THURSDAY, SEPTEMBER 23, 1880

THE PHOTOPHONE

EARLY in the summer of the current year it was announced in the columns of NATURE that Prof. Graham Bell had made a discovery which, for scientific interest, would rival the telephone and phonograph, and that he had deposited in a sealed packet, in the custody of the Smithsonian Institute, the first results of his new researches; that announcement has now received its due fulfilment in the lecture by Prof. Bell to the American Association for the Advancement of Science, on Selenium and the Photophone, which will be found on another page. In spite of those who ingenuously attempted at the time of our announcement to forestall Prof. Bell and to discredit the idea that he had done anything new, the discovery, which he has now published, is a startling novelty. The problem which he has attacked is that of the transmission of speech, not by wires, electricity, or any mechanical medium, but by the agency of light. The instrument which embodies the solution of this principle he has named the Photophone. It bears the same relation to the telephone as the heliograph bears to the telegraph. You speak to a transmitting instrument, which flashes the vibrations along a beam of light to a distant station, where a receiving instrument reconverts the light into audible speech. As in the case of that exquisite instrument, the telephone, so in the case of the photophone, the means to accomplish this end are of the most ridiculous simplicity. The transmitter consists of a plane silvered mirror of thin glass or mica. Against the back of this flexible mirror the speaker's voice is directed; a powerful beam of light is caught by a lens from the sun and directed upon the mirror, so as to be reflected straight to the distant station. This beam of light is thrown by the speaker's voice into corresponding vibrations. At the distant station the beam is received by another mirror, and concentrated upon a simple disk of hard rubber fixed as a diaphragm across the end of a hearing-tube. The intermittent rays throw the disk into vibration in a way not yet explained, yet with sufficient power to produce an audible result, thus reproducing the very tones of the speaker. Other receivers may be used, in which the variation in electrical resistance of selenium under varying illumination is the essential principle. The experimental details have been worked out by Prof. Bell in conjunction with Mr. Sumner Tainter. They have discovered that other substances beside hard rubber, gold, selenium, silver, iron, paper, and notably antimony, are similarly sensitive to light. This singular production of mechanical vibrations by rays of light is even more mysterious than the production of vibrations in iron and steel by changes of magnetisation. It was indeed this latter fact which led the discoverers to suspect the analogous phenomenon of photophonic sensibility in selenium and in other substances. Hitherto, in consequence of the mere optical difficulties of managing the beam of light, the distance to which sounds have been actually transmitted by the Photophone is less than a quarter of a mile, but there is no reason to doubt that the method can be applied to much greater distances, and that sounds can be transmitted from one station to another wherever a beam of light can be flashed; hence we may expect the slow spelling out of words in the flashing signals of the heliograph to be superseded by the more expeditious whispers of the Photophone.

We congratulate Prof. Bell most sincerely on this addition to his well-won laurels, and venture to predict for his photophone a great, if not a widely-extended, future of usefulness. SILVANUS P. THOMPSON

THE GEOLOGY OF LONDON

Guide to the Geology of London and the Neighbourhood. (An Explanation of the Geological Survey Map of "London and its Environs," and of the Geological Model of London, in the Museum of Practical Geology.) By William Whitaker, B.A., F.G.S. Third Edition. (London: Longmans and Co. and Edward Stanford, 1880.)

SINCE the appearance of the second edition of this most useful little work, some valuable additions have been made to our knowledge of the rocks which underlie the London Basin. The deep borings at Turnford, near Cheshunt, and at Ware, which were executed by the New River Company in 1879, have furnished new data to geologists for determining the position and characters of that great underground ridge of palæozoic rocks, the probable existence of which was so long ago indicated by Mr. Godwin-Austen. This underground ridge of palæozoic rocks has now been reached in no less than six borings, those of Kentish Town, Harwich, Crossness, Meux's Brewery, Turnford, and Ware. In four of these cases the age of the rocks which have been found unconformably underlying the Cretaceous strata is placed beyond question, by the discovery of the well-preserved and characteristic fossils, lists of which have recently been published by Mr. Etheridge. At Harwich the cores of dark-coloured indurated shale yielded *Posidoniæ*, which proved that the rock belongs to the lowest part of the Carboniferous system; at Meux's Brewery and at Turnford the purple shales yielded the characteristic fossils of the Upper Devonian; while at Ware cores were brought up crowded with the well-known fossils of the Wenlock shale.

But in the case of the Kentish-Town and Crossness borings no fossils have been detected in the cores of rock brought up from the infra-cretaceous rocks, and in these cases geologists have had to fall back upon the far less satisfactory evidence afforded by their mineral characters. Under these circumstances there has arisen considerable diversity of opinion as to the age of red, mottled, and variegated strata which have been reached in these two borings. While Prof. Prestwich is inclined to refer the rocks in both cases to the Old Red Sandstone, the officers of the Geological Survey are in favour of regarding them as belonging to some part of the New Red Sandstone or Poikilitic. Mr. Whitaker has so well stated the objections to the view that the rocks at Kentish Town and Crossness are of Old-Red-Sandstone age, that we cannot do better than quote what he says upon the subject:—

"I may here remark that there is a strong reason against the classification of the bottom beds at Kentish Town and Crossness with the Old Red Sandstone, which seems to have escaped notice. Having the series unmistakably present in the Devonian type at Cheshunt and at Meux's, it would be strange indeed were it to occur in its wholly distinct Old Red Type at Kentish Town, between those two places, and at Crossness, not very many miles from the latter of them! I believe that no such thing is known to occur anywhere; the two types of what is generally taken to be one great geological system being limited to separate districts, and not occurring together."

There is one other point upon which Mr. Whitaker's views as expressed in the work before us are well worthy of the attentive consideration of geologists. On p. 20 he remarks:—

"It has been said that although, from the cores brought up from some of the deep borings, we can estimate the angle of dip of the rocks, yet for calculations as to the probable extent of any of these rocks, and as to where other rocks may be expected either to come on above or to rise up from beneath them, it is needful to be able also to approximate to the *direction* of dip. I am led to think, however, that this is really of less importance than at first sight seems to be the case; for judging by what we generally see of the older rocks in districts where they are at the surface, they are much subject to disturbances and are thrown into great rolls or folds, so that whilst at one spot dipping north, near by they may turn over and dip south. We find, too, in the coal-fields of Belgium and of the north of France frequent evidence of sharp folding, and indeed of inversion of these older rocks underneath the even and almost undisturbed Cretaceous and Tertiary beds. I think therefore that were we able to find the direction of the dip of the rocks in any of our deep wells it would be unsafe to argue from that alone as to the probable succession of the beds in any direction for a considerable distance."

Mr. Whitaker's suggestions as to the direction in which it might possibly be of use to search for the Coal measures in the south-east of England are offered with a cautious reservation which is highly to be commended; it is evident, however, that he agrees with Mr. Godwin-Austen and Prof. Prestwich in the opinion that the southern suburbs of London and the line of the North Downs afford the most promising fields for future research. It is greatly to be regretted that no attempt has been made to obtain the necessary means, either from public funds or by private subscription, for carrying out this most interesting experiment.

It is not only in connection with his account of the great underground ridge of palæozoic rocks that Mr. Whitaker has been able to make valuable additions to this work. His account of the chalk and the boulder clay have been supplemented by some remarks giving the reader an idea of the latest views which have been published concerning the mode of origin of those formations. To students of Tertiary geology Mr. Whitaker's remarks on the pebble-gravel round London will prove interesting, while archæologists will find a valuable record of the discovery of a canoe (now placed in the British Museum) in the peat cut through in the works of the Victoria-Docks Extension.

This admirable guide to the geology of London is published at the low price of one shilling, and the demand which has already produced the exhaustion of two editions ought surely to convince the authorities of the Geological Survey that it would be wise of them so to curtail the expenses of publication in the memoirs which they issue, as to permit of their being sold at equally reasonable rates.

PROF. A. GRAY'S BOTANICAL TEXT-BOOK

The Botanical Text-Book (Sixth Edition). Part I.—*Structural Botany, or Organography on the Basis of Morphology, to which is added the Principles of Taxonomy and Phytography, and a Glossary of Botanical Terms.* By Asa Gray, LL.D., &c., Fisher Professor of Natural History (Botany) in Harvard University. (New York and Chicago: Ivison, Blakeman, Tayler, and Co., 1879. London: Macmillan and Co.)

THE near prospect of a visit from one who has for upwards of a quarter of a century held the undisputed position of the first botanist in the New World, both as a teacher and a writer, reminds us that his last and in some respects the most important of his many valuable publications, the "Botanical Text-Book," has not yet been noticed in this journal.

Prof. Asa Gray is the author of various elementary works upon the science he has so long cultivated and taught, some adapted for the old and some for the young, unequal in every respect except that of merit and admirable adaptation to their several purposes. Such are his "First Lessons in Botany," which contain just so much as is necessary to obtain such a knowledge of the botany of flowering plants as will enable a student to ascertain their names and affinities by the use of the ordinary descriptive manuals; his "How Plants Grow: a Simple Introduction to Structural Botany," which is the most lucidly written work of the kind known to us; his "Lessons in Botany and Vegetable Physiology;" his "Manual of the Botany of the Northern United States;" and, above all, his "How Plants Behave: an Introduction to a Study of the Habits and Movements of Plants, adapted for the Young," which is a marvel of good arrangement and clear expression, whilst the subject is rendered in the highest degree attractive to both old and young.

These and other elementary works on botany are, though sold by thousands in the United States, comparatively little known on this side of the Atlantic. The same cannot be said of a more comprehensive work by the same author, written for more advanced students, and as a book of reference for the working botanist, namely that which heads this article. The "Botanical Text-Book" appeared so long ago (as a first edition) as 1842, and immediately took rank as a first-class educational work in all English-speaking countries, and from that time to the present, during which the five editions which have appeared, being always brought up to date in point of matter, have been successively recognised by professors in British universities as the best introduction to botany extant in the language, and recommended by many to their pupils as the one they should use.

In all the five editions all the branches of botany have been treated of under the heads of Structure, Physiology, and System; but the twenty-three years that have elapsed since the appearance of the last edition have so greatly enlarged our knowledge of the physiology of vegetables, and of the vast assemblage of plants known as cryptogamic, that these subjects can no longer be treated of *pari passu* with the structural, if this latter is to be brought up to the present state of knowledge in a work of the scope and design of the author's "Text-Book." This has determined Dr. Gray to enlarge the scope of his work, to retain the authorship of one volume, which is devoted to Morphology, Taxonomy, and Phytography, re-writing these throughout, to assign another upon Vegetable Histology and Physiology to his colleague, Prof. Goodall; a third, which will be an Introduction to Cryptogamous Plants, to another colleague, Prof. Farlow; and to complete the series by a fourth, from his own pen, on the Morphology and Classification of Flowering Plants, their Distribution, Products, &c.

Thus, when complete, we shall have from the most eminent botanical professors in the New World as comprehensive an introduction to the study of the Vegetable Kingdom as the nineteenth century is likely to produce.

OUR BOOK SHELF

Light and Heat; the Manifestations to our Senses of the Two Opposite Forces of Attraction and Repulsion in Nature. By Capt. W. Sedgwick, R.E. (London: Hodgson and Son, 1880.)

THE reviewer who says what he thinks is sometimes thought unkind. The author's paradoxes require no commentary but themselves to be duly appraised by scientific readers.

"The explanation of the fact that a spot of light is seen alike when pressure is applied to the outside of the eye, and when a single ray of light passes into the eye, is that the ray of light really makes itself manifest to our sense of vision by exerting a pull upon the retina of the eye . . . it follows, of course, that light is a pulling or an attractive force, and is therefore opposed to heat, which, as is well known, is a pushing or repulsive force." (Pp. 14 and 15.)

"Light consists of a large amount of the attractive force, mixed with a small amount of the repulsive force. Heat, on the other hand, consists of a large amount of the repulsive, with a small amount of the attractive force." (P. 28.)

"We have in the growth of plants and trees a beautiful exemplification of the action of heat and light as expansive and attractive forces. The young shoots are extended by the expansive power of heat, and then the attractive power of light comes into play" . . . (P. 38.)

"It may be objected that gravity cannot be the same force as light, because, if it were, it would be greater by day than by night." (P. 42.)

"There is ample evidence all about us to testify to the fact that light is an attractive force. Indeed *we ourselves bear witness to the fact by our fondness for fireworks and illuminations*" . . . (P. 38.)

"Light being the manifestation, in the free state, to our senses of the attractive or cohesive force . . . the fact that the production of light is made the first act in the creation of the world, in the account given us in the Book of Genesis, becomes intelligible." (P. 42.)

"I ask for no other favour, and for no mercy." (P. 3.)

We believe we have sufficiently complied with the gallant captain's request. S. P. T.

The Land and Freshwater Shells of the British Isles. By Richard Rimmer, F.L.S. (London: David Bogue, 1880.)

THIS unpretending and well-written volume is dedicated to the artisans, with many of whom, especially in the North of England, the subject is very popular. The dedication is qualified, viz.: "To those of my countrymen among the working classes who wisely employ their leisure hours in the pursuit of useful and elevating knowledge, with the hope that others among their ranks may be induced to forsake the paths of profitless and degrading dissipation." William Edward, the Banff shoemaker, is (thanks to Mr. Smiles) a celebrated example of the more intelligent workman; and we know of others who, however, "carent vate sacro." The book is very readable: it gives an excellent account of the habits of our land and freshwater mollusks, as well as of their various habitats, and it is not burdened with any synonymy or useless aliases. It is founded on Dr. Gwyn Jeffreys' "British Conchology." But the present work has a drawback. Eight out of the eleven plates give photographs of the shells, which are produced by the "Albertype" process; and the figures, especially of the smaller species, are so blurred or smudgy as to be almost undistinguishable. Plate X. is very good, representing magnified views of the British species of *Vertigo*. There is a useful glossary.

LETTERS TO THE EDITOR

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

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

Novel Celestial Object

THE search for planetary nebulæ, described in NATURE, vol. xxii. p. 327, was continued for several evenings without revealing any new object, although it is estimated that the spectra of about 100,000 stars were examined. On the evening of August 28 an object entered the field which presented a faint continuous spectrum with a bright band near each end. Clouds interfered and barely permitted an identification with Oeltzen 17681, or a position in 1880 of R.A. 18h. 1m. 17s., Dec. − 21° 16′.

This object might be mistaken for a temporary star like that in Corona in 1863, and the bands assumed to correspond to the hydrogen lines C and F. This view is contraverted by the permanency of the object which was observed by Argelander in 1849, and at the Washington Observatory in 1848 and 1849. In all these cases its magnitude was estimated as 8, or very nearly its present brightness. No variation of light was detected between August 28 and September 1. The star Oeltzen 17648 precedes it very nearly a minute, and is only four minutes north, so that their light can be easily compared. As they are nearly equal, a slight change would be quickly recognised.

A further examination of the spectrum showed that the less refrangible band is near D, and the other between F and G. The images of both, but particularly of the second, are much elongated by the prism, showing that they are bands, and not lines. The limits of wave-length of the first band are approximately 5,800 and 5,850; those of the second, 4,670 and 4,730. A band at 5,400 and some other fainter bands were also suspected, but their existence is not certain.

The discovery of this object greatly increases the difficulty of distinguishing between a star and a planetary nebula. If the disk is used as a test, the first object described in the paper referred to above might easily be taken for a star. If we define a nebula by its spectrum of bright lines on a faint continuous spectrum, the object now under consideration becomes a nebula. Whether it is a mass of incandescent gas resembling a nebula in character, but not in constitution, or whether it is a star with a vast atmosphere of incandescent gas of a material not as yet known to us, are questions which cannot yet be decided.

Cambridge, U.S., September 2 EDWARD C. PICKERING

Experiments on the States of Matter

THE exploration of the new region which I have lately opened up has led to so many results with both scientific and technical bearings that I have been unable to leave this city for some time to attend any scientific meetings, and I would beg leave, with your kind permission, to make, through the medium of your valuable columns, a few remarks on some recent scientific work.

The quite independent confirmation of my discovery of the limit of the liquid state given in a letter in NATURE, vol. xxii. p. 435, by my old colleague, Dr. Carnelley, helps to dispel the idea of an intermediate state above the critical point, and confirms me in the use of the term "gas" for all fluids above their critical temperatures in speaking of the "solubility of solids in gases." The term vapour should only be applied to an aëriform fluid which by pressure alone can be reduced to the solid or liquid state, and above the critical temperature this cannot be done. As yet I have no evidence that vapours so defined are capable of dissolving solids, and this negative property may help to form a definition of that division of matter. As Dr. Carnelley does not mention the coincidence of our researches, perhaps you will permit me to quote from our respective papers. Dr. Carnelley says:—1. "In order to convert a gas into a liquid, the *temperature* must be *below* a certain point (termed by Andrews the *critical temperature* of the substance), otherwise no amount of pressure is capable of liquefying the gas."

As far back as May 24 I wrote ("On the State of Fluids at their Critical Temperatures," *Proc.* Roy. Soc. No. 205, 1880):—"The same results were obtained as before. When the temperature was below the critical point, the contents of the tube were liquid, and when over that temperature the reaction was always gaseous, notwithstanding the variations of pressure.

"I think we have in these experiments evidence that the liquid state ceases at the critical temperature, and that pressure will not materially alter the temperature at which the cohesion limit occurs." Dr. Carnelley will find the whole of my paper devoted to an experimental demonstration of what he has now deduced from his experiments. The paper was written with the title, "On the Cohesion Limit," but by the advice of Sir William Thomson, to whose great kindness in helping me with advice and information I am much indebted, I altered the title until I had the whole field explored. This I have since done, and have completely established the "cohesion limit" for all liquids—that for homogeneous liquids being an isotherm starting from the critical point. My paper being a very full one has taken much time and work, and the corrections for over a thousand experiments will take me some time yet. Prof. Stokes (whose kindly interest and encouragement have greatly lightened my labours) has been kept informed of my progress, and is cognisant of the work I have done in this direction. Dr. Carnelley's second conclusion is also very interesting, especially when applied to water; but surely we are not to understand that the solid ice was hot throughout, or that, if a thermometer had been imbedded in the ice, it would have risen. Although the vessel be red hot, the ice need never be allowed to melt, but made to pass directly into vapour, and yet its temperature remain 0° till it has been entirely volatilised.

I notice from your report of the British Association that Sir William Thomson calls attention to Cagniard de Latour's method of showing the critical state of a liquid by sealing the requisite proportion of liquid in a stout tube and heating it in a bath. It should not be forgotten that, although to Dr. Andrews undoubtedly belongs the credit of establishing the definite finish of the boiling-line and the apparent continuity of the liquid and gaseous states, to Baron Cagniard de Latour belongs the discovery of "l'état particulier" where the liquid state ends. Latour's method, although often used by Mr. Hogarth and myself, is not convenient for purposes of research. The method, your report goes on to say, was criticised by Prof. W. Ramsay, in what spirit we are not informed, but Dr. Ramsay added that he had found an apparatus in which a screw was employed to produce increase of pressure instead of using the expansion of the liquid itself. Dr. Ramsay, however, did not say whether the apparatus he had found was that invented by Mr. Hogarth and myself, and described by us in *Proc.* Roy. Soc., No. 201, 1880, in which india-rubber in a hollow cap is made by compression to yield a perfectly tight joint and to answer also for a screw when protected by a facing of leather. Dr. Ramsay visited my laboratory, and had the apparatus taken down and fitted up before his eyes, and with my permission had an apparatus made. The use of the compressed india-rubber for obtaining the requisite close fitting constitutes the important feature of my apparatus; the employment of iron for constructing the vessel enabling experimenters to dispense with the use of two liquids as in Andrews' apparatus—mercury being used alone.

