THURSDAY, APRIL 18, 1895.

THE EXPERIMENTAL PHYSIOLOGY OF PLANTS.

Practical Physiology of Plants. By Francis Darwin, M.A., F.R.S., and E. Hamilton Acton, M.A. Cambridge Natural Science Manuals, Biological Series. (Cambridge: University Press, 1894.)

THE physiological course which Mr. Francis Darwin gave at Cambridge in 1883, was the first systematic effort, in this country, to teach the phenomena of plant-life to students by means of actual experiments. As we are told in the preface to this book, the experiments were at first demonstrated in the lecture-room; some years later, the students were required to do the practical work for themselves in the laboratory. The example set at Cambridge has been followed in other universities and colleges, to the great benefit of botanical teaching. We all recognise now that practical laboratory work is no less necessary in physiological than in morphological botany, though in the former it is certainly more difficult to organise. The present book, which embodies the results of the experience gained in practical teaching, is in two parts. Part i., on General Physiology, is the more elementary, and therefore the more widely useful; Part ii., on the Chemistry of Metabolism, is of a more advanced character, and is adapted to those students who desire to make a special study of the chemical physiology of plants. The former, we believe, is mainly the work of Mr. Darwin; the latter, of Mr. Acton.

A volume of this kind was very much needed, and it is a matter for congratulation that the work has fallen into the most competent hands. There was nothing of the kind in English before, and the book will be of the greatest service to both teachers and students. It must be clearly understood that it is a strictly practical laboratory guide, which can only be used by those who are willing to experiment for themselves. The volume is in no sense a treatise on physiology, and thus differs from its German predecessor, Detmer's "Pflanzen-physiologisches Practicum," which is to some extent a compromise between a practical guide and a theoretical text-book. The thoroughly practical character of Messrs. Darwin and Acton's book seems to us a great merit; every word in it is of direct use to the experimental worker, and to him alone.

We cannot attempt to give anything like a summary of the contents of the work, which, in spite of its moderate bulk, covers a great deal of ground. Thus in part i. alone, no less than 265 distinct experiments are described. Of course they vary very much in character, some being quite simple and elementary, while others are more of the nature of original research. It goes without saying that a large proportion of the experiments are of Mr. Darwin's own devising, and that nearly all have been practically tested by the authors. Wherever this is not the case, the reader is told so; and if the experiment is, from any cause, at all likely to fail, he is warned of the possible disappointment. The candour with which the student is treated all through, is a very pleasant feature of the book.

The first chapter is on some of the conditions affecting the life of plants, and as the presence of oxygen is among the most important of these conditions, respiration is taken first. Besides the more usual experiments, ingenious demonstrations of intramolecular respiration, and of the excessive consumption of oxygen by germinating oily seeds, are given.

In the second chapter assimilation takes the first place, and many beautiful experiments are described, including Gardiner's ingenious modification of Sachs's iodine method, in which the sun is made to print off, in starch, a copy of a photograph, from a negative placed on the leaf. An experiment proving that excess of carbon dioxide stops assimilation, is especially interesting. When a second edition is called for, Mr. Blackman's new and important experiments on the function of stomata will no doubt find a place.

In the next chapter, which is also concerned with nutrition, particularly good and complete directions are given for the management of water-cultures; these are quite the best we have met with, and will save the experimenter from many failures. The use of Duckweed (*Lemna*) for demonstrating the effect of various food-solutions on growth, is, we believe, new, and is a very neat method. The same chapter includes experiments on the nutrition of the carnivorous plant, Sundew, a subject on which Mr. Darwin's investigations have become classical.

The question of the movement of water in plants is still unsolved. The data of this problem, however, are very thoroughly taught, by means of the experiments described in the sections on the functions of roots, and on transpiration. The latter process is investigated, in the first instance, by means of the potometer, an instrument devised by Mr. Darwin and his pupil Mr. Phillips, in which the speed of the transpiration-current is measured by the rate of ascent of an air-bubble, which is drawn up a capillary glass-tube by a transpiring shoot connected with it.

A particularly ingenious experiment is one in which the hygroscopic twisting and untwisting of an awn of the grass *Stipa*, is made use of as an index of transpiration.

A chapter on physical and mechanical properties treats of such phemonena as imbibition, turgor, osmosis, and the tensions of tissues. It may be pointed out that in the description of Traube's artificial cells, copper *sulphide* is evidently a misprint, either for copper *chloride*, or *sulphate* (p. 111).

The next chapter is on *growth*, and contains, among many other things, full directions for the use of the various kinds of auxanometer.

The remaining chapters are concerned with curvatures (geotropism, heliotropism, traumatic curvature, &c.), and with other movements. Some of the most fascinating experiments come in this part; we will only mention those on the decapitation of roots, an operation which, as Charles Darwin discovered, prevents the root from perceiving the geotropic stimulus, though it does not hinder the curvature of the growing region which may have been induced by a previous stimulation. Attention is here called to the brilliant experiments of Prof. Pfeffer, which have demonstrated conclusively that the tip of the root is alone sensitive to gravitation, thus finally confirming the conclusion drawn by Darwin from less decisive experiments. The announcement of this discovery by Prof. Pfeffer was one of the most interesting incidents in the Biological Section at the Oxford meeting of the British Association.

A self-recording method for studying the sleep-movements of leaves, strikes us as especially valuable.

