THURSDAY, SEPTEMBER 27, 1883

HERMANN MÜLLER'S "FERTILISATION OF FLOWERS"

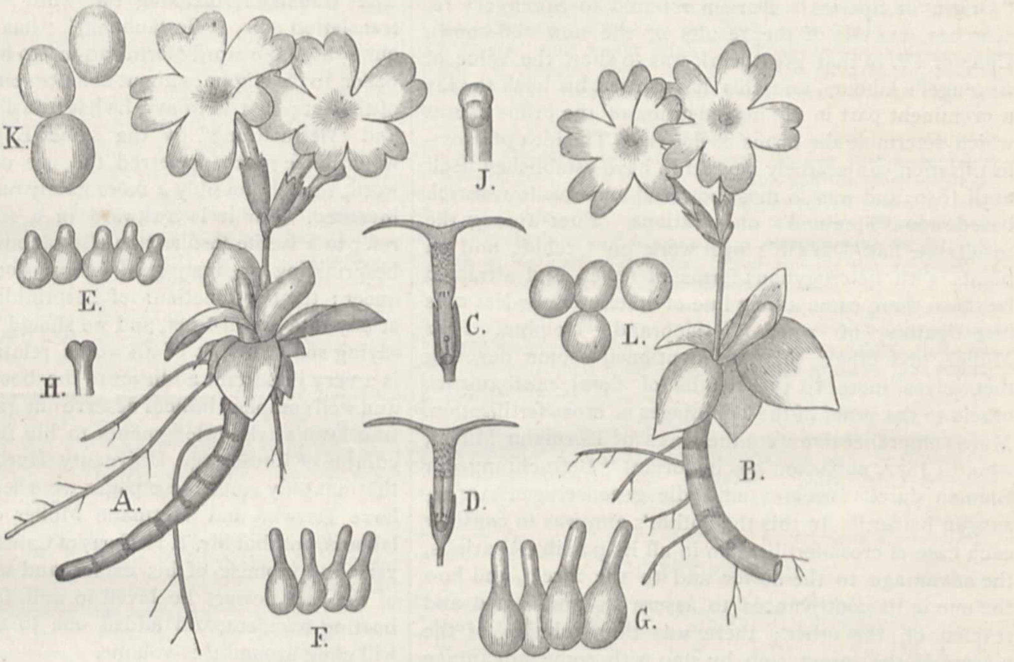
The Fertilisation of Flowers. By Prof. Hermann Müller. Translated and Edited by D'Arcy W. Thompson, B.A., Scholar of Trinity College, Cambridge, with a Preface by Charles Darwin. With Illustrations. (London: Macmillan and Co., 1883.)

CHRISTIAN CONRAD SPRENGEL'S treatise on the structure and fertilisation of flowers, after well nigh a century of oblivion, has come to be recognised as one of the most interesting of books, and his theory of the adaptation of flowers to fertilisation by insects is one that will ever be associated with his name. In the "Origin of Species" Darwin referred to Sprengel's researches, and one of the results of the now well-known Chapter IV. of that great work was to show the value of Sprengel's labours, and this has caused his book to play a prominent part in the investigation of the prime causes which determine the forms of flowers. The idea of cross-fertilisation can scarcely be said to have established itself until 1859, and was a most powerful impetus to research based upon Sprengel's observations. First among the results we had Darwin's own work on Orchids and on plants with heterogynous forms of styles, and attracted by these there came a long line of other more or less able investigators, of whom Hildebrand, Delpino, Fritz Müller, and others may be mentioned—some devoting themselves more to the details of floral mechanisms, others to the proof of the advantages of cross-fertilisation. More comprehensive were the views of Hermann Müller, who, in 1872, published his important "Befruchtung der Blumen durch Insekten und die gegenseitigen Anpassungen beider." In this the author's aim was to consider each case of cross-fertilisation in all its possible bearings, the advantage to the flower and to the insect, and how the one in its contrivances to assure its ends acted and reacted on the other; there was the evolution of the powers of the insect step by step with some advantage to the plant. Naturally the scheme was too vast, too grand to be entirely accomplished through the labours, direct or indirect, of any one man; and, so far as regarded anthophilous insects, Hermann Müller chiefly confined his attention to the bees, describing the modifications which fit them for a floral part, and proving that such modifications had been gradually evolved. This work of H. Müller's has been the guide-book of a host of workers during these last eleven years, and we most cordially greet its appearance now in an English translation by Mr. D'Arcy W. Thompson. The very recent death of its painstaking and worthy author adds a peculiar interest to its publication; in it he has incorporated all his most recent observations, so that it is not only a translation but a new and importantly enlarged edition—a monument to his fame. We regret that the translator did not think fit to give us the author's preface, which, though but four pages, contained much of practical interest, gave us an insight into Müller's labours as a teacher of natural science in the High School at Lippstadt, and would have been a worthy affix to the genial prefatory notice of Charles Darwin. One other regret and we are done. Why are the modest but pregnant words on the title-page of the original nowhere alluded to in the translation? This work, for which Darwin felt grateful—this book containing "an enormous mass of original observations on the fertilisation of flowers and on the part which insects play in the work," we quote again Darwin—the author himself styles "Ein Beitrag zur Erkenntniss des Ursächlichen Zusammenhanges in der Organischen Natur," but the translation says nothing of this.

So far as we have been able to judge, the translation has been most successfully accomplished, but a great deal of new material has been added. Some of the original is omitted, and many new figures have been introduced. The systematic part of the book, most happily for the reader, has been rearranged from Endlicher's system to that of Bentham and Hooker's "Genera Plantarum." The translator, disliking the word "pollination" as a translation for "Bestäubung," "has throughout [not quite] used the word 'fertilisation' to imply application of pollen to the stigma, without definite reference to the result of the act; that is to say, he has translated 'Bestäubung' and 'Befruchtung' by the same English word." We would have much preferred the use of the "ungainly" word, though possibly a more gainly one might have been invented. For it is awkward in a scientific treatise to refer to a sterile fertilisation (Bestäubung), while a sterile besprinkling or dusting of pollen would sound no way queer; the two functions of besprinkling and fertilising at any rate are distinct, and we should have some way of saying so. The list of all works relating to the subject is a very important addition to the book, and the copious and well compiled indices deserve our grateful thanks. The translator's acknowledgments to his friend the assistant curator of Cambridge University Herbarium reminds us that not only since these pages were sent by him to press have Darwin and Hermann Müller ceased from their labours, but that Mr. T. H. Corry of Caius College has, in the very first promise of his career, and while in the pursuit of the very flowers he loved so well, fallen a victim to a boating accident, and added one to the memories that will cling around this volume.

As an example of the illustrations we are enabled to give the accompanying woodcut of the pretty Alpine Primrose (*Primula integrifolia*). It is one of the red-flowered heterostyled species, and is adapted for Lepidoptera by its colour and the narrowing of the mouth of the tube. It will be remembered that the species of primrose were the subject of a series of interesting researches by Darwin which showed that in the common Primrose (*P. veris*) the stigma in the long-styled form possesses papillæ three times as long as those of the short-styled form, and that the pollen grains of the long stamen are half as large again as those of the short. The same holds good of *P. auricula* and *P. sinensis*, and these Primulas are very unproductive in the absence of insects, but fully productive when artificially fertilised or when insects have access to them.

The last few pages of this translation treat of the subject of the origin of flowers, which has chiefly been discussed by Hermann Müller, since the appearance of the first edition, in a series of essays, several of which appeared from time to time in these columns, tracing the first appearance of vegetation to aquatic forms. With the change to dry localities, from the vascular Cryptogams seem to have been developed wind-fertilised unisexual flowers—thus first the Gymnosperms, and from these afterwards the Angiosperms have arisen. Finally from the wind-fertilised Angiosperms entomophilous flowers arose; insects came first accidentally and afterwards regularly to seek their food on flowers, and natural selection fostered and perfected every change which favoured insect visits, and thereby aided cross-fertilisation. With the transition to insect-fertilisation came on the one hand great economy of pollen, but on the other hand the uncertainty of insect visits made it as a rule necessary that self-fertilisation should remain possible. Thus, though descended from unisexual (anemophilous) ancestors, entomophilous flowers are usually hermaphrodite, and are capable to a great extent of fertilising themselves when insect visits fail. But in the course of further development many of them have so increased their means of attracting insects (by colour, perfume, honey, &c.) that the power of self-fertilisation has become superfluous, and finally has been lost. Insects, in cross-fertilising flowers, endow them with offspring, which, in the struggle for existence, vanquish those individuals of the same species which are the offspring of self-fertilisation. The insects must therefore operate by selection in the same way as do unscientific cultivators among men, who preserve the most pleasing or most useful specimens, and reject or neglect the others. In both cases selection in course of time brings those variations to perfection which correspond to the taste or to the needs of the selective agent. Different groups of insects, according to their sense of taste or colour, the length of their tongues, their way of movement, and their dexterity, have produced various odours, colours, and forms of flowers, and thus have flowers and insects progressed together towards perfection. All this leads on to the final proposition with which this general retrospect ends, that the forms, colours, and odours of the flowers in a particular region must depend in the closest manner upon the insect fauna of the region, and especially upon the relative abundance of the various classes of insects in it.



Primula integrifolia, L. A.—Short-styled, B.—Long-styled plant (nat. size). C.—Short-styled, D.—Long-styled flower in section (nat. size). E.—Stigmatic papillæ of short-styled flower. F, G.—Ditto of long-styled flower. H.—Stigma of short-styled flower, I.—Ditto of long-styled flower (× 7). K.—Moistened pollen of short-styled flower, L.—Ditto of long-styled flower.

The whole subject of the fertilisation of flowers is one of still unexhausted resources. The student will in this volume have references to what is known, and will find out easily the immense amount of details still waiting investigation. An almost new subject is one that has been lately referred to in our columns by Prof. A. W. Bennett, on the constancy of insects in their visits to flowers; and several new lines of research are pointed out in Mr. Darwin's preface. It is a subject within the reach of all honest, patient observers. It is limited practically to no clime and season. Some of Müller's observations were made on flowers grown in windows, and all were carried on amid the somewhat scant leisure of a busy professional life.

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 insure the appearance even of communications containing interesting and novel facts.*]

Iguanodon

SINCE I wrote the account of M. Dollo's researches on Iguanodon, which appeared in NATURE of September 6 (p. 439), I have had the advantage of some conversation with Prof. Marsh on the subject, and am anxious to state one or two matters which I learned from him concerning questions raised in my review. Prof. Marsh has visited Brussels since I was there, and since M. Dollo's memoirs referred to by me were in print, and has examined the Iguanodon skeletons with M. Dollo, this being the second occasion on which he has seen the collection. After having examined the specimens now available he is of opinion that the question whether the bones considered by M. Dollo to be sternal are in reality such, or clavicles, is still an open one. The form of the bones, which are undoubtedly identical with those in the British Museum specimen determined by Prof. Marsh to be clavicles, is exactly that of clavicles and unlike that of any known sternal bones. There can be no doubt that they belong to the pectoral arch, but the position in which they have been found in two Bernissart specimens points to their belonging rather behind than in front of the coracoids. It is, however, Prof. Marsh believes, just possible that they may have fallen forward into the position in which they there occur, and he awaits the results to be attained from their examination in the other Bernissart specimens before making up his mind. In the closely-allied Hypsilophodon the sternum is a single broad-keeled plate. In the case of the British Museum specimen one of the bones is attached to the scapula. At all events, he points out that, should these bones really prove to be sternal, it does not follow that Iguanodon had no clavicles at all, for there is a process on the scapula indicating the presence of a clavicle, and such a bone, possibly very small and rudimentary, may yet be found to exist.

The statement of Prof. Marsh that the post-pubis in Iguanodon is long and slender, and *incomplete*, is correct; the conclusion that it was not so arose from a misunderstanding of the exact meaning of the term incomplete, as used. It denotes that in Iguanodon the bone does not extend, as it does in some Dinosauria (Hypsilophodon), backwards as far as the ischia, or farther, as in some birds, and this, as will be seen by reference to the figure is the case in Iguanodon, in which the post-pubis does not extend much further back than just beyond the ischial tuberosity. The fact is proved clearly by British specimens as well as by those of Bernissart. Prof. Marsh has observed that in two or three of the Bernissart skulls sutures are distinctly to be seen.

H. N. MOSELEY

Prof. Henrici's Address at Southport

THOUGH a member of many years' standing of the British Association, I have not had the advantage of being present at the current meeting, and am altogether indebted to the report in NATURE for a knowledge of Prof. Henrici's opening address in Section A.

It is much to our advantage to have our educational deficiencies in certain points indicated to us in so candid and, at the same time, so kindly a manner as Prof. Henrici has done on this recent as well as on former occasions; and I hope we shall profit by such friendly criticism. Had I been present, however, I should have ventured to remark on two heads of the address, that I thought Prof. Henrici underrated (1) the extent to which the modern geometry has been cultivated in these countries by many who have not been fettered by the "slavery of examinations" (an expression in which I entirely sympathise), chiefly under the influence of the great geometer Chasles' works; (2) the character of the instruction our youth receive in decimal arithmetic, the abbreviated methods of processes in which being certainly found in our better class of text-books, notably in that of the late Prof. de Morgan, dating back some fifty years, may be assumed to be taught generally in our higher-grade schools, as I certainly know to be the case in several. Other remarks, turning rather on matters of opinion than of fact, which occur to me, would be considered, probably, out of place here.

J. J. WALKER

Scientific Aspects of the Java Catastrophe

YOUR excellent leading article on this great event omits to call attention to a factor which I have long maintained to be of the greatest interest and importance from the point of view of meteorology and geology in general. I allude to the quantity of gases or vapour emitted during the eruptions. This must bear a direct relation to the quantity of matter emitted (whatever its form) and also to the height and distance to which the matter may be ejected or carried.

Now I hold that such vast quantities of gases as must have been liberated on this occasion cannot be passed over or taken as having no action on our atmosphere. Whatever the addition made, temperature and currents are influenced by it either locally or over great extents of the earth's surface, and if it were possible to take account of the height attained by the gases, their temperature of liberation, and the point of the surface of the globe whence proceeding, some judgment might be attempted of their action. In the present state of meteorology we know nothing of these quantities, but it is justifiable to assume that the upper currents of the air may be thus profoundly influenced, and that in certain cases cyclones may thus be generated. The present very fine dry weather we are enjoying here, with the high and steady barometer, may be a result of the great eruption, and it will be worth while to note if any abnormal conditions of atmosphere be found to prevail during the coming months.

J. P. O'REILLY

Dublin, September 16

"Elevation and Subsidence"

MR. YOUNG appears to think that I hold the view that rocky matter will melt at a lower temperature when under greater pressure. I did not intend, in my letter of August 24th, to express such an opinion as my own, but only to say that this was not a settled question; quoting the experiments upon which the doubt was founded.

Again, I merely mentioned the hypothesis that the matter of the nucleus may be above its own critical temperature as "conceivable." To all Mr. Young's present queries I should be disposed to answer in the affirmative, except to the second—"Do not the 'rigidity' calculations incontestably show that the earth is extremely rigid, *i.e.* solid?" As a geologist I do not concern myself anxiously about the nucleus. But to hold that the superficial parts are rigid I assert to be absolutely contrary to the known facts of geology. Perhaps it will be said that they ought to be, and therefore so much the worse for the facts.

Again, I say that mere plasticity of the upper layers will not explain the phenomena. The arrangement of rocks in the interior of mountain chains shows that the crust has been pushed over the surface towards them. It must, therefore, rest on a lubricating substratum. Again, mountains tend to rise and sedimental plains to sink. If mere plasticity were all, the reverse would happen.

As I understand it, the tidal argument for rigidity amounts to this. If the earth were not rigid, the fortnightly tide would be inappreciable. But Prof. Darwin, after most laborious and involved reductions of observations made at the instance of the Indian Government, has come to the conclusion that such a tide can be detected—not of its full amount, however (so far negativing absolute rigidity), but something less than three-quarters of that. The undiminished amount ought to be $4\frac{1}{2}$ inches only. The barriers caused by my "roots of the mountains," which, as noticed by Mr. Gardner, would break up the continuity of the substratum, would, as I have elsewhere pointed out, be great obstacles to the formation of tides in it.

O. FISHER

Harlton, Cambridge, September 20

A Complete Solar Rainbow

ALTHOUGH I quoted Capt. Winchester's figures as to the diameter of the circumsolar bow, mentioned in my letter on p. 436, I may add that this measurement was checked by that of the chief officer (Mr. Grant), who took the distances from the horizon to the inner rim of the bow on both sides, and subtracted them from 180°. In the case of the Captain's measurements, in the first instance, he measured from the inner rim of the bow to the edge of the sun. This was doubled, and the diameter of the sun added to it. Under these circumstances I can hardly believe there could have arisen the mistake suggested by Dr. Ingleby.

September 17 D. MORRIS

C. M. INGLEBY (p. 489) is clearly mistaken in supposing that D. Morris's description in NATURE (p. 436) referred to a real rainbow, for he makes no mention of any rain, the phenomenon being on a thin film over the sky. It must have been a solar halo, differing from an ordinary one only in being more distinctly coloured than usual. I have on rare occasions seen small portions of an ordinary halo very brilliantly coloured, but never saw a complete one so.

T. W. BACKHOUSE

Sunderland, September 24

Animal Intelligence

I AM not aware whether or not the following case has appeared among the numerous instances under this head already given in the columns of NATURE. It is to be found in Vogt and Specht's "Die Säugetiere in Wort und Bild" (p. 11). The writer of the text of that work says:—"I have myself seen a case in which a chimpanzee, who had got himself a little scratched by the point of a slightly projecting nail in the wall of his cage, first carefully examined the same, then sought to remove it, and afterwards, when he was let out, immediately proceeded to search for the head of the nail on the outside of the wall, and then, on finding it, began to try to pull out the nail with his fingers and teeth, and when this was done for him with a pair of pincers, broke out into lively demonstrations of joy."

Camberwell, September 18 GEO. G. CHISHOLM

THE BRITISH ASSOCIATION

SOUTHPORT, *Tuesday*.

CONSIDERING general results, the Southport meeting must be regarded as a decided success. The number of tickets sold has been over 2,650, and the funds will therefore be ample to provide for scientific research. The supply of papers has been kept up in all Sections, and the quality of them has certainly reached a fair average. The weather with two exceptions has been fine, and the accommodation ample. The *soirées* have been all that could be wished, to which the beautiful trees, ferns, and palms in the Winter Gardens have contributed, and the exhibition connected with it afforded points of interest for people of varied tastes. The local officers have worked well and shown both application and forethought, and the excursions, if not of a particularly scientific character, have certainly been the means of the Association receiving much hospitality, and seeing many places of interest, and some of beauty.

The General Committee meeting on Monday was very largely attended, and after the exceedingly well expressed speeches of Principal Dawson and Sir Charles Tupper, the feeling was strong that the meeting in Canada will be a success, and that the greater the number who go the better will the Canadians be pleased. Sir Charles Tupper stated that, after a long experience of the Canadian House of Commons, he never saw a vote so unanimously passed as the appropriation of 4000*l*. for the forthcoming meeting. There are many who still think it a mistaken policy for the Association to leave the shores of these islands, but all of those who were present fully sympathised with the very strong expression of approval that met announcement after announcement of cheap passages, free railway journeys, and magnificent hospitality offered by the Dominion. General satisfaction was expressed at the announcement that those who are unable to spare the time for the long excursions to the Rocky Mountains and elsewhere after the Canada meeting will be allowed to make those expeditions before the meeting, which will commence on August 27, under the presidency of Prof. Lord Rayleigh.

For the 1885 meeting Birmingham and Nottingham did not put in applications, the competing towns being Aberdeen and Bournemouth. In favour of the former it was urged that the members of the Association by that time will be so accustomed to long journeys that they will think nothing of the distance to Aberdeen, and that the Scotch meetings have always been a success, both as to numbers and as to the position of those who attended. In favour of Bournemouth it was urged that first meetings were always a great success, as at Brighton and the present meeting at Southport. The vote was for the northern University town; but there was an expression of feeling that the claims of the watering-place should not be forgotten in 1886.

Prof. Ball's lecture was the most successful of the addresses delivered in the Pavilion. The building, as originally constructed, was oval in shape, with a gallery extending round it, and its acoustic properties were then good; subsequently an ordinary theatrical stage and appointments were added to it, which latter were only partially removed for the meeting, and the building was certainly but ill adapted for the large audiences which endeavoured to find room in it. The Reception Room at the Cambridge Hall left nothing to be desired, except a wish that it had been on the ground floor.

