceptible sudden disturbance of either land or water."

I am glad to think that this question, to which I to some extent assisted to direct attention, has been so fully discussed, but I can hardly regard its discussion as being now finally closed. It appears to me doubtful whether eventually it will be found possible to concede to this globe that amount of solidity and rigidity which at present it is held to possess, and which, to my mind at all events, seems to be in entire disaccordance with many geological phenomena. Yet this, as the Rev. O. Fisher[6] has remarked, is presupposed in all the numerical calculations which have been made. I am also doubtful whether in the calculations which have been made, sufficient regard has been shown to the fact that a great part of the exterior of our spheroidal globe consists of fluid which, though of course connected with the more solid part of the globe by gravity, is readily capable of readjusting itself upon its surface, and may, to a great extent, be left out of the account in considering what changes might arise from the disturbance of the equilibrium of the irregular spherical or spheroidal body which it partially covers. It appears to me also possible that some disturbances of equilibrium may take place in a mysterious manner by the redistribution of matter or otherwise in the interior of the globe. Capt. F. J. Evans,[7] arguing from the changes now going on in terrestrial magnetism, has suggested the possibility of some secular changes being due to internal, and not to external causes; and it really be true that there is a difference between the longest and shortest equatorial radii of

[1] *Quart. Journ. Geol. Soc.*, 1878, p. 35.
[2] *Proc.* R. S., vol. xxv. p. 328. *Phil. Trans.*, clxvii. p. 271.
[3] *Proc.* R. S., 1877, 1878.
[4] *Geol. Mag.*, June, 1878.
[5] Rep. Brit. Assoc., 1876, p. 11.
[6] *Geol. Mag.*, July, 1878.
[7] NATURE, vol. xviii. p. 80.

the earth, amounting to six thousand three hundred and seventy-eight feet,[1] such a fact would appear to point to a great want of homogeneity in the interior of our planet, and might suggest a possible cause for some disturbance of equilibrium.

I have mentioned Prof. Haughton among those who, from mathematical considerations, have arrived at the conclusion that a geographical change in the position of the axis of rotation of the earth is not only possible but probable. In a recent paper, however, he has maintained that notwithstanding this possibility or probability, we can demonstrate that the pole has not sensibly changed its position during geological periods. He arrives at this conclusion by pointing out that in the Parry Islands, Alaska and Spitzbergen, there are triassic and Jurassic deposits of much the same tropical character, and then by a geometrical method fixing the north pole somewhere near Pekin, and the south pole in Patagonia, within seven hundred miles of a spot where Jurassic ammonites occur, shows that such a theory is untenable. In the same way he fixes the pole in miocene times near Yakutsk, within eight hundred miles of certain miocene coal-beds of the Japanese islands. These objections are at first sight startling, but I think it will be found that if, instead of drawing great circles through certain points, we regard those points as merely isolated localities in a belt of considerable width, there is no need of fixing the pole of either the Jurassic or the miocene period with that amount of nicety with which Prof. Haughton has ascertained its position. The belt may indeed be made to contain the very places on which the objection is founded. Still the method proposed is a good one, and I hope that as our knowledge of foreign geology extends it may be still further pursued. There is, however, one farther consideration to be urged, and that is as to the safety of regarding all deposits of one geological period as contemporaneous in time. Although an almost identical flora may be discovered in two widely-separated beds, it appears to me that chronologically they are more probably of different ages than absolutely contemporaneous; and, inasmuch as the duration of the miocene period must have been enormous, there would be time—if once we assume the wandering of the poles—for such wandering to have been considerable between the beginning and end of the period.

I must not, however, detain you longer upon this phase of geological speculation, but will advert to a subject of more practical interest, the discovery of palæozoic rocks under London. So long ago as 1856 the Kentish Town boring had shown that immediately below the gault red and variegated sandstones and clays occurred, which Professor Prestwich regarded as probably of old red or Devonian age. The boring of Messrs. Meux & Co. has now shown that under Tottenham Court Road, at a depth of little more than nine hundred feet from the surface, there are true Devonian beds, with characteristic fossils, and that Mr. Godwin Austen's prophecy of the existence of palæozoic rocks at an accessible depth under London has proved true. Prof. Prestwich, from a consideration of the French and Belgian coal-fields, inclines to the belief that in the district north of London carboniferous strata may be found. Unfortunately the expense of conducting deep borings, even with the admirable appliances of the Diamond Boring Company, is so great that I almost despair of another experimental borehole, like that carried out in the Wealden district under the auspices of Mr. Willett, being undertaken.

In the department of theoretical geology I would call your attention to some experiments by M. Daubrée, of which he has given accounts at different times to the French Academy of Sciences. In these experiments he has attempted to reproduce on a small scale various geological phenomena, such as faulting, cleavage, jointing, and the elevation of mountain chains. Although the analogy between work in the laboratory and that on the grand scale of nature may not in all cases be perfect, yet these experiments are in the highest degree instructive, and reflect no little credit on the ingenuity of the distinguished chief of the École des Mines.

With regard to recent progress in palæontology, I must venture to refer you to Prof. Alleyne Nicholson's inaugural address lately delivered to the Edinburgh Geological Society, but I cannot pass over in silence the magnificent discoveries in North America, which are principally due to the researches of Profs. Marsh, Leidy, and Cope. The *Diceratherium*, a rhinoceros with two horns placed transversely, and the *Dinoceras*, somewhat allied to the elephant, but with six horns, arranged in pairs, are as marvellous as some of the beasts seen by Sir John Maundeville on his travels, or heard of by Pliny. But perhaps the most remarkable series of remains ever discovered are those which so completely link the existing horse with the *Eohippus* and *Orohippus*, and still farther extend the pedigree of the genus *Equus*, which had already been some years ago so ably traced by Prof. Huxley.

Of these American discoveries, as well as those made in the tertiary beds of Europe, M. Albert Gaudry has largely availed himself in his recent beautiful volume on the links in the animal world in geological times, a work which will long be a text-book on the inter-relation of different orders, genera, and species. I am tempted to make use of some portions of M. Gaudry's own analysis of the book, which he communicated to the Geological Society of France. Beginning with the marsupials of the close of the secondary and beginning of the tertiary period, he shows that they are succeeded by such animals as the *Pterodon*, the *Hyænodon*, the *Proviverra*, and *Arctocyon*, which present a mixture of marsupial and placental characters, and to some extent justify a theory of the transition from one order to the other. He next examines the marine mammalia, and points out that, so far as at present known, they make their appearance later than those of the land, and that the examination of the pelvis of the *Halitherium* tends to support the idea that the mammals, such as the sirenians, which at the present day have no hind limbs, are descended from terrestrial quadrupeds, for those limbs in the *Halitherium* are much less reduced than in its recent successors, the dugong and manatee. After tracing the numerous links which are to be found between the extinct and living pachydermata, he proceeds to show that, notwithstanding the great distance between them and the ruminants, transitions may be seen. The earliest ruminants were devoid of horns and antlers, but possessed upper incisors, and by a comparison of the molars of different genera it may readily be conceived how the large bosses of the omnivorous teeth of the pachyderms gradually shaded into the small crescents of the teeth of the ruminants. At the same time the passage from the heavy and complicated extremities of the limbs of the pachyderms to the simpler and lighter feet of the ruminants can be traced. The history of the horse family is also discussed, and the descent of existing proboscidians from the mastodonts is shown to be probable, though the previous forms from which the mastodonts and dinotheria are derived are as yet unknown. Nor can the origin of the carnivora as yet be suggested, though passages between the six existing families of the order may be observed. In conclusion M. Gaudry devotes a chapter to the quadrumana, and thinks that palæontological observations tend to diminish the isolation in which these mammals now stand with regard to the other orders.

One of the most important features insisted on by M. Gaudry is that to which I have already alluded—the development of the complicated molars of most mammals. His view is that by a comparison with early and with fœtal forms the probability may be shown of these compound teeth being made up of what in earlier forms were simple teeth—or, as he has termed them, denticules—which have coalesced in the same manner as have some other parts of the normal bony skeleton. In the compound teeth the denticules in some cases preserve their original conical form, as in the pig tribe; in others are elongated transversely, so as by their junction to form ridges, as in the tapirs; while in others, again, they are drawn out into longitudinal crescents, as in the ruminants. Between these forms there are, of course, innumerable transitions. They do not, however, appear to me to affect the importance of M. Gaudry's observations, which must be regarded as of the highest value in all attempts to trace the inter-relation of different forms of mammalian life. I must not, however, detain you longer on this subject, as I trust that I have said enough to show the importance and interest of this book.

The discoveries of early forms of birds with teeth do not come within M. Gaudry's province; but Prof. Marsh has largely added to our knowledge of these remarkable forms. The tertiary *Odontopteryx toliapicus* from Sheppey, described by Prof. Owen, seems rather to be endowed with bony tooth-like processes in the jaw than actual teeth, and the head of the *Argillornis* from the same locality is at present unknown. But the *Hesperornis* and *Ichthyornis* from the cretaceous beds of America possess veritable teeth, in the one case set in a long groove in the jaw, and in the other in actual sockets. Such intermediate, or, as Prof. Huxley would term them, intercalary, forms, tend

[1] Thomson and Tait, "Phil." p. 648.

materially to bridge over the gap which at first sight appears to exist between reptiles and birds, but which to many palæontologists was far from being impassable, long before the discoveries just mentioned. The amphicœlous character of the vertebræ of *Ichthyornis* presents another most remarkable peculiarity, which is also of high significance. I hear rumours of the discovery of another *Archæopteryx* in the Solenhofen Slates, which is said to present the head in a much more complete condition than that in which it occurs on the magnificent slab now in the British Museum. As yet, I believe, the jaws have not had the matrix removed from them; but should they prove to be armed with teeth, it will to me be a cause of satisfaction rather than surprise, as confirming an opinion which some fifteen years ago[1] I ventured to express, that this remarkable creature may have been endowed with teeth, either in lieu of or combined with a beak.