The second part of the book, on the chemistry of metabolism, is of quite a different character from part i., and is evidently intended for students with an advanced chemical knowledge, who alone can make intelligent use of it. The object aimed at is sufficiently explained in the opening paragraph :

"The practical study of the transformations which plastic substances undergo in metabolism, is an application of organic chemistry: the immediate problem is generally to determine whether certain substances are present or absent, and, if present, in what amounts in particular tissues."

The mode of determination of all the important organic bodies occurring in plants, such as protein, amides, oils, carbohydrates, tannins, acids, and enzymes, is concisely explained.

There are two appendices, the first of which gives examples of quantitative results obtained in actual experiments, in order to show the degree of accuracy which may fairly be expected ; the second is a list of reagents.

Within the short space of ninety small pages, which is all that the second part occupies, it is obviously impossible to give full instruction in such a difficult and complicated subject as the practical physiological chemistry of plants. Those, however, who are already good chemists, will no doubt derive great help from the terse directions given here, especially as these are supplemented by abundant references to the more special literature.

The authors are much to be congratulated on their work, which fills a serious gap in the botanical literature of this country. We think it very desirable that a smaller edition of the book should be published for use in schools, bearing somewhat the same relation to the present handbook as Prof. Bower's "Practical Botany for Beginners" bears to his larger manual on the same subject. It is most important, now that physiological botany is supposed to be taught in so many schools throughout the country, that it should really be taught in the only efficient way, namely by experiment, and that it should no longer be made a mere matter of "cramming," as is now too often the case. A selection from the present book of the simplest and most fundamental experiments, such as could be performed with tolerable certainty of result in ordinary science-schools, would, we are sure, be of the greatest service to conscientious teachers, who desire to make their scientific instruction a reality.

D. H. S.

MUSSEL CULTURE.

Mussel Culture and the Bait Supply, with reference more especially to Scotland. By W. L. Calderwood. (London : Macmillan and Co., 1895.)

THIS little book has been written for the useful purpose of calling public attention to the urgent need of an increased supply of mussel bait in the interests of the line fishermen, and in order that the food supply of the country may be increased. It does not contain new facts, but it summarises many old ones, and puts the results of some biological inquiries in a form in which they will be readily available for consultation by members of County Councils and others who ought to be interested in fish-culture. The general conclusion arrived at is one which has been recently pointed out in the pages of NATURE, viz. that a systematic cultivation of our foreshores, such as is now carried on in several European countries—notably France and Holland—must soon be resorted to if we wish to stop the rapid depletion of our shellfish beds.

Sixty years ago the supply of mussels for bait must have seemed almost inexhaustible, but now that larger boats with more men and much longer lines are employed, the supply of bait is rapidly failing at many places round the coast, with the result that we have to import at considerable expense large quantities of mussels annually from Holland. The fact that it is necessary thus to import a mollusc which grows naturally in great abundance on our own shores, is in itself significant of the mismanagement—or total absence of management—of our bait beds in the past. Mr. Calderwood gives an account of the various mussel scalps or beds round the Scottish coast, describes their former extent and their present condition—in most cases a sad story of wicked waste resulting in woful want—and says : "We have seen our public oyster fisheries slowly decline, and all but expire ; we now are watching our mussel beds as they diminish in the same way."

It is a pleasant relief to notice that some few beds really *are* regulated and well managed, with the result that they are in a flourishing condition. For example, those at Montrose, where "the grounds now under cultivation were at one time all but destitute of mussels, but by the exertions of the Ferryden and Usan Society of Fishermen, led by Mr. James Johnston, 'seed' was collected and bedded, and the system of cultivation adopted which has since yielded such excellent results" (p. 24). The interesting account of the enterprise of the fishermen at Nairn, in experimenting with mussel culture on their own account, shows how readily new beds may be formed and important results obtained. If such work is to be done, however, to any great extent, we must adopt the French system of renting, on easy terms, the sea-bottom and foreshore to such individuals as will cultivate the beds, and so render them of increased value to the country.

We are reminded by Mr. Calderwood, as a proof of the enhanced value of bait, that the sands of Dun, in Scotland, which were not considered by the fishermen to be worth £5 a year at the beginning of the century, are now let for £500 per annum. Of the various baits for the long lines—mussel, scallop, squid, lugworm, whelk, cockle, &c.—mussel is the most generally distributed, the most easily grown, and altogether the most serviceable and important. The Mussel Commission stated in 1889 that "nearly all the 50,000 fishermen of Scotland use mussels as their bait during some part of the year." It has been calculated that the fishing lines used in Scotland in 1893 would, if tied together, nearly encircle the world twice, and probably about 47,000,000 hooks have to be baited (each with two mussels) every time all the lines are set. These statements give some rough idea of the magnitude of the demand for this bait. If squid could be obtained in sufficient quantity, it would probably be even more valuable than mussels, but its price is usually prohibitive to most fishermen. A fishing firm in Aberdeen paid during this last winter over £200 for squid bait for a single boat's lines for the three months October to December, and there are fifty to sixty of such boats north of the Tyne.

One section of this book gives a short account of the anatomy, and the reproduction, and an outline of the development of the mussel. Most of this is very simple; but it is not easy to understand the following statement (p. 44), where, speaking of the kidneys, Mr. Calderwood says: "Two internal openings also communicate with the cavity in which the heart is situated, and in this way the organ has a more powerful action in aërating the blood"! Whether "the organ" in question is the heart or the kidney is not very clear, and in either case the statement is equally mysterious.