In the Geographical Section much interest was felt in a long paper by Mr. H. H. Johnson, on a visit to Mr. Stanley's stations on the River Congo. The author read a letter he had just received from Mr. Stanley, in which that explorer gives expression to his belief that the River Congo will give civilisation and commerce to the lost Continent. In this Section also an interesting paper was read by Mr. Wm. Hancock of the Chinese Imperial Custom Service, on the volcanic and earthquake regions of Central America; by the Rev. S. J. Perry, on Nos Vey and the south-west of Madagascar, which he visited for the late transit of Venus.

The address of Sir Frederick Bramwell to the working men was a very great success; his good voice and easy style told with effect on the crowded audience of working men who came to learn about the telephone, which was clearly shown to be an important factor in commercial life.

The following is the list of grants of money appropriated by the General Committee to scientific purposes for next year:—

A—Mathematics and Physics	
Brown, Prof. Crum—Meteorological Observations on Ben Nevis	£50
Foster, Prof. G. Carey—Electrical Standards	50
Schuster, Prof.—Meteoric Dust	20
Abney, Capt.—Standard of White Light	20
Scott, Mr. R. H.—Synoptic Charts of the Indian Ocean	50
Stewart, Prof. Balfour—Meteorological Observatory near Chepstow	25
Shoolbred, Mr. J. N.—Reduction of Tidal Observations	10
Darwin, Prof. G. H.—Harmonic Analysis of Tidal Observations	45
B—Chemistry	
Odling, Prof.—Photographing the Ultra-Violet Spark Spectra	10
C—Geology	
Etheridge, Mr. R.—Earthquake Phenomena of Japan	75
Williamson, Prof. W. C.—Fossil Plants of Halifax	15
Sorby, Dr. H. C.—British Fossil Polyzoa	10
Prestwich, Prof.—Erratic Blocks	10
Etheridge, Mr. R.—Fossil Phyllopoda of the Palæozoic Rocks	15
Hull, Prof. E.—Circulation of Underground Waters	15
Evans, Dr. J.—Geological Record	15
Green, Prof. A. H.—Raygill Fissure	15
Prestwich, Prof.—International Geological Map of Europe	20
D—Biology	
Newton, Prof.—Zoological Bibliography	50
Sclater, P. L.—Natural History of Timor Laut	50
Lankester, Prof. Ray—Table at the Zoological Station at Naples	80
Harrison, J. Park—Facial Characteristics of Races in the British Isles	10
Hooker, Sir J.—Exploring Kilimandjaro and the adjoining Mountains of Equatorial Africa	500
Cordeaux, Mr. J.—Migration of Birds	20
Foster, Dr. M.—Coagulation of the Blood	50
Stainton, Mr. H. T.—Record of Zoological Literature	100
E—Geography	
Godwin-Austen, Lieut.-Col.—Exploration of New Guinea	100
F—Economic Science and Statistics	
Brabrook, Mr. E. W.—Preparation of the Final Report of the Anthropometric Committee	10

G—Mechanics

Bramwell, Sir F.—Patent Legislation £5

Total £1445

SECTION D

BIOLOGY

Opening Address by Prof. E. Ray Lankester, M.A., F.R.S., F.L.S., President of the Section.

It has become the custom for the presidents of the various Sections of this Association to open the proceedings of the departments with the chairmanship of which they are charged by formal addresses. In reflecting on the topics which it might be desirable for me to bring under your notice, as your president, on the present occasion, it has occurred to me that I might use this opportunity most fitly by departing somewhat from the prevailing custom of reviewing the progress of science in some special direction during the past year, and that, instead of placing before you a summary of the results recently obtained by the investigations of biologists in this or that line of inquiry, I might ask your attention and that of the external public (who are wont to give some kindly consideration to the opinions expressed on these occasions) to a matter which is even more directly connected with the avowed object of our Association, namely, "the Advancement of Science." I propose to place before you a few observations upon the provision which exists in this country for the advancement of that branch of science to which Section D is dedicated—namely, Biology.

I am aware that it is usual for those who speak of men of science and their pursuits to ignore altogether such sordid topics as the one which I have chosen to bring forward. A certain pride on the one hand, and a willing acquiescence on the other hand, usually prevents those who are professionally concerned with scientific pursuits from exposing to the public the pecuniary destitution and the consequent crippling and languor of scientific research in this country. Those Englishmen who take an interest in the progress of science are apt to suppose that, in some way which they have never clearly understood, the pursuit of scientific truth is not only its own reward, but also a sufficient source of food, drink, and clothing. Whilst they are interested and amused by the remarkable discoveries of scientific men, they are astonished whenever a proposal is mentioned to assign salaries to a few such persons sufficient to enable them to live decently while devoting their time and strength to investigation. The public are becoming more and more anxious to have the opinion or report of scientific men upon matters of commercial importance, or in relation to the public health; and yet in ninety-nine cases out of a hundred they expect to have that opinion for the asking, although accustomed to pay other professional men handsomely for similar service. There is, it appears, in the public mind a vague belief that men who occupy their time with the endeavour to add to knowledge in this or that branch of science are mysteriously supported by the State Exchequer, and are thus fair game for attacking with all sorts of demands for gratuitous service; or, on the other hand, the notion at work appears sometimes to be that the making of new knowledge—in fact, scientific discovery—is an agreeable pastime, in which some ingenious gentlemen, whose business in other directions takes up their best hours, find relaxation after dinner or on the spare hours of Sunday. Such mistaken views ought to be dispelled with all possible celerity and determination. It is in part owing to the fact that the real state of the case is not widely and persistently made known to the public, that no attempt is made in this country to raise scientific research, and especially biological research, from the condition of destitution and neglect under which it suffers—a condition which is far below that of these same interests in France and Germany, and even in Holland, Belgium, Italy, and Russia, and is discreditable to England in proportion as she is richer than other States.

It appears to me that, in placing this matter before you, I may remove myself from any suggestion of self-interest by at once stating that the great defect to which I shall draw your attention is *not* that the few existing public positions which are open in this country to men who intend to devote their chief energies to biological research are endowed with insufficient salaries; but that there is not anything like *a sufficiently large number* of those posts, and that there is in that respect, from a national point of view, a pecuniary starvation of biology, a withholding of money which (to use another metaphor) is no less the sinews of the war of science against ignorance than of other less glorious campaigns. Surely men engaged in the scientific profession may advocate the claim of science to maintenance and needful pecuniary provision! It seems to me that we should, if necessary, swallow, rather than be controlled by, that pride which tempts us to paint the scientific career as one far above and independent of pecuniary considerations; whereas all the while we know that knowledge is languishing, that able men are drawn off from scientific research into other careers, that important discoveries are approached and their final grasp relinquished, that great men depart and leave no disciples or successors, simply for want of that which is largely given in other countries, of that which is most abundant in this country, and is so lavishly expended on armies and navies, on the development of commercial resources, on a hundred injurious or meaningless charities—viz., money.

I have no doubt that I have the sympathy of all my hearers in wishing for more extensive provision in this country for the prosecution of scientific research, and especially of biological research. I need hardly remind this audience of the almost romantic history of some of the great discoveries which have been made in reference to the nature and history of living things during the past century. The microscope, which was a drawing-room toy a hundred years ago, has, in the hands of devoted and gifted students of nature, been the means of giving us knowledge which, on the one hand, has saved thousands of surgical patients from terrible pain and death, and, on the other hand, has laid the foundation of that new philosophy with which the name of Darwin will for ever be associated. When Ehrenberg and, later, Dujardin described and figured the various forms of Monas, Vibrio, Spirillum, and Bacterium which their microscopes revealed to them, no one could predict that fifty years later these organisms would be recognised as the cause of that dangerous suppuration of wounds which so often defeated the beneficent efforts of the surgeon and made an operation in a hospital ward as dangerous to the patient as residence in a plague-stricken city. Yet this is the result which the assiduous studies of the biologists, provided with laboratories and maintenance by Continental States, have in due time brought to light. Theodore Schwann, professor at Liége, first showed that these Bacteria are the cause of the putrefaction of organic substances, and subsequently the French chemist Pasteur, professor in the École Normale of Paris, confirmed and extended Schwann's discovery, so as to establish the belief that all putrefactive changes are due to such minute organisms, and that if these organisms can be kept at bay no putrefaction can occur in any given substance.

It was reserved for our countryman, Joseph Lister, to apply this result to the treatment of wounds, and by his famous antiseptic method to destroy by means of special poisons the putrefactive organisms which necessarily find their way into the neighbourhood of a wound, or of the surgeon's knife and dressings, and to ward off by similar means the access of such organisms to the wounded surface. The amount of death, not to speak of the suffering short of death, which the knowledge of Bacteria gained by the microscope has thus averted is incalculable.

Yet further, the discoveries of Ehrenberg, Schwann, and Pasteur are bearing fruit of a similar kind in other directions. It seems in the highest degree probable that the terrible scourge known as tubercular consumption or phthisis is due to a parasitic Bacterium (Bacillus), discovered two years since by Koch of Berlin, as the immediate result of investigations which he was commissioned to carry on at the public expense, in the specially erected Laboratory of Public Health, by the German Imperial Government. The diseases known as erysipelas and glanders or farcy have similarly, within the past few months in German State-supported laboratories, been shown to be due to the attacks of special kinds of Bacteria. At present this knowledge has not led to a successful method of combating those diseases, but we can hardly doubt that it will ultimately do so. We are warranted in this belief by the fact that the disease known as "splenic fever" in cattle and "malignant pustule" or anthrax in man has likewise been shown to be due to the action of a special kind of Bacterium, and that this knowledge has, in the hands of MM. Toussaint and Pasteur, led to a treatment in relation to this disease similar to that of vaccination in relation to small-pox. By cultivation a modified growth of the anthrax parasite is obtained, which is then used in order to inoculate cattle and sheep with a mild form of the disease, such inoculation having the result of rendering the cattle and sheep free from the attacks of the severe form of disease, just as vaccination or inoculation with cow-pox protects man from the attack of the deadly small-pox. One other case I may call to mind in which knowledge of the presence of Bacteria as the cause of disease has led to successful curative treatment. A not uncommon affliction is inflammation of the bladder accompanied by ammoniacal decomposition of the urine. Microscopical investigation has shown that this ammoniacal decomposition is entirely due to the activity of a Bacterium. Fortunately this Bacterium is at once killed by weak solutions of quinine, which can be injected into the bladder without causing any injury or irritation. This example appears to have great importance, because it is the fact that many kinds of Bacteria are not killed by solutions of quinine, but require other and much more irritant poisons to destroy their life, which could not be injected into the bladder without causing disastrous effects. Since some Bacteria are killed by one poison and some by another, it becomes a matter of the keenest interest to find out all such poisons; and possibly amongst them may be some which can be applied so as to kill the Bacteria which produce phthisis, erysipelas, glanders, anthrax, and other scourges of humanity, whilst not acting injuriously upon the body of the victim in which these infinitesimal parasites are doing their deadly work. In such ways as this biology has turned the toy "magnifying-glass" of the last century into a saver of life and health.

No less has the same agency revolutionised the thoughts of men in every branch of philosophy and speculation. The knowledge of the growth of the chick from the egg and of other organisms from similarly constituted beginnings has been slowly and continuously gained by prodigious labour, extending over generation after generation of students who have occupied the laboratories and lived on the stipends provided by the Governments of European States—not English, but chiefly German. It is this history of the development of the individual animal and plant from a simple homogeneous beginning to a complex heterogeneous adult which has furnished the starting-point for the wide-reaching Doctrine of Evolution. It is this knowledge, coupled with the knowledge of the myriad details of structure of all kinds of animals and plants which the faithful occupants of laboratories and the guardians of biological collections have in the past hundred years laboriously searched out and recorded—it is this which enabled Darwin to propound, to test, and to firmly establish his theory of the origin of species by natural selection, and finally to bring the origin, development, and progress of man also into the area of physical science. I have said enough, in referring only to two very diverse examples of the far-reaching consequences flowing from the discoveries of single-minded investigators in biological science, to remind my hearers that in the domain of biology, as in other sciences, the results attained by those who have laboured simply to extend our knowledge of the structure and properties of living things, in the faith that every increase of knowledge will ultimately bring its blessing to humanity, have in fact led with astonishing rapidity to conclusions affecting most profoundly both the bodily and the mental welfare of the community.

We who know the beneficent results which must flow more and more from the labours of those who are able to create new knowledge of living things, or, in other words, are able to aid in the growth of biological science, must feel something more than regret—even indignation—that England should do so small a proportion of the laborious investigation which is necessary, and is being carried on for our profit by other nationalities. It must not be supposed, because we have had our Harvey and our Darwin, our Hunter and our Lister, that therefore we have done and are doing all that is needful in the increase of biological science. The position of this country in relation to the progress of science is not to be decided by the citation of great names.

We require to look more fully into the matter than this. The question is not whether England has produced some great discoverers, or as many as any other nationality, but whether we might not, with advantage to our own community and that of the civilised world generally, do far more in the field of scientific investigation than we do.

It may be laid down as a general proposition, to which I know of no important exception, that scientific discovery has only been made by one of two classes of men, namely—(1) those whose time could be devoted to it in virtue of their possessing inherited fortunes; (2) those whose time could be devoted to it in virtue of their possessing a stipend or endowment especially assigned to them for that purpose.

Now it is a very remarkable fact that in England, far more than in any other country, the possessors of private fortunes have devoted themselves to scientific investigation. Not only have we in all parts of the country numerous *dilettanti*[1] who, especially in various branches of biology, do valuable work in continually adding to knowledge, quietly pursuing their favourite study without seeking to reach to any great eminence, but it is the fact that many of the greatest names of English discoverers in science are those of men who held no professional position designed to maintain an investigator, but owed their opportunity simply to the fact that they enjoyed a more or less ample income by inheritance. Thus, Harvey possessed a private fortune, Darwin also, and Lyell. Such also is true of some of the English naturalists, who more recently have most successfully devoted their energies to research. Those who wish to defend the present neglect of the Government and of public institutions to provide means for the carrying on of scientific research in this country are accustomed to declare as a justification for this neglect that we do very well without such provision, inasmuch as the cultivation of science here flourishes in the hands of those who are in a position of pecuniary independence. The reply to this is obvious. If those few of our countrymen who by accident are placed in an independent position show such ability in the prosecution of scientific research, how much more would be effected in the same direction were the machinery provided to enable those also who are *not* accidentally favoured by fortune to enter upon the same kind of work? The number of wealthy men who have distinguished themselves in scientific research in England is simply evidence that there is a natural ability and liking for such work in the English character, and is a distinct encouragement to those who have it in their power to do so to offer the opportunity of devoting themselves to research to a larger number of the members of the community. It is impossible to doubt that there are hundreds of men amongst us who have as great capacity for scientific discovery as those whom fortune has favoured with leisure and opportunity. It cannot be doubted that, were the means provided to enable even a proportion of such men to give themselves up to scientific investigation, great discoveries of no less importance to the world than those relative to the causes of disease and the development of living things from the egg—which I have cited—would be made as a direct consequence of their activity, whereas now we must wait until in due course of time these discoveries shall be made for us in the laboratories of Germany, France, or Russia.

It should further be pointed out that it is altogether a mistake to suppose that the existence amongst us of a few very eminent men is any evidence that we are contributing largely to the hard work of careful study and observation which really forms the material upon which the conclusions of eminent discoverers are based. You will find in every department of biological knowledge that the hard work of investigation is being carried on by the well-trained army of German observers. Whether you ask the zoologist, the botanist, the physiologist, or the anthropologist, you will get the same answer: it is to German sources that he looks for new information; it is in German workshops that discoveries, each small in itself, but gradually leading up to great conclusions, are daily being made. To a very large extent the business of those who are occupied with teaching or applying biological science in this country consists in making known what has been done in German laboratories; our English students flock to Germany to learn the methods of scientific research; and to such a state of weakness is English science reduced for want of proper nurture and support, that even on some of the rare occasions when a capable investigator of biological problems has been required for the public service, it has been necessary to obtain the assistance of a foreigner trained in the laboratories of Germany.

Let me now briefly explain what are the arrangements, in number and in kind, which exist in other countries for the purpose of promoting the advancement of biological science, which are wanting in this country.

In the German Empire, with a population of 45,000,000, there are twenty-one universities. These universities are very different from anything which goes by the name in this country. Amongst its other arrangements devoted to the study and teaching of all branches of learning and science, each university has five institutes, or establishments, devoted to the prosecution of researches in biological science. These are respectively the physiological, the zoological, the anatomical, the pathological,

[1] I use this word in its best and truest sense, and would refer those who have been accustomed to associate with it some implication of contempt, to the wise and appreciative remarks of Goethe on Dilettanti.

and the botanical. In one of these universities of average size, each of the institutes named consists of a spacious building containing many rooms fitted as workshops, provided with instruments, a museum, and, in the last instance, with an experimental garden. All this is provided and maintained by the State. At the head of each institute is the university professor respectively of physiology, of zoology, of anatomy, of pathology, or of botany. He is paid a stipend by the State, which in the smallest university is as low as 120*l.*, but may be in others as much as 700*l.*, and averages say 400*l.* a year. Considering the relative expenditure of the professional classes in the two countries, this average may be taken as equal to 800*l.* a year in England.[1] Besides the professor, each institute has attached to it, with salaries paid by the State, two qualified assistants, who in course of time will succeed to independent positions. A liberal allowance is also made to each institute by the State for the purchase of instruments, material for study, and for the pay of servants, so that the total expenditure on professor, assistants, laboratory service, and maintenance, averages 800*l.* a year for each institute—reaching as much as 2000*l.* or 3000*l.* a year in the larger universities. It is the business of the professor, in conjunction with his assistants and the advanced students, who are admitted to work in the laboratories free of charge, to carry on investigations, *to create new knowledge* in the several domains of physiology, zoology, anatomy, pathology, and botany. It is for this that the professor receives his stipend, and it is on his success in this field of labour that his promotion to a more important or better paid post in another university depends. In addition to and irrespectively of this part of his duties, each professor is charged with the delivery of courses of lectures and of elementary instruction to the general students of the university, and for this he is allowed to charge a certain fee to each student, which he receives himself; the total of such fees may, in the case of a largely attended university and a popular subject, form a very important addition to the professorial income; but it is distinctly to be understood that such payment by fees is only an *addition* to the professor's income, quite independent of his stipend and of his regular occupation in the laboratory: it is paid from a separate source and for a separate object. There are thus in the German Empire more than 100 such institutes devoted to the prosecution of biological discovery, carried on at an annual cost to the State of about 80,000*l.*, equal to about 160,000*l.* in England, providing posts of graduated value for 300 investigators, some of small value, sufficient to carry the young student through the earlier portion of his career, whilst he is being trained and acting as the assistant of more experienced men—others forming the sufficient but not too valuable prizes which are the rewards of continuous and successful labour.

In addition to these university institutes, there are in Germany such special laboratories of research, with duly salaried staff of investigators, as the Imperial Sanitary Institute of Berlin, and the large museums of Berlin, Bremen, and other large towns, corresponding to our own British Museum of Natural History.

Moreover, we must be careful to note, in making any comparison with the arrangements existing in England, that there are, in addition to the universities in Germany, a number of other educational institutions, at least equal in number, which are known as polytechnic schools, technical colleges, and agricultural colleges. These furnish posts of emolument to a limited number of biological students, who give courses of instruction to their pupils, but they have not the same arrangements for research as the universities, and are closely similar to those colleges which have been founded of late years in the provincial towns of England, such as Bristol, Nottingham, and Leeds. The latter are sometimes quoted by sanguine persons, who are satisfied with the neglected condition of scientific training and research in this country, as really sufficient and adequate representatives of the German universities. As a matter of fact, the excellent English colleges in question do not present anything at all comparable to the arrangements of a German university, and are, in respect of the amount of money which is expended upon them, the number of their teaching staff and the efficiency of their laboratories, inferior not merely to the smallest German university, but inferior to many of the technical schools of that country.