I have made some little progress with the construction of vessels to withstand pressure at high temperatures, and I expect in a few weeks (when I have prospect of leisure) to carry my crystallisation experiments to a scientific if not commercial success.

J. B. HANNAY

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Fascination

I EXPECTED some of your readers to refute the explanation of Mr. Stebbing on "Fascination." I see in NATURE, vol. xxii. p. 383 another paragraph which is not more to the purpose. Want of presence of mind and stupefaction are not fascination. In 1859 (twenty-one years ago) I followed in the rocks of Avon, close by the park of Fontainebleau, the fairy paths of Denecourt, when the approach of a storm induced me to leave the blue arrows, indicating the right path, for a short cut. I soon lost my way, and found myself in a maze of brambles and rocks, when I was startled by seeing on my left hand, at a distance of about ten yards, a snake, whose body lifted up from the ground at a height of about a yard, was swinging to and fro. I remained motionless, hesitating whether to advance or to retreat, but soon perceived that the snake did not mind me, but kept on maintaining its swinging motion, and some plaintive shrieks attracted my attention to a greenfinch perched on a branch of a young pine overhanging the snake, with his feathers ruffled, following by a nod of his head on each side of the branch the motions of the snake. He tottered, spread his wings, alighted on a lower branch, and so on until the last branch was reached. I then flung my stick at the snake, but the point of a rock broke it and the snake disappeared with the rapidity of an arrow. On approaching the spot, a real abode of vipers, which I did with the greatest precaution, knowing by observation that death may be the result of the bite of a viper, I saw the greenfinch on the ground agitated by convulsive and spasmodic motion, opening and shutting his eyes. I put him in my bosom to try the effect of heat, and hastened to reach the park of Fontainebleau. The little claws of the bird opening and shutting, perhaps as an effect of heat, made me think that he might perhaps be able to stand on my finger, and he did clutch it, and held on with spasmodic squeezes. In the park I got some water, and made him drink it. In short, he revived and finally flew off in the lime-trees of the park.

Now whilst following the motions of the snake and bird I experienced a singular sensation. I felt giddy; a squeezing like an iron hoop pressed in my temples, and the ground seemed to me to be heaving up and down. In fact the sensation was quite analogous to that experienced on a beginning of sea-sickness.

From these facts would it not seem probable that fascination is nothing more nor less than an extreme fatigue of the optic nerve, produced by a rapid gyratory motion of a shining object and resulting in a nervous attack and a coma? Curiosity rivets at first the attention of the bird, unconscious of any danger, and when giddiness warns him of his peril it is too late. The snake is as well aware of this as the *Lophius piscatorius* is of the effect of his membrane.

In this system the fact of the bird coming down from a higher to a lower branch would be explained by the supposition that, giddiness overtaking him, he opened instinctively his wings and clung to the next support that he found, the motion having partially removed the giddiness so as to enable him to hold fast.

Observe, that nothing hindered the bird from flying away, and that the snake being at most five feet long, could never have reached even the lowest branch.

Besides he could have no nest to protect, for in the rocks of Avon there is no water save rain-water in the hollows of the rocks, and this is not potable on account of microscopic leeches which people it, the instinct of birds teaching them to avoid it.

Jersey, August 29 CHATEL

P.S.—I inquired of Mr. Denecourt, "the sylvan of the forest," if he were aware of the existence of such large snakes in the forest, and he told me that he had only seen, in the "rocher Cuvier Chatillon," a snake about four and a half feet long, which he killed, but that even larger snakes had been seen in this very "rocher d'Avon" and in the "rocher St. Germain," but he thought that they were only "couleuvres" of a large size and quite inoffensive.

Meteor

ON the 19th inst., at 11.34 p.m. (within a minute of G.M.T.), I observed a large meteor in the east, towards which I happened to be looking, the sky being quite free from clouds, and clear.

When first seen it was half way between Capella and α Persei; it passed in a slightly curved direction, which was concave towards Auriga, downwards to a line joining β and α Tauri, disappearing at a point one-third of the distance from β towards α. It was very much brighter than Jupiter, and quite half the diameter of the moon; I made these comparisons immediately after it had passed.

Its passage occupied about eleven and a half seconds, and it left a bright continuous yellow streak in its wake, which did not fade until about two or three seconds after the meteor had disappeared; this enabled me the more readily to fix its position. The whole of its path was not seen, as it emerged from behind a tree with thick foliage, though its light attracted my attention before the meteor appeared.

It was pear-shaped, and its brightness appeared to slightly increase; its colour was a very bright, reddish yellow changing to deep purple at its disappearance; it was not followed by any noise. The latitude of place of observation is N. 51° 28′ 20″, the longitude W. 0° 20′ 17″. C. THWAITES

Isleworth, September 21

EVOLUTION AND FEMALE EDUCATION

ONE of the most remarkable features of the advance of science is perhaps the increasing facility afforded for bringing under the grasp of *physical* treatment questions formerly thought to be within the range of abstract reasoning alone. These two methods, if correct, will of course run parallel to each other, and at the same time tend reciprocally to confirm their truth:—the *physical* method being often the more easily followed, and therefore perhaps considered on that account the more certain of the two. Many instances may no doubt readily present themselves of conclusions formerly reasoned out on abstract grounds (more especially by the ancient philosophers), and subsequently confirmed by physical reasoning. As a modern example of this *double* treatment of the same subject we might mention the very important question of the higher mental training of women, dealt with by the late John Stuart Mill on substantially abstract grounds, and touched on by the theory of evolution on physical grounds. As we propose solely to notice the *physical* side of the question here, perhaps this brief essay may not be thought unsuited to the columns of NATURE. We do not expect to bring forward anything especially new, but we may perhaps exhibit the case in some novel aspects; at the same time we may avoid elements of uncertainty by carefully separating the facts supported by scientific evidence from the question of the desirability or undesirability of the measures to be taken upon these facts as a basis, and thus the paper may hope to attain that degree of reliability or solidity which is usually looked for in a journal of natural science.

Perhaps the most valuable characteristic of the doctrine of evolution (or the history of the past rise of man) is the lesson it gives for future progress. It will be apparent that an inquiry into the conditions affecting the progress of mankind would want one of its primary elements if the conditions bearing on the advancement of woman (as one half the race) were excluded therefrom; and the fact of this point being popularly underrated may be considered as rather in favour of its value and significance than not, inasmuch as all great reforms consist in the conquest of popular prejudice. That the value attached to this reform by Mill, which occupied a great part of his life, was not overestimated by him will, we think, become all the more evident when the subject is brought under the test of the theory of evolution.

Mr. Darwin in his work, "The Descent of Man" (second edition), remarks:—"It is indeed fortunate that the law of the equal transmission of characters to both sexes prevails with mammals, otherwise it is probable that man would have become as superior in mental endowment to woman as the peacock is in ornamental plumage to the peahen" (p. 565).

This therefore puts the question of the education of woman in a somewhat new light: though in a light probably suspected by some (including, it may be said, the writer) beforehand, on abstract grounds. For this would show, on a reliable physical basis, that one of the chief arguments for the intellectual training of woman must be for the direct benefit of *man*. For the above deduction, grounded on the evidence of natural science, would indicate clearly that man, by opposing the intellectual advance of woman for countless generations, has enormously injured his own advance—by inheritance. In other words, while man has been arbitrarily placing restrictions in the way of the mental progress of woman, nature has stepped in, and by the laws of inheritance has (to a large extent) corrected, at his expense, the injury which would otherwise have been inflicted, and which, without this interposition of natural law, would have made itself transparently obvious, centuries ago. Man, by hindering woman from performing her natural share in the work of brain development, has been compelled by nature to do the work for her, and valuable brain tissue (accumulated by mental discipline), which would have been man's own property as the fair reward of intellectual labour, has gone over by the rigorous laws of inheritance[1] to the female side, to fill up the gap artificially created by man through his persistent hindrance of woman from doing her part in the progressive development of the brain. The probable extent of the gap by accumulation (from all causes, including the very important factor of man's obstruction) is apparently roughly indicated by the comparison employed in the above quotation. It would seem, therefore, that it could scarcely be said to be altogether fortunate (in *one* sense at least) that "the law o equal transmission of characters to both sexes prevails with mammals;" for this fact has served to conceal an evil which in reality exists in all its magnitude, and which otherwise it would not have required the intellect of a Mill to detect, but which must have become glaringly apparent long ago. Physical science would therefore appear to show a remarkable confirmation of Mill's magnificent theoretic analysis, and of the reality of those evils, the clear exposure of which by him looked to some like exaggeration. In fact it would result from the scientific evidence that however monstrously women might have been treated, however much idleness might have been enforced, or healthy brain exercise prevented, nature would have infallibly corrected the irregularity *at the expense of man*, entailing of course the partial extinction of the progress of the race (as a whole). Possibly the not uncommon popular ridicule which (at first, at least) accompanied Mill's protests, the conceited independence of some men in ignoring the fact that they are descended from women, and their failure even now to realise so obvious a truth as the desirability of clearing away all obstacles to the intellectual advance of woman (by facilitating education, by removing the bars to healthy exercise of the brain in suitable professions, &c., in place of idleness) may itself be in part a consequence of the deficiency of brain tissue caused by the drain through inheritance which goes to counteract their efforts of obstruction. Some of the reasons urged against the higher mental training of woman are of so superficial a character as themselves to show the extensive magnitude of the evil. One notoriously not uncommon ground adduced

[1] Possibly (and we believe this may have been suggested by others) the less stability, or sometimes almost hysterical character of the female intellect may be naturally due to the brain qualities being gained mainly by *inheritance* instead of by hard practice, as in the case of man's brain attributes. While the faculties of man have acquired the steadiness produced by centuries of healthy intellectual discipline and exercise, the field for this has been closed to woman to a large extent. In fact the scientific evidence would appear to show that the common brain (*i.e.* the brain common to the race) has been built up mainly by man's efforts, while woman has to a great extent *inherited* her share at his expense, though no doubt if left entirely unfettered she would have largely contributed to the common good; and it may be inferred with tolerable safety that the race would then have been elevated far above its present *status*.

is that women already are, as a rule, somewhat inferior in mental power to men, forgetting that they were precisely made inferior by the obstacles thrown for centuries in the way of their advance (some of these specially fixed by legal enactment), and which are sometimes of such a kind as almost to amount to a tax on liberty. It may well be conceivable that the law of inheritance, though it has achieved a vast amount, may not have been able to combat these artificial conditions for producing inferiority with entire success. The above plea of existing inferiority in mental power, therefore, so far from being an argument *against* female education, ought, when justly viewed, to be regarded as the strongest reason the other way. For if obstruction has produced—in spite of the powerful countervailing influence of the law of inheritance—a certain degree of inferiority: so (conversely) by equally reliable casual sequence encouragement would produce an effect in the opposite direction. Moreover, precisely on account of the fact that woman is already somewhat handicapped by nature in the race of progress, would there be all the more reason why every encouragement should be given; *à fortiori*, all artificial hindrances in the way of advancement removed. It would be a great mistake if the idea were for one moment entertained that progress can be accomplished by letting matters generally drift under the influence of prevailing custom. If there is one thing more certain than another it is that man can never hope to progress with satisfactory rapidity without having a sharp eye to the conditions necessary for this object, and examining (by the light of reason and knowledge gradually acquired) all his customs, to see if they are desirable or not. To facilitate this end the history of past progress, unfolded in the theory of evolution, may afford some valuable instruction. The increasing appreciation of the value of co-operating with the weak, instead of domineering over them, may be perhaps regarded as one of the most pleasing accompaniments to the advance of science.

S. TOLVER PRESTON

THE YANG-TSE, THE YELLOW RIVER, AND THE PEI-HO[1]

THESE three rivers form conjointly the great river-system of China. Although at the present day each of them runs its separate course to the sea, there is good reason to believe that several centuries since they were united by a number of connecting branches in a manner somewhat resembling the junction of the Ganges and the Brahmapootra in our own time. Such is the inference to be drawn from an ancient Chinese map copied by Alvarez Semedo, a Portuguese Jesuit, and which must be assigned to a time preceding that of the construction of the Grand Canal by Ghenghis Khan in the beginning of the thirteenth century.[2] Linked together as these rivers were in the past, a brief consideration of their present condition will prove that they are labouring towards the same end in our own day. But before proceeding to examine them in their conjoint character, it will be necessary to consider briefly their leading hydrological features.

1. *The Yang-tse*—the largest and most important of these three rivers—has a course of about 3,000 miles, and drains an area which is variously estimated between 750,000 and 550,000 square miles: for my own calculations I will adopt the mean of these two estimates, namely, 650,000 square miles. Its waters, commencing to rise in February and March, reach their highest level in the month of June or July; and here they remain with occasional fluctuations till the end of August or the beginning of September, finally reaching their lowest level towards the close of January.

With regard to the *discharge of water* of this river, Capt. Blakiston[1] has estimated the average amount carried past T-chang, which is situated at about 960 miles from the sea, at 500,000 cubic feet per second; he founded this estimate on observations made during the months of April and June. When stationed at Hankow in the winter 1877-78, a place distant about 600 miles from the sea, I set to work to make a similar estimate of the water carried past that city for the twelve months included between May 1877 and May 1878. Having taken a line of soundings across the river and having ascertained the river's breadth (1,450 yards, by sextant measurement) at a point below the union of the Han with the main stream, I commenced a series of observations on the rise and fall of the river water and on the force of the current, which, combined with information received from the Custom-house and from other sources, supplied me with the necessary data for my calculation. The results are contained in the following table:—

1877.		Surface current. Knots per hour.	Average depth. Feet.	Water-discharge. Cubic feet per second.
May	31	$2\frac{1}{4}$	64	846,336
June	30	$2\frac{1}{2}$	61	896,293
July	31	3	58	1,022,656
August	31	$3\frac{1}{5}$	62	1,275,381
September	30	$2\frac{3}{4}$	63	1,018,248
October	31	2	53	622,997
November	30	$1\frac{1}{4}$	42	308,560
December	31	1	36	211,584
1878.				
January	31	$\frac{4}{5}$	30	141,085
February	28	$1\frac{4}{5}$	39	412,626
March	31	$1\frac{1}{2}$	45	396,720
April	30	2	57	670,016
				12)7,822,502
				651,875

We may therefore place the average water-discharge for the year at Hankow at *650,000 cubic feet per second.* Now, estimating the area of drainage above Hankow to be about $\frac{11}{13}$ of the whole area, and assuming that the portion of the Yang-tse valley below Hankow drains off its waters at the same rate as the remainder of the river-basin, it follows that the average water-discharge for the whole river may be placed at *770,000 cubic feet per second.*

With reference to the amount of *sediment* carried by the Yang-tse past the same city, I found as much as seven grains in the pint (nearly one drachm in the gallon) in the month of July, when the river was at its height; while in March, when the river was low, I found as little as three-fifths of a grain per pint. The average proportion of sediment during the twelve months in question I estimate at *four grains in the pint* (a little over half a drachm per gallon). This represents a proportion of $\frac{1}{2188}$ by "weight," or (taking the specific gravity of the dried mud at 1·9) of $\frac{1}{4157}$ by "bulk" of the average discharge of water. It is thus easy to obtain the total amount of sediment carried during the twelve months past Hankow, namely, 4,945,280,250 cubic feet: but to allow for the amount of mud a river pushes along its bed, one-tenth must be added according to the principle laid down by Messrs. Humphreys and Abbot in the case of the Mississippi. This will bring the *total annual discharge of sediment* at Hankow up to *5,439,808,275 cubic feet*, or at the rate of 172 cubic feet per second. Now, assuming that the drainage area below Hankow sup-

[1] The author, Surgeon H. B. Guppy, of H.M.S. *Hornet*, writing from Yokohama, February 11, says:—"I forward to you by this mail a paper containing the results of observations I have made during the last two years on the subject of the Yang-tse and the Pei-ho, together with similar information as regards the Yellow River. Looking on these three rivers as in reality one river-system, I have embodied in one paper all the 'data' concerning them; and have treated them both separately and in their conjoint character. I can answer for the accuracy of the various estimations, and have employed the usual methods in obtaining them."

[2] *Vide* a paper by Mr. S. Mossman on the "Double Delta of the Yellow River," published in the *Geographical Magazine* for April, 1878.

[1] "Five Months on the Yang-tse."

plies the same relative amount of sediment as the remainder of the catchment basin, I estimate the *total amount carried down to the sea annually at 6,428,858,255 cubic feet.*

The removal of this amount of sediment from an area of drainage of 650,000 square miles represents a lowering of the surface at the rate of *one foot* (of rock) *in 3,707 years.* This is therefore the rate of "subaërial denudation" of the valley of the Yang-tse as far as concerns the quantity of sediment removed. Of the proportion of solids in solution, I have had no opportunity of judging, but that the soluble matter is in considerable quantity is rendered probable from the extensive limestone districts traversed by this river.

2. *The Yellow River* or *the Hoang-ho* has derived the appellation of "China's Sorrow" from its frequent destructive inundations. It runs a course of about 2,500 miles; but, unlike the Yang-tse, its lower course has frequently shifted in the course of ages, and although it opens at the present day into the Gulf of Pe-chili, only a quarter of a century has passed since it emptied its waters into the Yellow Sea.[1] The mountainous district of the province of Shantung has in truth been the chief means in deflecting the waters of this great river on more than one occasion during the historical era from the Gulf of Pe-chili to the Yellow Sea, and *vice versâ*.

With reference to the *quantity of water discharged* by the Hoang-ho I have had no opportunity of personal observation. We have, however, an estimate not only of the water-discharge, but also of the sediment, which Sir George Staunton supplies us in his account of Lord Macartney's embassy to China in 1792. It was calculated that at the place where the British embassy crossed the Yellow River—its junction with the Grand Canal—the water was carried past at the rate of 418,176,000 cubic feet per hour, or *116,000 cubic feet per second.* The method employed in ascertaining the quantity of sediment was the measurement of the amount of mud deposited from a gallon and three-quarters of water when allowed to stand. From this experiment it was concluded that the sediment was in the proportion of $\frac{1}{200}$ of the original bulk of the water, and the *annual discharge of sediment* was assessed at *17,520,000,000 cubic feet.*

However carefully these observations may have been made, and however near they may approach the actual discharge of water and of sediment at the time in question, it seems to me that one is hardly justified in accepting the result of a single observation as typical of the average state of things throughout the year; and yet Sir George Staunton's estimate has never, as far as I am aware of, been questioned. A single glance at the foregoing table will convince one of the little dependence that can be placed on a solitary estimation; it will be there seen that the Yang-tse discharged nine times as much water when at its highest level in August as it did during the month of January, when its waters occupied their lowest level.[2] Or if the question of sediment is considered, to which the same objection would apply, I have the greatest diffidence in accepting Sir George Staunton's estimate as being of any value except as a trustworthy result of a single experiment; and yet, even considered as the maximum of the whole year, the result is a rather startling one. While the greatest amount of sediment I found in the water of the Yang-tse was seven grains in the pint, and in the case of the Pei-ho—as will subsequently be noticed—fifteen grains in the pint, Sir George Staunton estimates the sediment of the Yellow River at over eighty grains in the same measure of water. Even the muddy waters of the Ganges do not contain more than twenty grains of sediment in the pint of water.

It is therefore not with any surprise that I find the "subaërial denudation" of the Hoang-ho is estimated[3] at less than half that of the Yang-tse, namely, *one foot in 1,464 years.* This estimate only refers to the amount of sediment removed, and yet I cannot but consider it as very liable to correction by some future observer. As this is the only calculation that has ever been made, as far as I am aware, with reference to the quantities of sediment and of water discharged by the Yellow River, I am perforce obliged to accept it *pro tanto*.