As a single female mussel produces two or three million young, and as innumerable young mussels all round our coast perish miserably every year for want of suitable objects to attach to, there can be no reasonable doubt that the judicious erection of simple stakes, or "bouchots," would serve a useful purpose, at any rate in the collection of seed, even if the further rearing be carried on by means of the bed system. The importance of transplanting, of cropping the beds by rotation, of exterminating enemies such as starfish and whelks, and of avoiding overcrowding, is pointed out; and we are reminded of the opinion of Prof. M'Intosh, endorsed by the Scottish Fishery Board, that "if properly and wisely managed, a mussel fishery will rapidly repay the small initial expense, and might, indeed, be made largely profitable."

The book concludes with a chapter on the legal aspects of the matter, extracts from the Acts in regard to mussel fisheries, and information on the methods of obtaining fishery "orders" in England, Scotland, and Ireland. Some of these legal processes seem unnecessarily complicated and expensive, and it is certainly unfortunate that our local fishery committees can make regulations in regard to their mussel beds, but cannot improve them by cultivation; and yet in parts of England the mussel is even more important than in Scotland, as it is used not merely as bait, but very largely as food in some districts. The book seems singularly free from typographical errors—a curious slip on p. 11 suggests that the author's mind was so full of his subject that he has mis-spelled Mr. Bateson's name. W. A. H.

HISTORICAL EPIDEMIOLOGY.

A History of Epidemics in Britain. By Charles Creighton, M.A., M.D. Vol. ii. From the Extinction of the Plague to the Present Time. (Cambridge: University Press, 1894.)

THE first volume of this work was reviewed in these columns about three years ago, and Dr. Creighton has now brought his difficult task to completion. The labour of disinterring the facts of epidemiological history from the scattered chronicles in which they lie hidden is very considerable, and, when this is accomplished, the historian is further confronted with the difficulty of identifying, under the confused nomenclature of by-gone days, the various pestilences described, and of assigning to them their proper place in modern nosology.

Dr. Creighton has undoubtedly earned the thanks of all students of epidemiology for the painstaking and laborious compilation of facts and references which he has brought together in the present volume. The magnitude of the labour has been immense—the more so since, as the author remarks in the preface, he has had little help from predecessors in the same field. He has at times incorporated with the strictly historical portions of the work ætiological considerations which, we fear, will hardly commend themselves as of equal merit with the rest; these, however, form but a small part of the whole, and though they call for comment, should not be allowed to interfere with our appreciation of the value of the book.

The opening chapter deals with typhus and other continued fevers, and its historical interest is very great. It well illustrates the close connection which exists between the epidemic prevalence of this group of diseases, and the physical environment and social conditions of the population, nor could an instructive lesson in hygiene have been better presented. It is interesting, too, to trace the gradual rise of more exact pathological notions, whereby the old medley of spotted, putrid, miliary, and comatose fevers has been resolved into our modern typhus, enteric, and relapsing fevers. The grouping together of influenzas and "epidemic agues" in another chapter seems warranted by the etymology of the term ague (Latin: *acutus*), the expression being used in early times for any sharp fever: the term "influenza" did not reach England till 1743.

Small-pox naturally receives a full measure of attention. The statements put forward to show that during the seventeenth century the mortality from this disease had not that excessive incidence on infants which afterwards became the rule, are not, in the necessary absence of any statistical evidence, sufficiently convincing. The history and practice of inoculation are treated of at great length, and the account is full of interest. Dr. Creighton's opinions on the subject of vaccination are well known, and it is a pity that in a purely historical work, any bias should have been allowed to appear. He has perhaps done his best to avoid it by treating vaccination, after the year 1825, as "*ex hypothesi* irrelevant," though this proceeding may raise a smile on the faces of many of his readers. Even in the preface the assumption that variola and vaccinia are two perfectly distinct diseases, calls for some comment in the light of recent investigations, and the omission of any reference to the statistics of the Sheffield epidemic of 1887–8 is a serious blot on any work dealing with the history of small-pox. The immunity from small-pox which infants and children enjoy at the present day, receives the not very satisfying explanation that they now have measles, whooping-cough, scarlatina, and diphtheria instead!

In the later chapters the author deals with the last-mentioned diseases and with infantile diarrhœa, dysentery and cholera, the history of which is traced with great care and accuracy. It would indeed be difficult to praise too highly the pains which the author has taken in the collection and arrangement of his historical facts. But he has chosen to add, in many places, considerations as to the nature and causes of the diseases he chronicles, which frequently do not cover all that is known about the subject, and would have been better omitted or treated separately from the historical portions of the book. It is true that, in some cases, there are strong reasons for believing that the virus of a disease may reside in the soil, but it is by no means true for others. It is true that we are ignorant of the precise nature of the virus of many of the diseases discussed in the book; but it is not the case with all. Yet in no single passage dealing with ætiology do we find any reference to even well-established bacteriological facts. It may be that much studying of the records of the past begets a tendency to a mediæval frame of mind. Certainly Dr. Creighton's views on telluric influences will not commend themselves to the modern pathologist, though, like the subjects he treats of, they may possess a historical interest.

But it is a great merit of the book that it can be read with pleasure and instruction by all, however the reader may differ from the author in pathological creed; and Dr. Creighton may be congratulated upon the completion of so excellent and thorough a history of epidemic diseases in this country.

OUR BOOK SHELF.

Grundzüge der mathematischen Chemie. Von Dr. G. Helm. (Leipzig: Wilhelm Engelmann, 1894).

THE treatment of the subject-matter of this book is based on the view that in its present state of development, that branch of physical chemistry which relates to chemical change can be discussed from a general standpoint, inasmuch as it affords the clearest and most complete confirmation of the principle of the conservation of energy.