Passing from Germany, I would now ask your attention for a moment to an institution which is supported by the French Government, and which—quite irrespective of the French university system, which is not on the whole superior to our own—constitutes one of the most effective arrangements in any European State for the production of new knowledge. The institution to which I allude is the Collège de France in Paris—co-existing there with the Sorbonne, the École de Médecine, the École Normale, the Jardin des Plantes, and other State-supported institutions—in which opportunity is provided for those Frenchmen who have the requisite talent to pursue scientific discovery in the department of biology, and in other branches of science. I particularly mention the Collège de France, because it appears to me that the foundation of such a college in London would be one of the simplest and most direct steps that could be taken towards filling, in some degree, the void from which English science suffers. The Collège de France is divided into a literary and a scientific faculty. Each faculty consists of some twenty professors. Each professor in the scientific faculty is provided with a laboratory and assistants (as many as four assistants in some cases), and with a considerable allowance for the expenses of the instruments and materials required in research. The personal stipend of each professor is £400, which has been increased by an additional £100 a year in some cases from the Government Department charged with the promotion of higher studies. The professors in this institution, as in the German universities, when a vacancy occurs, have the right of nominating their future colleague, their recommendation being accepted by the Government. The professors are not expected to give any elementary instruction, but are directed to carry on original investigations, in prosecuting which they may associate with themselves pupils who are sufficiently advanced to join in such work; and it is further the duty of each professor to give a course of forty lectures in each year upon the results of the researches in which he is engaged. There are at present among the professors of the Collège de France four of the most distinguished among contemporary students of biological science: Professor Brown-Séquard, Professor Marey, Professor Balbiani, and Professor Ranvier. Every one who is acquainted with the progress of discovery in physiology, minute anatomy, and embryology, will admit that the opportunities afforded to these men have not been wasted; they have, as the result of the position in which they have been placed, produced abundant and most valuable work, and have, in addition, trained younger men to carry on the same line of activity. It was here, too, in the Collège de France, that the great genius of Claude Bernard found the necessary conditions for its development.

Let us now see how many and what kind of institutions there are in England devised so as to promote the making of new knowledge in biological science. Most persons are apt to be deceived in this matter by the fact that the terms "university," "professorship," and "college" are used very freely in England in reference to institutions which have no pecuniary resources whatever, and which, instead of corresponding to the German arrangements which go by these names, are empty titles, neither backed by adequate subsidy of the State nor by endowment from private sources.

In England, with its 25,000,000 inhabitants, there are only four universities which possess endowments and professoriates—viz., Oxford, Cambridge, Durham, and the Victoria (Owens College). Besides these, which are variously and specially organised each in its own way, there are the London Colleges (University and King's), the Normal School of Science at South Kensington, and various provincial colleges, which are to a small and varying extent in possession of funds which could be or are used to promote scientific research. Amongst all these variously arranged institutions there is an extraordinarily small amount of provision for biological research. In London there is one professorship only, that at the Normal School of Science, which is maintained by a stipend paid by the State, and has a laboratory and salaried assistants, similarly maintained, in connection with it. The only other posts in London which are provided with stipends intended to enable their holders to pursue researches in the domain of biological science, are the two chairs of physiology and of zoology at University College, which, through the munificence of a private individual (Mr. Jodrell), have been endowed to the extent of 300*l.* a year each. To these should be added, in our calculation, certain posts in connection with the British Museum of Natural History and the Royal Gardens at Kew, maintained by the State; though it must be remembered that a large part of the expenditure in those institutions is necessarily taken up in the preservation of great national collections, and is not applicable to the subvention of investigators. We may, however, reckon about six posts, great and small, in the British

[1] From the fact that the salaries of judges, civil servants, military and naval officers, parsons and schoolmasters, as also the fees of physicians and lawyers, are in Germany even less than half what is paid to their representatives in England, I think that we are justified in making this estimate.

Museum, and four at Kew, as coming into the category which we have in view. In London, then, we may reckon approximately some fourteen or fifteen subsidised posts for biological research. In Oxford there fall under this category the professorship of anatomy and his assistant, that of physiology, that of zoology, that of botany. The Oxford professorships are well supported by endowment, averaging 700*l*. or 800*l*. a year; but they are inadequately provided with assistance as compared with corresponding German positions. Whilst Oxford has thus five posts, Cambridge has at present the same number, though the stipends are of less average value. In regard to Durham, it does not appear that the biological professorships (which have their seat in the Newcastle College of Science) are supported by stipends derived from endowment: they fall under another category, to which allusion will be made below, of purely teaching positions, supported by the fees paid for such teaching by pupils. The Victoria University (Owens College, Manchester), supports its professors of physiology, anatomy, zoology, botany, and pathology, by means partly of endowment, partly of pupils' fees. By the provision of adequate laboratories and of salaries for assistants to each professor, and of student-fellowships, Owens College gives direct support to original investigation. We may reckon five major and eight minor posts as dedicated to biological research in this college. Altogether, then, we have 15 positions in London and 23 in the provinces (taking assistantships, and professorships, and curatorships together)—a total of 38 in all England with its 25,000,000 inhabitants, as against the 300 in Germany with its 45,000,000 inhabitants. In proportion to its population (leaving aside the consideration of its greater wealth), England has only about one-fourth of the provision for the advancement of biological research which exists in Germany.

It would not be fair to reckon in this comparison the various biological professorships in small colleges recently created, and paid to a small extent by stipends derived from endowments, in the provincial towns of England, for the holders of these chairs are called upon to teach a variety of subjects, for instance, zoology, botany, and geology combined; and not only is the devotion of the energies of their teaching staff to scientific discovery not contemplated in the arrangement of these institutions, but, as a matter of fact, the large demands made on the professors in the way of teaching must deprive them of the time necessary for any serious investigation. Such posts, in the fact that neither time, assistants, nor proper laboratories are provided to enable their holders to engage in scientific research, are schoolmasterships rather than professorships, as the word is used in German universities.

One result of the exceedingly small provision of positions in England similar to those furnished by the German university system, and of the irregular, uncertain character of many of those which do exist, is that there is an insufficient supply of young men willing to enter upon the career of zoologist, botanist, physiologist, or pathologist as a profession. The number of posts is too small to create a profession, *i.e.* an avenue of success; and consequently, whereas in Germany there is always a large body of new men ready to fill up the vacancies as they occur in the professorial organisation, in England it very naturally does not appear to our university students as a reasonable thing to enter upon research as a profession, when the chances of employment are so few and far between.

Before stating, as I propose to do, what appears to me a reasonable and proper method of removing to some extent the defect in our national life due to the want of provision for scientific research, I will endeavour to meet some of the objections which are usually raised to such views as those which I am advocating. The endowment of research by the State, or from public funds of any kind, is opposed on various grounds. One is that such action on the part of the Government is well enough in Continental States, but is contrary to the spirit of English statecraft, which leaves scientific as well as other *enterprise* to the individual initiative of the people. This objection is based on error, both as to fact and theory. It is well enough to leave to individual effort the conduct of such enterprises as are remunerative to the parties who conduct them; but it is a mistake to speak of scientific research as an "enterprise" at all. The mistake arises from the extraordinary pertinacity with which so-called "invention" is confounded with the discovery of scientific truth. New knowledge in biological or other branches of science cannot be sold; it has no marketable value. Koch could not have sold the discovery of the Bacterium of phthisis for as much as sixpence, had he wished to do so. Accordingly, we find that there is not, and never has been, any tendency among the citizens of this country to provide for themselves institutions for the manufacture of an article of so little pecuniary value to the individual who turns it out as is new knowledge. On the other hand, as a matter of fact, the providing of means for the manufacture of that article is not only not foreign to English statecraft, but is largely, though not largely enough, undertaken by the English State. The Royal Observatories, the British Museum, the Royal Gardens at Kew, the Geological Survey, the Government grant of 4,000*l*. a year to the Royal Society, the 300*l*. or 400*l*. a year (not a large sum) expended through the medical officer of the Privy Council upon the experimental investigation of disease, are ample evidence that such providing of means for creating new knowledge forms part of the natural and recognised responsibilities of the British Government. Such a responsibility clearly is recognised in this country, and does fall, according to the present arrangement of things, upon the central Government. What we have to regret is, that those who temporarily hold the reins of government fail to perceive the lamentable inadequacy of the mode in which this responsibility is met.

A second objection which is made to the endowment of research by public funds, or by other means, such as voluntary contributions, is this: it is stated that men engaged in scientific research ought to *teach*, and thus gain their livelihood. It is argued, in fact, that there is no need whatever to provide stipends or laboratories for researchers, since they have only to stand up and teach in order to make incomes sufficient to keep them and their families, and to provide themselves with laboratories. This is a very plausible statement, because it is the fact that some investigators have also been excellent lecturers, and have been able to make an income by teaching whilst carrying on a limited amount of scientific investigation. But neither by teaching in the form of popular lectures, nor by teaching university or professional students who desire as a result to pass some examination test, is it possible, where there is a fair field and no favour, for a man to gain a reasonable income and at the same time to leave himself time and energy to carry on original investigations in science.

In some universities, such as those of Scotland, the privilege of conferring degrees of pecuniary value to their possessors becomes a source of income to the professors of the university; they are, in fact, able to make considerable incomes, independently of endowment, by compelling the candidates for degrees to pay a fee to each professor in the faculty for the right of attending his lectures and of presentation to the degree. Consequently, teaching here appears to be producing an income which may support a researcher; in reality, it is the acquisition of the university degree, and not necessarily the teaching, for which the pupil pays his fee. Where the teacher is unprotected by any compulsory regulations (such as that which requires attendance on his lectures and fee-payment on the part of the pupils) it is *impossible* for him to obtain such an income by teaching for one hour a day as will enable him to devote the rest of the day to unremunerative study and investigation, for the following reason. Other teachers, equally satisfactory as teachers, will enter into competition with him, without having the same intention of teaching for one hour only, and of carrying on researches for the rest of the day. They will contemplate teaching for six hours a day, and they will accordingly offer to those who require to be taught either six hours' teaching for the same fee which the researcher charges for one, or one hour for a sixth part of that fee. Consequently the unprotected researcher will find his lecture-room deserted—pupils will naturally go to the equally good teacher who gives more teaching for the same fee, or the same teaching for a less cost. And no one can say that this is not as it should be. The university pupil requires a certain course of instruction, which he ought to be able to buy at the cheapest rate. It does not seem to be doing justice to the pupil to compel him to form one of a class consisting of some hundreds of hearers, where he can obtain but little personal supervision or attention from the teacher, whereas if he had the free disposal of his fee, he might obtain six times the amount of attention from another teacher. This arrangement does not seem to be justifiable, even for the purpose of providing the university professor with an income and leisure to pursue scientific research. The student's fee should pay for a given amount of teaching at the market value, and he has just cause of complaint if, by compulsory enactments, he is taxed to provide the country with scientific investigation.

Teaching must, in all fairness, ultimately be paid for as teaching, and scientific research must be provided for out of other funds than those extracted from the pockets of needy students, who have a reasonable right to demand, in return for their fees, a full modicum of instruction and direction in study.

In the German universities, the professor receives a stipend which provides for him as an investigator. He also gives lectures, for which he charges a fee, but no student is compelled to attend those lectures as a condition of obtaining his degree. Accordingly, independent teachers can, and do, compete with the professor in providing for the students' requirements in the matter of instruction. As a consequence, the fees charged for teaching are exceeding small, and the student can feel assured that he is obtaining his money's worth for his money. He is not compelled to pay any fee to any teacher as a condition of his promotion to the university degree. In a German university, if the professor in a given subject is incompetent, or the class over-crowded, the student can take his fee to a private teacher, and get better teaching; all that is required of the candidate, as a condition of his promotion to the Doctor's degree, is that he shall satisfy the examination-tests imposed by the faculty, and produce an original thesis.

Unless there be some such compelling influence as that obtaining in the Scotch universities, enabling the would-be researcher to gather to him pupils and fees without fear of competition, it seems impossible that he should gain an income by teaching whilst reserving to himself time and energy for the pursuit of scientific inquiry. It is thus seen that the necessity of endowment, in some form or another, to make provision for scientific research, is a reality, in spite of the suggestion that teaching affords a means whereby the researcher may readily provide for himself. The simple fact is that a teacher can only make a sufficient income by teaching, on the condition that he devotes his whole time and energy to that occupation.

Whilst I feel called upon to emphatically distinguish the two functions—viz., that of *creating new knowledge*, and that of *distributing existing knowledge*—and to maintain that it is only by arbitrary and undesirable arrangements, not likely to be tolerated, or, at any rate, extended, at the present day, that the latter can be made to serve as the support of the former, I must be careful to point out that I agree most cordially with those who hold that it is an excellent thing for a man who is engaged in the one to give a certain amount of time to the other. It is a matter of experience that the best teachers of a subject are, *cæteris paribus*, those who are actually engaged in the advancement of that subject, and who have shown such a thorough understanding of that subject as is necessary for making new knowledge in connection with it. It is also, in most cases, a good thing for the man engaged in research to have a certain small amount of change of occupation, and to be called upon to take such a survey of the subject in connection with which his researches are made, as is involved in the delivery of a course of lectures and other details of teaching. Though it is not a thing to be contemplated that the researcher shall sell his instruction at a price sufficiently high to enable him to live by teaching, yet it is a good thing to make teaching an additional and subsidiary part of his life's work. This end is effected in Germany by making it a duty of the professor, already supported by a stipend, to give some five or six lectures a week during the academical session, for which he is paid by the fees of his hearers. The fees are low, but are sufficient to be an inducement; and, inasmuch as the attendance of the students is not compulsory, the professor is stimulated to produce good and effective lectures at a reasonable charge, so as to attract pupils who would seek instruction from some one else if the lectures were not good or the fees too high. Indeed, in Germany this system works so much to the advantage of the students, that the private teachers of the universities at one time obtained the creation of a regulation forbidding the professors to reduce their fees below a certain minimum, since, with so low a fee as some professors were charging, it was impossible for a private teacher to compete! This state of things may be compared, with much advantage, with the condition of British universities. In these we hear, from one direction, complaints of the high fees charged and of the ineffective teaching given by the professoriate; and in other universities, where no adequate fees are allowed to the professors as a stimulus to them to offer useful and efficient teaching, we find that the teaching has passed entirely out of their hands into those of college tutors and lecturers. The fact is that a satisfactory relation between teaching and research is one which will not naturally and spontaneously arrange itself. It can hardly be said to exist in any British university or college, but the method has been thought out and carried into practice in Germany. It consists in giving a competent researcher a stipend and a laboratory for his research work, and then requiring him to do a small amount of teaching, remunerated by fees proportionate to his ability and the pains which he may take in his teaching. If you pay him a fixed sum as a teacher, or artifically insure the attendance of his class, instead of letting this part of his income vary simply and directly with the attractiveness of his teaching, you will find as the result that (with rare exceptions) he will not give effective and useful teaching. He will naturally tend to do the minimum required of him, in a perfunctory way. On the other hand, if you leave him without stipend as a researcher, dependent on the fees of pupils for an income, he will give all his time and energies to teaching, he will cease to do any research, and become, *pro tanto*, an inferior teacher.

A third objection which is sometimes made to the proposition that scientific research must be supported and paid for as such, is the following: It is believed by many persons that a man who occupies his best energies in scientific research can always, if he choose, make an income by writing popular books or newspaper articles in his spare hours; and, accordingly, it is gravely maintained that there is no need to provide stipends and the means of carrying on their work for researchers. To do so, according to this view, would be to encourage them in an exclusive reticence, and to remove from them the inducement to address the public on the subject of their researches, by which the public would lose valuable instruction.

This view has been seriously urged, or I should not here notice it. Any one who is acquainted with the sale of scientific books, and the profits which either author or publisher makes by them, knows that the suggestion which I have quoted is ludicrous. The writing of a good book is not a thing to be done in leisure moments, and such as have been the result of original research have cost their authors often years of labour apart from the mere writing. Mr. Darwin's books, no doubt, have had a large sale; but that is due to the fact, apart from the exceptional genius of the man who wrote them, that they represent some thirty or more years of hard work, during which he was silent. There is not a sufficiently large public interested in the progress of science to enable a researcher to gain an income by writing books, however great his literary facility. A school-book or class-book may now and then add more or less to the income of a scientific investigator; but he who becomes the popular exponent of scientific ideas, except in a very moderate and limited degree, must abandon the work of creating new knowledge. The professional *littérateur* of science is as much removed by his occupation from all opportunity of serious investigation as is the professional teacher who has to consume all his time in teaching. Any other profession—such as the Bar, Medicine, or the Church—is more likely to leave one of its followers time and means for scientific research than is that of either the popular writer or the successful teacher.

We have, then, seen that there is no escape from the necessity of providing stipends and laboratories for the purpose of creating new knowledge, as is done in continental States, if we are agreed that more of this new knowledge is needed and is among the products which a civilised community is bound to turn out, both for its own benefit and for that of the community of States, which give to and take from one another in such matters.

There are some who would finally attack our contention by denying that new knowledge is a good thing, and by refusing to recognise any obligation on the part of England to contribute her share to that common stock of increasing knowledge by which she necessarily profits. Among such persons are those who would prohibit altogether the pursuit of experimental physiology in England, and yet would not and do not hesitate to avail themselves of the services of medical men, whose power of rendering those services depends on the fact that they have learnt the results obtained by the experiments of physiologists in other countries or in former times. In reference to this strange contempt and even hatred of science, which undoubtedly has an existence among some persons of consideration, even at the present day, I shall have a few words to say before concluding this address. I have now to ask you to listen to what seems to me to be the demand which we should make, as members of a British Association for the Advancement of Science, in respect of adequate provision for the creation of new knowledge in the field of biology in England.

Taking England alone, as distinct from Scotland and Ireland, we require in order to be approximately on a level with Germany, forty new biological institutes, distributed among the five branches of physiology, zoology, anatomy, pathology, and botany—forty in addition to the fifteen which we may reckon (taking one place with another) as already existing. The average cost of the buildings required would be about 4,000*l.* for each, giving a total initial expenditure of 160,000*l.*; the average cost of stipends for the director, assistants, and maintenance we may calculate at 1,500*l.* annually for each, or 60,000*l.* for the forty—equal to a capital sum of 2,000,000*l.* These institutes should be distributed in groups of five—eight groups in all—throughout the country. One such group would be placed in London (which is, at present, almost totally destitute of such arrangements), one in Bristol, one in Birmingham, one in Nottingham, one in Leeds, one in Newcastle, one in Ipswich, one in Cardiff, one in Plymouth—in fact, one in each of the great towns of the kingdom where there is at present, or where there might be with advantage, a centre of professional education and higher study. The first and the most liberally arranged of these biological institutes—embracing its five branches, each with its special laboratory and staff—should be in London. If we can have nothing else, surely we may demand, with some hope that our request will eventually obtain compliance, the formation in London of a College of Scientific Research similar to that of Paris (the Collège de France). It is one of the misfortunes and disgraces of London that—alone amongst the capitals of Europe, with the exception of Constantinople—it is destitute of any institution corresponding to the universities and colleges of research which exist elsewhere.

Either in connection with a properly organised teaching university or as an independent institution, it seems to me a primary need of the day that the Government should establish in London laboratories for scientific research. Two hundred and fifty years ago Sir Thomas Gresham founded an institution for scientific research in the City of London. The property which he left for this purpose is now estimated to be worth three millions sterling. This property was deliberately appropriated to other uses by the Corporation of the City of London and the Mercers' Company about a hundred years since, with the consent of both Houses of Parliament. By this outrageous act of spoliation these Corporations, who were the trustees of Gresham, have incurred the curse which he quaintly inserted in his will in the hope of restraining them from attempts to divert his property from the uses to which he destined it. "Gresham's curse" runs as follows:—"And that I do require and charge the said Corporations and chief governors thereof, with circumspect Diligence and without long Delay, to procure and see to be done and obtained, as they will answer the same before Almighty God; (for if they or any of them should neglect the obtaining of such Licenses or Warrants, which I trust can not be difficult, nor so chargeable, but that the overplus of my Rents and Profits of the Premisses hereinbefore to them disposed, will soon recompense the same; because to soe good Purpose in the Commonwealth, no Prince nor Council in any Age, will deny or defeat the same. And if conveniently by my Will or other Convenience, I might assure it, I would not leave it to be done after my death, then the same shall revert to my heirs, whereas I do mean the same to the Commonwealth, and then THE DEFAULT THEREOF SHALL BE TO THE REPROACH AND CONDEMNATION OF THE SAID CORPORATIONS AFORE GOD)." I confess that I find it difficult to see how the present representatives of the Corporations who perverted Gresham's trust are to escape from justly deserving the curse pronounced against those Corporations, unless they conscientiously take steps to restore Gresham's money to its proper uses. Let us hope that Gresham's curse may be realised in no more deadly form than that of an Act of Parliament repealing the former one which sanctioned the perversion of Gresham's money. Such a sequel to the Report of the Commission which has recently inquired into the proceedings of the Corporation and Companies of the City of London is not unlikely.

Whilst we should, I think, especially press upon public attention the need for an institute of scientific research in London, and indicate the source from which its funds may be fitly derived, we must also urge the foundation of other institutes in the provinces upon the scale already sketched, because it is only by the existence of numerous posts, and of a series of such posts—some of greater and some of less value, the latter more numerous than the former—that anything like a professional career for scientific workers can be constructed. It is especially necessary to constitute what I have termed "assistantships," that is, junior posts in which younger men assist and are trained by more experienced men. Even in the few institutions which do already exist additional provision of this kind is what is wanted more than anything else, so that there may be a progressive career open to the young student, and a sufficient field of trained investigators from which to select in filling up the vacancies in more valuable positions.