[1] *Vide* Mr. Mossman's paper, already referred to.

[2] In the case of the Ganges at Ghazepoor the proportion is as 1 to 14.

[3] *Vide* NATURE, vol. xviii. p. 268.

3. *The Pei-ho* drains the great plain which constitutes the province of Pe-chili. Its length is said to be about 300 miles, but the lower part of its course below the city of Tientsin is so tortuous that a distance of thirty miles overland is converted into fifty by the river. It is at Tientsin that the Pei-ho proper and the Yu-ho unite together to form the main stream: the latter is generally known by Europeans as the Grand Canal, but as a matter of fact the canal joins the Yu-ho about 150 miles to the southward. During the three winter months—December, January, and February—the Pei-ho is usually frozen over, the ice having a thickness of about eighteen inches; in the same season there is generally a large quantity of ice in the Gulf of Pe-chili, which may completely fill up the head of the gulf.

With reference to the *water-discharge* of this river, I was enabled while wintering at Tientsin during the season 1878-79, to collect some "data" for its estimation during the four months from December to March. Although my estimate strictly applies to but a third of the year, still from the limited rise and fall of the water during the different seasons (it never exceeds six feet) I feel pretty confident that it fairly represents the average rate of discharge during the whole year. The breadth of the river at the place of observation below the city of Tientsin was 280 feet. The following table contains the results of my calculations:—

1878.	Surface current. Knots per hour.	Average depth. Feet.	Water-discharge. Cubic feet per second.
December	$1\frac{1}{5}$	14	6,355
1879.			
January	$\frac{4}{5}$	$14\frac{1}{2}$	4,389
February	$1\frac{3}{5}$	16	9,684
March	2	14	10,592
			4)31,020
			7,755

We may thus place the average discharge of water for the whole year at about *7,700 cubic feet per second.*

Now with regard to the *amount of sediment* carried past the city of Tientsin: I found the average quantity during the four months in question to be about *five grains per pint.* (It varied much at different times, for I found as much as fifteen grains in the middle of March, while in the months of January and February it did not equal a grain in the pint.) This represents a proportion of $\frac{1}{1750}$ by "weight," or $\frac{1}{3325}$ by "bulk" of the average discharge of water: and following the same method of calculation as was employed in the case of the Yang-tse, I estimate the annual discharge of sediment for this river at *80,000,000 cubic feet.*

Now the removal of this bulk of material from an area of drainage, which I estimate at 55,000 square miles, represents a lowering of the surface of *one foot in 25,218 years.* This is the rate of "subaërial denudation" of the Pei-ho basin, omitting of course the question of the solids in solution.

To show the rank that these three rivers hold in the fluvial system of the globe, I have subjoined a list of fourteen other rivers, which gives the quantities of water and sediment discharged by each, as well as the rate of subaërial denudation, as far as I have been able to ascertain.

	Water discharged per second.	Sediment per annum.	Subaërial denudation.
	Cubic feet.[1]	Cubic feet.	
Amazon	2,458,026	—	—
Congo	1,800,000 (By Behm and Capt. Tuckey)	—	—
Yang-tse	770,397 (By myself)	6,428,800,000 (By myself)	1 foot in 3,707 years. (By myself)
Plate	700,000 (By Mr. Higgin and Mr. Bateman)	1,543,500,000 (By Mr. Higgin)	1 foot in 29,400 years. (Calculated from Mr. Higgin's estimate)
Mississippi ...	618,000 (By Messrs. Humphreys and Abbot)	7,474,000,000 (By Messrs. Humphreys and Abbot)	1 foot in 6,000 years. (By Mr. Croll)
Danube	300,321	1,255,500,000 (By Mr. Ch. Hartley)	1 foot in 6,846 years.
Shat-el-Arab ...	295,461	—	—
Ganges, at Ghazepoor ...	203,485 (By the Rev. Mr. Everest)	6,368,000,000 (By the Rev. Mr. Everest)	1 foot in 2,358 years.
Indus	199,476	—	—
Atrato	185,274	—	—
Nile	130,032	—	—
Yellow River ...	116,000 (By Sir George Staunton)	17,520,000,000 (By Sir George Staunton)	1 foot in 1,464 years.
Rhone	91,935	594,000,000	1 foot in 1,528 years.
Rhine	69,741	—	—
Po	61,263	405,420,000 (By M. Lombardini)	1 foot in 729 years.
Pei-ho	7,755 (By myself)	80,000,000 (By myself)	1 foot in 25,218 years.
Thames, at Kingston ...	2,300 (By Prof. Prestwich)	1,865,900 (Huxley's "Physiography.")	1 foot in 9,600 years.[2] (Huxley's "Physiography.")

We have now the necessary "data" for considering these three rivers in their conjoint character. Together they drain an area of 1,105,000 square miles; they discharge a body of water equal to *894,000 cubic feet every second;* and they carry down every year to the sea *24,028,800,000*[3] *cubic feet of sediment*, which represents a rate of subaërial denudation equal to the removal of *one foot of solid rock in 1,687 years*.

If we look upon the Yang-tse, the Yellow River, and the Pei-ho as labouring, with the assistance of the gradual elevation of the sea-border which is at present going on, to extend the territory of China seaward towards her ancient coast-line—represented by a line running from Kamtschatka through the Kurile Islands, Japan, the Loo-choo group, Formosa down to the Malay Archipelago;[4] and carry ourselves forward into the future when such task is completed and the waves of the Pacific beat once more against this old sea-border, we shall not have much difficulty in picturing to ourselves what will then be the state of matters. In the place of the gulf of Pe-chili and the Yellow Sea there will be vast alluvial plains traversed by the waters of the Yang-tse, the Yellow River, and the Pei-ho; but before the ancient coast-line is reached they will have joined to form one great river and one united delta. If the Yellow River confines itself mostly in future ages to its course into the gulf of Pe-chili, that gulf will be filled up in process of time; and the Hoang-ho winding along through the bed of this obliterated sea will, after being joined by the Pei-ho, turn its course southward, deflected by the Corean peninsula, until it meets at length its sister stream. On the other hand, should the Yellow River be mostly occupied in future in advancing its southern delta it will join the Yang-tse at a period much less remote from the present; and their united waters will pursue an easterly direction subsequently to be joined by the Pei-ho, which will have been gradually finding its way through the gulf of Pe-chili and the Yellow Sea during the preceding ages. In either event the union of these three rivers would follow.

Such being the case, it may be interesting to speculate on the time required by these rivers to fill up the seas into which they discharge their sediment. Sir George Staunton estimated that at the rate the Hoang-ho was discharging sediment it would fill up the Yellow Sea and the gulfs of Pe-chili and Lian Tung in 24,000 years; but M. Elisée Reclus is of opinion that this estimate ought to be doubled, as the Yellow Sea is much deeper than Sir George Staunton stated it to be (20 fathoms). On carefully examining the latest charts of these seas I am inclined to consider that this estimate cannot be assailed on this point, as my own determination of the average depth is 22 fathoms.

We will now attempt to gauge the time that the three rivers in question would require to fill up by the sediment they deposit the portion of sea which is included by the gulfs of Pe-chili and Lian Tung, the Yellow Sea, and the Eastern Sea north of the 29th parallel and west of the 126th meridian. I have placed the total surface area at 200,000 English square miles, and the average depth at 26 fathoms; and following Sir George Staunton's mode of estimation I find that it would take sixty-six days to form an island a mile square reaching up to the surface of the sea. At this rate it would require 36,000 years to form all the sea in question into dry land, supposing of course that there was no elevation or depression of the sea-bottom during that period. But, the recent formation of several islands and shoals in the Yang-tse estuary, the occurrence of raised beaches and marine remains at Hang-chau, Wusung, and Chefoo, with other similar evidences, demonstrate that there is an elevation of the coast going on at present; and, in that case, it will require considerably less than 36,000 years to form the sea into *terra firma*. Perhaps Sir George Staunton's original estimate for the Yellow River may not be far wrong when applied to the whole sea in question.

[1] Where not otherwise mentioned I have obtained my information of the discharge of water and sediment from the "Earth," by Elisée Reclus.

[2] This estimate also includes the solids held in solution.

[3] In Page's "Advanced Text-book of Geology" Staunton's estimate of the sediment discharged by the Yellow River has been erroneously applied to all "the great Chinese rivers."

[4] *Vide* a paper on this subject, by Mr. A. S. Bickmore, read before the North China branch of the Asiatic Society in November, 1867.

PHYSICS WITHOUT APPARATUS[1]

VI.

ACOUSTICAL experiments require, for the most part, the aid of some good instrument or valuable piece of apparatus. Nevertheless a few instructive illustrations of the principles of the science can be improvised without difficulty. Firstly, there is the familiar experiment brought into fashion, we believe, by Prof. Tyndall, of setting a row of ivory billiard balls or glass solitaire marbles along a groove between two wooden boards, and showing how their elasticity enables them to transmit from one to another a wave of moving energy imparted to the first of the row, thus affording a type of the transmission of sound-waves from particle to particle through elastic media. Then we may show how sounds travel through solid bodies by resting against a music-box or other musical instrument, a broomstick, or any convenient rod of wood, at the other end of which we place our ear. A kindred experiment, illustrative of the transmission of sounds through threads, is depicted in Fig. 20. A large spoon is tied to the middle of a thin silken or hempen thread, the ends of which are thrust into the ears upon the ends of the thumbs. If the spoon be dangled against the edge of the table it will resound, and the tones reach the ear like a loud church bell. The thread telephone or "lover's telegraph," is upon the same principle, the thread transmitting the whispered words to a distance, without that loss by spreading in all directions which takes place in the open air.

[1] Continued from p. 464.

The discovery that a musical tone is the result of regularly recurring vibrations, the number of which determines the pitch of the tone, was made by Galileo without any more formal apparatus than a mill-edged coin, along the rim of which he drew his thumb-nail, and found it to produce a sound. We can show this better by taking a common toy gyroscope-top with a heavy leaden wheel, such as are sold at every toy-shop. With a strong penknife or a file, cut a series of fine notches or grooves across the rim, so that it shall have a milled edge like a coin. Now spin it, and while it spins, gently hold against the revolving wheel the edge of a sheet of stiff writing-paper or of a very thin visiting card. A loud clear note will be heard if the nicks have been evenly cut, which, beginning with a shrill pitch, will gradually fall with a dolorous cadence into the bass end of the scale, and finally die out in separately audible ticks.

Much notice was attracted some years ago by the discovery of singing and sensitive flames. A sensitive

FIG. 20.

flame is not easily made, unless where gas can be burned at a much higher pressure than is to be found in the case of the gas supplied by the companies for house-lighting. To make a singing-flame requires the proper glass tubes and an apparatus for generating hydrogen gas. The roaring-tube, which we are now about to describe, is a good substitute, however, and is also due to the generation of very rapid vibrations, although in this case the way in which the heat sets up the vibrations cannot be very simply explained. Let a common paraffin-lamp chimney be chosen, and let us thrust up loosely into its wider or bulbous portion a piece of iron-wire gauze such as is often employed for window-blinds. If this be not at hand a few scraps of wire twisted together, or even a few hair-pins will suffice. The lamp-chimney must then be held over the flame of a spirit-lamp, or other hot flame, until the wire-gauze glows with a red heat (see Fig. 21). Now remove the lamp or lift the chimney off it, so that the gauze may cool. It will emit a loud note like a powerful (though rather harsh) organ-pipe, lasting for about a quarter of a minute, or until the gauze has cooled Tubes of different sizes produce different notes.

It is now well known that the quality of different sounds depends upon the form or character of the invisible sound-waves, and that different instruments make sounds that have characters of their own, because their peculiar shapes throw the air into waves of particular kinds. The different vowel-sounds are caused by putting the mouth into particular shapes in order to produce waves of a particular quality. Take a jew's-harp and put it to the mouth as if you were going to play it. Shape the mouth as if you were going to say the vowel O, and on striking the harp you hear that sound. Alter the shape of the mouth to say A, and the harp sounds the vowel accordingly. The special forms of vibration corresponding to the different vowel-sounds can be rendered evident to the eye in a very beautiful way by the simplest conceivable means. A saucerful of soapy water (prepared

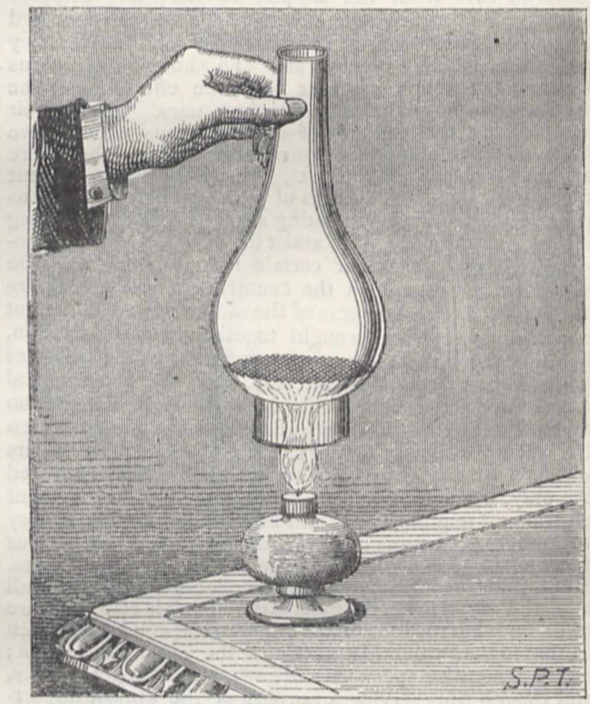


FIG. 21.

from yellow kitchen soap and soft water, or with cold water that has previously been boiled) and a brass curtain-ring, is all that is needed. A film of soapy water shows, as all children know when they blow bubbles, the loveliest rainbow-tints when thin enough. A flat film can be made by dipping a brass curtain-ring into the soapy water, and then lifting it out. When the colours have begun to show on the edge of the film, sing any of the vowels, or the whole of them one after the other, near the film. It will be thrown into beautiful rippling patterns of colour which differ with the different sounds. Instead of a curtain-ring the ring made by closing together the tips of finger and thumb will answer the purpose of providing a frame on which to produce the phoneidoscopic film.

(To be continued.)

GENERAL PITT RIVERS' (LANE FOX) ANTHROPOLOGICAL COLLECTION

THE collection which General Pitt Rivers, F.R.S., commenced to form in the year 1851 became well known to all immediately interested in the science of anthropology during the series of years in which it was exhibited at the Bethnal Green branch of the South Kensington Museum as Col. Lane Fox's collection, and no one visited it without picking up a great deal of interesting and curious information. To those who studied it with care it opened up a new field of exploration, and invested all objects of art and manufacture, from the simplest ornaments, weapons, or implements of savages, to works the product of the highest modern culture, with a certain peculiar interest over and above the gratification derived from the objects themselves without reference to the history of their origin. It is needless to say that the moving power of this peculiar interest was the evolution theory, for the object which General Pitt Rivers set before him was, as he explained, "so to arrange his collection of ethnological and prehistoric specimens as to demonstrate, either actually or hypothetically, the development and continuity of the material arts from the simpler to the more complex forms. To explain the conservatism of savage and barbarous races and the pertinacity with which they retain their ancient types of art. To show the variations by means of which progress has been effected and the application of varieties to distinct uses. To exhibit survivals or the vestiges of ancient forms which have been retained through natural selection in the more advanced stages of the arts, and reversion to ancient types. To illustrate the arts of prehistoric times as far as practicable by those of existing savages in corresponding stages of civilisation. To assist the question of the monogenesis or polygenesis of certain arts; whether they are exotic or indigenous in the countries in which they are found. To this end objects of the same class from different countries have been brought together in the collection, but in each class the varieties from the same localities have been placed side by side, and the geographical distribution of each class has been shown in distribution maps." The gradual growth of the arts has of course been the theme of many writers. But General Pitt Rivers was the first, and up till now has, we believe, remained the only, collector who has investigated the development of arts and manufactures, and brought home their history to students by means of series of the objects themselves arranged in groups so as to illustrate their actual pedigrees.

It is in the arrangement that the collection differs from all others. Very many of the objects of which it is composed are to be found in most ordinary ethnological collections, such as that in the British Museum, and the Christy collection; but in these the specimens are arranged geographically, and though thus serving a purpose of the utmost importance as showing in what matters of culture the various races of man are most clearly distinct and separate, or more or less allied, they do not afford that kind of information which it is the one aim of General Pitt Rivers' collection to convey and develop. In fact in the case of all series of objects of arts or manufactures two collections are absolutely required: the one to illustrate pedigree in accordance with the Darwinian theory, the other to illustrate geographical distribution. A collection arranged on General Pitt Rivers' plan is much needed in natural history galleries. What is specially required for the purposes of general instruction is a series which shall trace the pedigree of man and all the other highest types in the several groups as directly as possible from the lowest forms of life. Such a collection might be arranged in a series of galleries radiating from a central chamber in which should be placed the lowest forms, each gallery leading gradually up to the highest of the group to which it was alloted. Good models should represent in the series those links which are embryonic, or which require reconstruction from fossil remains.

Since the year 1851, when General Pitt Rivers' collection was first commenced, it has been continuously added to, and it has now reached very considerable dimensions. The space allotted to it at present in the South Kensington Museum will not be sufficient to display it sufficiently. General Pitt Rivers has most generously offered to present it to the nation on certain conditions, which will insure its being properly maintained in its present arrangement, and prevent the possibility of its being broken up and distributed amongst other collections by any future authorities who might not thoroughly comprehend its importance in its present condition. It is stipulated that General Pitt Rivers shall have the management of the collection during his lifetime, and that sufficient space shall be allotted to him to allow of his making additions and further developing it in accordance with the plans which he has formed.

A committee consisting of Sir P. Cunliffe Owen, Col. Donelly, Mr. Augustus Franks, Prof. Huxley, Sir John Lubbock, Mr. Poynter, and Prof. Rolleston, was appointed to consider the advisability of the acceptance of the collection by the nation, and it has, we believe, although the conclusions arrived at have of course not been officially announced, reported unanimously in favour of its being accepted. There can be no doubt that it has acted with the best judgment in so doing; indeed the eminent men of science and art of which it was composed could have arrived at no other conclusion. It would be a very serious matter if the country were to miss so excellent an opportunity, and there could be no better place for the collection than in the South Kensington Museum. It is, as it were, the key to the whole of the vast collections there gathered together. On the one hand, in the Pitt Rivers collection is traced the earliest history of inventions, showing plainly how every primitive implement and machine grew slowly from the simplest contrivances, thus leading up to and acting as a preliminary training for the study of the contents of the Patent Museum; whilst on the other is to be learnt the developmental history of all the arts, the gradual development of sculpture and painting, the history of the development of pattern ornaments, the growth of musical instruments, of the art of pottery, of clothes, and the history of the gradual development of ships. All these series and very many others lead directly up to the various large collections of paintings, sculpture, pottery, models of shipping, &c., which it is the main object of the Museum to exhibit, and cannot but greatly enhance their value and interest to the student. They serve to impress upon the observer the curious fact that all arts and inventions, even those apparently of extreme simplicity, have never been arrived at by jumps, but have grown slowly by degrees by means of a series of slight modifications, just as in the case of biological development. The collection, it should be remarked, does not in any way clash with the Christy and British Museum collections, which are arranged on a perfectly different plan, and which do not in any way bring together savage and civilised objects. There is full room for both collections, and indeed a necessity for them.