The applications of this principle to chemical interactions are first illustrated by means of the different kinds of thermal measurements, numerical examples being given, the solutions of which, here as elsewhere in the book, are particularly neat. Mechanical forms of energy attending chemical change, in particular the volume energy of gases, are also discussed. The author next points out that the measures of the different forms of energy are composed of two factors, one of which is all-important in determining the direction of the energy change. Temperature and entropy are shown to be the factors of heat energy, and a clear and concise account of the thermodynamics of perfect gases is given, in order to arrive at the shape of the entropy function, which is of course known in this particular case. The relations between heat energy and volume energy, and between heat energy and electrical energy, are then set out at length.

The author here indicates how terms involving what he calls the "chemical intensity" of the reacting substances enter into the energy equations. Chemical intensity is what Gibbs originally termed the "potential" of the substances, and this function, it is hoped, will eventually be shown to be the mathematical expression of chemical affinity.

The third section of the book is devoted to the properties of chemical intensity. The general method of deriving the law of mass action is given, and chemical equilibrium, the properties of dilute solutions, and the velocity of chemical reactions are brought under the sway of the energy equations. The last section contains the treatment of the phenomena which may be grouped around Gibbs's phase rule, and of reactions depending on several parameters.

The book is the only one which is exclusively devoted to chemical energetics, and to the student possessed of sufficient mathematical knowledge it offers an admirable account of the present state of the subject. J. W. R.

Die Bearbeitung des Glases auf dem Blasetische. Von D. Djakonow und W. Lermantoff. Pp. 154. (Berlin: R. Friedländer and Sohn, 1895.)

THE original edition of this book was in Russian, and the authors, one of whom, D. Djakonow, is now dead, were demonstrators in chemistry at St. Petersburg University. The instruments and methods employed by glass-blowers are set forth in detail, together with descriptions of the kinds of glass best suited for different work. A very full and practical account is given of the construction, graduation, and calibration of thermometers; but to carry out these operations thoroughly, some experience is required. Work more suitable for the 'prentice hand fills the greater part of the book. Every operation in glass-blowing and manipulation likely to be needed in physical and chemical laboratories, appears to be described; while the diagrams illustrating the stages in the construction of the different pieces of apparatus, will greatly assist in training students to become skilled workers.

Problems and Solutions in Elementary Electricity and Magnetism. By W. Slingo and A. Brooker. Pp. 108. (London: Longmans, Green, and Co.)

MODEL answers to examination questions may prove a blessing or a curse, according to the way in which teachers use them. Herein are answers to questions in electricity and magnetism (elementary stage), set at the Science and Art Department's examination from 1885 to 1894, together with a series of original questions. The teacher who wishes to train his class to answer questions clearly and concisely, will find suitable exercises in composition in this book, and he will also find the volume an inducement to cram his students with undigested information.

Qualitative Chemical Analysis of Inorganic Substances. (New York: American Book Company, 1895.)

THIS work consists of a series of analytical tables, supplemented by explanatory and descriptive notes, and working directions. It makes no pretence to originality, and is hardly a book we should like to see widely adopted by students of elementary practical chemistry. The tables, which were prepared for use in Georgetown College, Washington, D.C., present few points of interest or value to teachers of chemistry in our schools.

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 intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Professor Boltzmann's Letter on the Kinetic Theory of Gases.

IN common, I am sure, with all the physical readers of NATURE, I have read Herr Boltzmann's letter with great interest. And I am glad to observe that, though he appears to think I differ from him, that part of his letter which chiefly deals with my criticism on Dr. Watson's idea of what "Boltzmann's Minimum Theorem" is, is simply putting forward, with all his great authority, the view for which I contended. But it is a little hard that Dr. Boltzmann should represent me as endeavouring to *disprove* his theorem when I expressly stated that while I did not know *his* proof, *I supposed that it was all right.* True, I said that I found it hard to conceive how any proof on the lines of Dr. Watson's could be valid *because that proof appeared to me to be a purely dynamical proof,* and I applied the reversibility argument *to show that a purely dynamical proof was impossible,* so that the H-theorem could not be a purely dynamical theorem; and after indicating the lines on which it appeared that there might be an *average dynamical* theorem, I asked if some one would say what the H-theorem really was.

Thereupon Mr. Burbury wrote a helpful letter, which he followed up by a still more helpful correspondence, in which verbal misunderstandings were gradually cleared away, which showed that the proof of the H-theorem considered as a dynamical theorem, not as a theorem in probabilities, assumed that in one respect the configuration was, before each set of collisions, already perfectly average, and that this condition is violated in the reversed motion; so that the theorem, regarded as a dynamical theorem, is not proved for configurations in general, but for those possessing a certain amount of "average" already—a restriction which comes to the same thing as the limitation imposed by Prof. Boltzmann when he says *the theorem is not a dynamical theorem, but one in probabilities.*

Shortly after Mr. Burbury's letter appeared, Dr. Watson wrote denying that the criticism from reversibility applied, and claiming that the theorem was a general dynamical theorem, in the sense that it applied to *all* configurations. Enlightened by Mr. Burbury, I now see that Dr. Watson's reasoning is not open to the objection that it proves a general dynamical theorem; but I cannot blame myself for thinking that it did, for that was what Dr. Watson himself believed it to do, and what his language naturally implies. Moreover, after perceiving the oversight which vitiates the proof in its present form, I did not examine it further.