I am well aware that it will be said that the scheme which I have proposed to you is gigantic and almost alarming in respect of the amount of money which it demands. One hundred and sixty thousand pounds a year for biology alone must seem, not to my hearers, but to those who regard biology as an amusing speculation—that is to say, who know little or nothing about it—an extravagant suggestion. Unfortunately it is also true that such persons are very numerous—in fact, constitute an overwhelming majority of the community; but they are becoming less numerous every day. The time will come, it seems possible, when there will be more than one member of the Government who will understand and appreciate the value of scientific research. There are already a few members of the House of Commons who are fully alive to its significance and importance.

We may have to wait for the expenditure of such a sum as I have named, and possibly it may be derived ultimately from local rather than imperial sources, though I do not see why it should be; yet I think it is a good thing to realise *now* that this is what we ought to expend in order to be on a level with Germany. This apparently extravagant and unheard of appropriation of public money is *actually made every year in Germany*.

I think it is well to put the matter before you in this definite manner, because I have reason to believe that even those whom we might expect to be well informed in regard to such matters, are not so, and as a consequence there is not that keen sense of the inferiority and inadequacy of English arrangements in these matters which one would gladly see actuating the conduct of English statesmen. For instance, only a few years ago, when speaking at Nottingham, the present Prime Minister, who has taken an active part in rearranging our universities, and has, it is well known, much interest in science and learning, stated that 27,000*l.*, the capital sum expended on the Nottingham College of Science, was a very important contribution to the support of learning in this country, amounting, as he said he was able to state, from the perusal of official documents, to as much as one-third of what was spent in Germany during the past year upon her numerous universities, which were so often held up to England as an example of a well supported academical system. Now, I do not think that Mr. Gladstone can have ever had the opportunity of considering the actual facts with regard to German universities, for he was in this instance misled by the official return of expenditure on a single university, namely, that of Strasburg; the total annual expenditure on the twenty-one German universities being, in reality, about 800,000*l.*, by the side of which a capital sum of 27,000*l.* looks very small indeed. I cannot but believe that if the facts were known to public men, in reference to the expenditure incurred by foreign States in support of scientific inquiry, they would be willing to do something in this country of a sufficient and statesmanlike character. As it is, the concessions which have been made in this direction appear to me to be in some instances not based upon a really comprehensive knowledge of the situation. Thus the tentative grant of 4,000*l.* a year from the Treasury to the Royal Society of London appears to me not to be a well-devised experiment in the promotion of scientific research by means of grants of money, because it is on too small a scale to produce any definite effect, and because the money cannot be relied upon from year to year as a permanent source of support to any serious undertaking.

The Royal Society most laboriously and conscientiously does its best to use this money to the satisfaction of the country, but the task thus assigned to it is one of almost insurmountable difficulty. In fact, no such miniature experiments are needed. The experiment has been made on a large scale in Germany, and satisfactory results have been obtained. The reasonable course to pursue is to benefit by the experience, as to details and methods of administration, obtained in the course of the last sixty years in Germany, and to apply that experience to our own case.

It is quite clear that "the voluntary principle" can do little towards the adequate endowment of scientific research. Ancient endowments belonging to the country must be applied thereto, or else local or imperial taxes must be the source of the necessary support. Seeing that the results of research are distinctly of imperial, and not of local value—it would seem appropriate that a portion of the imperial revenue should be devoted to their achievement. In fact, as I have before mentioned, the principle of such an application of public money has long been admitted, and is in operation.

Whilst voluntary donations on the part of private persons can do little to constitute a fund which shall provide the requisite endowment for the scheme of biological institutes which I have sketched (not to mention those required for other branches of science), yet those who are interested in the progress of scientific investigation may by individual effort do something, however little, towards placing research in a more advantageous position in this country. Supposing it were possible, as I am sanguine enough to believe that it is, to collect in the course of a year or two from private sources a sum of 20,000*l*. for the maintenance of a biological laboratory and staff, it would be necessary, in expending so limited a sum, to aim at the provision of something which would be likely to produce the largest and most obvious results in return for the outlay, and to benefit the largest number of scientific observers in this department.

I believe that it is the general opinion among biologists that there could be no more generally useful institution thus set in operation than a biological laboratory upon the sea-coast, which, besides its own permanent staff of officers, would throw open its resources to such naturalists as might from time to time be able to devote themselves to researches within its precincts. There is no such laboratory on the whole of the long line of British coast. At Naples there is Dr. Dohrn's celebrated and invaluable laboratory, which is frequented by naturalists from all parts of the world; at Trieste the Austrian Government supports such a laboratory; at Concarneau, Roscoff, and Villefranche, the French Government has such institutions; at Beaufort, in North Carolina, the Johns Hopkins University has its marine laboratory; and at Newport, Professor Alexander Agassiz has arranged a very perfect institution also for the study of marine life. In spite of the great interest which English naturalists have always taken in the exploration of the sea and marine organisms—in spite of the fact that the success and even the existence of our fisheries-industries to a large extent depends upon our gaining the knowledge which a well-organised laboratory of marine biology would help us to gain, there is actually no such institution in existence.

This is not the occasion on which to explain precisely how and to what extent a laboratory of marine zoology might be of national importance. I hope to see that matter brought before the Section during the course of our meeting. But I may point out now, that though it appears to me that the great need for biological institutes, to which I have drawn your attention, can *not* be met by private munificence, and must in the end be arranged for by the continued action of the Government in carrying out a policy to which it has for many years been committed, and which has been approved by Conservatives and Liberals alike—yet such a special institution as a laboratory of marine biology, serving as a temporary workshop to any and all of our numerous students of the important problems connected with the life of marine plants and animals, might very well be undertaken from private funds. Should it be possible, on the occasion of this meeting of the British Association in Southport, to obtain some promise of assistance towards the realisation of this project, I think we shall be able to congratulate ourselves on having done something, though small perhaps in amount, towards making better provision for biological research, and therefore something towards the advancement of science.

In conclusion, let me say that, in advocating to-day the claim of biological science to a far greater measure of support than it receives at present from the public funds, I have endeavoured to press that claim chiefly on the ground of the obvious utility to the community of that kind of knowledge which is called biology. I have endeavoured to meet the opposition of those who object to the interference of the State wherever it may be possible to attain the end in view without such interference, but who profess themselves willing to see public money expended in promoting objects which are of real importance to the country, and which cannot be trusted to the voluntary enterprise arising from the operation of the laws of self-preservation and the struggle for wealth. There are, however, it seems to me, further reasons for desiring a thorough and practical recognition by the State of the value of scientific research. There are not wanting persons of some cultivation who have perceived and fully realised the value of that knowledge which is called science, and of its methods, and yet are anxious to restrain rather than to aid the growth of that knowledge. They find in science something inimical to their own interests, and accordingly either condemn it as dangerous and untrustworthy, or encourage themselves to treat it with contempt by asserting that "after all, science counts for very little"—a statement which is unhappily true in one sense, though totally untrue when it is intended to signify that the progress of science is not a matter which profoundly influences every factor in the well-being of the community. Amongst such people there is a positive hatred of science, which finds expression in their exclusion of it, even at this day, from the ordinary curriculum of public school education, and in the baseless though oft-repeated calumny that science is hostile to art, and is responsible for all that is harsh, ugly, and repulsive in modern life. To such opponents of the advancement of science, it is of little use to offer explanations and arguments. But we may, when we reflect on their instinctive hostility and the misrepresentations of science and the scientific spirit which it leads them to disseminate, console ourselves by bringing to mind what science really is, and what truly is the nature of that calling in which a man who makes new knowledge is engaged.

They mock at the botanist as a pedant, and the zoologist as a monomaniac; they execrate the physiologist as a monster of cruelty, and brand the geologist as a blasphemer; chemistry is held responsible for the abomination of aniline dyes and the pollution of rivers, and physics for the dirt and misery of great factory towns. By these unbelievers science is declared responsible for individual eccentricities of character, as well as for the sins of the commercial utilisers of new knowledge. The pursuit of science is said to produce a dearth of imagination, incapability of enjoying the beauty either of nature or of art, scorn of literary culture, arrogance, irreverence, vanity, and the ambition of personal glorification.

Such are the charges from time to time made by those who dislike science, and for such reasons they would withhold, and persuade others to withhold, the fair measure of support for scientific research which this country owes to the community of civilised States. Not in reply to these misrepresentations, but by way of contrast, I would here state what science seems to be to those who are on the other side, and how, therefore, it seems to them wrong to delay in doing all that the wealth and power of the State can do to promote its progress.

Science is not a name applicable to any one branch of knowledge, but includes all knowledge which is of a certain order or scale of completeness. All knowledge which is deep enough to touch the causes of things, is Science; all inquiry into the causes of things is scientific inquiry. It is not only co-extensive with the area of human knowledge, but no branch of it can advance far without reacting upon other branches; no department of Science can be neglected without sooner or later causing a check to other departments. No man can truly say this branch of Science is useful and shall be cultivated, whilst this is worthless and shall be let alone; for all are necessary, and one grows by the aid of another, and in turn furnishes methods and results assisting in the progress of that from which it lately borrowed.

We desire the increase and the support and the acceptance of Science, not only because it has a certain material value and enables men to battle with the forces of nature and to turn them to account, so as to increase both the intensity and the extension of healthy human life: that is a good reason, and for some persons, it may be, the only reason. But there is something to be said beyond this.

The pursuit of scientific discovery, the making of new knowledge, gratifies an appetite which, from whatever cause it may arise, is deeply seated in man's nature, and indeed is the most distinctive of his properties. Man owes this intense desire to know the nature of things, smothered though it often be by other cravings which he shares with the brutes, to an inherited race-perception stronger than the reasoning faculty of the individual. When once aroused and in a measure gratified, this desire becomes a guiding passion. The instinctive tendency to search out the causes of things, gradually strengthening as generation after generation of men have stumbled and struggled in ignorance, has at last become an active and widely extending force: it has given rise to a new faith.

To obey this instinct—that is, to aid in the production of new knowledge—is the keenest and the purest pleasure of which man is capable, greater than that derived from the exercise of his animal faculties, in proportion as man's mind is something greater and further developed than the mind of brutes. It is in itself an unmixed good, the one thing which commends itself as still "worth while" when all other employments and delights prove themselves stale and unprofitable.

Arrogant and foolish as those men have appeared who, in times of persecution and in the midst of a contemptuous society, have, with an ardour proportioned to the prevailing neglect, pursued some special line of scientific inquiry, it is nevertheless true that in itself, apart from special social conditions, Science must develop in a community which honours and desires it before all things, qualities and characteristics which are the highest, the most human of human attributes. These are, firstly, the fearless love and unflinching acceptance of truth; hopeful patience; that true humility which is content not to know what cannot be known, yet labours and waits; love of Nature, who is not less, but more, worshipped by those who know her best; love of the human brotherhood for whom and with whom the growth of Science is desired and effected.

No one can trace the limits of Science, nor the possibilities of happiness both of mind and body which it may bring in the future to mankind. Boundless though the prospect is yet the minutest contribution to the onward growth has its absolute and unassailable value; once made it can never be lost; its effect is for ever in the history of man.

Arts perish, and the noblest works which artists give to the world. Art (though the source of great and noble delights) cannot create nor perpetuate; it embodies only that which already exists in human experience, whilst the results of its highest flights are doomed to decay and sterility. A vain regret, a constant effort to emulate or to imitate the past, is the fitting and laudable characteristic of Art at the present day. There is, indeed, no truth in the popular partition of human affairs between Science and Art as between two antagonistic or even comparable interests; but the contrast which they present in points such as those just mentioned is forcible. Science is essentially creative; new knowledge—the experience and understanding of things which were *previously non-existent for man's intelligence*, is its constant achievement. And these creations never perish; the new is built on and incorporates the old; there is no turning back to recover what has lapsed through age; the oldest discovery is even fresher than the new, yielding in ever increasing number new results, in which it is itself reproduced and perpetuated, as the parent in the child.

This, then, is the faith which has taken shape in proportion as the innate desire of man for more knowledge has asserted itself—namely, that there is no greater good than the increase of Science; that through it all other good will follow. Good as Science is in itself, the desire and search for it is even better, raising men above vile things and worthless competitions to a fuller life and keener enjoyments. Through it we believe that man will be saved from misery and degradation, not merely acquiring new material powers, but learning to use and to guide his life with understanding. Through Science he will be freed from the fetters of superstition; through faith in Science he will acquire a new and enduring delight in the exercise of his capacities; he will gain a zest and interest in life such as the present phase of culture fails to supply.

In opposition to the view that the pursuit of Science can obtain a strong hold upon human life, it may be argued, that on no reasonable ground can it appear a necessary or advantageous thing to the individual man to concern himself with the growth and progress of that which is merely likely to benefit the distant posterity of the human race. Our reply is: Let those who contend for the reasonableness of human motives develop, if they can, any theory of human conduct in which reasonable self-interest shall be man's guide. We do not contend for any such theory. By reasoning we may explain and trace the development of human nature, but we cannot change it by any such process. It is demonstrably unreasonable for the individual man, guided by self-interest, to share the dangers and privations of his brother-man, and yet, in common with many lower animals, he has an inherited quality which makes it a pleasure to him to do so; it is unreasonable for the mother to protect her offspring, and yet it is the natural and inherited quality of mothers to derive pleasure from doing so; it is unreasonable for the half-starved poor to aid their wholly starving brethren, and yet such compassion is natural and pleasurable to those who show it, and is the constant rule of life. Unreasonable though these things are from the point of view of individual self-interest, yet they are done because to do them is pleasurable, to leave them undone a pain. The race has, as it were, in these respects befooled the individual, and in the course of evolution has planted in him, in its own interests, an irrational capacity for taking pleasure in doing that which no reasoning in regard to self-interest could justify. As with these lower and more widely distributed instincts, shared by man with some lower social animals, so is it with this higher and more peculiar instinct—the tendency to pursue new knowledge. Whether reasonable or not, it has by the laws of heredity and selection become part of us and exists: its operation is beneficial to the race: its gratification is a source of keen pleasure to the individual—an end in itself. We may safely count upon it as a factor in human nature; it is in our power to cultivate and develop it, or, on the other hand, to starve and distort it for a while, though to do so is to waste time in opposing the irresistible.

As day by day the old-fashioned stimulus to the higher life loses the dread control which it once exercised over the thoughts of men, the pursuit of wealth and the indulgence in fruitless gratifications of sense become to an increasing number the chief concerns of their mental life. Such occupations fail to satisfy the deep desires of humanity; they become wearisome and meaningless, so that we hear men questioning whether life be worth living. When the dreams and aspirations of the youthful world have lost their old significance and their strong power to raise men's lives, it will be well for that community which has organised in time a following of and a reverence for an ideal Good, which may serve to lift the national mind above the level of sensuality and to insure a belief in the hopefulness and worth of life. The faith in Science can fill this place—the progress of Science is an ideal Good, sufficient to exert this great influence.

It is for this reason more than any other, as it seems to those who hold this faith, that the progress and diffusion of scientific research, its encouragement and reverential nurture, should be a chief business of the community, whether collectively or individually, at the present day.

Department of Anthropology

ADDRESS BY WILLIAM PENGELLY, F.R.S., F.G.S., VICE-PRESIDENT OF THE SECTION.

ANTHROPOLOGY, on one of its numerous sides, marches with geology; and hence it is, no doubt, that I, for many years a labourer very near this somewhat ill-defined border, have been invited to assist my friends and neighbours in the work which lies before them during the Association week. I have the more cheerfully accepted the invitation from a vivid recollection that, when on a few occasions I have come uninvited into this Department, my reception has been so very cordial as to lead me to ask myself whether the reports which for many years (1864 to 1880) I laid annually before my geological brethren did not derive their chief interest from their anthropological bearings and teachings.

In 1858—a quarter of a century ago—I had the pleasure of reading to the Geological Section of the Association the first public communication on the exploration, then in progress, of Brixham Cavern (more correctly, Brixham Windmill Hill Cavern); and as any interest connected with that paper lay entirely in the evidence it contained of the inosculation and contemporaneity of human industrial relics, of a rude character, with remains of certain extinct mammals, I purpose on this occasion to lay before the Department a few thoughts, retrospective and prospective, which may be said to radiate from that exploration, confining myself mainly to South Devon.

Probably nothing will better show the apparent apathy and scepticism with which, up to 1858, all geological evidence of the antiquity of man was received by British geologists generally, than the following statement of facts:—

About the beginning of the second quarter of the present century the late Rev. J. MacEnery made Kent's Cavern, or Kent's Hole, near Torquay, famous by his researches and discoveries there. He not only found flint implements beneath a thick continuous sheet of stalagmite, but, after a most careful and painstaking investigation in the presence of witnesses, arrived at the conclusion that the flints "were deposited in their deep position before the creation of the stalagmite" (*Trans. Devon. Assoc.* iii. 330); and when it was suggested by the Rev. Dr. Buckland, to whom he at once and without reservation communicated all his discoveries, that "the ancient Britons had scooped out ovens in the stalagmite, and that through them the knives got admission to the 'diluvium,'" he replied, "I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam" (*Ibid.* p. 334).

He added, "It is painful to dissent from so high an authority, and more particularly so from my concurrence generally in his views of the phenomena of these caves, which three years' personal observation has in almost every instance enabled me to verify" (*Ibid.* p. 338).

It is, perhaps, not surprising that Dr. Buckland, one of the leading geologists of his day, should be too tenacious of his opinion, and feel too secure in his position to yield to the statements and arguments of his comparatively young friend MacEnery, then scarcely known to the scientific world.

That the position taken by Buckland retarded the progress of truth, and was calculated to check the ardour of research, is apparently certain, and much to be regretted; but it should be remembered that, at least, as early as 1819 he taught that "the two great points . . . of the low antiquity of the human race, and the universality of a recent deluge, are most satisfactorily confirmed by everything that has yet been brought to light by geological investigations" ("Vindiciæ Geologicæ," p. 24); that early in 1822 he reiterated and emphasised these opinions in his famous Kirkdale paper (*Phil. Trans.* for 1822, pp. 171-236), which the Royal Society "crowned with the Copley medal" (*Quart. Journ. Geol. Soc.* vol. xiii. p. xxxiii.); that in 1823, having amplified and revised this paper, he published it as an independent quarto volume under the attractive title of "Reliquiæ Diluvianæ," of which he issued a second edition in 1824; and that, though his acquaintance with Kent's Cavern was much less intimate than that of MacEnery, he, nevertheless, was, of the two, the earlier worker there, and in fact had discovered a flint implement in it before MacEnery had ever seen that or any other cavern—the first tool of the kind found in any cavern, it is believed, and which in all probability was met with under circumstances not in conflict with his published opinion on the low antiquity of man. I confess that under such circumstances, human nature being what it is, the line followed by Dr. Buckland seems to me to have been that which most men would have pursued.

It was, at any rate, the line to which he adhered as late, at least, as 1837, for in his well-known "Bridgewater Treatise," published that year, after describing his visit to the caverns near Liége, famous through the discoveries of Dr. Schmerling, he said, "The human bones found in these caverns are in a state of less decay than those of the extinct species of beasts; they are accompanied by rude flint knives and other instruments of flint and bone, and are probably derived from uncivilised tribes that inhabited the caves. Some of the human bones may also be the remains of individuals who, in more recent times, have been buried in such convenient repositories. M. Schmerling . . . expresses his opinion that these human bones are coeval with those of the quadrupeds, of extinct species, found with them; an opinion from which the author, after a careful examination of M. Schmerling's collection, entirely dissents" (*op. cit.* i. 602).

It may be doubted, however, whether his faith in these, his early, convictions remained unshaken to the end. I have frequently been told by one of his contemporary professors at Oxford, who knew him intimately, that Buckland shrank from the task of preparing for the press new editions of his "Reliquiæ Diluvianæ" and his "Bridgewater Treatise." "The work," he said, "would be not editing, but re-writing."

Mr. MacEnery intended to publish his "Cavern Researches" in one volume quarto, illustrated with thirty plates. In what appears to have been his second prospectus, unfortunately not dated, he said, "The limited circulation of works of this nature, being by no means equal to the expenses attendant on the execution of so large a series" [of plates], "the author is obliged to depart from his original plan, and to solicit the support of those who may feel an interest in the result of his researches."