We will now draw attention briefly to some few of the series of objects exhibited in the collection taken more or less at random as samples of the whole. The collection may be considered as consisting of three parts. Firstly, a collection of photographs of the various races of mankind which is not as yet far advanced, though it contains large and instructive series of portraits of Danes, Scandimavians, the people of Britanny, and Japanese; whilst together with the photographs is a small series of those skulls which show the best marked racial characteristics, and another which is to exhibit the various modifications in the forms of their skulls which are made by different races. Secondly, the very large collection showing the growth of weapons of all kinds. Thirdly, the various series illustrating the development of musical instruments, ornaments, sculpture, painting, and artistic design of all kinds; and fourthly, those which relate to the development of implements, utensils, houses, ships, machines, and strictly useful appliances of all kinds. Of course the two latter series run into one another, and it is impossible to draw a distinct line between them in the case of the lower terms of the series. General Pitt Rivers has especially drawn attention to the manner in which primitive implements subserve many uses: how, for example, a spear-head may do duty as a knife, as is the case with the obsidian-headed spears of the Admiralty Islanders. The earliest Palæolithic stone implements made for grasping in the hand were no doubt weapons of offence, diggers, hammers, nut-crackers, choppers, all in one.

We propose to give a slight sketch of some of the series in the collection, taken at random from its several departments, culling freely from the owner's published catalogue, and his papers read before the Anthropological Institute and elsewhere. We may state at the outset that there exists as yet a catalogue of the weapons only. General Pitt Rivers has not been able to complete a catalogue of the remainder of his collection, since it has been continuously in process of augmentation. The catalogue of the weapons contains so much valuable and curious information that the appearance of the remainder may be looked forward to with great interest.

One of the marked features of the collection is that specimens are usually introduced to show what natural objects may have first suggested primitive contrivances to savage man. Thus amongst the series of savage stone hatchets and adzes we find specimens of natural stone axes as it were (Fig. 1, 1), roots of trees which have grown round and attached themselves firmly to stones which have somewhat of an axe-blade shape, so as to appear like natural hatchets. It is quite conceivable that the first idea of the axe, the fixing the stone blade at the end of a lever, may have arisen from the observation by primitive man, and his possible use of such a natural hatchet.

Amongst the series of specimens illustrating the origin of weaving are placed specimens of bark cloth composed of naturally-interlaced fibres, and we may suggest that it would be well if there were added a specimen of a weaver-bird's nest, which may have given the first hint as to basket-work, and thus led to weaving. In this series is placed a collection of spindle-whorls from all parts of the world—Peru, Vancouver Island, Cyprus, Denmark, England, Ireland. It is most remarkable how closely alike are these implements, though from such widely separated localities. The collection of primitive looms is very interesting, though as yet one of the least complete in the collection. In its primitive condition, as at the Caroline Islands and Vancouver's Island, the loom is entirely portable, consisting of a few sticks only, and only narrow bands, to form belts or armlets, are woven with it. Some years ago we saw such a portable loom in use in Britanny, worked by a boy with his hands and feet, to make girth-like bands with. The boy was working by the road-side and playing about every now and then, with the whole apparatus in his hand. In the bark cloth, made of bark strips welded together by means of beating and the action of water, the "tappa" of Polynesia, we probably see the origin of paper, which in Japan is made from the bark of the same tree as tappa.

The collection of weapons commences with weapons of offence, and begins with a series illustrating the development of the shield out of the parrying-stick, such as now used by Australian blacks, the idea of the wide shield covering the whole body having apparently arisen as an improvement on the simple stick held in the centre, which gradually expanded and grew into a shield. The origin of the bow is a very interesting question. General Pitt Rivers, as explained in a learned disquisition on the subject in his catalogue, and also in his published lecture on "Primitive Warfare," believes that the first idea of the bow may have arisen from the use of an elastic throwing-stick, with the spring-trap of the Malay regions possibly as a stepping-stone. In several places in the world, as, for example, in the Admiralty Islands, the bow is a contrivance still unknown; and Mr. Brooke Low, whose fine collection of Bornean manufactures and implements is now on exhibition at the South Kensington Museum, informs us that it is not in use throughout Borneo, though the coast people necessarily know the weapon. The primitive arrow is merely a spear thrown with the bow. It is such in New Guinea, where the arrows are far too long for the bow, and though they fly for a dozen yards or so with great force, soon wobble and turn over. The arrows have no notch and no feather;

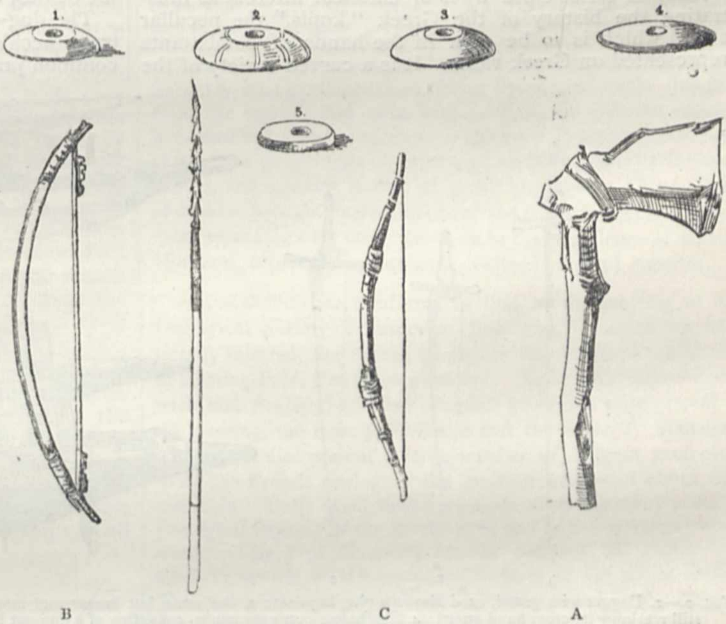


FIG. 1.—A, Natural stone axe formed by the growing of the root of a tree round a blade-shaped stone; B, Papuan bow with broad flat string and long arrow without notch or feather; C, Esquimaux composite bow; 1 to 5, stone spindle whorls. 1, from the Island of Cyprus; 2, from Peru; 3, from Denmark; 4, from Neuchâtel; 5, from Ireland.

the bow-string is wide and flat, made of split rattan cane (Fig. 1, B). The notch and feather are further improvements not yet attained, at all events, in the greater part of New Guinea. At the Aru Islands both notch and feather are in use, but the string is still of rattan narrowed to fit the notch. In some of the New Hebrides the arrows, which are beautifully finished, have the notch, but still no feather. The development of the composite bow made up of several pieces of horn, bamboo, wood, ivory, &c., and usually strengthened by the sinews of animals at the back, is illustrated by a special series (Fig. 1, C). It is concluded to have spread from a common centre in Central or Northern Asia to Turkey, Persia, Greenland, California, and elsewhere.

To speak of more civilised weapons, the origin of the bayonet is peculiarly interesting. Its history is set forth in a special small series, and thus explained in the catalogue:—"In the early part of the seventeenth century it was found necessary to retain the use of pikemen in the infantry, on account of the defenceless position of the firelock-men when the enemy approached to close quarters. To remedy this defect they were accustomed, about the middle of the century, to stick the handles of their daggers into the muzzles of their guns in order to use them as pikes." Implements modified on this principle were called "plug-bayonets" (Fig. 2, 1, 2). One of these in the collection has the date 1647 upon it. The objection to this was that the handles stopped up the muzzle, and the gun could never be fired with the bayonet fixed. Many of the dagger-handles had rings on the guard (Fig. 2, 3), and this suggested the idea of fastening the ring on to the muzzle, and the dagger or plug-bayonet was thus secured on to the outside of a spring, so that the firelock could be loaded and fired with fixed bayonets. The first introduction to this weapon was in one of the campaigns in Flanders, in the time of William III., and greatly were our men astonished at being fired at with fixed bayonets. The series contains all stages leading from the simple dagger with a wooden plug-like handle, through the same with a ring added, to the modern bayonet and its tube and catch.

Another series close by is of classical interest as illustrating the history of the Greek "kopis," the peculiar sword which is to be seen in the hands of combatants represented on Greek vases. It is a curved variety of the straight leaf-shaped bronze sword. It appears to have been brought to Spain by the Romans. It is identical in form with the koohrie of the Goorkas of Nepal, and the Turkish, Albanian, and Persian yataghans are direct descendants of this ancient weapon.

Leaving the series of weapons, we may refer to the collection illustrating the origin and development of boats and ships. Concerning this question General Pitt Rivers has published a valuable memoir, entitled "Early Modes of Navigation," in the *Journal* of the Anthropological Institute. He there divides the subject into five heads, treating of (1) Solid trunks or dug-out canoes; developing into (2) Vessels on which planks are laced or sewn together, and these developing into such as are pinned with plugs of wood, and ultimately nailed with iron or copper; (3) Bark canoes; (4) Vessels of skins and wicker-work; (5) Rafts, developing into outrigger canoes, and ultimately into vessels of broader beam, to which may be added rudders, sails, and contrivances which gave rise to parts of a more advanced description of vessel, such as the oculus, aplustre, forecastle, and poop.

The dug-out canoes probably originated from trunks of trees accidentally burnt hollow in consequence of the common practice of lighting fires at the bases of trees.

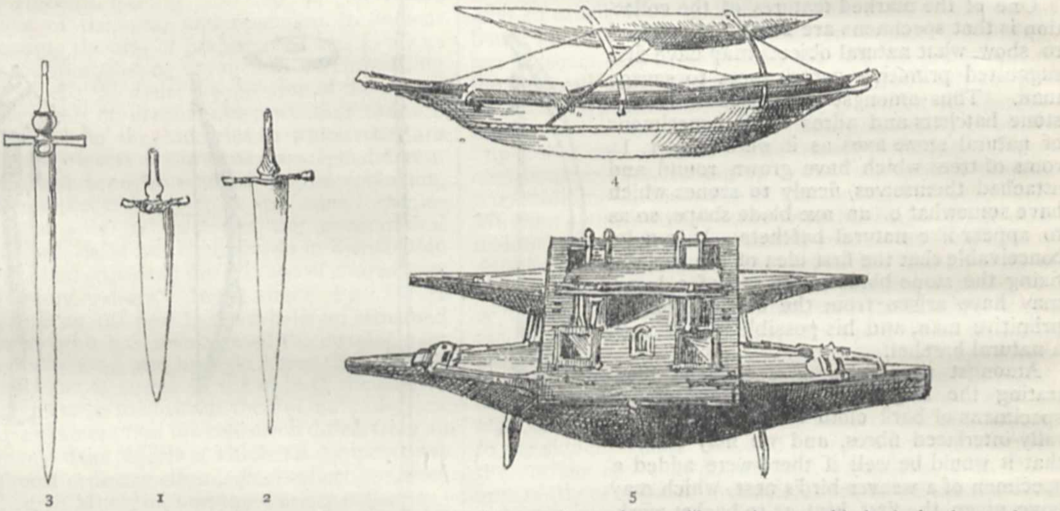


FIG. 2.—1, Dagger with guard, used also as a plug bayonet; 2, the same, but longer and more bayonet-like; 3, bayonet with simple ring for attachment still retaining its cross hand-guard; 4, Singhalese outrigger canoe, consisting of a dug-out base with planks sewn on above; 5, Fijian double canoe.

Some Australian blacks used to paddle about on logs shaped like canoes, but not hollowed out at all, sitting merely astride with their feet resting on a rail of small sticks driven in. As an improvement to the dug-out, wash-boards, or gunwale-pieces, narrow plank strips are added all round at the edge, to keep the wash of the water out. These wash-boards are gradually increased in height till, when the canoe is loaded, the dug-out trunk is entirely below water, and acts merely as a float to support the vessel of planks resting on it. In such a condition are the Cinghalese canoes which come alongside all the steamers at Point de Galle and take passengers on shore (Fig. 2, 4). There is a model of one of these in the series, and also another of a wide flat-bottomed boat, also from Ceylon, in which two dug-out trunks are fastened to the margins of the bottom, one on each side, so as to form lateral floats and give the boat very great stability, this primitive device being absolutely the same in principle as that adopted in the structure of the Czar's new yacht *Livadia*, lately described in NATURE. In progress of development, the dug-out portion of the canoe becomes proportionately less important, its functions being usurped by the superstructure of planks, and eventually the dug-out disappears, or rather survives as the keel only, and the ordinary boat built of planks is the result. The upper planks long remain laced together, and lashed to the dug-out by means of rattans or sennet, the boats having no ribs, but simply thawts as supports for the planks. In Fiji the ribs seen in the interior of the canoes are not used to bring the planks into shape, but are the last things inserted, and are used for uniting the deck more firmly to the body of the canoe. Wallace has described the boats and boat-builders of the Ké Islands. Here, though the ledges of the planks are pegged together by means of wooden pegs, the planks are still fastened to the ribs by means of rattans. The ribs themselves are an addition, after the boat is otherwise complete, and after the first year the rattan-tied ribs are generally taken out and replaced by new ones, fitted to the planks and nailed.

General Pitt Rivers develops the outrigger canoe from the raft. In all Africa and all America there has never existed an outrigger vessel of any kind. All the canoes are simple; but on the coast of South America rafts are used with sails elsewhere unknown in America. Those termed balzas, used on the Guayaquil, in Ecuador, are described by Ulloa. Some are seventy feet in length, and twelve in breadth. They are made of light wooden logs lashed together, and when they are sailing, planks are pushed down into the water between the logs, and, acting as centre-boards, enable the rafts to luff up or bear away, according as they are inserted in the fore or hinder part. On the raft theory the outrigger canoe is supposed to have been developed from an improved modification of the sailing raft, in which two logs were made use of instead of many, as opposing less resistance to the water, and were connected by a platform. Such two-log structures, of course without sails, have been described as in use by the Tasmanians. The use of the sailing rafts on the Pacific coast of America seems to lend probability to the theory, since the outrigger canoe is universal in Polynesia. On this theory the double canoe (Fig. 2, 5) is a highly-specialised development of the two-log rafts; and General Pitt Rivers points for additional proof to the fact that in all double canoes one vessel is always smaller than the other. This may however be merely a contrivance for aiding steering.

On the other hand it seems to us very probable that the outrigger canoe is really derived from the double canoes and that the outrigger float represents, not a log in process of development towards a canoe itself, but a degenerate second canoe. On some parts of the coast of New Guinea the Papuans are accustomed to lash side by side firmly several of their large canoes, when about to set out on a trading expedition of 200 or 300 miles and sail along the coast. Such a group of canoes is called a "lakatoi." It is very probable that the fastening of two dug-outs side by side may have early suggested itself, and that the two may have gradually been separated and fastened by longer and longer cross-pieces, as stability was found to be increased thereby. We merely suggest this other view of the matter as worthy of consideration. It is by means of collections such as that now under consideration that such points can be determined. Luckily, for some reason or other, possibly a religious one, savages all over the world make most carefully-constructed models of their canoes. These are not children's toys, but exact models, correct in all details. Even the wretched Fuegians do this, and the models are not made for purposes of barter originally, since they are made by such races as the Admiralty Islanders, who have no opportunity of disposing of them. We seem even ourselves to make more models than necessary, as the quantities of them in museums testify. General Pitt Rivers has collected a most valuable series of native models of boats and ships of all kinds.

(*To be continued.*)

NOTES

JUDGING from the papers and reports that have reached us, through the kindness of the permanent secretary, Mr. F. W. Putnam, the Boston meeting of the American Association has been a great success. The many attractions of Boston drew together a large concourse, including nearly all the great lights of American science. The people of Boston and Cambridge seem to have exerted themselves to the utmost to make the numerous visitors enjoy themselves, and, from the accounts of the many excursions and receptions, these exertions were completely successful. There were something like a thousand names registered on the books of the Association, and at the Cambridge dinner, on August 24, 870 persons were present. The number of papers entered was 280, all of them evidently duly considered before being admitted, and many of them of great scientific importance.

THE address of welcome of Prof. Rogers, of the Massachusetts Institute of Technology, briefly reviewed the origin of the various National Associations, predicting that the American would in time rival that which at the moment was meeting at Swansea. "Let us," Prof. Rogers said, "make it our special work to exclude from our annual reports all detailed publications which are not of a character actually to add to the stock of human knowledge, whether that knowledge be simply the gathering together of facts by careful processes of discernment, or the development of laws by careful mathematical investigation." Mr. Lewis H. Morgan, the president of the Association, in his brief reply to the addresses of welcome, made some remarks which are quite as deserving of attention here as on the other side of the water. "When the meetings of this Association become indifferent to the communities among which they are held, its usefulness will be near its end. There is a direct connection between the work upon which its members are engaged and the material prosperity of the country, in which all alike have an interest. Scientific investigations ascertain and establish principles which inventive genius then utilises for the common benefit. We cannot have a great nation without a great development of the industrial arts, and this, in its turn, depends upon the results of scientific discovery as necessary antecedents. Material development, therefore, is intimately related to progress in science." The address of Prof. A. Agassiz in Section A we gave in a recent number, and that of Prof. Asaph Hall we hope to be able to give next week. Prof. Bell's remarkable lecture will be found on another page.

THE German Association began its sittings at Danzig last Saturday, and continues them during the present week. Judging from the reports that have been sent us, the German *savants* have received a warm welcome in the great Prussian commercial city. The programme of papers, as we have already intimated, is long, and contains several of great importance. Prof. Cohn of Breslau brought forward at one of the public lectures important data, spreading over many years, as to the prevalence of colour-blindness, especially in Germany, Switzerland, and America.

A CORRESPONDENT informs us that at the meeting of the Geological Society of France at Boulogne, to which we have already referred, the French geologists did England the honour of electing Prof. Prestwich president. Besides Professors Prestwich and Seeley, two other English geologists were present at the meeting, the Rev. J. F. Blake and the Rev. T. Wiltshire. There were also present a large number of Belgian geologists. With the French geologists the meeting numbered about fifty members. Daily excursions were made to all the many places of geological interest in the Boulonnais, and in the evenings papers were read by Prof. Gosselet, Dr. E. Sauvage, M. Pellat, and Prof. Prestwich, on the geological features of the places visited. The geologists were most hospitably entertained by the municipality and other public bodies.

AT the Swansea meeting of the British Association Sir William Thomson, as an incidental illustration of a paper by him, gave the following method of "turning the world upside down." Suppose there to be no sea or other water on the earth, and no hills or hollows; and let the earth be a perfectly elastic or perfectly rigid solid, with no moon nor sun, nor other body to disturb it. Commencing anywhere in the northern hemisphere, walk a few miles northwards or southwards. This, by displacing the earth's axis makes a slope. Then walk up hill as long as you can; then walk a few miles southwards; then lie down and rest, and in time the thing is done; that is to say, what was the South Pole is found under Polaris.

THE autumn Congress of the Sanitary Institute was opened at Exeter on Tuesday, under the presidency of Lord Fortescue.

THE death, on August 2, is announced of Karl Ritter von Hauer, the director of the chemical laboratory of the Geological Institute of Vienna.

A CONGRESS on hygiene was held at Hamburg on September 13, 14, 15. The number of members was about 200. At the first sitting the hygiene of hospitals and public buildings was discussed; at the second the hygiene of shipping, after the delivery of an address by Dr. Reincke; and on the third day the ventilation of private dwellings, and other similar subjects. A resolution, proposed by Dr. Rietschel of Dresden, was passed to induce public authorities to study practically the ventilation of buildings, and another, by Dr. Prath of Dresden, that sanitary inspection should always take place by duly qualified officers. This session is the eighth of the Association.

THE Russian newspapers announce that the jubilee of the zoological museum of the Academy of Sciences, established in 1831 by the Emperor Nicholas, will take place in 1881. Russian and foreign zoologists will meet at St. Petersburg on this occasion.