Prof. Boltzmann has misunderstood Mr. Burbury and me in one or two particulars. He denies that there are as many configurations for which dH/dt is positive as there are for which it is negative. He evidently thinks that we mean something different from the bare meaning of the words, which are certainly true. It is easy to explain what we do not mean (I say *we*, for I am sure Mr. Burbury will agree with me). Suppose $H = 10$ to be the minimum value of H for a given system of molecules, we do not mean that among all the configurations for which $H = 50$, there are as many which will, if left to themselves, turn into configurations for which $H = 60$, as will turn into configurations for which $H = 40$. The illustration, which to my mind has most clearly removed the apparent contradiction in the statement that there are as many configurations for which H will increase as decrease, while yet the probability is that H will on the whole decrease, is that of a *y* turned upside down, thus ⅄. For every downward path there is an upward path, *i.e.* the reversed direction; yet starting from the angle there are two ways down for one way up, so that there is a greater probability of going down than up. If in the reversibility argument one could assert, not merely that there are *as many* configurations for which H tends to increase as to decrease, but that *for any given value of* H there were as many configurations which tend to increase as to decrease, then the conclusion that H was as likely to increase as to decrease could be deduced. But the argument is quite invalid when we set off a configuration for which H increases against one for which it decreases, *although the values of H for each are different.* As an illustration more closely allied to the case of a gas, we might take a tree turned upside down, with an infinite number of branches passing through each point of its substance in all directions, there being at every point more branches tending downward than upward (because those whose tangents are horizontal may be said to tend downward on each side), and every upward branch finally turns downward and tends to become nearly horizontal at last, when H is near its minimum value.

To my mind this appears a far better way of meeting the difficulty than Prof. Boltzmann's illustration of the dice, for so far as I can see, all that he has shown is that if you start from an exceptionally high ordinate, *i.e.* one over the average, you are likely, after a considerable time, to get to lower ordinates in whichever direction you go, and an opponent might answer that if you start from an exceptionally low ordinate you are likely to get higher ones in whichever direction you go, and that there must of course be as many deviations below the average as above it, so that if you start from an arbitrary point in an arbitrary direction, you are just as likely to get to higher as to lower ordinates. In point of fact this appears to be the case for his curve, while it is not true for the tree or for a gas.

Prof. Boltzmann must have put an entirely wrong construction on something or other, which I suppose I have written, when he says I object to the Maxwell Law of distribution because it would ultimately lead to the total kinetic energy of the universe being equally distributed among every degree of freedom of every particle in the universe. Instead of considering that to be *à priori* improbable, I hold exactly the view put forward by Prof. Boltzmann.

With regard to the first portion of Prof. Boltzmann's letter, there is so much that is speculative in it that any discussion would occupy more space than I feel entitled to claim. I will only say that the idea that a gas takes years to come to thermal equilibrium seems hardly consistent with vibrational portion of the kinetic theory being of practical value, when applied to gas which has only had a few hours to settle down.

EDWARD P. CULVERWELL.

Trinity College, Dublin, March 6.

IT seems to me that my meaning has not been expressed quite clearly; therefore, it may be worth while to add one remark. Not for every curve, but only for the particular form of the H-curve, disymmetrical in the upward and downward direction, can it be proved that H has a tendency to decrease. This particular form is very well illustrated by Mr. Culverwell's suggestion of an inverted tree. The H-curve is composed of a succession of such trees. Almost all these trees are extremely low, and have branches very nearly horizontal. Here H has nearly the minimum value. Only very few trees are higher, and have branches inclined to the axis of abscissæ, and the improbability of such a tree increases enormously with its height. The difficulty consists only in imagining all these branches infinitely short.

Finally there is the difference between the ordinary cases, where H decreases or is near to its minimum value, and the very rare cases, where H is far from the minimum value and still increasing. In the last cases, H will reach, probably in a very short time, a maximum value. Then it will decrease from that value to the well-known minimum value.

Paris, April 6. LUDWIG BOLTZMANN.

The Recent Auroral Phenomenon.

ON the evening of March 13, from 7.35 to 8.5, Greenwich mean time, I was a spectator of the abnormal display of Aurora Borealis which attracted so much attention at various places throughout the country. It appeared here as a belt of light spanning nearly the whole sky in a great circle from east to west. When first noticed by me at 7.35, the streak extended from near the hind quarters of Leo to the head of Aries, or from R.A. 169°, Decl. + 16° to R.A. 24°, Decl. + 22°.

At the time the streak was altogether cometary in appearance, beginning in a fine point, but it gradually changed in form, moving at the same time towards the south. Eventually it also shortened so considerably that just before my last view of it, it only extended from γ Geminorum to γ Ceti. Its greatest breadth was about 12°.

The following figures represent the most striking phases, as nearly as may be at intervals of five minutes.

Fig. 1 shows how the streak extended from the cometary head so as to form a long wavy tail, and represents also the streak at its greatest length. As indicated in the sketch, there was a central portion much more brilliant than the rest, running from the head into the body of the streak.

In Fig. 2 the streak is seen when it had more the appearance of a rainbow than of a comet; and it was very noticeable that one side—that towards the north—was much brighter than the other.

Fig. 3 shows how the "head" began to shrivel up—shortening the streak. The glimmering appearance of the "shrivelling" put me very much in mind of the motion of the air over a "hot heap" (of slag); the tail end began to broaden out somewhat.

In Fig. 4 the streak has taken a very pronounced arrow-headed shape, and, as if to complete the resemblance, the shimmering part took the form of the feathering; whereas in the preceding figure it had more the appearance of comb-teeth. The more brilliant parts are indicated by darker shading.