There is reason to believe that at least twenty-one of the plates were ready, and that the rough copy of much of his manuscript was written; but that, the support he solicited not being forthcoming, the idea of publishing had to be abandoned (see *Trans. Devon. Assoc.* iii. 198–201).

In 1840 Mr. R. A. C. Austen (now Godwin-Austen), F.G.S., read to the Geological Society of London a paper on the Bone Caves of Devonshire, which, with some amplifications, was incorporated in his memoir on the geology of the south-east of Devonshire, printed in the *Transactions* of the Society in 1842 (2nd ser. vi. 433–489). Speaking of his own researches in Kent's Cavern he said, "Human remains and works of art, such as arrow-heads and knives of flint, occur in all parts of the cave and throughout the entire thickness of the clay: and no distinction founded on condition, distribution, or relative position can be observed whereby the human can be separated from the other reliquiæ" (*Ibid.* p. 444).

He added, "My own researches were constantly conducted in parts of the cave which had never been disturbed, and in every instance the bones were procured from beneath a thick covering of stalagmite; so far, then, the bones and works of man must have been introduced into the cave before the flooring of stalagmite had been formed" (*Ibid.* p. 446).

Though these important and emphatic statements were so fortunate as to be committed to the safe keeping of print with but little delay, and under the most favourable circumstances, they appear neither to have excited any interest, nor indeed to have received much, if any, attention.

In 1846, the Torquay Natural History Society appointed a Committee, consisting of Dr. Battersby, Mr. Vivian, and myself—all tolerably familiar with the statements of Mr. MacEnery and Mr. Austen—to make a few diggings in Kent's Cavern for the purpose of obtaining specimens for their museum. The work, though more or less desultory and unsystematic, was by no means carelessly done, and the Committee were unanimously and perfectly satisfied that the objects they met with had been deposited at the same time as the matrix in which they were inhumed. At the close of their investigation they drew up a report which was printed in the *Torquay Directory* for November 6, 1846 (see *Trans. Devon. Assoc.* x. 162). Its substance, embodied in a paper by Mr. Vivian, was read to the Geological Society of London on May 12, 1847, as well as to the British Association in the succeeding June, and the following abstract was printed in the Report of the Association for that year (p. 73):—

"The important point that we have established is, that relics of human art are found *beneath* the unbroken floor of stalagmite. After taking every precaution, by sweeping the surface, and examining most minutely whether there were any traces of the floor having been previously disturbed, we broke through the solid stalagmite in three different parts of the cavern, and in each instance found flint knives. . . . In the spot where the most highly finished specimen was found, the passage was so low that it was extremely difficult, with quarrymen's tools and good workmen, to break through the crust; and the supposition that it had been previously disturbed is impossible."

It will be borne in mind that the same paper was read the month before to the Geological Society. The Council of that body, being apparently unprepared to print in their *Quarterly Journal* the statements it contained, contented themselves with the following notice, given here in its entirety (*op. cit.* iii. 353):—

"On Kent's Cavern, near Torquay," by Mr. Edward Vivian.—"In this paper an account was given of some recent researches in that cavern by a committee of the Torquay Natural History Society, during which the bones of various extinct species of animals were found in several situations."

It will be observed that the "flint knives" were utterly ignored, a fact rendered the more significant by the following announcement on the wrapper of the journal:—"The Editor of the *Quarterly Journal* is directed to make it known to the public that the authors alone are responsible for the facts and opinions contained in their respective papers."

Such, briefly, were the principal researches in Kent's Cavern, at intervals from 1825 to 1847. Their reception was by no means encouraging: Mr. MacEnery, after incurring very considerable expense, was under the necessity of abandoning the intention of publishing his "Cavern Researches;" Mr. Austen's paper, though printed unabridged, was given to an apathetic, unbelieving world, and was apparently without effect; and Mr. Vivian's paper, virtually the report by a committee of which he was a member, was cut down to four lines of a harmless, unexciting character.

For some years nothing occurred to break the quietude, which but for an unexpected discovery on the southern shore of Torbay would probably have remained to this day.

Early in 1858 the workmen engaged in a limestone quarry on Windmill Hill, overhanging the fishing town of Brixham in South Devon, broke unexpectedly a hole through what proved to be the roof of an unknown and unsuspected cavern. I visited it very soon after the discovery, and secured to myself the refusal of a lease to include the right of exploration. As the story of this cavern has been told at some length elsewhere (see *Phil. Trans.* clxiii. 471–572; or *Trans. Devon. Assoc.* vi. 775–856). it will here suffice to say that at the instance of the late Dr. H. Falconer, the eminent palæontologist, the subject was taken up very cordially by the Royal and Geological Societies of London, a Committee was appointed by the latter body, the exploration was placed under the superintendence of Mr. (now Prof.) Prestwich and myself, and, being the only resident member of the Committee, the actual superintendence fell of necessity to me.

The following facts connected with this cavern were no doubt influential in leading to the decision to have it explored:—

1. It was a virgin cave which had been hermetically sealed during an incalculably long period, the last previous event in its history being the introduction of a reindeer antler, found attached to the upper surface of the stalagmitic floor. It was therefore free from the objection urged sometimes against Kent's Cavern, that, having been known from time immemorial, and up to 1825 always open to all comers, it had perhaps been ransacked again and again.

2. It was believed, and it proved, to be a comparatively very small cavern, so that its complete exploration was not likely to require a large expenditure of time or of money.

It will be seen that the exploration was placed under circumstances much more likely to command attention than any of those which had preceded it. It was to be carried on under the auspices of the Royal and Geological Societies, by a Committee consisting of Mr. S. H. Beckles, Mr. G. Busk, Rev. R. Everest, Dr. H. Falconer, Mr. Godwin-Austen, Sir C. Lyell, Prof. Owen, Dr. J. Percy, Mr. J. Prestwich, Prof. (now Sir A. C.) Ramsay, and myself—all Fellows of the Geological Society, and almost all of them of the Royal Society also.

It was impossible not to feel, however, that the mode of exploration must be such as would not merely satisfy those actually engaged in the work, but such as would command for the results which might be obtained the acceptance of the scientific world generally. Hence I resolved to have nothing whatever to do with "trial pits" here and there, or with shafts to be sunk in selected places; but, first, to examine and remove the stalagmite floor; then the entire bed immediately below (if not of inconvenient depth) horizontally throughout the entire length of the cavern, or so far as practicable; this accomplished, to proceed in like manner with the next lower bed; and so on until all the deposits had been removed.

This method, uniformly followed, was preferable to any other, because it would reveal the general stratigraphical order of the deposits, with the amount and direction of such "dip" as they might have, as well as any variations in the thickness of the beds; it would afford the only chance of securing all the fossils, and of thus ascertaining, not only the different kinds of animals represented in the cave, but also the ratios which the numbers of individuals of the various species bore to one another, as well as all peculiar or noteworthy collocations; it would disclose the extent, character, and general features of the cavern itself; it was undoubtedly the least expensive mode of exploration; and it would render it almost impossible to refer bones or indications of human existence to wrong beds, depths, or associations.

The work was begun in July, 1858, and closed at the end of twelve months, when the cavern had practically been completely emptied; an official report was printed in the *Philosophical Transactions* for 1873, and all the specimens have been handed over to the British Museum.

The paper on the subject mentioned at the beginning of this address was read in September, 1858, during the meeting of the Association at Leeds, when I had the pleasure of stating that eight flint tools had already been found in various parts of the cavern, all of them inosculating with bones of mammalia, at depths varying from nine to forty-two inches in the cave-earth, on which lay a sheet of stalagmite from three to eight inches thick, and having *within* it and *on* it relics of lion, hyæna, bear, mammoth, rhinoceros, and reindeer.

It soon became obvious that the geological apathy previously spoken of had been rather apparent than real. In fact, geologists were found to have been not so much disinclined to entertain the question of human antiquity as to doubt the trustworthiness of the evidence which had previously been offered to them on the subject. It was felt, moreover, that the Brixham evidence made it worth while, and indeed a duty, to re-examine that from Kent's Cavern, as well as that said to have been met with in river deposits in the valley of the Somme and elsewhere.

The first fruits, I believe, of this awakening was a paper by Mr. Prestwich, read to the Royal Society, May 26, 1859, on the occurrence of flint implements, associated with the remains of animals of extinct species in beds of a late geological period, in France at Amiens and Abbeville, and in England at Hoxne (*Phil. Trans.* for 1860, pp. 277–317). This paper contains explicit evidence that Brixham Cavern had had no small share in disposing its author to undertake the investigation, which added to his own great reputation and rescued M. Boucher de Perthes from undeserved neglect. "It was not," says Mr. Prestwich, "until I had myself witnessed the conditions under which these flint implements had been found at Brixham, that I became fully impressed with the validity of the doubts thrown upon the previously prevailing opinions with respect to such remains in caves" (*op. cit.* p. 280).

Sir C. Lyell, too, in his address to the Geological Section of the British Association, at Aberdeen, in September, 1859, said, "The facts recently brought to light during the systematic investigation, as reported on by Dr. Falconer, of the Brixham Cave, must, I think, have prepared you to admit that scepticism in regard to the cave evidence in favour of the antiquity of man had previously been pushed to an extreme" (*Report Brit. Assoc.* 1859, *Trans. Sects.* p. 93).

It is probably unnecessary to quote further to show how very large a share the exploration at Brixham had in impressing the scientific world generally with the value and importance of the geological evidence of man's antiquity. That impression, begun as we have seen in 1858, has not only lasted to the present day, but has probably not yet culminated. It has produced numerous volumes, crowds of papers, countless articles in reviews and magazines, in various countries; and, perhaps, in order to show how very popular the subject became almost immediately, it is only necessary to state that Sir C. Lyell's great work on the "Antiquity of Man" was published in February, 1863; the second edition appeared in the following April, and the third followed in the succeeding November—three editions of a bulky scientific work in less than ten months! A fourth edition was published in May, 1873.

Few, it may be presumed, can now doubt that those who before 1858 believed that our fathers had under-estimated human antiquity, and fought for their belief, have at length obtained a victory. Nevertheless, every anthropologist has doubtless from time to time

"Heard the distant and random gun
That the foe was sullenly firing."

The "foe," to speak metaphorically, seems to consist of very irregular forces, occasionally unfair but never dangerous, sometimes very amusing, and frequently but badly armed or without *any* real armour. The Spartan law which fined a citizen heavily for going into battle unarmed was probably a very wise one.

For example, and dropping a metaphor, a pamphlet published in 1877 contains the following passage:—"With regard to all these supposed flint implements and spear- and arrow-heads found in various places, it may be well to mention here the frank confession of Dr. Carpenter. He has told us from the presidential chair of the Royal Academy that 'No logical proof can be adduced that the peculiar shapes of these flints were given them by human hands'" (see 'Is the Book Wrong? A Question for Sceptics,' by Hely H. A. Smith, p. 26). The words ascribed to Dr. Carpenter are put within inverted commas, and are the whole of the quotation from him. I was a good deal mystified on first reading them, for while it seemed likely that the president spoken of was the well known member of this Association —Dr. W. B. Carpenter—it was difficult to account for his being in the presidential chair of the Royal Academy, and not easy to understand what the Royal Academy had to do with flint implements. A little search, however, showed that the address which Dr. W. B. Carpenter delivered in 1872 from the presidential chair of, not the Royal Academy, but the British Association, contained the actual words quoted, followed immediately by others which the author of the pamphlet found it inconvenient to include in his quotation. Dr. Carpenter, speaking of "common sense," referred, by way of illustration, to the "flint implements" of the Abbeville and Amiens gravel beds, and remarked, "No logical proof can be adduced that the peculiar shapes of these flints were given to them by human hands; but does any unprejudiced person now doubt it?" (*Report Brit. Assoc.* 1872, p. lxxv.). Dr. Carpenter, after some further remarks on the "flint implements," concluded his paragraph respecting them with the following words:—"Thus what was in the first instance a matter of discussion, has now become one of those 'self-evident' propositions which claim the unhesitating assent of all whose opinion on the subject is entitled to the least weight."

It cannot be doubted that, taken in its entirety, that is to say, taken as every lover of truth and fairness should and would take it, Dr. Carpenter's paragraph would produce on the mind of the reader a very different effect to that likely, and no doubt intended, to be produced by the mutilated version of it given in the pamphlet.

A second edition of the pamphlet has been given to the world. Dr. Carpenter is still in the presidential chair of the Royal Academy, and the quotation from his address is as conveniently short as before.

It would be easy to bring together a large number of similar modes of "defending the cause of truth"—to use the words of the pamphlet just noticed—but space and time forbid.

I cannot, however, forego the pleasure of introducing the following recent and probably novel explanation of cavern phenomena. In 1882 my attention was directed to two articles, by one and the same writer, on "Bone-Cave Phenomena." The writer's theme was professedly the Victoria Cave, near Settle, Yorkshire, which he says was an old Roman lead mine, but his remarks are intended to apply to bone-caves in general. He takes a very early opportunity in the second article of stating that "We shall have to take care to distinguish between what is truly indicated in the 'science' view from what are purely imaginary exaggerations of its natural and historical phenomena"; and he no doubt believes that he has taken this care.

"We have now," he says, "to present our own view of the Victoria Cave and the phenomena connected with it, premising that a great many of the old mines in Europe were opened by Phœnician colonists and metal workers, a thousand years before the Romans had set foot in Britain, which accounts for the various floors of stalagmite found in most caves, and also for the variety of groups of bones embedded in them. The animals represented by them when living were not running wild about the hills devouring each other, as science men suppose, but the useful auxiliaries and trained drudges of the miners in their work. Some of them, as the bear, had simply been hunted and used for food, and others of a fierce character, as the hyæna, to frighten and keep in awe the native Britons. The larger species of mammalia, as the elephant, the rhinoceros and hippopotamus, and beasts foreign to the country, the Romans, no less than the Phœnicians, had every facility in bringing with them in their ships of commerce from Carthage, or other of the African ports. These, with the native horse, ox, and stag, which are always found in larger numbers in the caves than the remains of foreign animals, all worked peacefully together in the various operations of the mines. . . . The hippopotamus, although amphibious, is a grand beast for heavy work, such as mining, quarrying, or road-making, and his keeper would take care that he was comfortably lodged in a tank of water during the night. . . . The phenomena of the Victoria Cave Lead Mine differ in no material respect from those of hundreds of others, whether of lead, copper, silver, or iron, worked in Roman and pre-Roman times in all parts of Europe. Its tunnels have all been regularly quarried and mined, *not by ancient seas*, but by the hands of historic man. Double openings have been made in every case for convenient ingress and egress, during the process of excavation. Its roadways had been levelled, and holes made up with breccia, gravel, sand, and bones of beasts that had succumbed to toil, on which sledges, trolleys, and waggons could glide or run. . . . Near the entrance inside Victoria Cave were found the usual beds of charcoal and the hearths for refining the metal, while close by on the hillside may still be seen the old kilns in which the men 'roasted' the metallic ores and burned lime."

Should any one be disposed to ascribe these articles to some master of the art of joking, it need only be replied that they appeared in a religious journal (*The Champion of the Faith against Current Infidelity* for April 20, and May 11, 1882, vol. i. pp. 5 and 26), with the writer's name appended; and that I have reason to believe they were written seriously and in earnest.

It has been already intimated that Brixham Cavern has secured a somewhat prominent place in literature; and it can scarcely be needful to add that some of the printed statements respecting it are not quite correct. The following instances of inaccuracy may be taken as samples:—

The late Prof. Ansted, describing Brixham Cavern in 1861, said, "In the middle of the cavern, under stalagmite itself, and actually entangled with an antler of a reindeer and the bones of the great cavern bear, were found rude sculptured flints, such as are known to have been used by savages in most parts of the world" ("Geological Gossip," p. 209).

To be "entangled" with one another, the antler, the bones of the cave bear, and the flints must have been all lying together As a matter of fact, however, the antler was *on* the upper surface of the sheet of stalagmite, while all the relics of the cave bear and all the flints were in detrital beds below that sheet. Again, the flints nearest the bear's bones in question were two in number; they were twelve feet south of the bones, and fifteen inches less deep in the bed. There was no approach to entanglement.

Should it be suggested that it is scarcely necessary to correct errors on scientific questions in works, like "Geological Gossip," professedly popular and intended for the million, I should venture to express the opinion that the strictest accuracy is specially required in such books, as the great majority of their readers are entirely at the mercy of the compilers. Those who read scientific books of a higher class are much more capable of taking care of themselves.

Prof. Ansted's slip found its way into a scientific journal, where it was made the basis of a speculation (see *Geologist*, 1861, p. 246).

The most recent noteworthy inaccuracies connected with this famous cavern are, so far as I am aware, two in the English edition of Prof. N. Joly's "Man before Metals" (1883).

According to the first, "An entire left hind leg of *Ursus spelæus* was found lying above the incrustation of stalagmite which covered the bones of other extinct species and the carved flints" (p. 52).

It is only necessary in reply to this to repeat what has been already stated: all the bones of cave-bear found in the cavern were in beds *below* the stalagmite.

The following quotation from the same work contains the second inaccuracy, or, more correctly, group of inaccuracies, mentioned above: "We may mention among others the cave at Brixham, where, associated with fragments of rude pottery and bones of extinct species, heaps of oyster shells and other salt-water mollusks occur, as well as fish-bones of the genus *scarus*" (p. 104).

I am afraid there is no way of dealing with this paragraph except that of meeting all its statements with unqualified denials. In short, Brixham Windmill Hill Cavern contained no pottery of any kind whatever, not a single oyster-shell, nor even a solitary bone of any species of fish. One common limpet shell was the only relic of a marine organism met with in the cavern.

As already intimated, the result of the researches at Brixham quickened a desire to re-examine the Kent's Cavern evidence, and this received a considerable stimulus from the publication of Sir C. Lyell's "Antiquity of Man" in 1863. Having in the meantime made a careful survey of the cavern, and ascertained that there was a very large area in which the deposits were certainly intact, to say nothing of unsuspected branches which in all probability would be discovered during a thorough and systematic exploration, I had arrived at the conclusion that, taking the cavern at its known dimensions merely, the cost of an investigation as complete as that at Brixham would not be less than 1000*l*.

Early in 1864 I suggested to Sir C. Lyell that an application should be made to the British Association, during the meeting to be held at Bath that year, for the appointment of a Committee, with a grant of money, to make an exploration of Kent's Cavern; and it was decided that I should take the necessary steps in the matter. The proposal being cordially received by the Committee of the Geological Section, and well supported in the Committee of Recommendations, a Committee—consisting of Sir C. Lyell, Mr. J. Evans, Mr. (now Sir) J. Lubbock, Prof. J. Phillips, Mr. E. Vivian, and myself (Hon. Secretary and Reporter)—was appointed, with 100*l*. placed at their disposal. Mr. G. Busk was added to the Committee in 1866, Mr. W. Boyd Dawkins in 1868, Mr. W. Ayshford Sanford in 1869, and Mr. J. E. Lee in 1873. The late Sir L. Palk (afterwards Lord Haldon), the proprietor, placed the cavern entirely under the control of the Committee during the continuance of the work; the investigation was begun on March 28, 1865, and continued without intermission to June 19, 1880, the Committee being annually reappointed with fresh grants of money, which in the aggregrate amounted to 1900*l*., besides 63*l*. received from various private sources.

The mode of exploration was essentially the same as that followed at Windmill Hill, Brixham, but as Kent's Cavern, instead of being a series of narrow galleries, contained a considerable number of capacious chambers, and as the aim of the explorers was to ascertain not merely what objects the deposits contained, but their exact position, their distribution, their condition, their collocation, and their relative abundance, the details had to be considerably more elaborate, while they remained so perfectly simple that the workmen had not the least difficulty in carrying them out under my daily superintendence. The process being fully described in the First Annual Report by the Committee (see *Report. Brit. Assoc.* 1865, pp. 19, 20), it is unnecessary to repeat it here.

Mr. Godwin-Austen, while agreeing with Mr. MacEnery that flint implements occurred under the stalagmite, contended that they were found throughout the entire thickness of the cave earth. MacEnery, on the other hand, was of opinion that in most cases their situation was intermediate between the bottom of the stalagmite and the upper surface of the cave earth; and, while admitting that occasionally, though rarely, they had been met with somewhat lower, he stated that the greatest depth to which he had been able to trace them was not more than a few inches below the surface of the cave earth (*Trans. Devon. Assoc.* iii. 326–327). The Committee soon found themselves in a position to confirm Mr. Godwin-Austen's statement, and to say with him that "no distinction founded on condition, distribution, or relative position can be observed whereby the human can be separated from the other reliquiæ" (*Trans. Geol. Soc.* 2nd ser. vi. 444).