THE Association Scientifique de France has not continued the observations of meteors which was begun by Leverrier, its founder. No steps have been taken by the Observatory to fill up this important gap in the scientific work of the nation. The interest of observations taken during the last two years in the display of August meteors and the forthcoming inauguration of Leverrier's statue have attracted public attention to this circumstance, and it is hoped these observations will shortly be resumed.

UNIVERSITY COLLEGE, Bristol, has the credit of being the first in England in which the higher education of women has been conducted on a large scale in conjunction with that of men. Its Calendar, which is before us, shows that in the last session, its fourth, the College was attended by more than five hundred students, of whom nearly half were women. A wide range in science and literature is covered by the lectures, of which there are more than forty distinct courses in the day, and more than twenty in the evening. Its engineering department has derived great advantage from the plan under which the students spend the six winter months in the College, and the six summer months as pupils in engineering works in the neighbourhood. The want of space, which has hitherto pressed severely, will be relieved by the opening in October of a part of the new buildings.

THE crayfish is disappearing so rapidly in several French departments that energetic measures have been considered necessary for its protection. The fishing of it has been entirely prohibited in the departments of Meuse and Doubs by prefectorial decrees.

THE freedom of the City of London is to be conferred on Sir Henry Bessemer, F.R.S., on October 6.

M. LORTET gives a brief account in the *Comptes rendus* for September 13, of the results of his dredging in the Lake of Tiberias. The lake is 212 metres above the surface of the Mediterranean, and the greatest depth is 250 metres. M. Lortet finds proofs that the lake was formerly on the same level as the Mediterranean. It is probable, he thinks, that formerly the lake was very salt; and thus a study of the fauna of the lake is full of interest. At least a dozen species of fish were obtained, several of them new forms, which M. Lortet is now investigating. He gives the following list of species which have been determined:—*Clarias macranthus, Capocta damascena, Barbus Beddomii, Chromis Andræ, C. paterfamilias, C. Simonis, C. nilotica, C. nov. sp., C. nov. sp., C. nov. sp.*, (*un genre nouveau indéterminé*), *Labrobarbus canis.* Several new species of molluscs have also been obtained; M. Lortet gives the following list:—*Neritina Jordani*, Butt.; *Melania tuberculata*, Müller; *Melanopsis premorsa*, L.; *M. costata*, Olivier; *Cyrena fluminalis*, Müller; *Unio terminalis*, Bourg.; *U. tigridis*, Bourg.; *U. Lorteti*, Locard; *U. Pietri*, Locard.; *U. Maris Galilæi*, Locard. Melanopsis and Melania are of a marine appearance, and seem to M. Lortet to show the transition between salt and fresh water.

IN Vol. xii. of the *Transactions* of the New Zealand Institute Mr. J. W. Stack has some interesting notes on the colour-sense of the Maori. Mr. Stack asks what stage had the colour-sense of the Maori reached before intercourse with Europeans began? This can readily be ascertained by reference to the terms existing in the language at that date for giving expression to the sense of colour. We find, upon examination, that the language possessed very few words that conveyed to the mind an idea of colour, apart from the object with which the particular colour was associated. There are only three colours for which terms exist, namely, white, black, and red. White, *ma* (sometimes *tea*—very limited application). Black, *pouri, pango, mangu.* Red, *whero, kura, ngangana.* If we analyse these words they seem all to relate to the presence or absence of sunlight. *Ma* is doubtless a contraction for *Marama*, light, which is derived from *Ra*, the sun. *Pouri*, black, is derived from *Po*, night. The derivation of *pango* and *mangu* is not so apparent, but I venture to think that both *whero* and *kura* may be traced to *Ra*. *Ma* is not only the term for whiteness and clearness, but also for all the lighter tints of yellow, grey, and green. Grey hair is called *hina*, but the term was never used to designate anything else but hair; every other grey object was either *ma* or *pango*, as it inclined to a lighter or darker shade. All the words for expressing redness, except *ngangana*, may, Mr. Stack thinks, be traced to *Ra*, and connect the Maori idea of that colour with the brilliant rays of the sun. *Ngangana* is not the word generally used to express the quality of redness, but only certain appearances of it, as in flowers or blood-shot eyes. Yellow and green were recognised, not as abstract conceptions of colour, but only as they are associated with objects. Blue was not formerly recognised, as no word exists to represent it. Anything blue was classed with black, and went under the heading of *pouri*, or *pango*, or *mangu*. The blue depths of ocean and sky were *pouri*, or dark. No words are found in the Maori language to express violet, brown, orange, and pink colours; but there are no less than three words to express pied or speckled objects. *Kopurepure* = reddish speckle; *Kotingotingo* = dark speckle; *tongitongi* = spotted. The limited number of colour-expressions that exist in the Maori language cannot be attributed to the absence of objects presenting those colours for which the terms are wanting. The ornamental scroll-work, and the elaborate patterns employed in tatooing and carving, showed that the Maoris were capable of appreciating the beautiful, both in form and in colouring, and we can only account, Mr. Stack thinks, for their indifference to the more delicate tints of flowers which call forth our admiration by supposing that their colour-sense was not so well educated as our own.

MR. JOHN SCOTT has been appointed Professor of Agriculture and Estate Management to the Royal Agricultural College at Cirencester. Mr. Scott studied agriculture at the University of Edinburgh, and has had many years practical experience in farming, estate management, and land valuing, both at home and in the Colonies. He is the author of two well-known books on farm and estate valuations, and was formerly editor of the *Farm Journal.*

A NEW and revised edition of Bishop Stanley's well-known and deservedly popular "Familiar History of British Birds" has just been published by Messrs. Longmans and Co. It has been revised by "a practical ornithologist of much experience," and has been furnished with numerous additional illustrations.

ANOTHER Lake village, assigned by experts to the age of Bronze, has been discovered at Auvernier, near Neuchâtel. Several millstones quite new, others half made, have been brought to light, from which it is inferred that the place may have been the seat of a manufactory of these articles. Another conclusion drawn from this find is that Swiss pile buildings served as actual dwellings for the primeval inhabitants of the land, and were not, as has been supposed, used merely as storehouses.

MR. DAVID BOGUE will publish in November a new book by Mr. S. Butler, author of "Erewhon," "Life and Habit," &c., entitled "Unconscious Memory." The work will contain translations from the German of Prosper Ewald Hering of Prague, and of von Hartmann, with a comparison between the views of instinctive and unconscious actions taken by these two writers respectively.

THE British Museum is about to be enriched by a collection of natural history specimens made by the officers of Her Majesty's surveying ship *Alert*, which has been for some months engaged in making a complete survey of the Straits of Magellan.

WITH the view of promoting agricultural improvement in Bengal and encouraging the study of scientific agriculture, the Bengal Government has created two annual special scholarships of 200*l*. each, to be held by science graduates of the Calcutta University at Cirencester College.

A TERRIFIC hurricane passed over the Bermudas on August 29 and 30, stated to have exceeded in violence the historical hurricane of 1839.

Two years ago (NATURE, vol. xviii. pp. 104, 344) we directed attention to the discoveries made in Russia in regard to Fermat's asserted prime-form $2^{2^m}+1$. We have now to chronicle the fact that to the number of composite integers of this form another addition has just been made. M. Landry has found that $2^{64}+1$ is divisible by 274177. As at present ascertained therefore the composite members of the form are—

$m = 5$; divisor, $5 \cdot 2^7 + 1$ (Euler),
$m = 6$; „ $1071 \cdot 2^8 + 1$ (Landry),
$m = 12$; „ $7 \cdot 2^{14} + 1$ (Pervouchine),
$m = 23$; „ $5 \cdot 2^{25} + 1$ (Pervouchine).

MM. MARTINET AND LESSON have brought out vol. i. of their work on the origin and migrations of the Polynesians. The next volume is nearly ready, and the remainder will be published in 1881. The aim of this exhaustive work is to demonstrate that the Polynesians are neither Asiatics nor Americans, but Maoris, from the Middle Island.

M. C. DE UFALVY is engaged in editing the narrative of the voyage of M. Panagiotis Potagos in Central Asia; while to M. Henri Duveyrier has been confided, by the Paris Geographical Society, the task of preparing for publication in French that traveller's expedition in Equatorial Africa.

THREE French expeditions are being organised. One, by M. Revoil, to Aden, in the country of the Somalis; another, by M. Moindron, to the northern coasts of New Guinea, which, if practicable, is to advance beyond the points reached by Raffray, Meyer, and Albertis; and the third, by M. Flahant, to the Polar Seas, in conjunction, probably, with Nordenskjöld.

A FRENCH explorer, M. Lecart, who is at present on the banks of the Niger, writes home from "Koundian (Gangaran), July 25," that he has discovered a new vine, which promises to be of great economical value. He says the fruit of the vine is excellent and abundant, its cultivation very easy, its roots tuberose and perennial, while its branches are annual. It can be cultivated as easily as the dahlia. He himself had been eating the large grapes of the vine for eight days, and found them excellent, and he suggests that its culture ought to be attempted in all vine-growing countries as a possible remedy against the phylloxera. He is sending home seeds for experiment, both in France and Algeria, and will bring home specimens of the plant at all stages.

MR. F. J. CAMPBELL of the College for the Blind, Upper Norwood, he himself being blind, gives an interesting account of his successful ascent of Mont Blanc, the first time such a feat was accomplished by a blind man.

THE Report of the Cardiff Naturalists' Society for 1879 has to complain of a considerable falling off in the membership, attributable mainly to bad times. Otherwise the work of the Society has been fairly satisfactory.

AT a recent meeting of the Balloon Society of Great Britain, it was announced that a challenge had been received from M. de Fonvielle, president of the French Académie d'Aërostation, to a balloon contest during the present autumn on English soil. After a discussion it was decided to accept the challenge, the contest to take place between one member of each nationality, and the ascent to be made from the Crystal Palace.

ACCORDING to a table published by the *Statistiche Monatsschrift* of Vienna, the number of volumes in the National Library of Paris is 2,078,000, and in the British Museum only 1,000,000. But it should be noted that the number of volumes does not give an exact idea of the real importance of a library. The Vatican, which is stated to have only 30,000 volumes and 25,000 manuscripts, must be considered as ranking far above its numerical position. According to the provisions of the French law, the deposit is required of each re-impression, even where there is no alteration, and the National Library has not the right of disposing by sale of useless volumes, so that there is an accumulation of popular works of no value at all. There is a room full of Noël and Chapsal's Elements of Grammar, and endless numbers of *Petits Parisiens*. Popular novels are in the same case, and there are more than eighty copies of "Nana."

AN interesting prehistoric sketch of the Spreewald and the Schlossberg of Burg, with special map and illustrations, by Professors Virchow and Schulenburg, has been published by Wiegandt of Berlin.

THE additions to the Zoological Society's Gardens during the past week include a Brown-necked Parrot (*Pœocephalus fusicollis*) from West Africa, presented by Mr. H. Wood; a Jacaraca (*Craspedocephalus brasiliensis*), a —— Tree Snake (*Dryiophis acuminata*), a —— Amphisbœna (*Amphisbœna alba*) from Brazil, presented by Dr. A. Stradling, C.M.Z.S.; a —— Amphisbœna (*Bronia brasiliensis*) from Pernambuco, presented by Mr. W. A. Forbes, F.Z.S.; a Weeper Capuchin (*Cebus capucinus*) from Brazil, a Ring-tailed Coati (*Nasua rufa*), a Spotted Cavy (*Cœlogenys paca*) from South America, a Crab-eating Raccoon (*Procyon cancrivorus*) from West Indies, a Saturnine Mocking-Bird (*Mimus saturninus*), two Silky Hang-nests (*Amblyrhamphus holosericeus*), a Sulphury Tyrant Bird (*Pitangus sulphuratus*) from Monte Video, a Maximilian's Aracari (*Pteroglossus wiedi*) from Pernambuco, deposited; five Ruffs (*Machetes pugnax*), British, purchased; a Reeves's Muntjac (*Cervulus reevesi*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

COLOURS OF SOUTHERN STARS.—In the *Uranometria Argentina*, Dr. Gould has drawn attention to a number of stars presenting marked colour, and to several in which there appears to be change of colour: the following are amongst the more noticeable cases:—

β Hydri is remarkable for its clear yellow light (2·7m.); the Cordoba observations do not support Sir John Herschel's suspicion of variability of brightness. α Indi is also of a bright clear yellow; mag. 3·1. The blue colour of γ Tucanæ is very marked; Gould's magnitude is 4·0. ϕ Eridani (3·5) is remarkable for its blue colour, and ν Puppis (3·5) is decidedly blue; ϵ Pavonis, the estimates of magnitude of which star vary from 3·6 to 4·2, is of a remarkably blue colour. Gould's No. 9 in Dorado, Lacaille

1567, which varies from $5\frac{1}{2}$ to $6\frac{3}{4}$, is excessively red; π^1 Gruis is of a deep crimson, while its neighbour π^2 is conspicuously white; magnitudes respectively 6·7 and 5·9. The star No. 10163 of Oeltzen's Argelander is remarkable for its superb crimson colour; its place for 1875 is in R.A. 9h. 45m. 18s., N.P.D. 112° 25′. μ Muscæ (5·3m.) is intense orange-red.

γ Centauri, varying between 4·5 and 5·1, appears also to vary in colour, having been repeatedly noted as reddish, while at other times it was found without any marked tinge. N Velorum, which has a peculiar yellow colour, contrasting markedly with that of the numerous red stars in its vicinity, is suspected to vary in colour as well as in brightness, the period of the variations appearing to be not far from $4\frac{1}{2}$ days, though the number of observations is not sufficient to give the law of the fluctuations. The positions of these stars for 1875·0 are: γ Centauri, R.A. 13h. 42m. 13s., N.P.D. 123° 49′·5; N Velorum, R.A. 9h. 27m. 25s., N.P.D. 146° 29′·0.

Dr. Gould says that there is "a decidedly greenish tinge to the light of β Libræ, although its colour cannot properly be called conspicuous;" this confirms Smyth's judgment on its tinge—pale emerald.

TELEGRAPHIC DETERMINATIONS OF LONGITUDE.—The Hydrographic Office at Washington has published a number of geographical positions determined in 1878 and 1879 by parties under the direction of Lieut.-Commanders F. M. Green and C. H. Davis of the United States Navy. The longitudes were fixed by telegraphic exchanges of time-signals, the initial point of measurement being the meridian of the Royal Observatory at Greenwich. The latitudes (with the exception of Lisbon, determined by the Director of the Royal Observatory, Capt. F. A. Oom) result from numerous zenith-telescope observations of pairs of stars. The details of the observations are in the press, and will shortly appear; meanwhile we extract from No. 59 of the *Notices* of the Hydrographic Office the positions of the more important points—

Lisbon—Centre of dome of the Royal Observatory.
Lat. 38° 42′ 31″·3 N. ... Long. 0h. 36m. 44·68s. W.
Funchal (Madeira)—Flagstaff of Fort St. Jago.
Lat. 32° 38′ 4″·1 N. ... Long. 1h. 7m. 35·56s. W.
Pernambuco—Lighthouse near Fort Ricão.
Lat. 8° 3′ 22″·2 S. ... Long. 2h. 19m. 27·77s. W.
Bahia—San Antonio Lighthouse.
Lat. 13° 0′ 37″·4 S. ... Long. 2h. 34m. 8·37s. W.
Rio de Janeiro—Centre of dome of Imperial Observatory.
Lat. 22° 54′ 23″·8 S. ... Long. 2h. 52m. 41·41s. W.
Montevideo—Centre of south-east tower of Cathedral.
Lat. 34° 54′ 33″·0 S. ... Long. 3h. 44m. 49·02s. W.
Buenos Ayres—Centre of Cupola of Custom-house.
Lat. 34° 36′ 29″·8 S. ... Long. 3h. 53m. 28·95s. W.

FOUR-FIGURE LOGARITHMS AND ANTI-LOGARITHMS.—Messrs. Layton, Fleet Street, have lately published tables of logarithms of numbers to four places from 1000 to 9999, and anti-logarithms ·0000 to ·9999, arranged by General Hannyngton, similarly to the modern six and seven-figure tables. All the figures are printed, and the value sought is consequently found by mere inspection. The logarithms of numbers 0 to 999 would have required two more pages only, and probably would have been more used than any of the other pages. A complete manual of four-figure logarithms of numbers and trigonometrical functions is much to be desired, and would suffice for eclipses, occultations, star-corrections, and many subordinate astronomical calculations; it is to be regretted that the logarithms of trigonometrical functions are not given in the present publication.

ACTION OF PHOSPHORESCENT LIGHT ON SELENIUM[1]

A FEW weeks ago, when listening to Mr. Heaton's lecture on Balmain's luminous paint at the Society of Arts, it occurred to me to try whether the faint light of phosphorescence would exercise any sensible effect upon the electric conductivity of selenium. I lately made some experiments in this direction, for which I adopted the following arrangement:—One of Dr. Werner Siemens' selenium preparations, of the kind described by me in vol. vi. of the Society of Telegraph Engineers' *Journal*, was placed in the circuit of two Daniell cells, together with a delicate Thomson's reflecting galvanometer. The selenium was put into one end of a blackened brass tube, which was placed inside a dark box provided with a sliding door in front of the open end of the tube. The source of phosphorescence was a *sheet* of glass 20 × 30 centimetres, painted at the back with some phosphorescent material.[1] This *luminous sheet* was placed before the opening of the box, usually at a distance of about 60 centimetres from the selenium plate.

In the first series of experiments the phosphorescence of the *sheet* was excited by exposing it to light from different sources. The results were as follows:—

No. of experiment.	Description of light used for exciting the phosphorescence of the *sheet*.	Increase of conductivity of the selenium by the action of phosphorescent light.
	The sheet was exposed to:—	Per cent.
1	Light reflected from ceiling of semi-dark room for several minutes.	0·7
2	Light reflected from ceiling of moderately light room for fifteen minutes.	2·4
3	Light of the sky at 5 p.m. for several minutes.	4·6
4	Light from a few inches of burning magnesium ribbon.	5·1
5	Sunlight for two minutes exactly.	7·8
6	Sunlight for five minutes exactly.	6·3

During these experiments the phosphorescent light acted on the selenium immediately after the exposure of the sheet to light. After a lapse of some minutes the effect was found considerably lessened; for instance, the third experiment gave only about $\frac{1}{4}$th of the original value five minutes after the exposure. The sheet, exposed to strong light two hours previously, showed no perceptible action on the selenium. The curious fact that the effect is less when the *sheet* is exposed to sunlight for five minutes than when it is exposed for only two minutes, is probably due to the circumstance that the *sheet* becomes sensibly warm during the longer period of exposure.

In the second series of experiments different lengths of magnesium ribbon were burnt in front of the *sheet* and at a distance of about 15 centimetres from it. The intensity of the phosphorescence increased with the time of exposure to light, as the following little table shows:—

Length of magnesium ribbon.	Time the magnesium was burning.	Increase of conductivity of selenium.
Centimetres.	Seconds.	Per cent.
1	2	0·8
3	3	1·8
10	5	2·4
20	12	2·8
30	21	3·4

In the third series of experiments a length of 20 centimetres of magnesium ribbon was burnt in front of the *sheet* at 20 centimetres distance from it. The *sheet* was then, immediately after its exposure to light, placed at distances of 200, 150, 100, and 50 centimetres respectively from the selenium. It was found that the effect upon the selenium varied approximately as the inverse distance of the *sheet* from the selenium plate, or in other words, as the square root of the light intensity. The same relation has been found by Dr. Werner Siemens and others for considerably stronger light intensities.