I must not, however, detain you longer with any of these general remarks, which are, moreover, becoming somewhat egotistic, but will now proceed to the business of this section, in which I hope that more than one paper of great value and interest will be forthcoming.

SECTION D.

BIOLOGY.

OPENING ADDRESS IN THE DEPARTMENT OF ZOOLOGY AND BOTANY, BY PROF. W. H. FLOWER, F.R.S., PRESIDENT OF THE SECTION.

On the 10th of January, 1778, died the great Swedish Naturalist, Charles Linné, more commonly known as Linnæus, a name which will ever be mentioned with respect and regard in an assembly devoted to the cultivation of the sciences of Zoology and Botany, as whatever may be the future progress of those sciences, the numerous writings of Linnæus, and especially the publication of the "Systema Naturæ," can never cease to be looked upon as marking an era in their development. That work contained a systematic exposition of all that was known on these subjects expressed in language the most terse and precise. The accumulated knowledge of all the workers at zoology, botany, and mineralogy since the world began, was here collected together by patient industry, and welded into a complete and harmonious whole by penetrating genius.

Exactly a century has passed since Linnæus died. What of the progress of the subjects to which he devoted his long and laborious life? This one century is a brief space compared with the ages which have passed since man began to dwell upon the earth, surrounded by living objects, which have, more and more as time rolled on, awakened his curiosity, stimulated his faculties to observe, and impelled him to record the knowledge so gained for the benefit of those to come. How does it stand in comparison with those which preceded it, in the contributions it has thus acquired and recorded?

It may be not without interest in commencing our work at this meeting to cast our eyes back and take stock, as it were, of the knowledge of a hundred years ago, and of that of the present time, and see what advances have been made; to look at the living world as it was known to Linnæus and as it is known to ourselves. The "Systema Naturæ," the last edition of which, revised by the author, was published in 1766, will be a convenient basis for the comparison, but as the subject is one which, even in a most superficial outline, might reach such lengths as would well tire out the most patient of audiences, and absorb time which will be more profitably occupied by the valuable contributions which are forthcoming from other members of the Association, I will merely take a small section of the work, about 100 pages out of the first of the four volumes, those devoted to the first class Mammalia. The comparison of this part is perhaps the easiest, as the contrast is the least striking, and the progress has been comparatively the slowest. The knowledge of large, accessible, and attractive-looking animals had naturally preceded that of minute and obscure organisms, and hence, while in many other departments the advance has altogether revolutionized the knowledge of Linnæus, in the vertebrated classes, especially the one of which I shall now speak, it has only extended and reformed it.

In taking the "Systema Naturæ" of Linnæus, the comparison is certainly carried back somewhat beyond the hundred years which have elapsed since his death, and the brilliant contributions to the knowledge of the Mammalia of Buffon and Daubenton just then beginning to be known, and the systematic compilation of Erxleben (published in 1777), are ignored, but for the present purpose, especially considering the limited time at my disposal, it will be best not to go beyond the actual text of the work in question.

Before considering systematically the different groups into which Linnæus divides the class, I must remark in passing upon what is the greatest, and indeed most marvellous difference between the knowledge of zoology of our time and that of Linnæus. Now we know that the animals at present existing upon the earth are merely the survivors of an immensity of others, different in form, characters, and mode of life, which have peopled the earth through vast ages of time, and to which numerically our existing forms are infinitesimally small, and that the knowledge we possess of an immense number of them, fully justifies the expectation of an enormous further advance in this direction. In the time of Linnæus the existence in any past time of a species having no longer living representatives on the earth, though perhaps the speculation of a few philosophical minds, had not been received among the certainties of science, and at all events found no place in the great work we are now considering.

In the twelfth edition of the "Systema Naturæ" we find the class Mammalia divided into seven orders: I. *Primates*, II. *Bruta*, III. *Feræ*, IV. *Glires*, V. *Pecora*, VI. *Belluæ*, VII. *Cete*. These orders contain forty genera without any intermediate subdivisions. The genera are again divided into species, of which the total number is 220.

The first order, PRIMATES, contains four genera: *Homo*, *Simia*, *Lemur*, and *Vespertilio*.

The vexed question of man's place in the zoological system was thus settled by Linnæus. He belongs to the class *Mammalia*, and the order *Primates*, the same order which includes all known monkeys, lemurs, and bats: he differs only generically from these animals. But then we must remember that the Linnæan genera were not our genera, they correspond usually to what we call families, sometimes to entire orders. So that practically man's position is much the same as that to which, after several vicissitudes, as his separation as an order by Blumenbach and Cuvier, or as a subclass by Owen, he has returned in the systems of nearly all the zoologists of the present day who treat of him as a subject for classification upon zoological and not metaphysical grounds.

Yet since the time of Linnæus the whole science of anthropology has been created. There is certainly an attempt at the division of the species *Homo sapiens* into six varieties in the "Systema Naturæ," but it has scarcely any scientific basis. Zoological anthropology may be said to have commenced with Blumenbach, who, it is interesting to recall as an evidence of the rapid growth of the science, was a contemporary with most of us in this room, for he died as lately as 1840, although his first work on the subject, "De generis humani varietate nativa," was published three years before the death of Linnæus, too late, however, to influence the work we are now chiefly speaking of. The scientific study of the natural history of man is therefore, we may say, but one century old. To what it has grown during that time you are probably aware. Scarcely an important centre of civilisation in the world but has a special society devoted to its cultivation. It forms by itself a special department of the Biological Section of our Association, a department of such importance, that on this occasion no less distinguished a person than a former most eminent president of the whole Association was thought fit to take charge of it. From him you will doubtless hear what is its present scope, aim, and compass. I need only remind you that except the one cardinal point of the zoological relation of man to other forms of life, which Linnæus appears to have appreciated with intuitive perception, all else that you will now hear in that department was not dreamt of in his philosophy.

As might naturally be supposed, apes and monkeys have, for various reasons, attracted the attention of observers of nature from very early times, and consequently Linnæus was able to give rather a goodly list of species of these animals, amounting to thirty-three, but of their mutual affinities, and of the important structural differences which exist between many of them, he seems to have had no idea, his three divisions being simply regulated by the condition of the tail, whether absent, short, or long.

We now know that the so-called anthropoid or man-like apes,

[1] *Nat. Hist. Rev.*, vol. v. p. 421.

the gorilla, chimpanzee, orang and gibbons, form a group apart from all the others of such importance, that everything related to their history, structure, and habits, has been most assiduously studied, and there is now an immense literature devoted to this group alone. Nothing could better illustrate the advances we have made in a hundred years, than the contrast of our present knowledge of these forms with that of Linnæus. It is true that, as shown in the most interesting story of the gradual development of our knowledge relating to them in the first chapter of Huxley's "Man's Place in Nature," the animal now called gorilla was, without doubt, the pongo, well known to, and clearly described by, our countryman, Andrew Battle, a contemporary of Shakespeare; and that a really accurate and scientific account of the anatomy of the chimpanzee had been published as far back as 1699 by Dr. Edward Tyson, who as the first English comparative anatomist, I am proud to claim as in some sort a predecessor in the chair I have the honour to hold in London, as he is described on the title page of his work as "Reader of Anatomy at Chirurgeons' Hall."

Linnæus was, however, not acquainted with these, and his second species of the genus *Homo*, *H. troglodytes*, and his first of the genus *Simia*, *S. satyrus*, were both made up of vague and semi-fabulous accounts of the animals now known as chimpanzees and orangs, but hopelessly confounded together. Of the gorilla, and what is stranger still, of any of the important genus of gibbons or long-armed apes of South-eastern Asia, he had at the time he revised the "Systema" no idea.

The remaining monkeys, we now know, fall into three very distinct sections: the *Cercopithecidæ* of the Old World, and the *Cebidæ* and *Hapalidæ* of the New, or by whatever other names we may like to designate them. Although members of all three groups appear in the list in the "Systema," they are all confusedly mixed together. Even that the American monkeys belong to a totally different stock from those of the Old World, does not seem to have been suspected.

The genus *Lemur* of Linnæus comprehends five species, of which the first four were all the then known forms of a most interesting section of the Mammalia. These animals, mostly inhabitants of the great island of Madagascar, though some are found in the African continent, and others in some of the Southern and Eastern parts of Asia, constitute a well-defined group, but one of which the relations are very uncertain. At one time, as in the system of Linnæus, they were closely associated with the monkeys. As more complete knowledge of their organisation has been gradually attained, the interval which separates them structurally from those animals has become continually more evident, and since they cannot be placed within the limits of any of the previously constituted orders, it has been considered advisable by some naturalists to increase the ordinal divisions in their behalf and to allow them to take rank as a distinct group, related to the *Primates* on the one hand, and to the *Carnivora* and *Insectivora* on the other. The knowledge of their relations, however, bids fair to be greatly increased by the discoveries of fossil forms lately made both in France and America, some of which seem to carry their affinities even to the *Ungulata*.

Existing upon the earth at present, besides the more ordinary lemurs to which the species known to Linnæus belong, there are two aberrant forms, each represented by a single species. These are the little *Tarsius* of Borneo and Celebes, and the singular *Chiromys*, or Aye-aye, which, though an inhabitant of the head-quarters of the group, Madagascar, and living in the same forests and under the same conditions as the most typical lemurs, exhibits a most remarkable degree of specialization in the structure both of limbs and teeth, the latter being modified so as to resemble, at least superficially, those of the Rodents, a group with which in fact it was once placed. It was discovered by Sonnerat in Madagascar in 1780, two years after the death of Linnæus. The specimen brought to Paris by this traveller was the only one known until 1860. Since that date, however, its native land has been more freely open than before to explorers, and many specimens have been obtained, one having lived for several years in the Gardens of the London Zoological Society.