Mr. MacEnery's "Plate F" contains seven figures of three remarkable canine teeth, and the following statement respecting them:—"Teeth of *Ursus cultridens*, found in the cave of Kent's Hole, near Torquay, Devon, by Rev. Mr. McEnery, January, 1826, in Diluvial Mud mix'd with Teeth and Gnaw'd Bones of Rhinoceros, Elephant, Horse, Ox, Elk, and Deer, with Teeth and Bones of Hyænas, Bears, Wolves, Foxes, &c."

It is worthy of note that no other plate in the entire series names the date on which the specimens were found, or the mammals with whose remains they were commingled. This arose probably from the fact, well known to MacEnery, that no such specimens had been found elsewhere in Britain; and possibly also to emphasise the statements in his text, should any doubt be thrown on his discovery.

It is, no doubt, unnecessary to say here that the teeth belonged to a large species of carnivore to which, in 1846, Prof. Owen gave the name of *Machairodus latidens*. MacEnery states that the total number of teeth he found were five upper canines and one incisor, and the six museums in which they are now lodged are well known.

A considerable amount of scepticism existed for many years in some minds as to whether the relics just mentioned were really found in Kent's Cavern, it being contended that from its zoological affinities *Machairodus latidens* must have belonged to an earlier fauna than that represented by the ordinary cave mammals; and various hypotheses were invented to explain away the difficulty, most of them, at least, being more ingenious than ingenuous. Be this as it may, it was naturally hoped that the re-exploration of the cavern would set the question at rest for ever; and it was not without a feeling of disappointment that I had to write seven successive annual reports without being able to announce the discovery of a single relic of *Machairodus*. Indeed, the greater part of the Eighth Report was written with no better prospect; when, while engaged in washing a "find" met with on July 29, 1872, I found that it consisted of a well-marked incisor of *Machairodus latidens*, with a left ramus of lower jaw of bear, in which was one molar tooth. They were lying together in the first or uppermost foot-level of cave earth, having over it a continuous sheet of granular stalagmite 2·5 feet thick. There was no longer any doubt of MacEnery's accuracy; no doubt that *Machairodus latidens* was a member of the cave earth fauna, whatever the zoological affinities might say to the contrary; nor was there any doubt that man and *Machairodus* were contemporaries in Devonshire.

I cannot pass from this case without directing attention to its bearing on negative evidence: had the exploration ceased on July 28, 1872—the day before the discovery—those who had always declined to believe that *Machairodus* had ever been found in the cavern would have been able to urge, as an additional and apparently conclusive argument, that the consecutive, systematic, and careful daily labour of seven years and four months had failed to show that their scepticism was unwarranted. Nay, more, had the incisor been overlooked—and, being but a small object, this might very easily have occurred—they might finally have said "15·25 years' labour"; for, so far as is known, no other relic of the species was met with during the entire investigation. In all probability had either of these by no means improbable hypotheses occurred, geologists and palæontologists generally would have joined the sceptics; MacEnery's reputation would have been held in very light esteem; and—to say the least—his researches regarded with suspicion.

When their exploration began, and for some time after, the Committee had no reason to believe or to suspect that the cavern contained anything older than the cave earth; but at the end of five months, facts, pointing apparently to earlier deposits, began to present themselves; and, at intervals more or less protracted, additional phenomena, requiring apparently the same interpretation, were observed and recorded; but it was not until the end of three full years that a vertical section was cut, showing, in undisturbed and clear succession, not only the cave earth with the granular stalagmite lying on it, but, under and supporting the cave earth, another, thicker, and continuous sheet of stalagmite—appropriately termed crystalline, and below this again an older detrital accumulation, known as the breccia, made up of materials utterly unlike those of the cave earth.

The breccia was just as rich as the cave earth in osseous remains; but the lists of species represented by the two deposits were very different. It will be sufficient to state here that, while remains of the hyæna prevailed numerically very far above those of any other mammal in the cave earth, and while his presence there was also attested by his teeth-marks on a vast number of bones, by lower jaws—including those of his own kith and kin—of which he had eaten off the lower borders as well as the condyles, by long bones broken obliquely just as hyænas of the present day break them, and by surprising quantities of his coprolites, there was not a single indication of any kind of his presence in the breccia, where the crowd of bones and teeth belonged almost entirely to bears.

No trace of the existence of man was found in the breccia until March, 1869, that is about twelve months after the discovery of the deposit itself, when a flint flake was met with in the third foot-level, and was believed to be not only a tool, but to bear evidence of having been used as such (see *Report Brit. Assoc.* 1869, pp. 201, 202). Two massive flint implements were discovered in the same deposit in May, 1872, and at various subsequent times other tools were found, until at the close of the exploration the breccia had yielded upwards of seventy implements of flint and chert.

While all the stone tools of both the cave earth and the breccia were Palæolithic and were found inosculating with remains of extinct mammals, a mere inspection shows that they belong to two distinct categories. Those found in the breccia—that is, the more ancient series—were formed by chipping a flint nodule or pebble into a tool, while those from the cave earth—the less ancient series—were fashioned by first detaching a suitable flake from the nodule or pebble, and then trimming the flake—not the nodule—into a tool.

It must be unnecessary to say that the making of nodule tools necessitated the production of flakes and chips, some of which were no doubt utilised. Such flakes, however, must be regarded as accidents, and not the final objects the workers had in view.

It is worthy of remark that in one part of the cavern, upwards of 130 feet in length, the excavation was carried to a depth of nine feet, instead of the usual four feet, below the bottom of the stalagmite; and that, while no bone of any kind occurred in the breccia below the seventh foot-level, three fine flint nodule tools were found in the eighth, and several flint chips in the ninth, or lowest foot-level.

It may be added that the same fact presented itself in the lowest or corresponding bed in Brixham Windmill Hill Cavern. In short, in each of the two famous Devonshire caverns, the archæological zone reached a lower level than the palæontological.

That the breccia is of higher antiquity than the cave earth is proved by the unquestionable evidence of clear undisturbed superposition; that they represent two distinct chapters and eras in the cavern history is shown by the decided dissimilarity of the materials composing them, the marked difference in the osseous remains they contained, and the strongly contrasted characters of the stone implements they yielded; and that they were separated by a wide interval of time may be safely inferred from the thickness of the bed of stalagmite between them.

It is probable, however, that the fact most significant of time and physical change is the presence of the hyæna in the cave earth or less ancient, but not in the breccia or more ancient, of the two deposits. I called attention to this fact in a paper read to this Department ten years ago (see *Report Brit. Assoc.* 1873, pp. 209–214), and at greater length elsewhere in 1875 (see *Trans. Plym. Inst.* v. 360–375). Bearing in mind the cave-haunting habits of the hyæna, the great preponderance of his remains in the cave earth, and their absence in the breccia, it seems impossible to avoid the conclusion that he was not an occupant of Britain during the earlier period.

The acceptance of this conclusion, however, necessitates the belief (1) that man was resident in Britain long before the hyæna was.

(2) That it was possible for the hyæna to reach Britain between the deposition of the breccia and the deposition of the cave earth. In other words, that Britain was a part of the Continent during this interval.

Sir C. Lyell, it will be remembered, recognised the following geographical changes within the British area between the Newer Pliocene and historical times (see "Antiquity of Man," edition 1873, pp. 331, 332).

Firstly, a pre-Glacial Continental period, towards the close of which the Forest of Cromer flourished, and the climate was somewhat milder than at present.

Secondly, a period of submergence, when the land north of the Thames and Bristol Channel, and that of Ireland, was reduced to an archipelago. This was a part of the Glacial age, and icebergs floated in our waters.

Thirdly, a second Continental period, when there were glaciers in the higher mountains of Scotland and Wales.

Fourthly, the breaking up of the land through submergence, and a gradual change of temperature, resulting in the present geographical and climatal conditions.

It is obvious that if, as I venture to think, the Kent's Cavern breccia was deposited during the first Continental period, the list of mammalian remains found in it should not clash with the list of such remains from the Forest of Cromer, which, as we have just seen, flourished at that time. I called attention to these lists in 1874, pointing out that according to Prof. Boyd Dawkins ("Cave-Hunting," p. 418) the forest bed had at that time yielded twenty-six species of mammals, sixteen of them being extinct, and ten recent; that both the breccia and the forest bed had yielded remains of the cave-bear, but that in neither of them had any relic or trace of hyæna been found. A monograph on the "Vertebrata of the Forest Bed Series" was published in 1882 by Mr. E. T. Newton, F.G.S., who, including many additional species found somewhat recently, but eliminating all those about which there was any uncertainty, said: "We still have forty-nine species left, of which thirty are still living, and nineteen are extinct" (p. 135). Though the number of the species has thus been almost doubled, and the presence of the cave-bear remains undoubted, it continues to be the fact that no trace of the hyæna has been found in the forest bed, and no suspicion exists as to his probable presence amongst the eliminated uncertain species.

It should be added that no relic or indication of hyæna was met with in the "Fourth Bed" of Brixham Windmill Hill Cavern, believed to be the equivalent of the Kent's Hole breccia.

I am not unmindful of the fact that my evidence is negative only, and that raising a structure on it may be building on a sandy foundation. Nevertheless, it appears to me, as it did ten years ago, strong enough to bear the following inferences:—

1. That the hyæna did not reach Britain until its last Continental period.

2. That the men who made the Palæolithic nodule-tools found in the oldest known deposit in Kent's Cavern arrived during the previous great submergence, or, what is more probable—indeed, what alone seems possible unless they were navigators—during the first Continental period. In short, I have little or no doubt that the earliest Devonians we have sighted were either of Glacial, or, more probably, of pre-Glacial age.

It cannot be necessary to add that while the discovery of remains of hyæna in the forest bed of Cromer, or any other contemporary deposit, would be utterly fatal to my argument, it would leave intact all other evidence in support of the doctrine of British Glacial or pre-Glacial man.

Some of my friends accepted the foregoing inferences in 1873, while others, whose judgment I value, declined them. Since that date no adverse fact or thought has presented itself to me; but through the researches and discoveries of others in comparatively distant parts of our island, and especially in East Anglia, the belief in British pre-Glacial man appears to have risen above the stage of ridicule, and to have a decided prospect of general scientific acceptance at no distant time.

I must, before closing, devote a few words to a class of workers who are "more plague than profit."

The exuberant enthusiasm of some would-be pioneers in the question of human antiquity results occasionally in supposed "discoveries" having an amusing side; and not unfrequently some of the pioneers, though utter strangers, are so good as to send me descriptions of their "finds," and of their views respecting them. The following case may be taken as a sample:—In 1881, a gentleman, of whom I had never heard, wrote, stating that he was one of those who felt deeply interested in the antiquity of man, and that he had read all the books he could command on the subject. He was aware that it had been said by one palæontologist to be "unreasonable to suppose that man had lived during the Eocene and Miocene periods," but he had an indistinct recollection that another eminent man had somewhere said that "man had probably existed in England during a tropical Carboniferous flora and fauna." He then went on to say, "I have got that which I cannot but look upon as a fossil human skull. I have endeavoured to examine it from every conceivable standpoint, and it seems to stand the test. The angles seem perfect, the contour the same but smaller in size than the average human head; but that, in my opinion, is only what should be expected if we assume that man lived during the Carboniferous period, in spite of what Herodotus says about the body of Orestes." Finally, he requested to be allowed to send me the specimen. On its arrival it proved, of course, to be merely a stone; and nothing but a strong "unscientific use of the imagination" could lead any one to believe that it had ever been a skull, human or infrahuman.

It may be added that a few years ago a gentleman brought me what he called, and believed to be, "three human skulls and as many elephants' teeth," found from time to time, during his researches in a limestone quarry. They proved to be nothing more than six oddly shaped lumps of Devonian limestone.

So far as Britain is concerned, cave-hunting is a science of Devonshire birth. The limestone caverns of Oreston, near Plymouth, were examined with some care in the interests of palæontology as early as 1816, and subsequently as they were successively discovered. The two most famous caverns of the same county—one on the northern, the other on the southern shore of Torbay—have been anthropological as well as palæontological studies; and, as we have seen, have had the lion's share in enlarging our estimate of human antiquity. The researches have, no doubt, absorbed a great amount of time and of labour, and demanded the exercise of much care and patience; but they have been replete with interest of a high order, which would be greatly enhanced if I could feel sure that your time has not been wasted nor your patience exhausted in listening to this address respecting them.

JOSEPH-ANTOINE-FERDINAND PLATEAU.

THE career of this indefatigable investigator, as we announced last week, has just closed. Born in the second year of the present century, he has occupied a notable position in the scientific world for more than fifty years. Before he reached middle age he met with the terrible misfortune of losing his eyesight while trying venturesome experiments on the physiological effects of light. His scientific career seems to have become only more active in consequence! When we think of the ease and success with which certain chess-players can, blind-fold, carry on some dozen or two simultaneous games, there seems little to surprise us in the mathematical career of Euler after he became blind. But the difficulties which stood in the way of the physicist, and which he successfully overcame, were of a far more formidable character. Had his chief investigations related to sound, the loss of eyesight might have but little interfered with them. But to carry out by the help of others' eyes a long series of investigations connected with Physiological Optics was a triumphal feat with which we know nothing to compare, except, perhaps, the composition of those marvellous master-works which Beethoven elaborated after he had become stone-deaf.

Plateau's really great contributions to physical science were, however, not optical, but molecular. They were collectively republished in 1873 in two volumes, with the title, *Statique expérimentale et théorique des Liquides soumis aux seules Forces moléculaires.* This work was appreciatively reviewed in our columns (vol. x., p. 119) by the then greatest authority on the subject, the late Prof. Clerk Maxwell, so that it is unnecessary for us to analyse it here. Few of the readers of the recently published biography of Maxwell can have forgotten the humorous but accurately expressive lines in which he alludes to this work:—

"And just as that living Plato, whom foreigners nickname Plateau,
Drops oil in his whiskey and water—for foreigners sweeten it so:—
Each drop keeps apart from the other, inclosed in a flexible skin,
Till touched by the gentle emotion evolved by the prick of a pin," &c.

When we look at the Royal Society's Catalogue, we find that up to 1873 Plateau is credited with fifty-three papers on subjects of the most varied character. One large section of these, of course, forms the matter of the volumes already mentioned. Another large section is devoted to the persistence of visual impressions, subjective impressions of colour, irradiation, and other questions of physiological optics. In connection with these, there are several controversies and reclamations, with and against authorities such as Chevreul and von Helmholtz. In these contests, it must be confessed that Plateau usually has the worse. In fact, he appears very much in the same light as did Brewster a little earlier. He furnished to others, who knew how to interpret and to use them, a great array of novel facts: but his strength lay mainly in the patience and ingenuity which led him to these facts; not in the power of interpreting, explaining, or generalising them.

Besides the two main subjects above mentioned, we find in Plateau's *répertoire* a number of curiosities taken from widely different branches of science. Thus we have a chemical analysis of the mineral waters of Spa; the geometrical problem of describing an equilateral triangle whose several corners shall be on three given circles in one plane; arithmetical recreations; photometry; the "ghosts" produced by various series of rotating spokes; and a centrifugal air-pump.

Plateau occupied with success, until practically disabled, the Chair of Physics in the University of Ghent; and, if he did not attain to the foremost rank among experimental physicists, he at least did much good and useful work under circumstances which would have effectually closed the career of many men who have been more successful than he. He was occupied in his later years in compiling a valuable catalogue of all the papers he could meet with which bore on his special optical inquiries. It is to be hoped that the as yet unpublished part of this collection has been left in a state approaching completion.

OFFICIAL REPORTS ON CHOLERA IN EGYPT

SURGEON-GENERAL HUNTER, who was commissioned by the Government to make inquiry as to the circumstances attending the cholera epidemic in Egypt, has sent two reports to the Foreign Office. Neither pretends to afford full information on the subject which has been under investigation, but the more recent one, which gives information up to August 19, supplies some indication as to the opinion Dr. Hunter has formed with regard to the etiology of the epidemic. In his first report Dr. Hunter gives the cholera deaths registered up to July 31 as 12,600, but he adds that, owing to defective registration, the total mortality will probably be found to have been nearly double that number. Since that date some 15,000 more deaths have been registered, and if the same faulty system of registration has been maintained, the total mortality up to the present date cannot have fallen far short of some 55,000. The inquiry undertaken by Dr. Hunter relates therefore to a matter of the greatest magnitude, the more so as Egypt has apparently been free from cholera ever since 1865. It is however precisely this question of immunity from cholera that will be raised by Dr. Hunter, and already we are able to gather what opinion will be expressed on this point.

Thus, the possibility of the importation of the disease into Egypt from India is discussed, and it is stated that even some of those who originally were firmly convinced of this method of origin have been forced to a different conclusion. The spontaneous origin of the contagium is also regarded as not being supported by facts; and Dr. Sierra, in a communication which is appended to Dr. Hunter's, distinctly asserts that such a generation of the infection in the Nile Delta cannot be regarded as proved merely because the choleraic germ is often produced at the mouth of the Ganges. Prominence is, however, given to the fact that Egypt has been visited by five epidemics since that of 1831, namely, in 1848, 1850, 1855, 1865, and 1883, and independent testimony is brought forward to show that during the early part of the present year, as also at occasional intervals since 1865, there have been cases of a disease known as "cholerine," which have been characterised by some of the symptoms of true cholera. And further, Dr. Hunter, in expressing an opinion as to these cases, says that he has arrived at the conclusion that many of them were "what in India we should call cholera."

A further step in the argument is embodied in a description of the filthy conditions under which the Egyptians live, and especially of the foul state of the Nile at Damietta and other places, both owing to the floating carcasses of animals who had died of bovine typhus and otherwise. Having regard to all these points, the report implies that a number of cases, which for the moment we may describe as sporadic cholera, have formed a somewhat continuous series of attacks ever since the 1865 outbreak, and that the potency of the infection for spread in an epidemic form was developed under the influence of the foul conditions which obtained immediately antecedent to the date of the last epidemic. This view is by no means a new one; it was specially dealt with in a series of papers which were brought before the Epidemiological Society in 1878, when the possibility of a "progressive development of the property of infectiveness" under favourable conditions was insisted on; and it is more than probable that, as regards some of the infectious diseases, it may turn out to be a true explanation of their origin.

It must, however, be borne in mind that in England, and indeed in all thickly peopled countries, cases which are clinically of a similar character constantly occur during the warmer months of the year; indeed, the term "English cholera" is of by no means infrequent occurrence in our mortality tables. And not only so, but Dr. Sierra, in arguing against the spontaneous development of the contagium under the conditions which were found at Damietta, says that the same "cosmo-telluric conditions" have appeared often enough at the mouth of the Nile, that the same accumulation of carcasses in the river has before now taken place, and yet that no cholera has broken out in Egypt. The evidence mainly needed with a view to support the theory which is foreshadowed in Dr. Hunter's reports, should go to point out what were the peculiar conditions which, during the past summer, led to the development of a special potency for mischief in a disease which is always more or less present. The subject is one of the greatest scientific interest, and we trust it will be fully dealt with in the final report.

NORDENSKJÖLD'S GREENLAND EXPEDITION

BARON NORDENSKJÖLD telegraphed as follows to the *Times* from Thurso on Friday night:—"An inland ice party started on July 4 from Auleitswik Fjord. When they were 140 kilometres east of the glacier border and 5000 feet above the sea level they were prevented by soft snow from proceeding with sledges. They sent the Laplanders further on snowshoes. These advanced 230 kilometres eastwards over a continual snow desert to a height of 7000 feet. The conditions for a snow-free interior consequently did not exist here; but this expedition, during which men have reached for the first time the interior of Greenland, has given important results as to the nature of the interior of an ice-covered continent. Over the whole inland there is ice. There occur masses of fine dust, partly of cosmical origin, with the ice. The rest of the expedition, under the command of Dr. Nathorst, visited the north-western coast between Waigattel (?) and Cape York. The Esquimaux told our Esquimaux interpreter (Hans Christian, formerly of Capt. Hall's expedition) that two members of the American Polar Expedition had died, and the rest had returned to Littleton Island (Sofia). On August 16 the expedition sailed from Egedesmunde for the south, with rich collections, zoological, botanical, and geological. Short stays were made at Iviktit, Julianshaab, and Frederiksdal. We tried to proceed eastwards thrice through the sounds north of Cape Farewell and once along the coast, but were hindered by ice. We then went outside the ice field to 66° latitude, remaining constantly in sight of land, having twice in vain tried to find an ice-free shore more to the south. The band of drift ice was forced south of Cape Dan. On September 4 we anchored in a fjord which had been newly visited by Esquimaux, and where we found some remains from the Norse period. It was the first time since the fifteenth century that a vessel had succeeded in anchoring on the east coast of Greenland south of the Polar Circle. We tried in vain to anchor in another fjord more to the north, and returned. The expedition arrived at Reikiavik (Iceland) on September 9. Our observations on the temperature of the sea prove that the cold current which packs the ice along the east coast of Greenland is very insignificant; that the glaciers of the east coast are few and of no great size; and that the fjords are free from ice. Probably the coast may be reached by suitable steamers in the autumn of most years."