In the fourth series of experiments the phosphorescent light was made to pass through differently-coloured sheets of glass before acting on the selenium. It was found that colourless glass transmitted all the active rays. Blue glass transmitted $\frac{4}{5}$ths of the total amount; green glass transmitted $\frac{1}{5}$th; red (almost monochromatic); and yellow glass transmitted no perceptible action.

In the fifth series of experiments the action of the *sheet* upon the selenium was compared with that of a spermaceti candle, the phosphorescence of the *sheet* being excited by diffused daylight. Two separate sets of measurements with the standard

[1] This article was sent to us by Dr. Obach in April last.—ED.

[1] A so-called *Aladdin's lamp* from Messrs. Ihlee and Horne, London.

candle at different distances ($2\frac{1}{2}$ and $3\frac{1}{2}$ metres) from the selenium gave tolerably concordant results when calculated on the supposition that the effect upon the selenium varies as the square root of the light intensity. The influence of about 350 square centimetres of the *luminous sheet* on the selenium was found equal to that of 0·0014 standard candle, or 0·04 standard candle per square metre.

In conclusion I wish to remark that the above must be considered only as preliminary experiments, and the figures given as only approximate. I am now engaged in making further experiments on this subject with the endeavour to obtain more accurate results and to extend these researches, as it seems probable that the sensitive selenium plate may render similar services to the study of phosphorescent light as the thermopile has rendered to the study of radiant heat. EUGEN OBACH

AGRICULTURAL CHEMISTRY[1]

II.

IT has been shown that the plant may receive abundance of nitrogen, may produce abundance of chlorophyll, and may be subject to the influence of sufficient light, and yet not assimilate a due amount of carbon. On the other hand, it has been seen that the mineral constituents may be liberally provided, and yet, in the absence of a sufficient supply of nitrogen in an available condition, the deficiency in the assimilation of carbon will be still greater. In fact, assuming all the other necessary conditions to be provided, it was seen that the amount of carbon assimilated depended on the available supply of nitrogen.

In a certain general sense it may be said that the success of the cultivator may be measured by the amount of carbon he succeeds in accumulating in his crops. And as, other conditions being provided, the amount of carbon assimilated depends on the supply of nitrogen in an available form within the reach of the plants, it is obvious that the question of the sources of the nitrogen of vegetation is one of first importance. Are they the same for all descriptions of plants? Are they to be sought entirely in the soil, or entirely in the atmosphere, or partly in the one and partly in the other?

These are questions which Mr. Lawes and myself have discussed so frequently that it might seem some apology was due for recurring to the subject here, especially as I considered it in some of its aspects before this Section at the Sheffield meeting last year. But the subject still remains one of first importance to agriculture, and it could not be omitted from consideration in such a review as I have undertaken to give. Moreover, there are some points connected with it still unsettled, and some still disputed.

It will be remembered that De Saussure's conclusion was that plants did not assimilate the free or uncombined nitrogen of the atmosphere, and that they derived their nitrogen from the compounds of it existing in the atmosphere, and especially in the soil. Liebig, too, concluded that plants do not assimilate nitrogen from the store of it existing in the free or uncombined state, but that ammonia was their main source, and he assumed the amount of it annually coming down in rain to be much more than we now know to be the case.

Referring to our previous papers for full details respecting most of the points in question, I will state, as briefly as I can, the main facts known—first in regard to the amount of the measurable, or as yet measured, annual deposition of combined nitrogen from the atmosphere; and secondly as to the amount of nitrogen annually assimilated over a given area by different crops—so that some judgment may be formed as to whether the measured atmospheric sources are sufficient for the requirements of agricultural production, or whether, or where we must look for other supplies?

First, as to the amount of combined nitrogen coming down as ammonia and nitric acid in the measured aqueous deposits from the atmosphere.

Judging from the results of determinations made many years ago, partly by Mr. Way, and partly by ourselves, in the rain, &c., collected at Rothamsted; from the results of numerous determinations made much more recently by Prof. Frankland in the deposits collected at Rothamsted, and also in rain collected elsewhere; from the results obtained by Boussingault in Alsace; from those of Marié-Davy at the Meteorological Observatory at Montsouris, Paris; and from those of many others made in France and Germany—we concluded, some years ago, that the amount of combined nitrogen annually so coming down from the atmosphere would not exceed 8 or 10 lbs. per acre per annum in the open country in Western Europe. Subsequent records would lead to the conclusion that this estimate is more probably too high than too low. And here it may be mentioned in passing, that numerous determinations of the nitric acid in the drainage water collected from land at Rothamsted, which had been many years unmanured, indicate that there may be a considerable annual loss by the soil in that way; indeed, probably sometimes much more than the amount estimated to be annually available from the measured aqueous deposits from the atmosphere.

It should be observed, however, that the amount of combined nitrogen, especially of ammonia, is very much greater in a given volume of the minor aqueous deposits than it is in rain; and there can be no doubt that there would be more deposited within the pores of a given surface of soil than on an equal area of the non-porous even surface of a rain-gauge. How much, however, might thus be available beyond that determined in the collected and measured aqueous deposits, the existing evidence does not afford the means of estimating with any certainty.

The next point to consider is—What is the amount of nitrogen annually obtained over a given area, in different crops, when they are grown without any supply of it in manure? The field experiments at Rothamsted supply important data relating to this subject.

Thus, over a period of 32 years (up to 1875 inclusive), wheat yielded an average of 20·7 lbs. of nitrogen per acre per annum, without any manure; but the annual yield has declined from an average of more than 25 lbs. over the first 8, to less than 16 lbs. over the last 12, of those 32 years; and the yield (it is true with several bad seasons) has been still less since.

Over a period of 24 years barley yielded 18·3 lbs. of nitrogen per acre per annum, without any manure; with a decline from 22 lbs. over the first twelve, to only 14·6 lbs. over the next 12 years.

With neither wheat nor barley did a complex mineral manure at all materially increase the yield of nitrogen in the crops.

A succession of so-called "root-crops"—common turnips, Swedish turnips, and sugar-beet (with 3 years of barley intervening after the first 8 years)—yielded, with a complex mineral manure, an average of 26·8 lbs. of nitrogen per acre per annum over a period of 31 years. The yield declined from an average of 42 lbs. over the first eight years, to only 13·1 lbs. (in sugar-beet) over the last 5 of the 31 years; but it has risen somewhat during the subsequent 4 years, with a change of crop to mangolds.

With the leguminous crop, beans, there was obtained, over a period of 24 years, 31·3 lbs. of nitrogen per acre per annum without any manure, and 45·5 lbs. with a complex mineral manure, including potass (but without nitrogen). Without manure the yield declined from 48·1 lbs. over the first 12 years to only 14·6 lbs. over the last 12; and with the complex mineral manure it declined from 61·5 lbs. over the first 12, to 29·5 lbs. over the last 12, years of the 24.

Again, an ordinary rotation of crops of turnips, barley, clover, or beans, and wheat, gave, over a period of 28 years, an average of 36·8 lbs. of nitrogen per acre per annum without any manure, and of 45·2 lbs. with superphosphate of lime alone, applied once every four years, that is for the root crop. Both without manure, and with superphosphate of lime alone, there was a considerable decline in the later courses.

A very remarkable instance of nitrogen yield is the following—in which the results obtained when barley succeeds barley that is when one gramineous crop succeeds another, are contrasted with those when a leguminous crop, clover, intervenes between the two cereal crops. Thus, after the growth of six grain crops in succession by artificial manures alone, the field so treated was divided, and, in 1873, on one half barley, and on the other half clover, was grown. The barley yielded 37·3 lbs. of nitrogen per acre, but the three cuttings of clover yielded 151·3 lbs. In the next year, 1874, barley succeeded on both the barley and the clover portions of the field. Where barley had previously been grown, and had yielded 37·3 lbs. of nitrogen per acre, it now yielded 39·1 lbs.; but where the clover had previously been grown, and had yielded 151·3 lbs. of nitrogen, the barley succeeding it gave 69·4 lbs., or 30·3 lbs. more after the removal of 151·3 lbs. in clover, than after the removal of only 37·3 lbs. in barley.

[1] Opening Address in Section B (Chemical Science), at the Swansea meeting of the British Association, by J. H. Gilbert, Ph.D., F.R.S., V.B.C.S., F.L.S., President of the Section. Continued from p. 476.

Nor was this curious result in any way accidental. It is quite consistent with agricultural experience that the growth and removal of a highly nitrogenous leguminous crop should leave the land in high condition for the growth of a gramineous corn crop, which characteristically requires nitrogenous manuring; and the determinations of nitrogen in numerous samples of the soil taken from the two separate portions of the field, after the removal of the barley and the clover respectively, concurred in showing considerably more nitrogen, especially in the first nine inches of depth, in the samples from the portion where the clover had been grown, than in those from the portion whence the barley had been taken. Here then the surface soil at any rate had been considerably enriched in nitrogen by the growth and removal of a very highly nitrogenous crop.

Lastly, clover has now been grown for twenty-seven years in succession, on a small plot of garden ground which had been under ordinary garden cultivation for probably two or three centuries. In the fourth year after the commencement of the experiment, the soil was found to contain, in its upper layers, about four times as much nitrogen as the farm-arable-land surrounding it; and it would doubtless be correspondingly rich in other constituents. It is estimated that an amount of nitrogen has been removed in the clover crops grown, corresponding to an average of not far short of 200*l*. per acre per annum; or about ten times as much as in the cereal crops, and several times as much as in any of the other crops, growing on ordinary arable land; and, although the yield continues to be very large, there has been a marked decline over the second half of the period compared with the first. Of course, calculations of the produce of a few square yards into quantities per acre can only be approximately correct. But there can at any rate be no doubt whatever that the amount of nitrogen annually removed has been very great; and very far beyond what it would be possible to attain on ordinary arable land; where, indeed, we have not succeeded in getting even a moderate growth of clover for more than a very few years in succession.

One other illustration should be given of the amounts of nitrogen removed from a given area of land by different descriptions of crop, namely, of the results obtained when plants of the gramineous, the leguminous, and other families, are growing together, as in the mixed herbage of grass-land.

It is necessary here to remind you that gramineous crops grown separately on arable land, such as wheat, barley, or oats, contain a comparatively small percentage of nitrogen, and assimilate a comparatively small amount of it over a given area. Yet nitrogenous manures have generally a very striking effect in increasing the growth of such crops. The highly nitrogenous leguminous crops (such as beans and clover), on the other hand, yield, as has been seen, very much more nitrogen over a given area, and yet they are by no means characteristically benefited by direct nitrogenous manuring; whilst, as has been shown, their growth is considerably increased, and they yield considerably more nitrogen over a given area under the influence of purely mineral manures, and especially of potass manures. Bearing these facts in mind, the following results, obtained on the mixed herbage of grass land, will be seen to be quite consistent.

A plot of such mixed herbage, left entirely unmanured, gave over twenty years an average of 33 lbs. of nitrogen per acre per annum. Over the same period another plot, which received annually a complex mineral manure, including potass, during the first six years, but excluding it during the last fourteen years, yielded 46·3 lbs. of nitrogen; whilst another, which received the mixed mineral manure, including potass, every year of the twenty, yielded 55·6 lbs. of nitrogen per acre per annum. Without manure there was some decline of yield in the later years; with the partial mineral manuring there was a greater decline; but with the complete mineral manuring throughout the whole period, there was even some increase in the yield of nitrogen in the later years.

Now, the herbage growing without manure comprised about fifty species, representing about twenty natural families; that growing with the limited supply of potass comprised fewer species, but a larger amount of the produce, especially in the earlier years, consisted of leguminous species, and the yield of nitrogen was greater. Lastly, the plot receiving potass every year yielded still more leguminous herbage, and, accordingly, still more nitrogen.

The most striking points brought out by the foregoing illustrations are the following:—

1. Without nitrogenous manure, the gramineous crops annually yielded, for many years in succession, much more nitrogen over a given area than is accounted for by the amount of combined nitrogen annually coming down in the measured aqueous deposits from the atmosphere.

2. The root crops yielded more nitrogen than the cereal crops, and the leguminous crops very much more still.

3. In all cases—whether of cereal crops, root crops, leguminous crops, or a rotation of crops—*the decline in the annual yield of nitrogen, when none was supplied, was very great.*

How are these results to be explained? Whence comes the nitrogen? and especially whence comes the much larger amount taken up by plants of the leguminous and some other families, than by the gramineæ? And lastly, what is the significance of the great decline in the yield of nitrogen in all the crops when none is supplied in the manure?

Many explanations have been offered. It has been assumed that the combined nitrogen annually coming down from the atmosphere is very much larger than we have estimated it, and that it is sufficient for all the requirements of annual growth. It has been supposed that "broad-leaved plants" have the power of taking up nitrogen in some form from the atmosphere, in a degree, or in a manner not possessed by the narrow-leaved gramineæ. It has been argued that, in the last stages of the decomposition of organic matter in the soil, hydrogen is evolved, and that this nascent hydrogen combines with the free nitrogen of the atmosphere, and so forms ammonia. It has been suggested that ozone may be evolved in the oxidation of organic matter in the soil, and that, uniting with free nitrogen, nitric acid would be produced. Lastly, it has by some been concluded that plants assimilate the free nitrogen of the atmosphere, and that some descriptions are able to do this in a greater degree than others.

We have discussed these various points on more than one occasion; and we have given our reasons for concluding that none of the explanations enumerated can be taken as accounting for the facts of growth.

Confining attention here to the question of the assimilation of free nitrogen by plants, it is obvious that, if this were established, most of our difficulties would vanish. This question has been the subject of a great deal of experimental inquiry, from the time that Boussingault entered upon it, about the year 1837, nearly up to the present time. About twenty years ago it was elaborately investigated at Rothamsted. In publishing the results of that inquiry those of others relating to it were fully discussed; and although the recorded evidence is admittedly very conflicting, we then came to the conclusion, and still adhere to it, that the balance of the direct experimental evidence on the point is decidedly against the supposition of the assimilation of free nitrogen by plants. Indeed, the strongest argument we know of in its favour is, that some such explanation is wanted.

Not only is the balance of direct experimental evidence against the assumption that plants assimilate free or uncombined nitrogen, but it seems to us that the balance of existing indirect evidence is also in favour of another explanation of our difficulties.

I have asked what is the significance of the gradual decline of produce of all the different crops when continuously grown without nitrogenous manure? It cannot be that, in growing the same crop year after year on the same land, there is any residue left in the soil that is injurious to the subsequent growth of the same description of crop; for (excepting the beans) more of each description of crop has been grown year after year on the same land than the average yield of the country at large under ordinary rotation, and ordinary treatment—provided only that suitable soil conditions were supplied. Nor can the diminishing produce, and the diminishing yield of nitrogen, be accounted for on the supposition that there was a deficient supply of available mineral constituents in the soil. For, it has been shown that the cereals yielded little more, and declined nearly as much as without manure, when a complex mineral manure was used, such as was proved to be adequate when available nitrogen was also supplied. So far as the root crops are concerned the yield of nitrogen, though it declined very much, was greater at first, and on the average, than in the case of the cereals. As to the leguminosæ, which require so much nitrogen from somewhere, it is to be observed that on ordinary arable land the yield has not been maintained under any conditions of manuring; and the decline was nearly as marked with mineral manures as without any manure. Compared with the growth of the leguminosæ on arable land, the remarkable result with the garden clover would seem clearly to indicate that the question was one of soil, and not of atmospheric supply. And the fact that all the other crops will yield full agricultural results even on ordinary arable land, when proper manures are applied, is surely very strong evidence that it is with them, too, a question of soil, and not of atmospheric supply.

But we have other evidence leading to the same conclusion. Unfortunately we have not reliable samples of the soil of the different experimental fields taken at the commencement of each series of experiments, and subsequently at stated intervals. We have nevertheless, in some cases, evidence sufficient to show whether or not the nitrogen of the soil has suffered diminution by the continuous growth of the crop without nitrogenous manure.

Thus we have determined the nitrogen in the soil of the continuously unmanured wheat plot at several successive periods, and the results prove that a gradual reduction in the nitrogen of the soil is going on; and, so far as we are able to form a judgment on the point, the diminution is approximately equal to the nitrogen taken out in crops; and the amount estimated to be received in the annual rainfall is approximately balanced by the amount lost by the land as nitrates in the drainage water.

In the case of the continuous root-crop soil, on which the decline in the yield of nitrogen in the crop was so marked, the percentage of nitrogen, after the experiment had been continued for twenty-seven years, was found to be lower where no nitrogen had been applied than in any other arable land on the farm which has been examined.

In the case of the experiments on the mixed herbage of grass land, the soil of the plot which, under the influence of a mixed mineral manure, including potass, had yielded such a large amount of leguminous herbage and such a large amount of nitrogen, showed, after twenty years, a considerably lower percentage of nitrogen than that of any other plot in the series.

Lastly, determinations of nitrogen in the garden soil which has yielded so much nitrogen in clover, made in samples collected in the fourth and the twenty-sixth years of the twenty-seven of the experiments, show a very large diminution in the percentage of nitrogen. The diminution, to the depth of 9 inches, only represents approximately three-fourths as much as the amount estimated to be taken out in the clover during the intervening period; and the indication is that there has been a considerable reduction in the lower depths also. It is to be supposed however that there would be loss in other ways than by the crop alone.

I would ask, Have we not in these facts—that full amounts of the different crops can be grown, provided proper soil conditions are supplied; that without nitrogenous manure the yield of nitrogen in the crop rapidly declines; and that, coincidently with this, there is a decline in the percentage of nitrogen in the soil—have we not in these facts cumulative evidence pointing to the soil, rather than to the atmosphere, as the source of the nitrogen of our crops?

In reference to this point I may mention that the ordinary arable soil at Rothamsted may be estimated to contain about 3,000 lbs. of nitrogen per acre in the first nine inches of depth, about 1,700 lbs. in the second nine inches, and about 1,500 lbs. in the third nine inches—or a total of about 6,200 lbs. per acre to the depth of twenty-seven inches.

In this connection it is of interest to state that a sample of Oxford clay obtained in the sub-Wealden exploration boring, at a depth of between 500 and 600 feet (and which was kindly given to me by the President of the Association, Prof. Ramsay, some years ago), showed, on analysis at Rothamsted, approximately the same percentage of nitrogen as the subsoil at Rothamsted taken to the depth of about 4 feet only.

Lastly, in a letter received from Boussingault some years ago, referring to the sources whence the nitrogen of vegetation is derived, he says:—

"From the atmosphere, because it furnishes ammonia in the form of carbonate, nitrates, or nitrites, and various kinds of dust. Theodore de Saussure was the first to demonstrate the presence of ammonia in the air, and consequently in meteoric waters. Liebig exaggerated the influence of this ammonia on vegetation, since he went so far as to deny the utility of the nitrogen which forms a part of farmyard manure. This influence is nevertheless real, and comprised within limits which have quite recently been indicated in the remarkable investigations of M. Schlösing.

"From the soil, which, besides furnishing the crops with mineral alkaline substances, provides them with nitrogen, by ammonia, and by nitrates, which are formed in the soil at the expense of the nitrogenous matters contained in diluvium, which is the basis of vegetable earth; compounds in which nitrogen exists in stable combination, only becoming fertilising by the effect of time. If we take into account their immensity, the deposits of the last geological periods must be considered as an inexhaustible reserve of fertilising agents. Forests, prairies, and some vineyards, have really no other manures than what are furnished by the atmosphere and by the soil. Since the basis of all cultivated land contains materials capable of giving rise to nitrogenous combinations, and to mineral substances, assimilable by plants, it is not necessary to suppose that in a system of cultivation the excess of nitrogen found in the crops is derived from the free nitrogen of the atmosphere. As for the absorption of the gaseous nitrogen of the air by vegetable earth, I am not acquainted with a single irreproachable observation that establishes it; not only does the earth not absorb gaseous nitrogen, but it gives it off, as you have observed in conjunction with Mr. Lawes, as Reiset has shown in the case of dung, as M. Schlösing and I have proved in our researches on nitrification.