The history of a name is often not a little curious. Linnæus applied the term *Lemures*, *i.e.*, the departed spirits of men, to these animals on account of their nocturnal habits and ghost-like aspect. The hypothetical continent in the Indian Ocean, supposed to have connected Madagascar with the Malayan Archipelago is called by Mr. Sclater, *Lemuria*, as the presumed original home of the lemur-like animals. Although the steps are not numerous, it might puzzle a classical scholar, ignorant of zoology, to explain the connection between this continent and the Roman festival of the same name.

The fifth animal which Linnæus places in his genus *Lemur*, under the name of *L. volans*, is the very singular creature to which the generic term *Galeopithecus* has since been applied. It is one of those completely aberrant forms, which having no near existing relations, and none yet discovered among extinct forms, are perfect puzzles to systematic zoologists. It is certainly not a lemur, and not a bat, as has been supposed by some. We shrink from multiplying the orders for the sake of single genera containing only two closely allied species; so we have generally allowed it to take refuge among the *Insectivora*, though without being able to show to which of that somewhat heterogeneous group it has any near affinities.

The fourth genus of the *Primates* is *Vespertilio*, comprising six species of bats. This genus has now by universal consent expanded into an order, and one of the best characterised and distinctly circumscribed of any in the class: indeed those who have worked most at the details of the structure of bats, find so much diversity in the characters of the skull, teeth, digestive organs, &c., associated with the modification of the fore-limbs for flight common to all, as almost to entitle them to be regarded rather as a sub-class. Anatomical, as well as palæontological evidence, show that they must have diverged from the ordinary mammalian type at a very far distant date, as the earliest known forms, from the eocene strata, are quite as specialised as any now existing, and no trace has hitherto been discovered of forms linking them to any of the non-volant orders. By the publication within the last few weeks of a valuable monograph on the existing species of the group, entitled "A Catalogue of the Chiroptera in the Collection of the British Museum," by G. E. Dobson, we are enabled to contrast our present knowledge with that of the time of Linnæus. Although the author has suppressed a large number of nominal species which formerly encumbered our catalogues, and wisely abstained from the tendency of most monographists to multiply genera, he describes four hundred species, arranged in eighty genera: nearly double the number of species, and exactly double the number of genera, of the whole class Mammalia in the "Systema Naturæ," and these Dr. Günther remarks in his preface are probably only a portion of those existing. The small size, nocturnal habits, and difficulty of capture of these animals, are sufficient reasons for the supposition that there are still large numbers unknown to science. In the list of Linnæus, the first primary group of Dobson, the *Megachiroptera*, now containing seventy species, is represented by a single one *V. Vampyrus*, obviously a *Pteropus*, to which the bloodthirsty habits of the fabulous vampyre are attributed, but which is not absolutely identified with any one of the known species. The other species described by Linnæus can almost all be identified with bats at present well known.

A curious example of the results of basing classification upon a few, and those somewhat artificial characters, is afforded by one of the true bats, now called *Noctilio leporinus*, though admitted by Linnæus to be "*simillimus vespertilionibus similiter pedibus alatus*," being separated from the others, not only generically, but even placed in another order, that of the *Glires* or Rodents, because it did not, or was supposed not to, fall under the definition of the order *Primates*, which begins "*Dentes primores incisores superiores IV. paralleli.*' In reality this bat has four upper incisors, but the outer ones are so small as to have been overlooked when first examined. But even if this were not so no one would now dream of basing an animal's position upon such a trivial character when opposed to the totality of its organization and habits.

The characters of the incisor teeth are placed in the first rank in the definitions of all the orders in the "Systema Naturæ," and hence the next order called BRUTA, characterized by "*dentes primores nulli superius aut inferius*," contains a curious mixture of heterogeneous animals, as the names of the genera *Elephas*, *Trichechus*, *Bradypus*, *Myrmecophaga*, *Manis*, and *Dasypus* will indicate. In contains, in fact, all the animals then known comprised in the modern orders of *Proboscidea*, *Sirenia*, and *Edentata*, together with the walrus, one of the *Carnivora*. The name *Bruta* has been revived for one of these orders, that more generally called *Edentata*, but I think very inappropriately, for it was certainly not equivalent, and if retained at all, should rather belong to the Proboscidea, as *Elephas* stands first in the list of genera, and was probably in the mind of Linnæus when he assigned the name to the group.

It is curious to find that the striking differences between the African and the Indian elephants, now so well understood by every beginner in zoology, and all the facts which have already been accumulated relating to the numerous extinct forms of Proboscideans, whether Mammoths, Mastodons, or Dinotheria, were quite unknown to Linnæus. One species only, *Elephas maximus*, represented in the zoology of a hundred years ago, was all that was known of the elephants or elephant-like animals.

The genus *Trichechus* of this edition exhibits a very curious phase of zoological knowledge. It contains two species. (1) *T. rosmarus*, the Walrus, now known to be a modified seal, and therefore a member of the Linnæan order Feræ, and (2) *T. manatus*, a name under which were included all the known forms of Manatees and Dugongs, in fact the whole of the modern order *Sirenia*; animals widely removed in all essential points of their organisation from the walrus, with which they are here generically united. Their position, however, between the elephant on the one hand and the sloths on the other, is far better than their association with the Cetacea, as in Cuvier's system, an association from which it has been most difficult to disengage them, notwithstanding their total dissimilarity, except in a few external characters. Although the discovery of many fossil forms has done much to link together the few existing species and to show the essential unity of the group, it has thrown no light upon their origin, or their affinities to other mammals. They still stand, both by their structure and their habits, a strangely isolated group, and it baffles conjecture to say whence they have been derived, or how they have attained their present singular organisation.

The remaining genera of the Linnæan order *Bruta* constitute the group out of which Cuvier, following Blumenbach, formed his order *Edentata*, a name certainly not happily chosen for a division which includes species like the great armadillo, having a larger number of teeth than any other land mammal, but which, nevertheless, has been so generally adopted, and is so well understood, that to attempt to change it would only introduce an element of confusion. Four out of five of the principal modifications of form in the group at present known, are indicated by the four Linnæan genera, *Bradypus* or Sloth, *Myrmecophaga* or Ant-eater, *Manis* or Pangolin, and *Dasypus* or Armadillo. The advances during the century have consisted in the accumulation of a great mass of details respecting these groups, the addition of a fifth and very distinct existing form, the *Orycteropus* or Cape Anteater, and the discovery of numerous and very remarkable extinct forms, such as the megatheriums and glyptodons of South America, so fully known by their well-preserved osseous remains. There is, however, still much to be done in working out the real relationship of the somewhat isolated members of the order, if it be a natural order, both to each other, and to the rest of the Mammalia, from which they stand widely removed in many points of organisation.

The third order of Linnæus, FERÆ, contained all the then known animals, which, with whatever diversities of general structure, agreed in their predatory habits, and possessed certain general characters of teeth and claws to correspond, though the terse definition of "*Dentes primores superiores sex, acutiusculi, canini solitarii*," is by no means universally applicable to them. This order was broken up by Cuvier into the orders Carnivora and Insectivora, and the genus *Didelphys*, included in it by Linnæus, has been since by universal assent removed to another group.

The first six genera belong to the very well-defined and probably natural group now called *Carnivora*. The one placed at the head of the list, *Phoca*, is equivalent to the large and important modern sub-order *Pinnipedia*, the walrus, however, though essentially a seal, having been, as before mentioned, relegated by Linnæus to another order on account of its aberrant dentition. But three species are recorded in the genus. *P. ursina*, the Sea-bear of the North Pacific (now *Otaria ursina*), *P. leonina*, founded on Anson's sea-lion, now commonly called the elephant seal, or sea-elephant (*Macrorhinus proboscideus*, or more properly *leoninus*), and *P. vitulina*, the Common Seal.

The terrestrial sub-order of Carnivora is represented by five genera. (1) *Canis*, including the dog, wolf, hyæna, fox, arctic fox, jackal, &c. (2) *Felis*, with only six species, but still one of the few Linnæan genera, which covers exactly the same ground as at present in the opinion of the majority of zoologists, although it may be mentioned as an example of the tendency towards excessive and unnecessary multiplication of generic names which exists in some quarters, that it has been divided into as many as fourteen. (3) *Viverra*, a heterogeneous group, containing ichneumons, coatis, and skunks, animals belonging to three very distinct families, according to modern ideas. (4) *Mustela*, a far more natural group, being nearly equivalent to the modern family *Mustelidæ*; and, lastly, a very comprehensive genus, *Ursus*, consisting of *U. meles*, the Badger, *U. lotor*, the Raccoon, *U. luscus*, the Wolverene, and all the true bears known, comprised in the single species *U. arctos*. Many interesting forms of Carnivora, as *Cryptoprcta*, *Proteles*, *Eupleres*, *Ailurus* and *Ailuropus*, have no place in the Linnæan system, being comparatively modern discoveries. The very recent date (1869) at which the last-named remarkable animal was made known to science by the enterprising researches of the Abbé David into the fauna of Eastern Thibet, gives hope that we may not yet be at the end of the discovery of even large and hitherto unsuspected forms of existing mammals.

Next in the Linnæan system comes the genus *Didelphys*, constituted for the reception of five species of American opossums. This is a very interesting landmark in the history of the progress of the knowledge of the animal life of the world, as these five opossums, forming a genus in the midst of the order Feræ, were all that was then known of the great sub-class *Marsupialia*, now constituting a group entirely apart from the ordinary members of the class. It is difficult now to imagine an animal world without kangaroos, without wombats, without phalangers, without thylacines, without dasyures, and so many other familiar forms, and yet such was the animal world known to Linnæus. It is true that a species of kangaroo from one of the islands of the Austro-Malayan Archipelago was described as long ago as 1714 by De Bruijn, who saw it alive at the house of the Dutch governor of Batavia, and that Captain Cook and Sir Joseph Banks saw and killed kangaroos on the east coast of Australia in 1770, and had published figures and descriptions of them in 1773, or five years before the death of Linnæus, but the work we are now considering contains no traces of knowledge of the existence of such a remarkable and now so well-known animal.