It will thus be seen that for once Baron Nordenskjöld has failed to fulfil his predictions. But his expedition must be regarded as in all respects successful. He has succeeded in penetrating into the very heart of Greenland, and the idea of taking Lapps with him to skate their way over the rough ice-bound land was a happy one. Greenland thus appears to be what has always been conjectured, a land everywhere covered with a thick ice-sheet. We cannot gather from the telegram whether Nordenskjöld's theory as to the position of the old Norse settlements has been confirmed, but he has, at all events, succeeded in bringing back remains of the old colonies. The analysis of the cosmical dust which has been collected will be eagerly looked for, and the detailed account of the collections made in Northern Greenland.

NOTES

MR. J. Y. BUCHANAN has been invited to accompany the expedition which sailed last week from the Thames to survey the route and lay the cables connecting Cadiz and the Canary Islands, and these islands with Senegal, on the west coast of Africa. During the laying of the cable from Lisbon to Madeira, over a route that had been carefully sounded, into what was believed to be close on 2000 fathoms, it suddenly parted. Soundings taken immediately revealed the existence of a bank with no more than 110 fathoms of water on it, which had been missed while surveying the route. Again, quite recently—indeed, during the last two weeks—the French exploring vessel *Talisman*, which has been investigating this part of the ocean with a numerous scientific staff, under the direction of the veteran Milne-Edwards, discovered another bank to the southward of the "Seine Bank," with as little as 70 fathoms on it. This bank was found to be about thirty miles long from east to west, and six miles broad from north to south. Apart from the special investigation of these banks, the survey of the line o route, which is carried out by two ships working in concert along a zigzag course, sounding every seven miles, must necessarily furnish much important information. Between Madeira and the Canary Islands lies the small group of the Salvage Islands, which may be said to be almost unknown. It is intended to carry the soundings round them, so as to determine whether they are connected with any of the new banks or with known land. It is also intended to land on the islands, from which interesting collections may be expected. In addition to the instruments ordinarily carried in the ships, Mr. Buchanan takes out a new sounding tube, constructed for use with the ordinary wire sounding apparatus. With it it will be possible at every station to secure a good sample of the mud and of the water from the bottom without altering the routine work of the ship. As the route crosses the mouth of the Mediterranean it will thus be possible to determine the extent to which the dense warm water which leaves that sea as a bottom current affects the density and temperature of the deep water of the North Atlantic in its neighbourhood. The ships to be used are the *Dacia* and the *International*, both belonging to the Telegraph Construction and Maritime Company.

IN a communication to the *Sonntags-Beiloge zur Norddeutschen Allgemeinen Zeitung* for September 16 Dr. Reichenow, the well-known ornithologist of the Berlin Museum, describes a new ostrich under the name of *Struthio Molybdophanes*. A living example is in the Zoological Gardens at Berlin, and others are expected at Cologne and Paris. The habitat of this species is stated to be the deserts of Somali Land and the Western Galla country, extending on the east coast of Africa from 10° N. lat. to the Equator.

THE United States steamer *Yantic* has, we regret to learn, failed to reach and rescue Lieut. Greeley and his observing party, who have been stationed at Lady Franklin Bay, in Kennedy Channel, about 81° N., since the summer of 1881. This at first sight looks as if there were little hope of the safety of the party, as they had only two years' provisions with them. But Sir George Nares, who knows the region intimately, writes to the *Times* to show that there is no reason for despair. He gives in detail his reasons for believing that Lieut. Greeley, when the relief ship failed to reach him in 1882, would, like a prudent leader, prepare for the worst and husband his provisions to the utmost. Moreover, he would probably be able to add considerably to his supply by hunting, and on the route southwards there are depots at various accessible points. So, even if another year should have to be spent in the north, there is good reason to hope for the ultimate safety of the party.

ACCORDING to the *Izvestia* of the Russian Geographical Society, the young Tashkend Observatory carries on very useful scientific work. Col. Pomerantseff and his assistants are not only engaged in the verification of those instruments which are used every year for determinations of latitudes and longitudes in Turkestan, and in the computation of the results; they also pursue independent work, such as the observation of the small planets Juno and Pallas; the observation of the last solar eclipse at Penjakent; of the stars eclipsed by the moon, which are given in the *Nautical Almanac*; magnetical and meteorological observations. These last were made in 1882, at twenty stations, out of which eight are first-rank stations, and that of Tashkend makes observations every hour.

THE prize of 500 francs presented by Prof. A. P. de Candolle for the best monograph on a genus or family of plants is announced as open for competition for 1884. Papers in Latin, French, German, English, or Italian should be sent to Prof. Alph. de Candolle, Geneva, before October 1, 1884.

THE *China Mail*, in referring to the Hong Kong Observatory, says that Dr. Doberck will first be instructed to draw up a report for His Excellency the Governor, on the minor stations now in existence. He will examine past records, and, if these are found fairly accurate, will endeavour to furnish certain data as to the climatic conditions prevalent throughout the colony during the different months of the year. When this is done, it will probably be found feasible to make these stations cooperate with the central station at Kowloon, especially in observations connected with typhoons. Another important item will be the determination of the magnetic elements, and the investigation of the magnetic attraction of the various mountains and hills in the colony and its neighbourhood. It is also possible that, under instructions from the Governor, Dr. Doberck will proceed to Manilla, Shanghai, and other places on the coast of China, to inspect the observatories there, and put himself into communication with the directors of those institutions, with the view of having their reports sent regularly to the Hong Kong Observatory to receive careful discussion here with the object of eventually furnishing trustworthy weather forecasts.

MESSRS. ALLEN, COUES, AND BREWSTER, according to *Science*, sign a call for a convention of American ornithologists, to be held in New York City, beginning on September 26, 1883, for the purpose of founding an American Ornithologists' Union, upon a basis similar to that of the "British Ornithologists' Union." The object of the Union will be the promotion of social and scientific intercourse between American ornithologists and their co-operation in whatever may tend to the advancement of ornithology in North America. A special object, which it is expected will at once engage the attention of the Union, will be the revision of the current lists of North American birds, to the end of adopting a uniform system of classification and nomenclature, based on the views of a majority of the Union, and carrying the authority of the Union. It is proposed to hold meetings at least annually, at such times and places as may be hereafter determined, for the reading of papers, and the discussion of such matters as may be brought before the Union. Those who attend the first meeting will be considered *ipso facto* founders. Active and corresponding members may be elected in due course after organisation of the Union, under such rules as may be established for increase of membership. Details of organisation will be considered at the first meeting.

AN enthusiastic meeting of 3000 working men was held in Nottingham recently, at which resolutions were passed maintaining the great importance of sound technical instruction for the manufactures of the country. In connection therewith we may say that it is expected that the technical schools which are attached to the University College, Nottingham, will be opened some time in October next. It is intended in these schools to provide a complete course of instruction in mechanical and electrical engineering, and in the sciences most intimately connected with these professions; also to give instruction to artisan classes in mechanics, and in the details and history of the machinery employed in the lace and hosiery manufactures. The students attending the school will be divided into day and evening classes. It is expected that the day students will consist of young men who intend taking up engineering as a profession, or, being the sons of manufacturers, and looking forward to the management of a manufacturing business, consider it desirable to gain some knowledge of the construction of machinery. For these students the College provides chemical and physical laboratories, and lecture theatres, and class rooms for drawing, mathematics, theoretical mechanics, &c. The workshops now added will comprise tools and mechanism in all departments of work. The shops will be supplied with steam power, and lighted by the electric light. In the evenings classes will be held for artisans. On these occasions opportunities will be given to engineers' apprentices and others to prepare themselves for the annual competition for Whitworth Scholarships. The Mechanical Museum will form a very important feature in the means of instruction provided for lace-makers and hosiers. In this museum will be exhibited models of all the mechanical movements which are generally recognised, with short printed or written descriptions pointing out the special features of each, and their function in lace and hosiery machinery when so employed. Specimens of lace and hosiery machines which can be set in motion, will also be shown, their moving parts being labelled in such a way as to point out their relation to the models above mentioned. The workshops are under the general direction of Prof. Garnett.

DURING the ensuing winter session of the Liverpool Science and Art Classes there will be conducted by Miss Helen Fryer a class for the study of Hygiene. The lectures will follow the course of the syllabus lately published by the Government Science and Art Department. Miss Fryer will also give a course of lectures on Animal Physiology.

THE Directors of the Crystal Palace have completed arrangements for holding an International Exhibition of Arts, Manufactures, Science, and Industry during 1884. It is intended that the Exhibition should open on April 3 and close at the end of October. All the arrangements will be under the control of Mr. G. C. Levey.

BY the kindness of the Trustees of the Gilchrist Fund, the Committee of the Victoria Coffee Hall have been able to arrange for the delivery of six Penny Science Lectures by eminent lecturers on Tuesdays, beginning on October 2. The Committee are anxious that lectures such as these, which are rarely within the reach of the London working men, should be made widely known beyond the circle of the usual frequenters of the Hall, and the Hon. Secretary would be glad to hear from any one willing to help by getting a poster hung up, or distributing handbills among working men in districts within reach of the Victoria. The following are the lecturers and subjects:—October 2 and 9: Lecture by Mr. Wm. Lant Carpenter, F.C.S., on "Ice, Water, and Steam." October 16: Mr. P. H. Carpenter, on "Life under the Ocean Wave." October 23: Mr. E. B. Knobel, Sec.R.A.S., on "Comets." October 30: Mr. C. A. V. Conybeare, on "The Rights and Feelings of an Animal." November 6: Dr. B. W. Richardson, M.D., LL.D., F.R.S., on "Food and Feeding."

WE are glad to see that science has a place in the first number of *The English Illustrated Magazine* (Macmillan and Co.), which contains Prof. Huxley's Royal Institution lecture on the oyster. Mr. Grant Allen contributes an interesting article with some beautiful illustrations on "The Dormouse at Home."

TWO strong shocks of earthquake were felt on Sunday at Casamicciola. A house situated in the upper town was wrecked and fell in ruins. No lives were lost.

CAPT. EDWARD ASHDOWN, Commander of the P. and O. steamer *Siam*, writes as follows to the *Times*:—"It may be interesting to some of your scientific readers to know that the steamship *Siam*, on her voyage from King George's Sound to Colombo, on August 1, when in lat. 6° S., long. 89° E., passed, for upwards of four hours, through large quantities of lava, which extended as far as could be seen (the ship was going 11 knots at the time). The lava was floating in a succession of lanes of from five to ten yards wide, and trending in a direction north-west to south-east. The nearest land was the coast of Sumatra (distant 700 miles), but as there was a current of fifteen to thirty miles a day, setting to the eastward, the lava could not have come from there, and I can only imagine it must have been an upheaval from somewhere near the spot. I may mention the soundings on the chart show over 2000 fathoms. There was a submarine volcano near the spot in 1789."

EXPERIMENTS on the liquefaction of oxygen and nitrogen are described by Wroblewski and Olszewski (*Compt. Rend.* xcvi. 1140 and 1225). At $-136°$ oxygen liquefies under a pressure of $22\frac{1}{2}$ atmospheres; nitrogen at the same temperature does not liquefy, even under a pressure of 150 atmospheres, but if the pressure is somewhat slowly diminished, care being taken that it does not become less than 50 atmospheres, the nitrogen becomes liquid. Carbon disulphide solidifies at about $-116°$, and alcohol at $-130°.5$.

FROM the annual report on the mineral statistics of Victoria, we see that the quantity of gold raised in 1882 was 864,609 oz., as against 833,378 oz. in 1881. The deepest shaft in the colony is the Magdala, at Slawell, 2409 feet deep.

THE additions to the Zoological Society's Gardens during the past week include a White-fronted Capuchin (*Cebus albifrons* ♂) from Brazil, presented by Capt. Harrison; a Puma (*Felis concolor*) from South America, presented by Mr. B. M. Whithard; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. Murray Dickinson; a Ruddy Ichneumon (*Herpestes smithi*) from South Africa, presented by Col. J. H. Bowker, F.Z.S.; a Fallow Deer (*Cervus dama* ♀), European, presented by Sir Henry Bessemer; a Persian Gazelle (*Gazella subgutterosa* ♂), two Persian Sheep (*Ovis aries*, var. ♂ ♂) from Persia, presented by Lady Brassey; a Grey Seal (*Halichærus gryphus*) from Wales, presented by Mr. J. J. Dodgshon; two Rufous Tinamous (*Rhynchotus rufescens*) from Uruguay, presented by Mr. J. Brown; a Spanish Terrapin (*Clemmys leprosa*), South European, presented by Mr. Aitchison; a Yellow-billed Sheathbill (*Chionis alba*) from Antarctic America, two Yarrell's Curassows (*Crax carunculata* ♂ ♀) from South-east Brazil, purchased; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, two Small Hill Mynahs (*Gracula religiosa*) from Southern India, deposited.

THE IRON AND STEEL INSTITUTE

THE Iron and Steel Institute has this year resolved to revisit the place of its birth—in other words, the young and flourishing town of Middlesbrough-on-Tees, where the association was founded some fifteen years ago. The arrangements for its reception and for visits to different works in the neighbourhood (though marred in practice by a grievous disaster) left nothing to be desired; but the papers, though sufficient in number and value for practical metallurgists, offer very little that is of interest to the student of science generally. Hence our notice will be brief. It is somewhat to be regretted (especially seeing that the Eston Works formed the first day's excursion) that no paper was devoted to the development of the Thomas-Gilchrist or "basic" process of steel-making. This process has been widely and successfully adopted in Germany, but has made little progress as yet in the Cleveland district, for which it may be said to have been specially designed, and where it was first put in practice. On this disappointment, however, it is useless to dwell. Passing over three adjourned discussions—on tin-plate making, coal-washing machinery, and the manufacture of anthracite pig iron respectively—we come to the new papers prepared for the meeting. There were two dealing with the important manufacture of coke: one by Mr. R. Dixon, on the Simon-Carve's process, and one by Mr. Jameson, on the process which bears his name. We hail these as a further assurance that the barbarous, costly, and offensive beehive oven, which still continues to disgrace our English coking districts, is far on the high road to extinction. In Belgium it has altogether ceased to exist, being superseded by more rational methods; and the same will soon be the case for the rest of the Continent. The two papers before us do not, however, contribute very much to our knowledge. Mr. Dixon's deals simply with the cost of erecting ovens on the particular system described, which cost is unfortunately high, and on the yield and quantity of coke produced, which are both satisfactory. Some difficulty is experienced with the bituminous coal of Durham in keeping the valve-boxes and mains free from pitch; but this, it is hoped, will shortly be overcome. He also describes a method just introduced of heating the air required for combustion by the waste gases passing away from the ovens, by which the time needed for coking is expected to be largely reduced. Mr. Jameson's system, as our readers will remember, consists in burning the coal from the top in a closed oven, and withdrawing the gases, as they form, from the bottom, by means of an exhausting apparatus. These waste gases are condensed, and give valuable results in ammonia, tar, &c. The amount of this yield has been largely increased, since former papers were read on the subject, by new extracting and condensing appliances, and the percentage of coke made appears also to have improved. One great advantage of the system is that any beehive oven can be adapted to it at a co t of some 10*l*. or 15*l*. The oils extracted, the value of which had been questioned, find a ready sale at 2*l*. to 3*l*. per ton.

A paper on raw coal in the blast furnace, prepared by Mr. I. Lowthian Bell, F.R.S., was postponed, in consequence of its author's serious illness—an illness from which we are glad to hear that he is recovering. We pass on to a paper by Mr. E. A. Cowper, Past President of the Institute of Mechanical Engineers, on the results obtained with the hot-blast stove which bears his name. This, as is well known, is an application to the blast furnace of the fire-brick "regenerator" invented by Sir Wm. Siemens for gas furnaces. In the earlier days of the hot-blast process, the best known means of heating the air was to pass it through a sort of coil of cast-iron pipes, inclosed within a tall furnace. The limit of endurance with such pipes is, however, reached at about 1000° F.; whereas by employing two inclosed stacks of fire-bricks, one of which is always being heated from below, while the other is being cooled from above by air passing through it to the furnace, temperatures of 1500° are attainable. The advantages of so far increasing the temperature were hotly contested, from a theoretical point of view; but "the proof of the pudding is in the eating," and Mr. Cowper has proved beyond doubt that a blast of 1500°, combined with a very large and slowly-working furnace, will realise an economy (in fuel consumed per ton of iron made) which, in these days of competition, means just the difference between a fair profit and a heavy loss. The chief element of success in these stoves appears to be the making of the bricks as thin as possible, so that there may be but little depth for the heat to soak into or soak out of; and the author describes a form of brick, making what he calls "honeycomb filling," with which there is nowhere a greater thickness than two inches, and this is always heated from both sides.

Two papers on hydraulic cranes for steel works, by Mr. R. M. Daelen and Mr. T. Wrightson, and another by Mr. J. E. Stead, on a new form of gas sampler, do not require any special comment. Finally we have a paper on blast furnace economy in relation to design, by Mr. R. Howson, which is of a somewhat more suggestive character. The almost universal form of the interior of a blast furnace is as follows:—From the throat, where the materials are charged and the gases collected, it widens slowly to a point about two-thirds of the way down, called the boshes. From thence it narrows again, but more rapidly, and ends in a shallow circular pit called the hearth. Mr. Howson asks whether this form has not, from beginning to end, been a "rule-of-thumb business" with English engineers; and whether the rapid narrowing below the boshes does not in fact favour the lodgment of half-melted cinder, and the consequent building up of "scaffolds," which are known to be the most serious of all impediments to the successful working of a blast furnace. It is supposed that the hearth needs "relief from pressure;" but as a matter of fact the difficulty is to get the materials down quickly enough, and the easier their descent is made the better. He proposes a barrel-shaped form, having a regular curve at the boshes, instead of a sharp angle—a form actually adopted by the late Mr. Menelaus at Treforest, and with great success as to economy of fuel. With the same object he advocates the charging of the coke towards the sides of the furnace, and the stone towards the middle, and the preserving of this distribution throughout, so as to have as much combustible material as possible above and near to the tuyeres.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

UNIVERSITY COLLEGE, LONDON.—The Department of Applied Science and Technology in this college opens on October 2, along with the rest of the college. The instruction in this department includes (1) lectures on different branches of civil and mechanical engineering and surveying and levelling, drawing and practical experimental work in the engineering laboratory; (2) lectures and practical laboratory work in electricity and allied branches of physics; (3) lectures in architecture and architectural construction; and (4) lectures and practical laboratory work in different branches of chemical technology, including brewing, heating and lighting, metallurgy, chemistry of the alkali trade, and agricultural chemistry. Besides these technical and professional lectures, the Faculty of Science provides very complete courses of lectures in mathematics, physics, chemistry, and geology, the sciences upon which the professional knowledge must be based.

ON Tuesday last Mr. F. J. M. Page, B.Sc., F.C.S., was elected Demonstrator of Practical Chemistry at the London Hospital Medical College.

SCIENTIFIC SERIALS

THE *Journal of Anatomy and Physiology*, vol. xvii. part 4, July, 1883, contains: On the action of saline cathartics, by Dr. Matthew Hay.—On the anatomy and physiology of the urinary bladder and of the sphincters of the rectum, by F. Le Gros Clark, F.R.S.—On ten cases of congenital contraction of the stomach, with remarks, by W. Roger Williams (plate 17).—A new rule of epiphyses of long bones, and on the ossification of the temporal bone, by J. B. Sutton (plate 18).—On three cases of cerebellar disease, by Dr. Thomas Oliver.—A contribution to the anatomy of the Indian elephant, by Dr. R. J. Anderson.—On a case of semi-agnatha or synotia in a lamb, by Frederic Eve.—On a case of primary epithelioma of the lung with secondary deposits in the kidney, vertebræ, and ribs, by W. E. Hoyle, M.A. (plate 19).—Researches into the histology of the central gray substance of the spinal cord and medulla oblongata, by Dr. W. A. Hollis (plate 20).—On the membrana tympani, by Dr. J. M. Crombie.—An account of an obturator hernia, and of a fibrous body attached to the hydatid of Morgagni, by W. S. Richmond.