North.

Fig. 1.

Fig. 2.

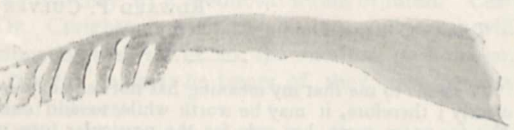


Fig. 3.

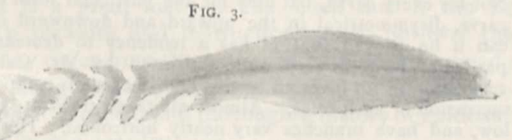


Fig. 4.

Fig 5.

South.

In Fig. 5 the streak has considerably shortened and broadened out in the west, where it soon afterwards mingled with faint auroral rays which had come round from the northern horizon.

I may say, in general, that the appearances were singularly noticeable and brilliant. The sky was very clear at the time, and every star was visible through the most brilliant parts of the streak. During the time the streak was visible there was a faint display of aurora on the northern horizon, which, as I have already said, worked round to the west and caught the last of the streak.

Jas. G. Richmond.

Muirkirk, N.B.

THE AGE OF THE EARTH.[1]

ILL-HEALTH has hitherto prevented my making the comments which seemed called for by Lord Kelvin's friendly article of March 7, in reply to my communication of January 3. Perhaps I may be allowed not merely to restrict my remarks to this article, but to deal more generally with the subject, in the hope of clearing away the misapprehensions which exist between modern geologists and palæontologists, who are no longer uniformitarians, and physicists who are represented by Lord Kelvin.

The arguments as to the age of life on the earth are based on considerations of (1) geology and palæontology; (2) tidal retardation and shape of the earth; (3) the cooling of the earth from an initially hot condition; (4) the age of the sun.

(1) From geology. The leading geologists declare that the great thickness of sedimentary rocks created since the Lower Cambrian, which are almost the oldest fossiliferous rocks, can only have been produced during many millions of years.

It is difficult to get geologists to give even wide limits for the age of the Lower Cambrian.[2] Their calculations are based not upon the rate of accumulation of sediment in one of our quiet oceans, but upon the rate of degradation in valleys where the rate is greatest at the present time. They make this declaration, thinking that for the last thirty-three years it has been authoritatively declared by physicists that such an estimate is absurdly great. I have no doubt that they have done their best to keep this estimate as low as possible, for they have a great interest in making geological theory agree with physics. Some physicists tell them that the flaw in the geologists' reasoning consists in their not taking into account the much greater tidal actions of the past. When tides rose and fell many hundreds of feet, and swept over tens or hundreds of miles of foreshore, there must undoubtedly have been a more rapid formation of sedimentary rock than anything of which we now have experience. The geologists' answer is:—We acknowledge that all nature's actions were on the whole, possibly, more intense in the past. We know from Prof. Darwin's development of Prof. Purser's theory that the moon was undoubtedly nearer the earth in palæozoic times, and the tide influence was therefore greater. But there seems to be no method of even approximately calculating how much greater the tidal influence was. Whilst one great astronomical authority speaks of tides of 500 feet deep in palæozoic times, Prof. Darwin himself thinks that two or three times as great as at present may be an excessive estimate. There is a good deal of geological evidence for much smaller seas than at present, and even if tidal influence were greater the actual tides may have been much smaller than now. Of positive evidence in our favour, we have the fact that numerous examples exist of palæozoic rocks which are identical in almost every physical way with tertiary rocks, and it is difficult to believe that they can have been deposited under very different conditions. Again, nearly all the old sedimentary rocks were laid down near coasts where tidal action would be most violent. Yet even low down in the Cambrian we find the remains of creatures

[1] In this paper free use has been made of many suggestions from Prof. Fitzgerald.

[2] Their data are of this nature:—Of fossiliferous rocks successively formed the total thickness may be taken as not less than 80,000 feet. Over the areas of the basins drained by many rivers the rate of denudation is known with sufficient accuracy for approximate calculation. Of the basin of the Mississippi a thickness of one foot of rock is removed in 6000 years; the Ganges, 2358; the Hoang Ho, 1464; the Rhone, 1528; the Danube, 6846; the Po, 729; the Nith, 4723 (Sir A. Geikie, Geol. Soc. of Glasgow, 1868). I have heard that Prof. Sollas demands less time than other geologists; but since this paper was written, I have seen (Nature, April 4) that even he does not care to put the age of the Lower Cambrian at much less than 17 million years.

which still have attached to them delicate antennæ. In sandstones we find most delicate ripple marks and the marks of rain-drops. But over and above all this, denudation along coast-lines can hardly be regarded as of much importance compared with subaerial denudation (Sir A. Geikie, *Trans.* Geol. Soc. of Glasgow, 1868). Was there more rain? and did it fall more suddenly? Did the wind blow more strongly? Were atmospheric actions more vigorous in the past? There is no great reason for believing that they were. As Prof. G. Darwin observes, fossil trees do not seem to have been built more strongly than modern trees, and this gives some evidence as to the relative violence of aerial forces.

All the geological evidence points to rates of denudation and deposition in the past which may, on the average, have been greater than the average rate at present, but which were not on the average greater than the greatest rates at present.