The three remaining genera of FERÆ, *Talpa*, *Sorex*, and *Erinaceus*, contained all the known species of the present order INSECTIVORA, which now embraces many and very varied forms, quite unsuspected a century ago, and to which it is probable that others will be added by the time the exploration of the animal products of the world is completed.

The fourth order, GLIRES, has remained practically unchanged to our day, although the name *Rodentia* has generally superseded that bestowed upon it by Linnæus. The five genera of the "Systema Naturæ," *Hystrix Lepus*, *Castor*, *Mus*, and *Sciurus*, have been vastly increased, partly by subdivision and partly by the discovery of new forms. *Noctilio* is, as before mentioned, removed to the Chiroptera, but its loss is well compensated for by *Hydrochærus*, the well-known Capybora, the largest existing member of the group, which in the Linnæan system is placed among the Belluæ, in the same genus with the pigs.

The fifth Linnæan order, PECORA, is a fairly natural group, equivalent to Cuvier's *Ruminantia*; but it is no longer considered of the value of an order, since the animals composing it have now been shown to be as closely related to certain of those belonging to the next order as they are to each other. The first genus, *Camelus*, contains both the American lamas and the Old World camels, the demonstration of the common origin and close affinities of which has been one of the important results of the recent discoveries in the palæontology of the Western continent. In the next genus, *Moschus*, were placed the well-known musk deer of the highlands of Central Asia, and two small African antelopes, which have no special affinity with it. The subsequent inclusion in the same genus of the small chevrotains (*Tragulinæ*), which was very natural at the time, as they agree perfectly with the musk in the absence of horns and the presence of large canine tusks, by which artificial characters the genus was defined by Linnæus, was one of those unfortunate associations which has greatly retarded the progress of knowledge of the true affinities of the group. Judging by the popular works on Zoology, it is still as difficult to apprehend that a chevrotain is not a musk deer, as it is that a manati is not a cetacean; both errors of the same kind, if not quite so gross, as that of regarding a whale as a fish, or a bat as a bird. The genus *Cervus* contains six species of true deer, including the moose, reindeer, red deer, fallow and roe, associated with the giraffe.

The twenty-one species at that time recognised of the great group of hollow-horned Ruminants are distributed quite artificially in three genera, *Capra*, *Ovis*, and *Bos*. Though subsequent investigations have greatly increased the number of species known, we are still in much uncertainty about their mutual affinities and generic distinctions. Being a group of comparatively modern origin, and only just attaining its complete development, variation has chiefly affected the less essential and superficial organs, and the process of extinction of intermediate forms has not operated sufficiently long to break it up into distinctly separated natural minor groups, as is the case with many of the older families, which yield, therefore, far more readily to the needs of systematic classification, especially as long as the extinct forms are unknown or ignored.

The sixth order of land mammals, BELLUÆ, corresponding to the *Pachydermata* of Cuvier, contains what is now known to be a heterogeneous collection, viz., the horses, the hippopotamus, the pigs, rhinoceros, and the rodent capybara. The abolition of these two last orders and the entire rearrangement of the ungulate mammals into two different natural groups, now called *Artiodactyla* and *Perissodactyla*, first indicated by Cuvier in the "Ossimens Fossiles," from the structure of the limbs alone, and afterwards confirmed by Owen from comparison of every part of the organisation, has been one of the most solid advances made in our knowledge of the relations of the mammalia during the present century.

The past history of this, as of so many other groups of vertebrated animals, has been brought to light in an unexpected manner by the wonderful discoveries of fossil remains made during the last ten years in the Rocky Mountains of America, discoveries the importance of which will only be fully appreciated when the elaborate and beautifully illustrated work which Prof. Marsh has now in progress is completed.

The last Linnæan order, CETÆ, is exactly conterminous with the order so named, or rather more generally modified to *Cetacea*, in the best modern systems, for Linnæus did not commit the error of Cuvier and others, of including the Sirenia among the whales. His knowledge of the animals composing the group was necessarily very imperfect, indeed it is only within the last few years, especially since the impulse given to their study by Eschricht of Copenhagen, that the great difficulties which surround the investigation of the structure and habits of these denizens of the open sea have been so far surmounted that we have begun to obtain clear views of their organisation, affinities, and geographical distribution.

Two most remarkable forms of mammals, so abnormal in their organisation as now to be generally considered deserving the rank of a distinct sub-class, the *Echidna* and *Ornithorhynchus*, were first made known to science in 1792 and 1799 respectively, and consequently have no place in the "Systema Naturæ." The very recent discovery of a third form to this group, or at least a very striking modification of one of the forms, the large New Guinea echidna (*Acanthoglossus bruijnii*), is the last important acquisition to our knowledge of the class.

In this brief review of the progress of one small section of one branch of zoological knowledge it will be seen that it is chiefly of systems of arrangement, of classification, and of names that I have been treating. By many biologists of the present day these are looked upon as the least attractive and least profitable branches of the subject. The interest of classification, though it has lost much in some senses by the modern advances of scientific biology, has, however, gained vastly in others. The idea that has now, chiefly in consequence of the writings of Darwin, taken such strong hold upon all working naturalists—the idea of a gradual growth and progressive evolution, and therefore genetic connection between all living things—breaks down the artificial barriers which zoologists raise around their groups, and shows that such names as *species*, *genera*, *families*, *orders*, &c., are merely more or less clumsy attempts to express various shades of differences among creatures connected by infinite gradations, and in this sense destroys the importance attached to them by our predecessors. On the other hand, it immensely increases the interest contained in the word relationship, as it implies that the word is used in a real and not, as formerly, in a metaphorical sense. There is a kind of classification, such as we might apply to inanimate substances or manufactured articles. We may say, for instance, that a tumbler, a wine-glass, and a tea-cup are more closely related to each other than either one is to a chair or a table, and that they might be formed into one group, and the last-named objects be placed in a second. This kind of classification is certainly useful in its way for methodical arrangement and descriptive purposes. It is the kind of arrangement which Linnæus and his contemporaries applied to animals. It is, however, a very different classification from that which supposes that the members of a group having common essential characters are descended from a common ancestor, and have gradually, by whatever cause or means, become differentiated from other groups. On this view a true classification, if it could be obtained, would be a revelation of the whole secret of the evolution of animal life, and it is no wonder that many are willing to devote so large a share of their energies to endeavour to attain it.

The right application of the principles of nomenclature, first clearly established by Linnæus, to the groups we form is, again, by no means to be despised, as laxity and carelessness in this respect are becoming more and more the greatest hindrances to the study of zoology. The introduction of any new term, especially a generic name, and indeed the use of an old one by any person whose authority carries weight, has an appreciable effect upon the progress of science, and should never be done without a full sense of the responsibility incurred. All beginners are puzzled and often repelled by the confused state of zoological nomenclature to an extent to which those who have advanced so far as only to care for the things, and to whom the actual names by which they are called are comparatively indifferent, have little idea. Those whose special gift or inclination leads them to the pursuit of other branches of biology, as morphology, physiology, embryology, &c., must have definite names for the objects they observe, depict, or describe, and are dependent upon the researches of the systematic zoologist for supplying them, and should not neglect to take his counsel, otherwise much of their work will lose its value.

Several times has the British Association thought this a worthy subject for the consideration of its members, and through the instrumentality of a committee of working naturalists drew up in 1842 an excellent code of regulations and suggestions on the subject of zoological nomenclature. These rules were revised and reprinted in 1865, and in accordance with a resolution adopted at the last annual meeting at Plymouth they have been again republished at the cost of the Association during the present year. The mere issue of such rules must have had a beneficial effect, as they have undoubtedly been a guide to many careful and conscientious workers. Unfortunately there are no means of enforcing them upon those of a different class, and there is still something wanting short of enforcing them, which possibly may be within the power of the Association to effect. In the administration of the judicial affairs of a nation, besides the makers of the laws, we have an equally essential body to interpret or apply the law to particular cases—the judges. However carefully compiled or excellent a code of regulations may be, dubious and difficult cases will arise, to which the application of the law is not always clear, and about which individual opinions will differ. The necessary permission given in the Association rules to change names which are either "glaringly false," or not "clearly defined," opens the door to considerable latitude of private interpretation. As what we are aiming at is simply convenience and general accord, and not absolute justice or truth, there are also cases in which the rigid law of priority, even if it can be ascertained, requires qualification, and other cases in which it may be advisable to put up with a small error or inconvenience to avoid falling into a larger one. I may name such cases as the propriety of reviving an obsolete or almost unknown name for one which, if not strictly legitimate, has been universally accepted, or the retention of a name when already applied to a different genus, instead of the institution of another in its place. For instance, should the name *Echidna*, by which the well-known monotrematous mammal is known in every text-book and catalogue in every language, be superseded by *Tachyglossus*, because the former name had previously been applied to a genus of snakes? or should the chimpanzee be no longer called *Troglodytes* lest it should be confounded with a wren? Should *Chiromys* be discarded for *Daubentonia*, *Trichechus* for *Odobenus*, and *Tapirus* for *Hydrochærus?* Should the Java slow lemur be called *Loris*, *Stenops*, or *Nycticebus?* Should Sowerby's whale be placed in the genus *Physeter*, *Delphinus*, *Delphinorhynchus*, *Heterodon*, *Diodon*, *Aodon*, *Nodus*, *Ziphius*, *Micropterus*, *Micropteron*, *Mesodiodon*, *Dioplodon*, or *Mesoplodon*, in all of which it may be found in various systematic lists? Should one of the largest and best known of the Cetaceans of our seas be called *Balænoptera musculus*, *Physalus antiquorum*, or *Pterobalæna communis*, all names used by authors of high authority? Should the smallest British seal be called *Phoca hispida, fœtida*, or *annellata*?