THE *Quarterly Journal of Microscopical Science* for July contains:—On the ancestral form of the Chordata, by Prof. W. Hubrecht (plate 23).—On the renal organs of Patella, by J. T. Cunningham.—On a rare form of the blastoderm of the chick, and its bearing on the question of the formation of the vertebrate embryo, by Dr. C. O. Whitman (plates 24 and 25).—On the development of the pelvic girdle and skeleton of the hind limb in the chick, by Alice Johnson (plates 26 and 27).—On the development of the mole (*Talpa europea*), by Walter Heape (plates 28 to 31).—On the tongue of *Ornithorhynchus paradoxus*: the origin of taste bulbs and the parts upon which they occur, by Edward B. Poulton, M.A. (plate 32).—Observations upon the fœtal membranes of the opossum and other marsupials, by Dr. H. F. Osborn (plate 33).

THE *Journal of the Royal Microscopical Society* for August contains:—On the red mould of barley, by C. G. Matthews (plates 5 and 6).—On the spicules of *Cucumarea hyndmanni*, *C. calcigera*, and two allied forms, by Prof. F. Jeffrey Bell, M.A. (plate 8).—On a method of preserving the freshwater medusa, by Peter Squire (four grains of bichloride of mercury to a pint of distilled water).—The usual summary of current researches and *Proceedings* of the Society.

THE *American Journal of Science*, September.—On the existence in both hemispheres of a dry zone and its cause, by Arnold Guyot. The presence is determined of two nearly rainless belts on both sides of the tropics, extending round the globe, and embracing most of the so-called deserts of both hemispheres. It is argued that the atmospheric currents, which are the great regulators of aqueous precipitation, are the primary cause of these subtropical dry zones.—On the relations of temperature to glaciation, by George F. Becker. Assuming the correctness of the generally received opinion that the sun is a gradually cooling body, it is concluded that the absolute maximum in the development of glaciers is past, and that the Glacial period was not one of general cold, but one of higher mean temperature at sea-level than the present.—Analysis of two varieties of lithiophilite (manganese triphilite) from Tubbs Farms, Maine, and Branchville, Connecticut, by S. L. Penfield.—On the intensity of sound. I. The energy and coefficient of damping of a tuning-fork, by Charles K. Wead.—The decay of rocks geologically considered, by Dr. T. Sterry Hunt. In this comprehensive memoir the author insists (*a*) on the evidence afforded by recent geological studies of the universality and antiquity of subaërial decay both of silicated crystalline rocks and of limestones, and of its great extent in pre-Cambrian times; (*b*) on the preservation of the disintegrated materials *in situ*, wherever they have been protected from denudation by overlying strata, or by their position in places sheltered from erosion, as in the Appalachian and St. Lawrence valleys; (*c*) on the insignificant results of this process of decay since the Glacial period owing to the relatively short duration of that period, and probably also to changed atmospheric conditions in recent times; (*d*) on the fact that the process has furnished the materials both for the clays, sands, and iron-oxides from the beginning of the Palæozoic to the present time, and for the corresponding Eozoic rocks formed from the older feldspath rocks by the partial loss of protoxide bases. The decay of sulphuretted ores in the Eozoic rocks has also given rise to oxidised iron ores and to deposits of rich copper ores in various geological regions; (*e*) that the rounded masses of crystalline rocks left in the process of decay constitute not only the boulders of the drift, but, judging from analogy, the similar masses in conglomerates of various ages from Eozoic times.—On Mr. Glazebrook's paper on the aberration of concave gratings, by H. A. Rowland.—On the stibnite from Japan, by Edward S. Dana. The author fully describes and illustrates the remarkable series of specimens of crystallised stibnite from Mount Kosang in the Island of Shikoku, South Japan, which have recently come into the possession of the Yale Museum.—Notes on the volcanoes of Northern California, Oregon, and Washington Territory, by Arnold Hague and Joseph P. Iddings.—Cassiterite, spodumene, and beryl in the Black Hills, Dakota, by William P. Blake.—Discovery of a new planetoid on the night of August 12, by C. H. F. Peters.

THE *American Naturalist* for June, 1883, contains:—Pearls and pearl fisheries, part i., by W. H. Dall.—Aboriginal quarries: soapstone bowls and the tools used in their manufacture, by J. D. McGuire.—Annelid messmates with a coral, by J. W. Fewkes.—Progress of invertebrate palæontology in the United States for the year 1882, by Dr. C. A. White.—Notes on the genus Campeloma of Rafinesque, by R. E. Call.—Mosses, by W. W. Bailey.—Emotional expression, by A. T. Bruce.—The developmental significance of human physiognomy, by C. D. Cope.

July, 1883, contains:—The Naturalist Brazilian expedition, No. 2: the lower Jacuhy and Sao Jeronymo, by H. S. Smith.—Growth and development, by C. Morris.—Pearls and pearl fisheries, part 2, by W. H. Dall.—Catlinite: its antiquity as a material for tobacco pipes, by E. A. Barber.

August, 1883, contains:—Means of plant dispersion, by E. I. Hill.—On the classification of the Linnean orders of Orthoptera and Neuroptera, by A. S. Packard, jun.—On the power of scent in the turkey vulture, by S. N. Rhoads.—The Siphonophores (illustrated), by T. Walter Fewkes.

Annalen der Physik und Chemie, July 15.—Theory of dispersion, by L. Lorenz.—On the elliptical polarisation by reflection from the surfaces of transparent bodies, by A. C. van Ryn van Alkemade.—The coefficient of refraction of some mixtures of alcohol and aniline, by W. Johst (with tables).—Remarks on E. Lommel's treatise "Concerning Newton's Rings," by Karl Exner.—On a method of comparing electrical resistances independent of the resistance of the leads, by F. Kohlrausch.—Some determinations of the absolute resistance of a chain by means of an earth inductor and a galvanometer.—Concerning the effect of polarisation with alternating currents, by A. Winkelman.—Quantitative determination of the influence of the changes of temperature produced by extension upon the measurement of the former, by Dr. A. Miller of München.—On the admissability of the acceptance of an electric sun potential and the effect of its interpretation on terrestrial phenomena, by Werner Siemens.—Researches in gaseous constitution of heavenly bodies, by A. Ritter of Aachen.—On the reduction of the fundamental units of mechanics to their elements, by E. Budde.—On a new fluid of high specific weight, of high refractive index and great dispersion, by Carl Rohrbach (with tables).—On the correct writing of some expressions of Arabic origin used in the art of measuring, by K. Zöppritz.

Bulletin of the Belgian Royal Academy of Sciences, July 27.—On the influence exercised by the respiratory process on the circulation of the blood, by Messrs. Em. Legros and Griffé. From experiments made on the dog, cat, horse, pig, sheep, rabbit, and other animals, Magendie's dictum that pressure is diminished during inspiration and increased during expiration appears to be normally true in the case of the pig alone.—On the existence and cause of a monthly periodicity of the aurora borealis, by M. Terby. The paper is accompanied by a table of magnetic disturbances at Brussels during the years 1870-82 arranged in monthly decades. The existence of a monthly periodicity is demonstrated, and from a series of remarkable coincidences it is suggested that in this periodicity is reflected the duration of the rotation of the sun round its axis. It is further argued that the magnetic perturbations accompanying the aurora borealis, which are closely associated with the appearance of solar spots, are probably subject to the same vicissitudes as the auroras, and to the same periodicity.—Two memoirs on steam-engines, locomotives, breaks, and railway rolling stock, by M. Delacy.—Remarks on the force of the word *discovery* as applied to the Iguanodons of Bernissart, by M. P. J. van Beneden. The discovery of the large specimen recently exposed to public view in the court of the Brussels Natural History Museum, a full account of which appeared in NATURE, September 6 (p. 439), is referred to M. Fagès. But M. van Beneden shows that he was the first to determine the connection of these gigantic fossils with the Iguanodon family.—On some remains of fossil Cetacea collected in the phosphorated rocks between the Elbe and Weser, by M. P. J. van Beneden.—The following theorem is communicated by M. Catalan: a, x, y being integers, every value of x satisfying the equation $(a^2 + 1)\ x^2 = y^2 + 1$, is the sum of three positive squares, with the exception of $x_1 = 1$ and $x_2 = 4a^2 + 1$.—On some autographs of Grétry, the famous composer of Liége, by M. Ed. Fétis.—On some desiderata in the history of art in Belgium, by M. Ed. Mailly.

Archives of Physical and Natural Sciences, Geneva, August 15.—On some remarkable movements occasionally accompanying the fall of hailstones, by M. Daniel Colladon.—Memoir on earthquakes and volcanoes, by Prof. F. Cordenons. In this first part of a comprehensive study of underground phenomena the author gives a general classification of seismic disturbances, and examines the various hypotheses hitherto proposed to account for them.—On the nomenclature of fossils in connection with the recent discussions on botanic nomenclature, by M. Alph. de Candolle.—On the American ants (concluded), by M. H. de Saussure.—On the movements of the ground recorded at the Neuchatel Observatory, by Dr. Hirsch.—Meteorological observations with tables of temperature and barometric pressure made at the Observatory of Geneva and on the Great Saint Bernard during the month of July.

Rendiconti of the Reale Istituto Lombardo di Scienze e Lettere, July 26, 1883.—Experimental studies on the parasite of tuberculosis (Robert Koch's *bacillus*), by Prof. G. Sormani and Dr. E. Brugnatelli. The conclusions of Charnley Smith (*Brit. Med. Jour.*, January, 1883) regarding the detection of the bacilli of tubercle in the breath of consumptive patients are not confirmed. Hence consumption would not appear to be infectious.—Cure of pneumonitis effected by the cold water method of treatment, by Prof. C. Golgi.—On the quaternary vegetable fossils recently discovered by G. B. Dell' Angelo in the Re district, Val Vegezzo, by Prof. F. Sordelli.—Remarks on the various methods of distributing the current to a system of electric lamps, by Prof. R. Ferrini.—On the Institution of International Law and its operations during the years 1879-83, by C. C. Norsa.—Meteorological tables for the month of July prepared at the Royal Brera Observatory, Milan.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 21.—"Contributions to our Knowledge of the Connection between Chemical Constitution, Physiological Action, and Antagonism." By T. Lauder Brunton, M.D., F.R.S., and J. Theodore Cash, M.D.

In this paper the authors show that the physiological action of salts of ammonia varies considerably according to the acid with which the ammonia is combined. They all affect the spinal cord, motor nerves, and muscles, and tend finally to paralyse these structures. The course of poisoning varies: the chloride has at first a stimulant action on the cord while with the iodide this is less marked, and the paralysing action is more distinct. The iodide, sulphate, and phosphate paralyse motor nerves more powerfully than other salts, the iodide being the most powerful of all.

Nineteen salts of the compound ammonias were investigated. They affect the spinal cord, motor nerves, and muscles.

There is a marked difference in action between ammonia and the compound ammonias; while ammonia causes well marked tetanus, compound ammonias as a rule produce symptoms of motor paralysis, with the exception of those in which only one atom of hydrogen is substituted by an alcohol radical. This paralysis appears to be partly due to their action on the spinal cord and nerve centres, and partly to a curara-like action on the motor nerves.

Some of them apparently increase somewhat the excitability of the spinal cord at first, but this is temporary, and is shown rather by hyperæsthesia or tremor than by convulsion; and tetra-methyl and ethyl-ammonium salts differ from the di- or tri-methyl or ethyl-ammonias in having a much greater tendency to cause convulsions.

The effect of the acid radical on the physiological action is less marked in the case of the compound ammonias than in the salts of ammonia itself. The iodides of the compound ammonias paralyse motor nerves more quickly than either chlorides or sulphates.

Salts of methyl, ethyl, amyl ammonium are more active than the corresponding ones of the di- and tri-compounds, but the tetra-compounds are most active of all.

In the next part of the paper the effect of the salts of alkalies on muscle and nerve are considered. The substances investigated were the chlorides of lithium, sodium, potassium, rubidium, and cæsium. These differ from ammonia in having very little tendency to stimulate the spinal cord, and the chief symptom of poisoning by them is increasing torpor. Slight excitement of reflex action is noted at first in the case of potassium and rubidium.

The motor nerves are not paralysed by cæsium or rubidium, except in very large doses, but the other substances of this group paralyse them to a greater or less extent. Lithium and potassium are the most powerful.

The contractile power of muscle (as shown by the height of curve) is increased by rubidium, ammonium, potassium, and cæsium. It is unaffected by sodium excepting in large doses, and is almost invariably diminished by lithium.

The action of substances belonging to the alkaline earths and earths is discussed in the next section. The substances investigated were the chlorides of calcium, strontium, barium, beryllium, didymium, erbium, and lanthanum. In regard to their action upon the nervous system, these substances fall into two groups: (*a*) containing beryllium, calcium, strontium, and barium; and (*b*) containing yttrium, didymium, erbium, and lanthanum. Group *a* has a tendency to increase reflex action, as evidenced by spasm or tremor. Group *b*, reflex action in the cord appears to be little affected, but they appear to have a tendency to paralyse motor centres of the brain in the frog. Group *a* all paralyse motor nerves to some extent. Lanthanum has also a slight paralysing action, but the other members of group *b* have not, agreeing in this respect with sodium and rubidium, and differing from all the others. The *contracture* produced by barium is enormous, resembling that produced by veratria, as the authors have shown in a former paper. It is like that of veratria diminished by heat, cold and potash, and may be abolished by these agents. It is not so well marked when the drug is injected into the circulation, as when locally applied to the muscle.

The action of some of the more important of those drugs can be graphically represented by a spiral, the terminal members of which are potassium and barium, and these two are to a certain extent connected by ammonium as an intermediate link.

The alterations effected in the action of the different members of these groups on muscle by the subsequent application of another is next discussed, and it is shown that the effect of one substance upon muscle may be increased or diminished by the application of another. One of the most curious points is that two substances having a similar action may, instead of increasing, neutralise each other's effect.

Barium, calcium, strontium, yttrium, and beryllium cause a great prolongation of the muscular curve or *contracture*. Some relations are pointed out between the atomic weights of antagonising elements of which the data are too limited to draw from them any general rule, but the authors think that they may possibly lead by and by to some useful result. Thus rubidium in large doses has the same effect as barium in causing a veratria-like curve, but barium destroys the effect of rubidium before producing its own effect.

Rb 85·4 × 8 = 683·2
Ba 137 × 5 = 685.

In the next division the authors show that by alternate application of acids and alkalies the muscle of the frog may be made to describe, on a slowly revolving cylinder, curves which almost exactly resemble those described on a quick cylinder by the normal contraction of a muscle on stimulation; and also those which the muscle describes on irritation after it has been poisoned by barium. They consider that the contraction of muscle may be possibly due in some measure at least to alterations in acid or neutral salts which the muscle contains.

Entomological Society, September 5.—Mr. J. W. Dunning, F.L.S., president, in the chair.—Baron Osten-Sacken of Heidelberg was elected a member of the Society.—Sir S. S. Saunders exhibited *Idarnella caricæ*, Hasselq., which had been lost sight of for more than a century; and other interesting fig-insects.—Mr. F. Enock exhibited an hermaphrodite specimen of *Macropis labiata*, Panz.—Mr. J. Coverdale exhibited specimens of *Grapholitha cæcana*, Schläger, a *Tortrix* new to Britain.—The Rev. H. S. Gorham read a revision of the genera and species of Malacoderm *Coleoptera* of the Japanese fauna, part i., *Lycidæ* and *Lampyridæ*.

SYDNEY

Linnean Society of New South Wales, July 25.—Prof. W. J. Stephens, M.A., in the chair.—The following papers were read:—On the myology of the Frilled Lizard (*Chlamydosaurus Kingii*), by Charles De Vis, B.A. The author does not find there is any special muscular mechanism connected with the reptile's habit of elevating the frill and of occasionally assuming the erect attitude. The function of the frill he regards as being partly to frighten assailants, partly to aid in the collection and concentration of the waves of sound.—Descriptions of Australian Microlepidoptera, No. 9, by E. Meyrick, B.A.—Some remarks on the action of tannin on Infusoria, by Harry Gilliatt.

PARIS

Academy of Sciences, September 10.—M. Blanchard, president, in the chair.—On certain predictions relative to seismic disturbances, by M. Faye. The author exposes the groundless character of the theory recently advanced by M. Delauney and others, regarding the connection of earthquakes with the planetary movements, and more particularly with the supposed transit of Jupiter through the August meteors.—Separation of gallium (continued). Separation from titanic acid, by M. Lecoq de Boisbaudran.—A new method of filtration for highly diluted precipitates, by M. Lecoq de Boisbaudran.—Memoir on induction, by M. P. Le Cordier. In this paper the author adopts the theory of a continuous and incompressible medium, by the translations and pressures of which are produced electric currents and electrostatic phenomena. Electromotor and electrostatic effects of induction are calculated approximately for a hollow sphere forming an insulated conductor, homogeneous, isotropic, and non-magnetic, turning with a constant angular velocity round a fixed axis in a uniform and permanent magnetic field.—Experiments made at Grenoble, by M. Marcel Deprez, on the transmission of force by electricity. Note communicated by M. Boulanger on behalf of the Committee appointed by the city of Grenoble to follow these experiments.—Cholera from the standpoint of chemistry, by M. Ramon de Luna. From his chemical and physiological studies in Madrid and the Philippines the author concludes that cholera is propagated exclusively through the respiratory organs, and that the only safe treatment is the inhaling of hypoazotic vapour mixed with air. The best prophylactic is also found in hypoazotic fumigations of rooms, utensils, &c., twice a day. During the terrible outbreak at Manilla, in 1882, this treatment was adopted with complete success in the case of three hundred artisans employed in the mint.—Observations of the new comet discovered by Mr. Brooks on September 2, and of the planet 234 made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—Proposition on a question of mechanics touching the figure of the earth, by M. E. Brassinne.—Laws of induction due to the variation of intensity in currents of diverse forms; circular current, by M. Quet.—On the absorption of the ultra-violet rays by albuminoid substances, by M. J. L. Soret. From his experiments, in which he was assisted by MM. Danilewsky and Denis Monnier, the author concludes that all albuminoid substances hitherto studied contain a common principle, to which is due their characteristic absorptive band. Gelatine, which in so many other respects differs from albumen, acts quite differently. It is much more transparent, and gives rise to no bands.—On the proportion of food consumed by dogs under various temperatures, by M. Guimaraes. In the normal state the average daily consumption varied from one-tenth to one-sixteenth of the weight of the body; in a temperature of 10° to 12° C. from one-ninth to one-twelfth.—On the division of the cellular nucleus in plants, by M. L. Guignard.—On the structure of the leaf of the fossil genus *Sphenophyllum*, ranging from the Lower Carboniferous to the Upper Permian systems, by M. B. Renault.—General conclusions on the causes of chemical change in wheaten flour, and on the best conditions for preserving it for long periods in a sound state, by M. Balland.

September 17.—M. Blanchard, president, in the chair.—Allusion was made by the president to the loss sustained by the Academy in the person of M. Puiseux, member of the Geometrical Section, who died at Frontenay on September 9.—On the destructive fires caused by lightning, with some suggested improvements in lightning conductors (one illustration), by M. D. Colladon.—On the possibility of increasing the irrigating waters derived from the Rhone by regulating the discharge from the Lake of Geneva, by M. Ar. Dumont. The author dwells on the great benefits likely to be conferred on the southern departments of France by the project recommended by the Geneva Commission. This project, which might be carried out at an expenditure of about 180,000*l*., involves the creation of a hydraulic force of 7000 horse-power, by which the level of the lake at high water might be reduced by at least 0·60 m., and the minimum discharge of the Rhone at the outlet increased by 80 mc. per second.—Elements and ephemerides of the Pons-Brooks comet of 1812, by MM. Schulhof and Bossert.—Search for the red star observed during the total eclipse of the sun on May 6, 1883, by M. E. L. Trouvelot. The subsequent disappearance of this object might perhaps justify the supposition that it was an intra-Mercurial planet. But pending more accurate observations the author suspends his judgment on this point.—On the double star Σ 2400 of the Dorpat Catalogue, by M. Perrotin.—Electric law of the conservation of energy under all forms at entrance and issue of any material system traversed by the electric current, by M. G. Cabanellas.—On a new capillary electrometer, by M. A. Chervet.—Note on Hall's electric phenomenon, by M. Aug. Righi.—Qualitative research of manganese in the zinc of commerce, in zinc ashes and zinc spar, and search for bismuth in the lead of commerce by means of electrolysis, by M. A. Guyard.—New observations on the microbes of fishes, by MM. L. Olivier and Ch. Richet.—On the olfactory apparatus in the antennæ of Vanessa Io, by M. J. Chatin.—On the venomous properties of the jequirity, by MM. Cornil and Berlioz.—On the microbes found in the liver and kidneys of victims to yellow fever (three illustrations), by M. Babes.

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