The palæontologist now comes in. A study of fossils shows that there has been a gradual development, sometimes more quickly perhaps, and sometimes more slowly, but on the whole a continuous development of animal life in the past. We believe from all our study of nature that the development has been continuous. As more and more strata are studied, many of the apparent discontinuities are being converted into continuities. Now even in the lower parts of the Cambrian, *Brachiopoda* are found. Biologists tell us that in all probability these were gradually developed from creatures like worms; their structures are sufficiently complex for us to know that the time taken to develop the Brachiopod from the worm may have been as great as the age of known fossiliferous rocks. There are many rocks, evidently sedimentary, enormously older than the Cambrian, and when laid down there was certainly water on the earth, and hence it was neither too hot nor too cold for animal life. In these lower formations there are conglomerates containing pieces of still older rocks. Although in pre-Cambrian strata traces of animal remains are said to occur, we may say that the palæontological record is almost lost below the Cambrian, most of the earlier rocks having been subjected to great metamorphic action. If we keep to our principle of continuity in nature's actions, we see that the first beginning of life must have taken place at a date many times earlier than the very earliest geological record.

But the most experienced geologists and palæontologists state that they are satisfied with a few hundred million years as the possible age of life or the existence of water on the earth.

2. The considerations drawn from tidal retardation are as follows:—

(α) The shape of the earth now is the same as its shape when it solidified. (β) The shape of a liquid earth tells us its rate of revolution on its axis, therefore we know the rate of revolution of the earth on its axis when it solidified. (γ) Assuming that we know, with a fair amount of accuracy, the rate at which the length of the day is altering, we know the date of the earth's solidification, and certainly this is later than 1000 million years ago.

When I referred to the fallacy in this argument, I did not know that it had already been pointed out by the Rev. M. H. Close and Mr. Clarence King and Prof. George Darwin. It lies in the fact that (α) is certainly wrong. A solid body like the earth will, under the action of great forces, alter its shape in time. Such alteration is continually going on. Again (γ) is very doubtful.

(3) I now come to the considerations from the cooling of the earth. Lord Kelvin proved that, if the earth was once at a uniform temperature of 7000° F. or 3870° C., of material the heat properties of which are the same as the average of three rocks experimented upon at Edinburgh —these remaining constant throughout—and if the rate of increase of temperature downwards in the crust is now 1 Centigrade degree for every 90 feet, 100 million years have elapsed since cooling began; but there is a possible maximum of 400 millions.

In the article on this subject, published in NATURE, January 3, 1895, I showed that, if we assume greater conductivity in the interior than at the surface, we increase this limit of age. I took a number of examples, which could be worked mathematically. I did not pretend that any one of these represented the actual state of the earth. They merely proved that there were *possible* internal conditions which might give enormously greater ages than physicists had been inclined to allow. Of my various results, I did not give one as more correct than another, although some may have seemed more probable than others. It was not my object to obtain a correct estimate. Indeed I tried to show that it was impossible for a physicist to obtain such an estimate, as there were all kinds of possible assumptions which led to many different answers.

The validity of my reasoning in no degree rests upon the accuracy of R. Weber's results as quoted by me. Indeed, I only discovered these results when writing to Prof. Tait. In NATURE, February 7, p. 341, I have shown the extent to which the possible limit of the earth's age is increased if k and c increase with temperature and k/c remains constant. But I published this as an interesting mathematical result, and was careful to add— "It must be understood that my conclusions are really independent of whether R. Weber's results are correct or not." It is comparatively unimportant, but R. Weber has published another set of results which confirm those which I quoted. The results, published on March 7 for the first time, differ so utterly from the two previous sets, that I venture to think there may be mistakes in transcribing. However that may be, I am not concerned either to support or refute them.

I mentioned the possible great quasi-conductivity due to the interior of the earth being a honey-combed mass containing liquid, and to the possible greater conduction due to the presence of iron and other metals. Almost anything is possible as to the present internal state of the earth. Dr. Ramsay seems to think that there must be great quantities of sulphides inside, and these would probably be much better conductors than the surface rocks.

Prof. Schuster, in discussing the diurnal variation of terrestrial magnetism (*Phil. Trans.* 1889, p. 467), comes to the conclusion that the *electric* conductivity of the earth must be considerably greater inside than at the surface.

In all probability there are no great masses of liquid inside the earth *at the present time*, but it is quite possible that until recent times convection in such masses may have been conveying heat from the very inner earth towards its surface, and the latent heat given out by such masses of liquid as they solidified would be another potent factor. Some distinguished geologists say that the excessive folding which has occurred on the earth's surface cannot be accounted for by the current assumption of physicists, which involves the result that, practically, no cooling has yet taken place below the depth of 120 miles: my assumption is that cooling has taken place to much greater depths.

All these things, like the numbers published by R. Weber, support the argument if they are correct, but they do not in any way destroy it if they are wrong. I was not looking for a *probable* age of the earth from the point of view of mere physics. I wished to show that the physics' higher limit was greater than a few hundred of millions of years.

Mr. Clarence King's paper appears somewhat inconclusive. He assumes, possibly rightly, that the earth's crust may have the properties of *Diabase*; experiment has shown what is the rate of increase of the melting temperature with increase of pressure of this rock: Laplace's hypothetical law of increase of density downwards in the earth cannot be very wrong, and from this a law of increase of pressure downwards may be formulated. From these data Mr. King finds what are the temperatures at various depths, which if exceeded would mean liquidity. A liquid layer inside the earth's crust being assumed to be impossible, Mr. King, trying all sorts of Kelvin solutions of a solid earth of uniform conductivity and uniform temperature, initially finds a maximum age of 25 million years, the initial temperature being not greater than 2000° C.! Furthermore, higher initial temperatures are not possible!