I might go on indefinitely multiplying instances which will be answered differently by different naturalists, the arguments for one or the other name being often nicely balanced. What is wanted, therefore, is some kind of judicial authority for deciding what should in future be used. If a committee of eminent naturalists, selected from various nations and divided into several sections according to the subjects with which each member is most familiar, could be prevailed upon to take up the task of revising the whole of our existing nomenclature upon the basis of the laws issued by the Association in 1842, occasionally tempering their strictly legal decisions with a little discretion and common sense, and with a view, as much as possible, of avoiding confusion, and promoting general convenience; and if the working zoologists of the world generally would agree to accept the decisions of such a committee as final, we should dispose of many of the difficulties with which we are now troubled. There seems to me no more reason why the nomenclature of such a committee, if it were composed of men in whose judgment their fellow-workers would have confidence, should not be as universally accepted as is the nomenclature of the last edition of the "Systema Naturæ" of Linnæus. We have agreed not to look beyond that work for evidence of priority, and why should we not agree in the same way to accept decisions which would probably be arrived at with even fuller knowledge and greater sense of responsibility?

Whether this suggestion will be received with favour or not it appeared to me that it was one not inappropriate for the consideration of this section which has already dealt with the question in a manner so advantageous to science, and also for this year, which has witnessed the hundredth anniversary of the death of the great teacher of systematic zoology.

Our knowledge of the living inhabitants of the earth has indeed changed since that time. Our views of their relations to the universe, to each other, and to ourselves, have undergone great revolutions. The knowledge of Linnæus far surpassed that of any of his contemporaries; but yet of what we now know he knew but an infinitesimal amount. Much that he thought he knew we now deem false. Nevertheless, some of the oldest words to be found in all his writings contain sentiments which still claim a response in the hearts of many. Although we are less accustomed to see such words in works of science, that is no proof that their significance has been impaired by the marvellous progress of knowledge. With the words which Linnæus selected to place at the head of his great work I will conclude—

"O Jehova,
Quam ampla sunt tua opera!
Quam sapienter ea fecisti!
Quam plena est terra possessione tua!"

NOTES

THE International Congress of Meteorology, to be held in Paris from August 24 to 28 next, at the Trocadéro, will discuss a long series of questions having an important bearing on the progress of meteorology, and especially on combined action on the part of the meteorologists of various countries. Sixteen subjects are down for discussion relating to the study of storms in Europe and America, the means of carrying on and recording meteorological investigations on a uniform plan, the origin and propagation of cyclones, meteorology and aeronautics, terrestrial magnetism, sun-spots and meteorology, influence of the configuration and nature of soil, and other physical conditions on climate, earthquakes, the measures to be adopted in observatories to hasten the progress of meteorology &c. This is certainly a comprehensive programme, and we trust there will be a good attendance of competent meteorologists of all nations, and that some good practical results will be the outcome of the meeting.

WE notice that Prof. Fuller has resigned the professorship of mathematics at Aberdeen, which he has held with so much distinction. All of the long list of Senior Wranglers who have come from the University of Aberdeen within the last twenty-five years have been his pupils. Prof. Fuller was tutor of Peterhouse before his appointment to Aberdeen. The chair will probably be filled up in September by the University Court.

THE *Journal Officiel* has published the dates of three new congresses to be held at the Trocadéro and Tuileries:—Weights and Measures, September 2, 3, 4; Silk-culture, September 5, 6, 7, 9, 11; Legal Medicine, August 11, 13, 14.

WE are informed that a course of six lectures on meteorology will be given under the auspices of the council of the Meteorological Society, commencing in October next. The subjects of the lectures will be:—"The Nature and Physical Properties of the Atmosphere;" "Air Temperature, its Distribution and Range;" "Atmospheric Pressure, Wind, and Storms;" "Clouds and Weather Signs;" "Rain, Snow, Hail, and Electricity;" and "The Nature, Methods, and General Objects of Meteorology." It is intended that these lectures shall give a concise account of the present state of knowledge on the above subjects. The lectures will be open to the public, admission being by ticket, to be obtained at the office of the Society, 30, Great George Street, Westminster, S.W. Further particulars, giving full information as to the time, place, &c., will be duly announced.

SCIENTIFIC study does not yet appear to have attracted a superfluity of women, to judge by the numbers of candidates at the Cambridge Higher Local Examinations recently held. Only about thirty out of 500 took the science subjects; twenty-one took botany, one failed, and three obtained distinction; twenty-six geology and physical geography, of whom two failed, and seven were distinguished; seven geology, one failed, three distinguished; nine chemistry, three failed, none distinguished. Ten of the science candidates sat at Cambridge, and among them they gained ten out of fourteen of the distinctions given. Miss E. M. Clarke, of Cambridge, was distinguished in geology, zoology, and botany, and passed in chemistry. Mathematics got only twenty-three candidates, of whom four failed; only two, however, were placed in the first class (being Cambridge students), and two in the second. We are glad to learn that two new subjects are to be set in the science group next year, namely, physics and physiology, the latter so much needed in all girls' schools. Also, students will be allowed to take this group without having to pass Group A (literature and history) first, although it will be required for a full certificate.

SIR SAMUEL BAKER, in a letter to a contemporary, advocates the establishment of a botanical garden in Cyprus, similar to that in Ceylon, under the charge of a competent official, by whom experiments will be made, and the trees most suitable for the climate and varying altitudes of mountain ranges be selected.

A PERUVIAN newspaper, the *Bolsa*, we learn from the *Colonies*, says that extraordinary phenomena have been observed in connection with the "Corpuna" volcano in the province of Castilla, which have caused great alarm among the population. The immense banks of snow which have crowned its summit from time immemorial have suddenly melted away with such rapidity as to cause torrents to rush down the sides of the mountain, washing out immense quantities of stones and earth. The river below, being unable to contain the great body of water so suddenly added to it, overflowed its banks, causing great damage and distress. A great chasm or lateral crater next opened on one side, throwing out volumes of smoke and steam as well as tongues of flame, which were distinctly visible at night, accompanied with loud subterranean rumblings. It had never been supposed that the Corpuna was or could be a volcano. and there is no tradition that it was ever in a state of eruption, Nor within the memory of man has its crown of snow ever been absent.

THE British Medical Association concluded its meeting at Bath on Friday. The Association meets next year at Cork, Dr. O'Connor being the president elect.

EXPERIMENTS have been made at Paris with the telephone between the Exhibition building and Versailles, and they proved very successful. It is intended to make use of the telephone during the great military manœuvres.

THE Paper Exhibition which we intimated some time ago was to be held in Berlin is now opened, and an instructive and interesting account of the variety of objects exhibited—from flimsy to paper carpets, chairs, and even boats—will be found in the *Times* of the 13th. Both the articles manufactured and the materials, chemical and other, connected with the manufacture of paper, are exhibited, and judging from the description, the exhibition althogether must be at least as attractive as the Caxton Exhibition of last year. One of the principal exhibitors is Prince Bismarck, who is a partner in a large firm at Varzin; one of his specialties is paper slips for the Morse telegraph apparatus. The following interesting statistics are given by the *Times*' Correspondent from the Catalogue:—

	Number of Inhabitants.	Kilos consumed.	Kilos per head.
United States ...	39,000,000 ...	535,000,000 ...	14
Germany	43,000,000 ...	244,000,000 ...	6
England	33,000,000 ...	168,000,000 ...	5
France	37,000,000 ...	138,000,000 ...	3·6
Austria-Hungary ...	36,000,000 ...	92,000,000 ...	2·5
Russia	27,000,000 ...	67,000,000 ...	0·9
Italy	28,000,000 ...	38,000,000 ...	1·4
Scandinavia	6,000,000 ...	3,000,000 ...	0·5
Belgium	5,500,000 ...	27,000,000 ...	5·1
Switzerland	2,500,000 ...	17,000,000 ...	6·3

According to inscriptions put up in the hall and illustrated by paste-board cubes of different size, 600,000,000 men employ Chinese paper, while 366,000,000 use the European and 130,000,000 the Arabian article; 24,000,000 write on leaves, bark, and wood, 280,000,000 dispensing with writing and reading, and consequently taking no interest in this enlightened exhibition.

THE excursionists who went to the Boulonnais last week under the auspices of the Geologists' Association, led by Prof. Morris, seem to have had a good time of it. Their intention having been communicated to M. A. Huguet, Senator, Maire of Boulogne, he convoked the local geologists, the members of the Société Académique de Boulogne, the Société Médicale, the committees of the Public Library and of the Museum, and invitations were at the same time sent to the Presidents of the Geological Societies of Paris, Lille, &c., to meet their scientific brethren from across the Channel and give them a proper welcome on this the first occasion of English geologists in any number visiting France. Among those assembled to meet the excursionists on their arrival at Boulogne were M. Edmond Pellat, ex-president of the French Geological Society, Dr. Ch. Barrois, Vice-president of the North of France Geological Society, Prof. Giard of the Zoological Laboratory at Wimereux, the British Vice-Consul and other persons of note. The Maire bade the party welcome, and hoped he would have the pleasure of welcoming other deputations of scientific inquirers. Indeed the occasion seems to have assumed somewhat of an international character, and we should not be surprised if the Maire of Boulogne has to welcome many such deputations. Perhaps, should the North of France Geological Society organise an excursion to the London Basin, the Geologists Association may succeed in getting the Lord Mayor to return the pleasant compliment paid the English geologists by the civic chief of Boulogne; a Mansion House dinner might even be possible.

M. BISCHOFSHEIM, the well-known generous Parisian banker, having recently visited Montsouris Naval Observatory, and found the canvas roof of the equatorial house in wretched condition, Admiral Mouchez told him, in answer to his inquiry, that the budget of the observatory was not rich enough to meet an expense of 50*l*. required for the construction of a comfortable zinc cover. Next morning Admiral Mouchez received a cheque for 120*l*. from his visitor of the previous evening.

WE believe it is deeply regretted in France that M. André and M. Angot, the two French astronomers sent out to America to observe the recent transit of Mercury, did not stay to observe the solar eclipse of July 29, although the two phenomena were visible from the same place. M. André is now in Lyons, and M. Angot arrived in Paris almost on the day when the telegram announced the detection of Vulcan by Prof. Watson.