"If there is one fact perfectly demonstrated in physiology it is this of the non-assimilation of free nitrogen by plants; and I may add by plants of an inferior order, such as mycoderms and mushrooms (translation)."

If, then, our soils are subject to a continual loss of nitrogen by drainage, probably in many cases more than they receive of combined nitrogen from the atmosphere—if the nitrogen of our crops is derived mainly from the soil, and not from the atmosphere—and if, when due return is not made from without, we are drawing upon what may be termed the store of nitrogen of the soil itself—is there not, in the case of many soils at any rate, as much danger of the exhaustion of their available nitrogen as there has been supposed to be of the exhaustion of their available mineral constituents?

I had hoped to say something more about soils to advance our knowledge respecting which an immense amount of investigation has been devoted of late years, but in regard to which we have yet very much more to learn. I must however now turn to other matters.

(*To be continued.*)

IMPROVED HELIOGRAPH OR SUN SIGNAL[1]

THE author claims to have contrived a heliograph, or sun telegraph, by which the rays of the sun can be directed on any given point with greater ease and certainty than by those at present in use.

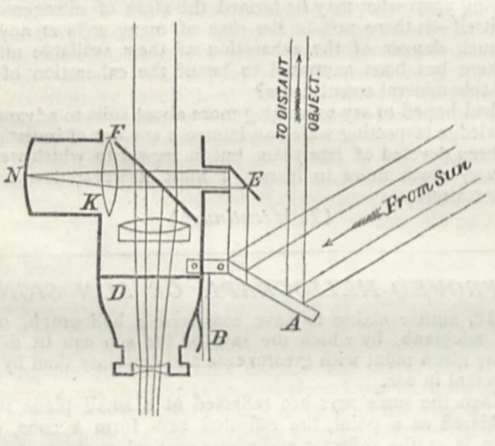
When the sun's rays are reflected at a small plane surface considered as a point, the reflected rays form a cone, whose vertex is at the reflector and whose vertical angle is equal to that subtended by the sun. Adding to the size of the mirror adds other cones of light, whose bounding rays are parallel with those proceeding from other points of the mirror, and only distant from them the same distance as the points on the mirror from which they are reflected. Hence increasing the size of the mirror only adds to the field to which the sun's rays are reflected a diameter equal to the diameter of the mirror, and this at any distance at which the sun signal would be used is quite inappreciable. Adding to the size of the mirror adds to the number of rays sent to each point, and hence to the brightness of the visible flash, but not to the area over which it is visible.

By the author's plan an ordinary field-glass is used to find the position of the object to be signalled to, and to it is attached, in the position of the ordinary sun-shade, a small and light apparatus, so arranged that when the mirror is turned to direct the cone of rays to any object within the field view of the glass, an image of the sun appears in the field, at the same time as the image of the distant object, and magnified to the same degree, and the part of the field covered by this image is *exactly* that part to which the rays are reflected, and at which some part of the sun's disk is visible in the mirror.

A perfectly plane silvered mirror, *A*, takes up the rays of the sun, and when in proper position reflects them parallel with the axis of *D*, which is one barrel of an ordinary field-glass. The greater part of the light passes away to the distant object, but some is taken up by the small silvered mirror, *E*, which is placed at an angle of 45° to the axis of *D*, and reflected at a right angle through the unsilvered plane mirror, *F*, and the convex lens, *K*, by which it is brought to a focus on the white

[1] Paper read at the British Association by Tempest Anderson, M.D., B.Sc.

screen, *H*, which is placed in the principal focus of *K*. The rays from this image diverge in all directions, and some are taken up by the lens *K* and restored to parallelism; some of these are reflected by the unsilvered mirror, *F*, down to the field-glass, *D*, and if this is focussed for parallel rays, as is the case in looking at distant objects, an image of the sun is seen projected on the same field of view as that of the distant object. As the mirrors *E* and *F* are adjusted strictly parallel, the rays proceeding from *F* into the field-glass are parallel and in the opposite direction to those going from the mirror *A* to *E*, which form part of the same pencil as those going to the distant object. Hence the image of the sun seen in the field exactly covers the object to which the sun-flash is visible, and in whatever direction the mirror *A* is moved so as to alter the direction in which rays are reflected to the distant object, and the angle at which part impinge on *E* and are reflected through the lens *K*, the image visible in the glass moves in the same direction. Several attempts to produce this result were made by the use of mirrors and prisms, before the lens *K* was introduced, but they all failed. It was easy to make the image of the sun cover the object when the two occupied the centre of the field of view, but directly the mirror was inclined so as to direct the rays not strictly parallel to the axis of the field-glass, the apparent image diverged generally in the same direction along one co-ordinate, and in the opposite along one at right angles to it, so that nowhere, but in one line across the field, did the image lie in the desired position. The mirrors *E* and *F* are adjusted parallel once for all, by noticing the position on a screen of the small



spot of light reflected from the front of *F* as the light passes from *E* to *K*. The mirrors are moved by the adjusting screws till this spot has, to the bright reflection from the mirror *A*, the same relative position that the centre of mirror *F* has to the mirror *A*.

In actual use the field-glass is first fixed in position pointing to the object, either by holding steadily in the hand, or better by a clamp attached, by which it can be screwed into a tree or post, or fixed in the muzzle of a rifle. The instrument is turned on the barrel of the glass till the sun is in the plane passing through the two axes of the instrument, and the mirror *A* is turned till the bright image of the sun is seen on the screen *H*, through a hole left for the purpose in the side of the tube. On looking through the glass the sun's image is seen, and by then slightly rotating the instrument or moving the mirror, is made to cover the object. The mirror *A* is connected not directly to the body of the instrument, but to a lever *B*, on which it works stiffly, so as to retain any position in which it is placed. Lever *B* works easily and has a limited range of motion, to one end of which it is pressed by a spring; slight pressure with the finger moves it and its attached mirror, so as to throw the light on and off the object in a succession of long and short flashes by which letters and words may be indicated. Flashes may also be given by moving the instrument if held in the hand.

The above instrument answers well for all positions of the sun except when very low behind the observer's back. For this case another mirror is provided by which the light is reflected on to the mirror *A*.

The instrument, which is made by Cook and Sons, York, was exhibited. It was favourably criticised by the president, Prof. W. G. Adams, F. Galton, and others.

SELENIUM AND THE PHOTOPHONE [1]

IN bringing before you some discoveries made by Mr. Sumner Tainter and myself, which have resulted in the construction of apparatus for the production and reproduction of sound by means of light, it is necessary to explain the state of knowledge which formed the starting point of our experiments. I shall first describe the remarkable substance selenium, and the manipulations devised by various experiments; but the final result of our researches has evidenced the class of substances sensitive to light vibrations, until we can propound the fact of sensitiveness being a general property of all matter. We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, German silver, Jenkin's metal, Babbitt's metal, ivory, celluloid, gutta-percha, hard rubber, soft vulcanised rubber, paper, parchment, wood, mica, and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscopic glass. We find that when a vibratory beam of light falls upon these substances they emit sounds—the pitch of which depends upon the frequency of the vibratory change in the light. We find further that, when we control the form or character of the light-vibration on selenium, and probably on the other substances, we control the quality of the sound and obtain all varieties of articulate speech. We can thus, without a conducting wire as in electric telephony, speak from station to station, wherever we can project a beam of light. We have not had opportunity of testing the limit to which this photophonic influence can be extended, but we have spoken to and from points 213 metres apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available. I shall now speak of selenium.

In the year 1817 Berzelius and Gottlieb Gahn made an examination of the method of preparing sulphuric acid in use at Gripsholm. During the course of this examination, they observed in the acid a sediment of a partly reddish, partly clear brown colour, which, under the action of the blowpipe, gave out a peculiar odour, like that attributed by Klaproth to tellurium. As tellurium was a substance of extreme rarity, Berzelius attempted its production from this deposit; but he was unable, after many experiments, to obtain further indications of its presence. He found plentiful signs of sulphur mixed with mercury, copper, zinc, iron, arsenic, and lead, but no trace of tellurium. It was not in the nature of Berzelius to be disheartened by this result. In science every failure advances the boundary of knowledge as well as every success, and Berzelius felt that, if the characteristic odour that had been observed did not proceed from tellurium, it might possibly indicate the presence of some substance then unknown to the chemist. Urged on by this hope he returned with renewed ardour to his work. He collected a great quantity of the material, and submitted the whole mass to various chemical processes. He succeeded in separating successively the sulphur, the mercury, the copper, the tin, and the other known substances whose presence had been indicated by his tests; and, after all these had been eliminated, there still remained a residue which proved upon examination to be what he had been in search of—a new elementary substance. The chemical properties of this new element were found to resemble those of tellurium in so remarkable a degree, that Berzelius gave to the substance the name of "Selenium," from the Greek word *selene*, the moon ("tellurium," as is well known, being derived from *tellus*, the earth).

Although tellurium and selenium are alike in many respects, they differ in their electrical properties, tellurium being a good conductor of electricity, and selenium, as Berzelius showed, a non-conductor. Knox discovered, in 1837, that selenium became a conductor when fused; and Hittorff, in 1852, showed that it conducted, at ordinary temperatures, when in one of its allotropic forms. When selenium is rapidly cooled from a fused condition it is a non-conductor. In this its vitreous form it is of a dark brown colour, almost black by reflected light, having an exceedingly brilliant surface. In thin films it is transparent, and appears of a beautiful ruby red by transmitted light. When selenium is cooled from a fused condition with extreme slowness it presents an entirely different appearance, being of a dull lead

[1] Lecture delivered at the Boston meeting of the American Association by Prof. A. Graham Bell.

colour, and having throughout a granulated or crystalline structure, and looking like a metal. In this form it is perfectly opaque to light, even in very thin films. This variety of selenium has long been known as "granular" or "crystalline" selenium, or, as Regnault called it, "metallic" selenium. It was selenium of this kind that Hittorff found to be a conductor of electricity at ordinary temperatures. He also found that its resistance to the passage of an electrical current diminished continuously by heating up to the point of fusion, and that the resistance suddenly increased in passing from the solid to the liquid condition. It was early discovered that exposure to sunlight hastens the change of selenium from one allotropic form to another; and this observation is significant in the light of recent discoveries.

Although selenium has been known for the last sixty years, it has not yet been utilised to any extent in the arts, and it is still considered simply as a chemical curiosity. It is usually supplied in the form of cylindrical bars. These bars are sometimes found to be in the metallic condition; but more usually they are in the vitreous or non-conducting form. It occurred to Willoughby Smith that, on account of the high resistance of crystalline selenium, it might be usefully employed at the shore end of a submarine cable, in his system of testing and signalling during the process of submersion. Upon experiment, the selenium was found to have all the resistance required—some of the bars employed measuring as much as 1,400 megohms—a resistance equivalent to that which would be offered by a telegraph wire long enough to reach from the earth to the sun! But the resistance was found to be extremely variable. Experiments were made to ascertain the cause of this variability. Mr. May, Mr. Willoughby Smith's assistant, discovered that the resistance was less when the selenium was exposed to light than when it was in the dark.

In order to be certain that temperature had nothing to do with the effect, the selenium was placed in a vessel of water, so that the light had to pass through from 1 in. to 2 in. of water in order to reach the selenium. The approach of a lighted candle was found to be sufficient to cause a marked deflection of the needle of the galvanometer connected with the selenium, and the lighting of a piece of magnesium wire caused the selenium to measure less than half the resistance it did the moment before.

These results were naturally at first received by scientific men with some incredulity, but they were verified by Sale, Draper, Moss, and others. When selenium is exposed to the action of the solar spectrum, the maximum effect is produced, according to Sale, just outside the red end of the spectrum, in a point nearly coincident with the maximum of the heat rays; but, according to Adams, the maximum effect is produced in the greenish-yellow or most luminous part of the spectrum. Lord Rosse exposed selenium to the action of non-luminous radiations from hot bodies, but could produce no effect; whereas a thermopile under similar circumstances gave abundant indications of a current. He also cut off the heat rays from luminous bodies by the interposition of liquid solutions, such as alum, between the selenium and the source of light, without affecting the power of the light to reduce the resistance of the selenium; whereas the interposition of these same substances almost completely neutralized the effect upon the thermopile. Adams found that selenium was sensitive to the cold light of the moon, and Werner Siemens discovered that, in certain extremely sensitive varieties of selenium, heat and light produced opposite effects. In Siemens' experiments, special arrangements were made for the purpose of reducing the resistance of the selenium employed. Two fine platinum wires were coiled together in the shape of a double flat spiral in the zig-zag shape, and were laid upon a plate of mica so that the discs did not touch one another. A drop of melted selenium was then placed upon the platinum wire arrangement, and a second sheet of mica was pressed upon the selenium, so as to cause it to spread out and fill the spaces between the wires. Each cell was about the size of a silver dime. The selenium cells were then placed in a paraffine bath, and exposed for some hours to a temperature of 210 deg. C., after which they were allowed to cool with extreme slowness. The results obtained with these cells were very extraordinary; in some cases the resistance of the cells, when exposed to light, was only one-fifteenth of their resistance in the dark.

Without dwelling farther upon the researches of others, I may say that the chief information concerning the effect of light upon the conductivity of selenium will be found under the names of Willoughby Smith, Lieutenant Sale, Draper and Moss, Professor W. G. Adams, Lord Rosse, Day, Sabini, Dr. Werner Siemens, and Dr. C. W. Siemens. All observations by these various authors had been made by means of galvanometers; but it occurred to me that the telephone, from its extreme sensitiveness to electrical influences, might be substituted with advantage. Upon consideration of the subject, however, I saw that the experiments could not be conducted in the ordinary way for the following reason:—The law of audibility of the telephone is precisely analogous to the law of electric induction. No effect is produced during the passage of a continuous and steady current. It is only at the moment of change from a stronger to a weaker state, or *vice versâ*, that any audible effect is proposed, and the amount of effect is exactly proportional to the amount of variation in the current. It was, therefore, evident that the telephone could only respond to the effect produced in selenium at the moment of change from light to darkness, or *vice versâ*, and that it would be advisable to intermit the light with great rapidity, so as to produce a succession of changes in the conductivity of the selenium, corresponding in frequency to musical vibrations within the limits of the sense of hearing. For I had often noticed that currents of electricity, so feeble as to produce scarcely any audible effects from a telephone when the circuit was simply opened or closed, caused very perceptible musical sounds when the circuit was rapidly interrupted, and that the higher the pitch of sound the more audible was the effect. I was much struck by the idea of producing sound by the action of light in this way. Upon farther consideration it appeared to me that all the audible effects obtained from varieties of electricity could also be produced by variations of light acting upon selenium. I saw that the effect could be produced at the extreme distance at which selenium would respond to the action of a luminous body, but that this distance could be indefinitely increased by the use of a parallel beam of light, so that we could telephone from one place to another without the necessity of a conducting wire between the transmitter and receiver. It was evidently necessary, in order to reduce this idea to practice, to devise an apparatus to be operated by the voice of a speaker, by which variations could be produced in a parallel beam of light, corresponding to the variations in the air produced by the voice.

I proposed to pass light through a large number of small orifices, which might be of any convenient shape, but were preferably in the form of slits. Two similarly perforated plates were to be employed. One was to be fixed and the other attached to the centre of a diaphragm actuated by the voice, so that the vibration of the diaphragm would cause the movable plate to slide to and fro over the surface of the fixed plate, thus alternately enlarging and contracting the free orifices for the passage of light. In this way the voice of a speaker could control the amount of light passed through the perforated plates without completely obstructing its passage. This apparatus was to be placed in the path of a parallel beam of light, and the undulatory beam emerging from the apparatus could be received at some distant place upon a lens, or other apparatus, by means of which it could be condensed upon a sensitive piece of selenium placed in a local circuit with a telephone and galvanic battery. The variations in the light produced by the voice of the speaker should cause corresponding variations in the electrical resistance of the selenium employed; and the telephone in circuit with it should reproduce audibly the tones and articulations of the speaker's voice. I obtained some selenium for the purpose of producing the apparatus shown, but found that its resistance was almost infinitely greater than that of any telephone that had been constructed, and I was unable to obtain any audible effects by the action of light. I believed, however, that the obstacle could be overcome by devising mechanical arrangements for reducing the resistance of the selenium, and by constructing special telephones for the purpose. I felt so much confidence in this that, in a lecture delivered before the Royal Institute of Great Britain, upon May 17, 1878, I announced the possibility of hearing a shadow by interrupting the action of light upon selenium. A few days afterwards my ideas upon this subject received a fresh impetus by the announcement made by Mr. Willoughby Smith before the Society of Telegraph Engineers that he had heard the action of a ray of light falling upon a bar of crystalline selenium, by listening to a telephone in circuit with it.

It is not unlikely that the publicity given to the speaking telephone during the last few years may have suggested to many minds in different parts of the world somewhat similar ideas to my own.

Although the idea of producing and reproducing sound by the action of light, as described above, was an entirely original and independent conception of my own, I recognise the fact that the knowledge necessary for its conception has been disseminated throughout the civilised world, and that the idea may therefore have occurred to many other minds. *The fundamental idea, on which rests the possibility of producing speech by the action of light, is the conception of what may be termed an undulatory beam of light in contradistinction to a merely intermittent one.* By an undulatory beam of light, I mean a beam that shines continuously upon the selenium receiver, but the intensity of which upon that receiver is subject to rapid changes, corresponding to the changes in the vibratory movement of a particle of air during the transmission of a sound of definite quality through the atmosphere. The curve that would graphically represent the changes of light would be similar in shape to that representing the movement of the air. I do not know whether this conception had been clearly realised by "J. F. W.," of Kew, or by Mr. Sargent, of Philadelphia; but to Mr. David Brown, of London, is undoubtedly due the honour of having distinctly and independently formulated the conception, and of having devised apparatus—though of a crude nature—for carrying it into execution. It is greatly due to the genius and perseverance of my friend, Mr. Summer Tainter, of Watertown, Mass., that the problem of producing and reproducing sound by the agency of light has at last been successfully solved. The first point to which we devoted our attention was the reduction of the resistance of crystalline selenium within manageable limits. The resistance of selenium cells employed by former experimenters was measured in millions of ohms, and we do not know of any record of a selenium cell measuring less than 250,000 ohms in the dark. *We have succeeded in producing sensitive selenium cells measuring only* 300 *ohms in the dark, and* 155 *ohms in the light.* All former experimenters seem to have used platinum for the conducting part of their selenium cells, excepting Werner Siemens, who found that iron and copper might be employed. We have also discovered that brass, although chemically acted upon by selenium, forms an excellent and convenient material; indeed, we are inclined to believe that the chemical action between the brass and selenium has contributed to the low resistance of our cells by forming an intimate bond of union between the selenium and brass. We have observed that melted selenium behaves to the other substances as water to a greasy surface, and we are inclined to think that when selenium is used in connection with metals not chemically acted upon by it, the points of contact between selenium and the metal offer a considerable amount of resistance to the passage of a galvanic current. By using brass we have been enabled to construct a large number of selenium cells of different forms. The mode of applying the selenium is as follows:—The cell is heated, and, when hot enough, a stick of selenium is rubbed over the surface. In order to acquire conductivity and sensitiveness, the selenium must next undergo a process of annealing.