THE new hotel of the Paris Geographical Society is fast approaching completion, and the date for the opening will be determined soon.

WE regret to state that M. Cochery, the director of the French Postal Telegraph Department, has announced his intention of charging the several communes receiving the daily telegrams of the Agricultural Service a sum of 4*l*. a year. M. Mascart, the new director of the Central Bureau, has obtained the postponement of this step for a month. If M. Cochery carries his resolution into effect it is pretty certain that the greater number of rural communes will refuse to pay, and the organisation which M. Leverrier had so well organised will be thrown into a state of disorganisation less than a year after his death. It is certain that the Meteorological Congress, which is to meet at the Trocadéro ten days hence, will interfere and lay the case before the public.

M. BARDOUX has sent to the prefects of the several departments of France a circular, asking them to collect information on the resources, working, and composition of the meteorological commissions established by Leverrier. The minister intends to give to these boards a uniform organisation.

IT is stated in the French papers that Mr. Edison is to have no reward whatever at the Paris Exhibition for his phonograph. The reasons alleged for this apparent denial of justice are somewhat amusing. The jury of the class of instruments of precision declared that the phonograph could not be considered as at all an instrument of precision, but merely a toy; consequently they sent it to the class of telegraphy to be rewarded. But the telegraphists replied that it was of no use whatever in telegraphy, and refused to examine it. The consequence is that the most wonderful invention, probably, in the Exhibition, will be passed by unmentioned and unrewarded.

ON Friday, August 9, the Société Française d'Hygiène held a banquet at the Continental Hotel, Paris, for the reception of the Sanitary Institute of Great Britain. Dr. Ricord was in the chair, assisted by M. de Lesseps and Anatole de la Forge, the Director of the Press Department in the Home Office. The toast of the Sanitary Institute was replied to by Mr. Chadwick, who handed to the Société Française d'Hygiène a diploma of affiliation signed by the Duke of Northumberland and Dr. Richardson. Some members of the English Ladies' Sanitary Association were also present at the banquet.

IT is not generally known, we are told by the *China Mail* of Hongkong, that official rank is, to a certain extent, hereditary in China. Thus, when an officer of the first rank dies—the four grand secretaries, viceroys, and chief presidents of the "boards" at Peking, for instance—his sons inherit the "full fifth" rank, and are entitled to commence their public career as *Langchung*, or junior lord, of one of the boards. The sons of officers of the "full-second" rank—such as vice-presidents of the boards and governors—may enter public life as assistant secretaries, with the "half-fifth" rank. The sons of officers of the "half-second" rank become under-assistants, with "full-sixth" rank. The sons of officers of the third rank obtain the honorary degree of bachelor of arts, carrying the seventh rank. The sons of deceased officers of lower grades inherit no rank.

THE thirty-ninth anniversary meeting of the Royal Botanic Society was held in the Gardens, Regent's Park, on Saturday, Mr. James Heywood, F.R.S., in the chair. The annual reports of the council, auditors, and secretary were read. From these reports it appears that the affairs of the society are in a satisfactory state; the receipts in each of the several items had exceeded those of 1877, the balance being some 600*l*. better. The number of new Fellows elected was 112. Four hundred and eighty-one free students' orders for terms of two to six months each had been issued, including sixty-three to artists. The number of cut specimens given to students, professors, and teachers at the several medical and other schools was 63,414—an increase of 20,000 over last year, and 40,000 more than in 1871. The usual exchange of plants and seeds has been maintained with vigour; valuable contributions to the Society's collections were received from correspondents, including the Botanic Gardens of South Australia, Mauritius, Dublin, &c., and also from the Royal Gardens, Kew.

THE additions to the Zoological Society's Gardens during the past week include two European Lynx (*Felis lynx*) from Norway, presented by Major Chadwick; a Hobby (*Hypotriorchis subbuteo*), British Isles, presented by Mr. Howel Scratton; a Solitary Thrush (*Monticola cyanea*), European, presented by Mr. W. Verner; a Common Marmoset (*Hapale jacchus*), a Weeper Capuchin (*Cebus capucinus*), three Bluish Finches (*Spermophila cærulescens*) from Brazil, a Coati (*Nasua nasica*), a Common Boa (*Boa constrictor*), a Common Teguexin (*Teius teguexin*) from South America, a Red and Blue Macaw (*Ara macao*) from Central America, deposited; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, two Common Kingfishers (*Alcedo ispida*), British Isles, purchased; a Wapiti Deer (*Cervus canadensis*), born in the Gardens; a Black-crested Cardinal (*Gubernatrix cristatella*), two Talpacoti Ground Doves (*Chamæpelia talpacoti*), bred in the Gardens.

THE NORWEGIAN NORTH ATLANTIC EXPEDITION

THE Expedition left Hammerfest on July 13 on its trip westwards. The depths sounded on the following days were not great, the greatest being 1,440 fathoms in 72° 16′ N. lat. and 8° 9′ E. long. On the 17th, late in the evening, we unexpectedly met the ice of the Greenland Arctic current, in 73° 10′ N. lat. and 3° 22′ W. long., and were obliged to turn eastwards and northwards. Our next cross section commenced in 75° 16′ N. lat. and 0° 54′ W. long., and went along the 75th parallel to a point north-east of Bear Island. On the westernmost point we had a depth of 1,985 fathoms, but later only smaller depths. The bank west of Bear Island lies much more easterly and nearer this island than shown in the charts, there being a depth of 1,100 fathoms in the place where the French Expedition with *La Recherche* in 1839 gives 259 fathoms. The weather was in the beginning favourable, but under the ice we had strong northerly winds with a temperature of 2° C, and a heavy swell from the south-east. Having passed the meridian of Bear Island the temperature of the water fell to 0·2° C. on the Spitzbergen Bank. From our last sounding and dredging station north-east of Bear Island we sailed down to the east side of the island and stopped outside the south-east side, but the heavy wind and sea did not allow us to go on shore. With the glass we saw distinctly the Russian hut and its environs, but were not able to see our flag, which we had planted by the Dutch port, nor any column or cairn which could show us that the Dutchmen had been there. At 10 P.M. on July 23 we made sail for Norway, and with a fair but rather heavy gale and storm the *Vöringen* went on, heavily rolling, till we, twenty-four hours afterwards, made the Norwegian coast in thick rainy weather. At 4 A.M. on the 24th we dropped our anchor in Hammerfest.

Our soundings show that there is a sort of ridge 1,200–1,300 fathoms deep, between Bear Island and Jan Mayen. The trawl has brought up from the ice-cold water in these depths many rare and several new fishes, particularly of the genus Lycodes. A good many other new animals have been secured by our zoologists. The line of 0° C. lies in our last two cross-sections still at a depth of 500 fathoms. In the eastern part of our sea, the boundary line of the polar current, where 0° is to be found is less than 30 fathoms, and the surface water is less than 5°, lies in lat. 72½° N., long. 6° E., and in lat. 75° N., long. 7½° E. North and east of Bear Island the ice-cold Spitzbergen current runs southward over a very shallow bottom, the depths being only some 20 fathoms. The border of the Greenland Arctic current shows the same phenomenon as that observed in our northern fjords, viz., a minimum of temperature in, say, 40 fathoms depth, a second maximum about 0° C. in, say, 100 fathoms, and the absolute minimum of − 1·4° at the sea bottom.

We are now fitting out for our last trip, to the sea west of Spitzbergen, and expect to be ready to sail on the 30th.

At this moment we have news from the Dutch expedition, whose commander has left a letter, sent us from Vardö. He tells us that they made Jan Mayen on the east side but were not able to land in the heavy sea. From Jan Mayen they proceeded along the edge of the ice to Hackluyt's Headland, North Spitzbergen, which they reached on June 19. North of Spitzbergen they spent some fourteen days, and went as far east as to the Verlegin Hook. Thence they sailed southwards, called at Kobbe Bay, and found our mail at Bear Island, but were obliged to leave that place in a hurry, the wind being rather strong. Their letters home have probably been taken by a Norwegian fisherman off Vardö. Prof. Nordenskjöld passed the North Cape some days ago.

Hammerfest, July 27 H. MOHN

THE ECLIPSE OF THE SUN

ON another page we print an article, just received from Mr. Lockyer, on the recent eclipse, which, from its title and date, it will be seen was written before the event. The following telegrams from the *New York Tribune* of July 31 will perhaps enable our readers, in the meantime, to form a more complete idea of the results achieved.

A telegram to the *Tribune* from Lebanon, Mo., July 30, states that the Fort Worth party of observers, consisting of L. Waldo and R. W. Willson, of Harvard University, Prof. J. K. Ries and W. H. Pulsifer, of St. Louis, and Mr. F. E. Seagrave, of Providence, R. I., had fine weather for their observations, and met with general success. The four contacts were observed both with and without spectroscopes. The reversion of the spectral lines at totality, and the corona and its spectrum were studied, and five photographs, two of them polariscopic, were secured during totality. A number of sketches by local observers, for extent and form of corona, were made. The observers stationed by this party at McKinney, Allen, Cleburne, Waco and Dallas, were also generally successful in observing the duration of totality.