We simply heat the selenium over a gas stove and observe its appearance. When the selenium attains a certain temperature, the beautiful reflecting surface becomes dimmed. A cloudiness gradually extends over it, somewhat like the film of moisture produced by breathing upon a mirror. This appearance gradually increases, and the whole surface is soon seen to be in the metallic, granular, or crystalline condition. The cell may then be taken off the stove, and cooled in any suitable way. When the heating process is carried too far, the crystalline selenium is seen to melt. Our best results have been obtained by heating the selenium until it crystallises, and continuing the heating until signs of melting appear, when the gas is immediately put out. The portions that had melted instantly recrystallise, and the selenium is found upon cooling to be a conductor, and to be sensitive to light. The whole operation occupies only a few minutes. This method has not only the advantage of being expeditious, but it proves that many of the accepted theories on this subject are fallacious. Our new method shows that fusion is unnecessary, that conductivity and sensitiveness can be produced without long heating and slow cooling; and that crystallisation takes place during the heating process. We have found that on removing the source of heat immediately on the appearance of the cloudiness, distinct and separate crystals can be observed under the microscope, which appear like leaden snow-flakes on a ground of ruby red. Upon removing the heat, when crystallisation is further advanced, we perceive under the microscope masses of these crystals arranged like basaltic columns standing detached from one another, and at a still higher point of heating the distinct columns are no longer traceable, but the whole mass resembles metallic pudding-stone, with here and there a separate snow-flake, like a fossil, on the surface. Selenium crystals formed during slow cooling after fusion present an entirely different appearance, showing distinct facets.

We have devised, about fifty forms of apparatus for varying a beam of light in the manner required, but only a few typical varieties need be shown. The source of light may be controlled, or a steady beam may be modified at any point in its path. The beam may be controlled in many ways. For instance, it may be polarised, and then affected by electrical or magnetic influences in the manner discovered by Faraday and Dr. Kerr. The beam of polarised light, instead of being passed through a liquid, may be reflected from the polished pole of an electro-magnet. Another method of affecting a beam of light is to pass it through a lens of variable focus. I observe that a lens of this kind has been invented in France by Dr. Cusco, and is fully described in a recent paper in *La Nature;* but Mr. Tainter and I have used such a lens in our experiments for months past. The best and simplest form of apparatus for producing the effect remains to be described. This consists of a plane mirror of flexible material—such as silvered mica or microscope glass. Against the back of this mirror the speaker's voice is directed. The light reflected from this mirror is thus thrown into vibrations corresponding to those of the diaphragm itself.

In arranging the apparatus for the purpose of reproducing sound at a distance any powerful source of light may be used, but we have experimented chiefly with sunlight. For this purpose a large beam is concentrated by means of a lens upon the diaphragm mirror, and, after reflection, is again rendered parallel by means of another lens. The beam is received at a distant station upon a parabolic reflector, in the focus of which is placed a sensitive selenium cell, connected in a local circuit with a battery and telephone. A large number of trials of this apparatus have been made with the transmitting and receiving instruments so far apart that sounds could not be heard directly through the air. In illustration I shall describe one of the most recent of these experiments. Mr. Tainter operated the transmitting instrument, which was placed on the top of the Franklin schoolhouse in Washington, and the sensitive receiver was arranged in one of the windows of my laboratory, 1325 L street, at a distance of 213 metres. Upon placing the telephone to my ear I heard distinctly from the illuminated receiver the words: "Mr. Bell, if you hear what I say come to the window and wave your hat." In laboratory experiments the transmitting and receiving instruments are necessarily within earshot of one another, and we have, therefore, been accustomed to pooling the electric circuit connected with the selenium receiver, so as to place the telephones in another room. By such experiments we have found that articulate speech can be reproduced by the oxyhydrogen light, and even by the light of a kerosene lamp. The loudest effects obtained from light are produced by rapidly interrupting the beam by the perforated disk. The great advantage of this form of apparatus for experimental work is the noiselessness of its rotation, admitting the close approach of the receiver without interfering with the audibility of the effect heard from the latter; for it will be understood that musical tones are emitted from the receiver when no sound is made at the transmitter. A silent motion thus produces a sound. In this way musical tones have been heard even from the light of a candle. When distant effects are sought another apparatus is used. By placing an opaque screen near the rotating disk the beam can be entirely cut off by a slight motion of the hand, and musical signals, like the dots and dashes of the Morse telegraph code, can thus be produced at the distant receiving station.

We have made experiments, with the object of ascertaining the nature of the rays that affect selenium. For this purpose we have placed in the path of an intermittent beam various absorbing substances. Prof. Cross has been kind enough to give me his assistance in conducting these experiments. When a solution of alum or bisulphide of carbon, is employed, the loudness of the sound produced by the intermittent beam is very slightly diminished; but a solution of iodine in bisulphide of carbon cuts off most, but not all, of the audible effect. Even an apparently opaque sheet of hard rubber does not entirely do this. When the sheet of hard rubber was held near the disk interrupter the rotation of the disk interrupted what was then an invisible beam, which passed over a space of about twelve feet before it reached the lens which finally concentrated it upon the selenium cell. A faint but perfectly perceptible musical tone was heard from the telephone connected with the selenium. This could be interrupted at will by placing the hand in the path of the invisible beam. It would be premature, without further experiments, to speculate too much concerning the nature of these invisible rays; but it is difficult to believe that they can be bent rays, as the effect is produced through two sheets of hard rubber containing between them a saturated solution of alum. Although effects are produced as above shown by forms of radiant energy which are invisible, we have named the apparatus for the production and reproduction of sound in this way "the photophone," because an ordinary beam of light contains the rays which are operative.

It is a well-known fact that the molecular disturbance produced in a mass of iron by the magnetising influence of intermittent electrical current can be observed as sound by placing the ear in close contact with the iron. It occurred to us that the molecular disturbance produced in crystalline selenium by the action of an intermittent beam of light should be audible in a similar manner without the aid of a telephone or battery. Many experiments were made to verify this theory without definite results. The anomalous behaviour of the hard rubber screen suggested the thought of listening to it also. This experiment was tried with extraordinary success. I held the sheet in close contact with my ear, while a beam of intermittent light was focussed upon it by a lens. A distinct musical note was immediately heard. We found the effect intensified by arranging the sheet of hard rubber as a diaphragm, and listening through a hearing-tube. We then tried the crystalline selenium in the form of a thin disk, and obtained a similar, but less intense effect. The other substances which I enumerated at the beginning of my address were now successively tried in the form of thin disks, and sounds were obtained from all but carbon and thin glass. We found hard rubber to produce a louder sound than any other substance we tried, excepting antimony, and paper and mica to produce the weakest sounds. On the whole we feel warranted in announcing as our conclusion that sounds can be produced by the action of a variable light from substances of all kinds, when in the form of thin diaphragms. We have heard from interrupted sunlight very perceptible musical tones through tubes of ordinary vulcanised rubber, of brass, and of wood. These were all the materials at hand in tubular form, and we have had no opportunity since of extending these observations to other substances.

I am extremely glad that I have the opportunity of making the first publication of these researches before a scientific society, for it is from scientific men that my work of the last six years has received its earliest and kindest recognition. I gratefully remember the encouragement which I received from the late Prof. Henry at a time when the speaking telephone existed only in theory. Indeed, it is greatly due to the stimulus of his appreciation that the telephone became an accomplished fact. I cannot state too highly also the advantage I received in preliminary experiments on sound vibrations in this building from Prof. Cross, and near here from my valued friend Dr. Clarence J. Blake. When the public were incredulous of the possibility of electrical speech, the American Academy of Arts and Sciences, the Philosophical Society of Washington, and the Essex Institute of Salem, recognised the reality of the results and honoured me by their congratulations. The public interest, I think, was first awakened by the judgment of the very eminent scientific men before whom the telephone was exhibited in Philadelphia, and by the address of Sir William Thomson before the British Association for the Advancement of Science.

At a later period, when even practical telegraphists considered the telephone as a mere scientific toy, Prof. John Peirce, Prof. Eli W. Blake, Dr. Channing, Mr. Clarke, and Mr. Jones, of Providence, Rhode Island, devoted themselves to a series of experiments for the purpose of assisting me in making the telephone of practical utility; and they communicated to me from time to time the result of their experiments with a kindness and generosity I can never forget. It is not only pleasant to remember these things, and to speak of them, but it is a duty to repeat them, as they give a practical refutation to the often repeated stories of the blindness of scientific men to unaccredited novelties, and of their jealousy of unknown inventors who dare to enter the charmed circle of science. I trust that the scientific favour which was so readily accorded to the telephone may be extended by you to this new claimant—the photophone.

SCIENTIFIC SERIALS

THE *Quarterly Journal of Microscopical Science*, July, contains—F. M. Balfour, on the structure and homologies of the germinal layers of the embryo (with woodcuts).—On Hubrecht's researches on the nervous system of nemertines (with a plate) abstract of.—A. G. Bourne, on the structure of the nephridia of the medicinal leech (with two plates).—Prof. Ray Lankester, on intra-epithelial capillaries in the integument of the medicinal leech (with a plate); and on the connective and vasifactive tissues of the same (with two plates).—Dr. H. Gibbes, on the use of the Wenham binocular with high powers.—On the structure of the spermatozoon.—P. H. Carpenter, on some disputed points in Echinoderm morphology.—Prof. Pouchet, on the origin of the red-blood corpuscles (translated from the *Revue Scientifique*).—Prof. Ray Lankester, on *Limnocodium sowerbii*, a new trachomedusa inhabiting fresh water (with woodcuts and two plates) [for an abstract *vide* NATURE, vol. xxii. p. 147].—Notes and memoranda.—*Proceedings* of the Dublin Microscopical Club for November and December, 1879.

Revue d'Anthropologie, tome iii. fascic. 3 (July).—Prof. J. Delbos, of Nancy, gives a brief report of the discovery, made in 1869, of a number of human skeletons in the loam beds of Bollwiller (Haut-Rhin). His paper, which describes the general geognostic character of the soil in which these remains were found, is followed by a detailed description, by Dr. René Collignow, of each of the seven distinct skeletons that have been recovered. Of these, five were adult males, two females, and one a child of about seven. In general characteristics they resemble the Cannstatt remains.—Dr. Bérenger-Féraud, whose position in Senegal as Médecin-en-chef de la Marine gave him favourable opportunities of studying the habits of the natives, has drawn up an interesting report of all that is known on the spot in regard to the mysterious sect of the Simos, which exercises an important influence on the tribes of the west coasts of Africa, from Cape Vert as far as the Gabon settlements on the equator. The Simo of these regions is the dreaded Mombo-Yombo of other races.—Dr. Gustave Lagneau's paper, "De quelques Dates reculées," is a scholarly dissertation on the community of race traceable in the Belgæ, Galli, and Germani, and on the evidence supplied in reference to the period of their immigration into Keltic lands by the introduction of a dolichocephalic character, in addition to the purely brachycephalic type observable in the skulls of Keltic and Kimmerian races. In discussing the question of the occupation of Western Europe by Iberians, M. Lagneau enters at length into the historical and anthropological grounds for accepting the testimony of Plato and others as to the defeat of those tribes by a powerful race, the Atlantæ, and the existence of a great western continent, or archipelago, the submerged Atlantis, from which the latter peoples made their inroads on West Africa and West Europe.—M. Martinet's enumeration of the prehistoric monuments of Berry deserves special notice for the interesting information it supplies in reference to the so-called "Mardelles," a kind of conically shaped excavations, the purport of which has not been determined, and which, although found elsewhere, as in Normandy, Provence, &c., is of exceptional frequency in Berry, where between 300 and 400 have been explored. In diameter they vary from 20 to 100 metres, in depth from 50 centimetres to 8 metres. Traces of ashes, calcined animal bones, and coarse potsherds, with a few broken flints, have been found at the bottom of these depressions, of which several are generally ranged in a line near natural or artifically constructed caverns.

Journal de Physique, August.—Experimental researches on rotatory polarisation in gases, by M. H. Becquerel.—Magnetic rotatory power of liquids and of their vapours, by M. Bichat.—Experiments on flames, by M. Neyreneuf.

Journal of the Franklin Institute, August.—The limitations of the steam-engine, by W. D. Marks.—Economical cut-off in steam-engines, by S. W. Robinson.—The involute of the circumference of a circle, by J. J. Skinner.—Holman's new compressorium and moist chamber, by J. A. Ryder.

Rivista Scientifico-Industriale, No. 15, August 15.—Periodic spontaneous movement of the stamens of *Ruta bracteosa*, D.C., and of *Smyrnum rotundifolium*, by Dr. Macchiati.—Synthesis of meteorological observations in Modica and Syracuse on the fall of meteoric powders, from the end of 1876 to April 16, 1880, by Prof. Lancetta.

No. 16, August 31.—On types of rocks, by Prof. de Stefani.

—New apparatus for the electric light.—Parallelogram of forces, by Prof. Lancetta.—Further contributions to the Aphides of Sardinia; description of three new species, by Prof. Macchiati.

Atti della R. Accademia dei Lincei, June.—On an apparatus for determining the mechanical equivalent of heat, by Dr. Bartoli.—On the laws of galvanic polarisation, by the same.—On a human skeleton of the age of stone in the Roman province, by Dr. Incoronato.—Liassic limestone of Gozzano, and its fossils, by Dr. Parona.—Works on the Tiber, and varied conditions of the Roman land, by S. Ponzi.—Reply to S. Ferrari's observations (relating to anomalous induction of a magnetic declinometer), by Prof. Keller.—On the mechanism of movements of the iris, by S. Morizzia.—On some derivatives of natural and synthetic thymol, by Professors Paterno and Canzonesi.—Analysis of an augite of Lazio, by Dr. Piccini.—Chemical researches on the lava of Montecompatri, &c., by Dr. Mauro.—On the hæmatopoetic function, by SS. Tizzoni and Fileti.—On the diffusion of the metals of cerite, by S. Cossa.—On tungstate of didymium, by the same.— On a proposition of Jacobi, by S. Siacci.—On a class of differential equations integrable by elliptic functions, by S. Brioschi.—Verification and use of a new formula for calculation of planetary perturbations, by S. De Gasparis.

Rendiconto delle Sessioni dell'Accademia delle Scienze dell' Istituto di Bologna, 1879-80.—We note here the following:—On the placenta of cartilaginous fishes and mammalia, and its applications in zoological taxonomy and anthropogeny, by Prof. Ercolani.—Variations of human temperature resulting from bodily movements, by Prof. Villari.—Dimensions of the electric spark of condensers, by the same.—On variation of length due to magnetism, by Prof. Righi.—On some products of decomposition of albumen at the temperature of the human body, and at slightly lower temperatures, by Prof. Selmi.—On the singular verticillate configuration of laminæ of crystalline snow, &c., by Prof. Bombicci.—On a case of permanent polarity in a magnet opposite to that of the inducing helix, by Prof. Righi.—Laws relative to the dimensions of electric sparks of condensers, by Prof. Villari.—Investigation of phosphorus in the urine in cases of poisoning, and products which may occur, by Prof. Selmi.—A mercury pneumatic machine with double action, by S. Liuzzi.—Verification of ptomaines in most cases of chemico-legal investigation, and formation of some of them, of poisonous nature, in animal substances kept three years in spirits, by Prof. Gianetti.—On the principal changes in the course of the Po, and means of obviating disaster threatened by it, by Dr. Predieri.—On the intimate structure of the eyes of Diptera, and on the eyes of blind Talpa, by Prof. Ciaccio. (This *Rendiconto* contains a considerable number of papers relating to anatomy and local geology.)

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xiii., fasc. xv., July 15.—Outlines of a Government sanitary organisation, by Dr. Zucchi.—On the theory of hallucinations, by Prof. Tamburini.—Triassic fossils of the African Alps, by S. De Stefani.—The learned friends of Alexander Volta, by S. Z. Volta.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, September 1.—H. T. Stainton, F.R.S., vice-president, in the chair.—Miss Emily A. Smith, Assistant State Entomologist of Illinois, was elected a Foreign Member.—Mr. J. Jenner Weir exhibited specimens of *Odonestis potatoria* and *Smerinthus populi*, which possessed the peculiarities of both sexes.—Sir Sidney Saunders exhibited six winged examples of the Stylopideous genus *Hylecthrus*, and also various other Hymenoptera, and contributed remarks thereon.—Miss E. A. Ormerod exhibited some galls found on *Tanacetum vulgaris*, which she described at length.—Mr. T. R. Billups exhibited a female specimen of *Polyblastus whalbergi*, an ichneumon not previously recorded in Britain.—Mr. E. Boscher exhibited living specimens of the two varieties of the larvæ of *Smerinthus ocellatus*, and contributed a note thereon.—Mr. Meldola exhibited some specimens of *Camptogramma bilineata*, a large number of which had been found by Mr. English near Epping, attached firmly to the leaves of the "tea tree" (*Lycium barbarum*) by the abdomen, in which position they had died, possibly from the effects of a fungoid disease.—Mr. A. H. Swinton communicated a note on *Luciola italica*, an Italian fire-fly.

PARIS

Academy of Sciences, September 13.—M. Edm. Becquerel in the chair.—The following papers were read:—Observations of Faye's comet and of comet *b* 1880 (Schäberle) at Paris Observatory with the equatorial of the western tower, by M. Bigourdan.—On the probable orbital motion of some binary systems of the southern heavens, by M. Cruls. This is from the Imperial Observatory at Rio; and the author's data are compared with those of Sir J. Herschel at the Cape, and Capt. Jacob at Poonah.—Spectroscopic researches on some stars not hitherto studied, by M. Cruls. This relates to stars in the Bee, the Cross, and the Centaur.—On some solar phenomena observed at Nice, by M. Thollon. He gives several sketches of the spectral phenomena of protuberances, &c. He does not hesitate to say that every movement of the solar surface having, along the line of observation, a component which is not *nil*, causes a displacement of the spectral lines. It is also extremely probable, but not certain, that every displacement of a line corresponds to a movement.—On the law of electromagnetic machines (continued), by M. Joubert. With a given intensity of field, whatever the other conditions in which the machine works, from the moment when it gives maximum work, the retardation is equal to $\frac{1}{8}$ of the entire period; the intensity is constant and equal to the quotient by $\sqrt{2}$ of the absolute maximum of intensity; the electromagnetic work is proportional to the velocity; and the velocity is in a constant ratio to the resistance.—On boroduodecitungstic acid and its salts of potassium, by M. Klein.—On the subcutaneous lymphatics of the python of Séba, by M. Jourdain. The arrangement presents an evident similarity to that in Teleosteans (a ventral trunk and two lateral ones, &c.). When the direction of circulation of lymph has been ascertained, it will probably be found the same in both.—Deep dredging in the Lake of Tiberias (Syria), in May, 1880, by M. Lortet. The surface of the lake is 212 m. under that of the Mediterranean, but probably was at one time level with it; the greatest depth met with was about 250 m. at the northern extremity. It was thought that the waters, formerly saline, had probably contained special animal forms, traces of which might still be found at great depths. Some twelve species of fishes were met with, and some new forms. *Chromis* preponderated; indeed, they swarm in the lake. Twelve forms of mollusca were met with, some new species. The *Melanopsis* and *Melania* were of marine character. At the borders of the lake were some shrimps, crabs, and tortoises. Diatoms, foraminifera, &c., were obtained in the fine slime of the bottom, but no algæ or coniferæ were met with (the water indeed was brackish, and had a temperature of $+ 24°$; that at the bottom was not more brackish than that at the surface).—On the existence in Soudan of wild vines with herbaceous stem, vigorous roots, and eatable fruit, by M. Lécard.—On a thunderstorm observed at Laigle (Orne) on August 6, 1880, by M. Royer. During an hour and a half he counted 4,700 flashes, or about 53 a minute. Sometimes there were 100 a minute. The storm lasted two hours in all. The lightning struck twice, viz., a house and a poplar-tree.

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