The following official telegrams were received at the U.S. Naval Observatory from the Naval Professors who were in charge of parties sent out to observe the eclipse :—

"*Las Junta, Col., July* 29.
"Good observations of the eclipse at Las Junta. Complete set of photographs. "ASAPH HALL"

"*Creton, W.T., July* 29
"Sky cloudless and observations perfectly successful. Six photographs of corona. Four polariscope photographs of corona and a fine drawing obtained. No ultra violet spectrum visible during totality.
"W. HARKNESS"

"*Central City, Col., July* 29
"Whole eclipse perfectly observed. I find no Vulcan as large as sixth magnitude. Hastings finds consistent tangential polarisation. Drawings and photographs of corona. Diffraction shade bands observed.
"E. J. HOLDEN"

"*Separation, W.T., July* 29
"Observations here very successful. Saw wings of light, supposed to be zodiacal light, extending 6° on each side of the moon in the direction of the ecliptic. Commander Sampson, U.S.N., found no dark lines in continuous spectrum of corona. Line 1,474 seen near sun's limb. No bright lines visible a few seconds after totality.
"S. NEWCOMB"

"*Pike's Peak, Col., July* 29
"Fair weather after a week's storm. Observations successful in a marvellously clear sky. Corona resembling zodiacal light followed in one direction twelve diameters from the sun. "S. P. LANGLEY."

"Eclipse successfully observed at Dallas, Texas. All four contacts satisfactory. No inter-Mercurial planet seen with comet-seeker. Thin clouds. No stars seen near the sun. Corona very brilliant. Several drawings secured and photographs taken. "D. P. TODD"

It is telegraphed to the *Tribune* from—

"*Havana*, July 30.—Yesterday the total eclipse of the sun was visible in this latitude. The sky was perfectly clear, and complete observations were made. A report of the results obtained was expected to-day, but the scientific commission which took observations at Mariel, where the meridian passes, has not returned yet."

"*Quebec*, July 30.—The eclipse of the sun yesterday was witnessed under the most favourable circumstances."

"*Washington*, July 30.—The Signal Service Observer at Virginia City, Mont., reports to the Chief Signal Officer as follows:—'Our four telescopic stations have got all the contacts nicely, and three sketches of the corona.'"

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—The following ephemeris of Hyperion is deduced from the elements which were calculated by Prof. Asaph Hall, upon his observations in 1875 with the Washington 26-inch refractor.

AT GREENWICH MIDNIGHT.

	Position.	Distance.		Position.	Distance.
	°	"		°	"
Aug. 24	268	147	Sept. 4	89	125
,, 25	263	93	,, 5	82	53
,, 26	243	37	,, 6	301	30
,, 27	125	36	,, 7	281	99
,, 28	105	93	,, 8	277	167
,, 29	100	146	,, 9	276	213
,, 30	97	189	,, 10	274	239
,, 31	96	218	,, 11	273	244
Sept. 1	94	228	,, 12	272	231
,, 2	93	216	,, 13	271	201
,, 3	92	181	,, 14	269	158

The plane of the orbit of the satellite is assumed to coincide with that of the ring, and as the earth has passed through the plane since the period included by the ephemerides which appeared in this column last year, the apparent motion of the satellite is now reversed, or the angles of position diminish. The above ephemeris includes an entire revolution of the satellite, and will serve, if necessary, to afford an idea of its position at any time during the present opposition, remarking that the satellite will be at its peri-saturnium at the following times: August 15·0939, September 5·4052, September 26·7165, October 18·0278, and November 8·3391.

OLBERS' STAR NEAR γ PEGASI.—On September 27, 1820, Olbers remarked a star of 6·7 m. not entered upon Harding's map, and which Harding had not seen during two comparisons of it with the heavens. It was somewhat brighter than 39 and nearly equal to 40 Piscium. Olbers accounted for it not having been observed on the meridian by the fact of its culminating within a few seconds of γ Pegasi. He watched it during the remainder of the season without noticing any change in brightness, and it was from the circumstance of his attention being thus directed to this quarter of the sky that he made an independent discovery of the comet of 1821. The star is in the *Durchmusterung* as 7·5 m., and was observed once by Argelander (October 24, 1861), the resulting position for 1855·0 being R.A. 0h. 5m. 41·8s., N.P.D. 73° 52′ 56″. Bessel did not observe it, but it is suspicious that he has a star 9m. only, preceding Argelander's position of Olbers' star 7·0s. and 3′ 58″ to the north, where the *Durchmusterung* has no star. It is a case for some one of those observers who are occupied with the variable stars to explain.

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.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A New Mineral

MR. BARNETT, of Chyandour, near Penzance, sent to the British Museum some time ago a mineral which appears to be new.

It is white with a slight tint of blue or greenish blue, and occurs as a layer sometimes of a quarter of an inch thickness, generally of a uniform fibrous structure, lining hollows or encasing quartz and other minerals. It is associated with earthy chlorite and quartz; iron pyrites, some copper pyrites, and mispickel being disseminated in the lode-material. Scorodite in bosslike aggregations also occurs with it, and in at least one instance the interior of the bosses of scorodite is filled with the mineral in question.

Dr. Flight has analysed it, and, though the mode in which the water is present has to be established more certainly, the general result of the analysis may be stated to be the assigning to the mineral a formula expressed, in "old style," as

$$3R_2O_3, As_2O_5 + 16H_2O,$$

R_2O_3 representing alumina with a notable amount of ferric oxide. The tint seems due to about one per cent. of copper, and a small amount of a sulphate is also present. The presence of the sulphate and general character of the composition would lead one to place the mineral with Pitticite, or "iron-sinter." But the formula, as Dr. Flight has pointed out, is so nearly that of an arsenical (instead of a phosphatic) Evansite that the true place of the mineral seems to be near the Evansite of David Forbes. I propose to call it Liskeardite, and to describe it more precisely hereafter. N. S. MASKELYNE

British Museum, Mineral Department, August 12

The Colouring of Birds' Eggs

WITHIN the last few months several notices have appeared in both England and Germany of supposed newly-discovered colouring matters in the shells of birds' eggs. Permit me to call attention, through your journal, to a long and detailed paper on this subject published by me, about four years ago, in the *Proceedings* of the Zoological Society of London. Apparently the authors who have lately treated on this subject have not been aware of the existence of this paper. As far as they go, the facts they have described fully confirm my conclusions; but, as I have shown, different birds' eggs contain at least five perfectly distinct coloured substances, and not, as Liebermann says, only two. One of these is closely related to a product of the decomposition of hæmoglobin and another to the bile pigments, it and the bile of birds yielding the same well-marked product on oxidisation.

H. C. SORBY

Kingstown, Dublin, August 6

The Limbs

IN his interesting papers on "The Genesis of the Limbs," which have recently appeared in NATURE, Mr. Mivart mentions that I have represented the limbs as modified portions of a primitively continuous inferior azygos fin.

That view was stated by me at length in a paper in the *Journal of Anatomy and Physiology*, 1871, vol. v., p. 59. It was formed from the following considerations among others—First, that the mesial, or azygos fins, are essentially double organs, being formed from the coalesced elements of the dorsal and ventral plates of the two sides, and being furnished with muscles, nerves, and blood-vessels, from the two sides. They may therefore be the representatives of organs which remain double, that is, of organs in which the lateral elements do not coalesce. Secondly, the limbs and the azygos fins do not co-exist in the same region. The limbs are formed where the ventral plates are kept apart and expanded by the presence of the visceral cavity, so that the elements which in the dorsal and postanal regions meet and unite into azygos fins, are here separated and grow out as lateral limbs. The ventral plates are, moreover, continued onwards beyond the outgrowing line of the limbs, and form the median portion of the visceral wall which lies beneath and between the limbs on the two sides. Thirdly, there is such a marked resemblance between the ventral fins in some fishes and the anal fins, that the transition from the one to the other is easy; the dagger-shaped pelvic bones being the representatives of two or more coalesced intraspinous bones and the ray bones of the one set being, in like manner, the representatives of the ray bones of the other.

Mr. Balfour in his admirable papers on "The Development of Elasmobranch Fishes," in the *Journal of Anatomy and Physiology* shows (vol. xi. p. 133), that the limbs are the remnants of continuous lateral fins, and that the ridges from which they are developed are in every way like the folds from which the unpaired fins are formed, but the development and growth are confined to two special points on each side instead of being continued, as in the case of the dorsal and anal fins, along a greater length of the fold. He further remarks that, externally, they closely resemble the unpaired fins, and both their position and nervous supply indicate that they do not belong to one special segment of the body. The lateral ridges, from which they are developed, I conceive to be the continuations of the diverging lateral halves of the essentially double ridge of the caudal fin kept asunder by the presence of the visceral cavity. These are but little separated in the position of the ventral fins, and are more so in the position of the pectoral fins. If this be so, the limbs are specialised differentiations of primitively continuous lateral folds—of portions, that is, of the diverging plates of the median fold from which the caudal fin is developed.

I cannot, however, assent to Mr. Mivart's view that the limbs are mere appendages to the axial system, or admit that either they or the limb girdles are the result of centripetal growth, "due to the in growth of originally superficial structures—exoskeletal hardenings which have grown inwards and become endoskeletal." The limb girdles are found in the same plane of the mesoblast as the ribs, and are, as I have shown in the "Anatomy of the Cryptobranch" (*Journal of Anatomy*, vol. vi. p. 9, and *Observations on Myology*), though not necessarily the serial homologues of the ribs, yet like them the result of ossification in the ventral transverse intermuscular septa. As they grow out they carry before them envelopes not only of skin, but of muscle, derived from the body-wall, which become differentiated according to the requirements. Moreover, although the ridges from which they are primarily developed may appear at first as epiblastic projections, these are soon supplemented by accumulations of mesoblastic tissue in which the components of the limbs are chiefly formed.

G. M. HUMPHRY

Cambridge, August 1

The Darkness of Caverns

THE impenetrable darkness of caverns has been for a very long time a recognised fact, without its cause having been satisfactorily explained. This darkness vanishes but partially before torch-light, and that only in a very limited radius. I, in my explorations in the caverns of Spain, had also noticed this circumstance, and now that I have verified it in others in Switzerland, I venture to think that I have found the explanation of this phenomenon.

The walls as well as the roof and floor of caverns are continually covered with moisture, which works without interruption in condensing the corpuscles that float in that circumscribed space. It thus performs the same function that the glycerine does which varnishes the sides of the crystal box by means of which Prof. Tyndall obtains an optical vacuum, the light diffusing itself imperfectly from want of those atoms which act as reflecting bodies. I have had occasion to verify my supposition by scattering around the torch very fine dust of different substances. The brightness diffused itself regularly all the time that the dust maintained itself in the required state of closeness and fluctuation, and vanished again slowly as the dust spread or deposited itself. The earth or common dust is the one which, in my experience, has produced the best effect.

SALVADOR CALDERON

Scent and Colour in Flowers

THE extension of our perceptive faculties of sight and hearing by various optical and acoustical instruments may enable us to comprehend the possibility of these faculties existing in other creatures to a degree so far surpassing ours as to seem a difference almost of kind. So the sight of the vulture would seem to be paralleled by the faculty of smell in moths, as evidenced by the detection of distant females by males. It would seem probable that the sense of smell may guide insects at a far greater distance than that of vision; for a consideration of the structure of the eyes of insects leads to the belief that they are not capable of forming clear images of distant objects. While, then, the scent of its blossoms may attract insects to a plant, their colour will act as a subsequent guide to the individual flowers, just as variegations undoubtedly act as honey-guides when the insect reaches the flower. This view is borne out, firstly, by the undoubted connection between perfume and pollination, shown by Morren in the case of the orchid *Maxillaria*, whose aromatic perfume lasts till pollination; and, secondly, by the well known connection of odour both with colour and with natural groups, white flowers being mostly sweet-scented, brown and orange ones most fetid. The insect could thus identify species before seeing them. Mr. Wallace has been, perhaps justly, blamed by a writer in the *Gardener's Chronicle* for saying that brightly-coloured flowers are seldom scented, and Dr. Taylor by "J. S. G." (NATURE, vol. xviii. p. 277), for saying that white flowers open mostly at night. It would, I think, be truer to say that few flowers are both variegated and scented, *i.e.*, that scented flowers are mostly monochromous, and that the majority of night-blowing flowers are white. The latter is a very different matter from saying that the majority of white flowers are night-blowing. We can perceive with difficulty that one part of a flower is more scented than another, yet scent may replace the dots and point-indicating lines of variegation to the senses of an insect. Nature not only often effects one purpose by divers means, but also uses one means for divers ends; so just as colour exists in plants, not only to attract insects, we can understand it being absent in some white flowers simply as a phenomenon of degradation and not as one of specialisation. The dog-rose, white convolvulus, and daisy, mentioned by Mr. Gardner as closing at eventide, are all scentless. The first, according to Dr. Hermann Müller, is visited by six hymenoptera, two diptera, and twelve coleoptera. The convolvulus does not close till between eight and ten P.M., and re-opens by moonlight. It is visited by two diptera, *Podura*, *Thrips*, one coleopteron, two hymenoptera, and the *Sphinx con-*

volvuli, L. This is a dusk-loving hawk-moth, which also visits the honeysuckle. The daisy is visited by nine hymenoptera, thirteen diptera, three coleoptera, and two lepidoptera, viz., the least meadow brown and the common blue butterflies. Many flowers, like *Lychnis vespertina*, remain open without exhaling their perfume, and I think Mr. Gardner will find that most of the subduedly-coloured flowers which are open at night give off most perfume, and are visited and fertilised by moths rather at dusk than in the dark, whilst the white ones remain fragrant still later. The clearly-cut discs of white of *Lychnis vespertina* are the last objects our eyes can often discern on a midsummer night's ramble. Of course variegation on the moths themselves would be as useless, from the point of view of sexual selection, as on the flowers from that of insect-fertilisation. Though it is to a certain extent true that like causes produce like effects, in investigations into phenomena so complex in their etiology as those of biology we must, I think, be more mindful that the converse that like effects are the result of like causes by no means necessarily follows. G. S. BOULGER

11, Burlington Road, Westbourne Park, W.

SCIENTIFIC SERIALS

Rendiconto delle Sessioni dell' Accademia delle Scienze dell' Istituto di Bologna.—The more important papers read at the Academy during the academical year 1877-8 were the following:—On the metamorphosis of plants, by Prof. G. B. Ercolani. The author specially refers to the transformation of a cryptogamous plant of the genus *Uromyces* into a phanerogamous dicotyledon, *Cuscuta europæa* L., and the return to the primitive cryptogamous form apparent in the seeds and branches of the *Cuscuta*.—On the velocity of light in transparent magnetised bodies, by Prof. A. Righi.—On the concentration of a magnetic solution near the pole of a magnet, by the same.—On the curves with equal normal principals, by Prof. A. Fais.—On some researches to ascertain whether from cadaverous matter, from albumen, or yolk of egg, volatile phosphoretted products are evolved, and on an excellent means of discovering free phosphorus in minute quantities, by Prof. Francesco Selmi.—Crystallographical researches, by Prof. Carlo Maragoni. The author describes some theoretical and experimental researches, and their application to the natural history of crystallised minerals.—Contributions to the flora of the Bolognese province, by Prof. Girolamo Cocconi.—Researches on the varying nature of the caloric emitted by various bodies heated to 100° C., by Prof. E. Villari.—On four species of noxious insects, inhabiting pines and birches, by G. Bertoloni.—On the calcareo-siliceous conglomerates of Sasso Cardo and of the Rio Fonti, and on the origin of pyrites, by Prof. Domenico Santagata.—Critical observations regarding some recent Italian crystallographical publications, by Prof. Luigi Bombicci.—On comparative psychogeny and the attempts to establish a zoopsychological classification, by Prof. Siciliani.—Note on a theorem in the theory of binary forms, by Prof. Francesco d'Arcais.—Observations regarding the existence of rudiments of upper canine teeth and incisors in the embryos of oxen and sheep, by Dr. G. P. Piana.—Anatomy and physiology of *Surilla Neapolitana*, by Prof. Salvatore Trinchese.—On a problem in undetermined analysis occurring in the geometrical theory of the transformation of plane figures, by Prof. F. P. Ruffini.—Geometrical studies on the molecular equilibrium, by S. Canevazzi.—On some gigantic bird-remains, probably belonging to *Aepyornis* or *Ruck*, by Prof. G. Bianconi.—On the internal texture of the eye of *Sphinx*, by Prof. G. V. Ciaccio.—On the origin and structure of the humor vitreous, particularly in the embryos of the two first classes of vertebrates, by the same.—On the whale of Taranto and the *Macleayius* of the Paris Museum, by Prof. G. Cappellini.—On the emery from S. Lucca and Paderno, and its fossils, by Dr. Lodovico Foresti.—On the reticular structure of the red corpuscles in the blood of *Torpedo*, and of the nerve substance of frogs, by Prof. Salvatore Trinchese.—Researches on the central nervous system of *Squilla Mentis*, by Dr. G. Bellonci.—Results of experiments made at the Royal Botanical Gardens of Bologna upon some species of *Eucalyptus*, and upon a new grass recently introduced in Italy, by A. Bertoloni.—On the nerve ends in the skin of bat's wings, by Dr. Agostino Rossi.—On the formation of protoxide of iron in the metallic state in the wet way, by Prof. Francesco Selmi.

SOCIETIES AND ACADEMIES

MANCHESTER

Literary and Philosophical Society, April 2.—E. W. Binney, F.R.S., F.G.S., president, in the chair.—On aurin, by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.—The origin of some ores of copper. Part II., by Charles A. Burghardt, Ph.D.

April 16.—Note on the occurrence of dioptase on Chrysocolla, from Peru, by Charles A. Burghardt, Ph.D.—On the internal cohesion of liquids and the suspension of a column of mercury to a height more than double that of the barometer, by Prof. Osborne Reynolds, F.R.S. The object of this communication is in the first place to show that certain facts already fully established afford grounds for believing that almost all liquids, and particularly mercury and water, are capable of offering resistance to rupture commensurate with the resistance offered by solid materials; in the second place to describe certain experimental results which, as far as they go, completely verify these conclusions and subvert the general ideas previously mentioned as to the limits to the height to which mercury can be ssupended in a tube or water raised by suction; and, in conclusion to explain the nature of the circumstances which have resulted in the practical limits to these phenomena.—On the estimation of hyposulphites and sulphites, by J. Grossmann, Ph.D.—Note on the action of iodine trichloride upon carbon bisulphide, by J. B. Hannay, F.R.S.E., F.C.S.

PARIS

Academy of Sciences, August 5.—M. Peligot in the chair.—Probable new observation of the planet Vulcan by Prof. Watson, by M. E. Mouchez.—On the orbito-ocular phenomena produced in mammals by excitement of the central end of the sciatic nerve, after excision of the superior cervical ganglion and the superior thoracic ganglion, by M. A. Vulpian.—New note on the progress of phylloxera in the two departments of Charente, in connection with the last communication of M. de la Vergne, by M. Bouillaud.—Rate of propagation of excitations in the motor nerves of the red muscles, abstracted from the power of the will, by M. A. Chauveau.—On the fundamental covariants of a cubo-quadratic binary system, by Prof. Sylvester.—On the baking of plaster, and on the manufacture of plasters by slow coagulation, by M. Ed. Landrin.—No mycelium intervenes in the formation and in the normal destruction of swellings developed under the influence of phylloxera, by M. Maxime Cornu.—On the abnormal solubility of certain bodies in soaps and alcaline resinates, by M. Ach. Livache.—On the vibratory forms of solid bodies and of liquids, by M. C. Decharme.—Note on the intra-Mercurial planet, by M. Gaillot.—Results of solar observations during the second quarter of 1878, by M. Tacchini.—Action of chloride of zinc on methylic alcohol; hexamethylbenzine, by MM. Le Bel and Greene.—Researches on the connections which exist between the weight of various bones of the Biscayan whale (*Balæna biscayensis*), by M. S. de Luca.—On *Prosopistoma punctifrons*, Latr., by MM. E. Joly and A. Vayssière.—On the influence of atmospheric electricity on vegetation, by M. L. Grandeau.—Age of the Mont-Dol bed; constitution and formation of the low plain called Marais de Dol, by M. Sirodot.

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