Now it is evident that if we take any probable law of temperature of convective equilibrium at the beginning and assume that there may be greater conductivity inside than on the surface rocks, Mr. King's ingenious test for liquidity will not bar us from almost any great age.

(4) There remain, lastly, considerations drawn from the age of the sun. On the assumption that all the energy possessed by the sun was that due to the mutual gravitation of its parts, and that the sun is now of uniform density, Helmholtz found that the sun may have in the past radiated as much as 22 million times his present annual loss. Langley found that the sun's present rate of radiation was under-estimated, and the statement of Prof. Newcomb may be taken as that of Helmholtz, corrected. Newcomb says ("Popular Astronomy," p. 523): "If we take the doctrine of the sun's contraction as furnishing the complete explanation of the solar heat during the whole period of the sun's existence, we can readily compute . . . It is thus found that if the sun had, in the beginning, filled all space, the amount of heat generated by his contraction to his present volume would have been sufficient to last 18 million years at his present rate of radiation."

Lord Kelvin pointed out (pp. 364-65, vol. i. "Pop. Lectures") that Helmholtz had assumed a sun of uniform density, whereas the sun's density must increase very much towards his centre, and as a result of calculation on the assumption that only half of the original energy was available (p. 374), that the radiation was greater in the past, and that the original collisions occurred practically simultaneously, he says: "We may therefore accept as the lowest estimate for the sun's initial heat 10,000,000 times a year's supply at present rate, but 50,000,000 or 100,000,000 as possible, in consequence of the sun's greater density in his central parts." And again (p. 375): "It seems therefore, on the whole, most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. This last number, then, is Lord Kelvin's higher limit. After six years, in 1868, Lord Kelvin returned to the question, and he says (p. 53, vol. ii. "Pop. Lect. and Addresses"): "The estimates here are necessarily very vague, but yet vague as they are, I do not know that it is possible, upon any reasonable estimate, founded on known properties of matter, to say that we can believe the sun has really illuminated the earth for five hundred million years."

In his R.I. address of 1887 Lord Kelvin gave no higher limit. I think that, on his specified assumptions in giving these large numbers, he has been very generous; for, taking Mr. Homer Lane's determination of the internal density of the sun, I find that the Helmholtz total energy need only be multiplied by about $2\frac{1}{2}$. If, however, instead of taking, as Mr. H. Lane did, 1·4 as the ratio of specific heat, we take a less number, and there is no reason why we should not, we find much greater densities towards the centre, and a much greater total energy and age. Still, I think that it is only when we escape from the above assumptions that we can see our way to increase the higher limits which have been quoted.

To justify the Helmholtz hypothesis of mere mutual attraction, initially, between the portions of matter which form the sun, Lord Kelvin ("Pop. Lect.," vol. i., pp. 411-3) dwells upon the great improbability that any parts of the sun possessed much initial velocity. He shows that if two bodies, A and B, came together to form the sun, when the bodies were still far apart before collision, the motion of the centre of B relatively to A, must have been directed with great exactness to pass nearly through the centre of A (as the sun has a comparatively small moment of momentum), and this was very improbable if the bodies had initial velocities. But this argument is only satisfactory when the bodies coming together are two in number. For example, let us imagine in early times a sun of half the mass of the present one, but of many times its diameter. It is possible that its radiant energy was supplied by meteors. If the meteor feeding was in excess, the sun became larger in volume. If there was too little meteor feeding, the sun became smaller. Even if there was a very excessive supply for a short time, say by the incoming of a huge meteor, we need not assume excessive radiation in consequence. Such meteors may have come from stellar space with great initial velocities, and may have possessed before collision many times the kinetic energy which a mere solar system meteor of the same mass would possess.[1] If there were many such meteors, their paths might be enormously out of line with one another and with the centre of the sun, and yet we need not imagine them to alter much the moment of momentum of the sun about its axis. If we look for the *probable* age of the sun as deduced from mere physics, we ought to take Helmholtz' condition of mere mutual attraction, the Helmholtz calculation being corrected of course for greater internal density; but if we look for a higher limit to the age of the sun, it is difficult to see why we may not multiply Lord Kelvin's total energy and age of 500 million years.

Again, the ages determined by Von Helmholtz, Prof. Newcomb, and Lord Kelvin, are given on the uniformitarian assumption that the sun has been radiating energy always at his present rate. If we may imagine that for long periods the sun radiated at a smaller rate, whether because his mass was smaller, or because of his atmosphere, we again have an increase to the calculated age. Prof. Newcomb seems to have noticed this, and to meet the objection (p. 525, "Popular Astronomy") he says, "that a diminution of the solar heat by less than one-fourth of its amount would probably make our earth so cold, that all the water on its surface would freeze, while an increase by much more than one-half would probably boil the water all away." On account of this exigency, indeed, he reduces his previous estimate in the ratio of nine to five. This statement ought to have the careful consideration of men who know more about astronomical physics than I do. It means that if the earth were now $15\frac{1}{2}$ per cent. further away from the sun, there would be no water and no life, only ice; and if we were 18·4 per cent. nearer the sun, there would be again no water and no life, only steam. It becomes an important question, is there no life, is there no water on the planet Venus which has twice our solar radiation? Is all its water in its atmosphere as steam? Again, Mars has only 40 per cent. of our solar radiation; is there no life, no water, only ice upon Mars? I have no right to speak on such a subject, but I understood that the atmosphere of Venus was much like that of our own planet, and that the water of Mars is not all ice, for his polar

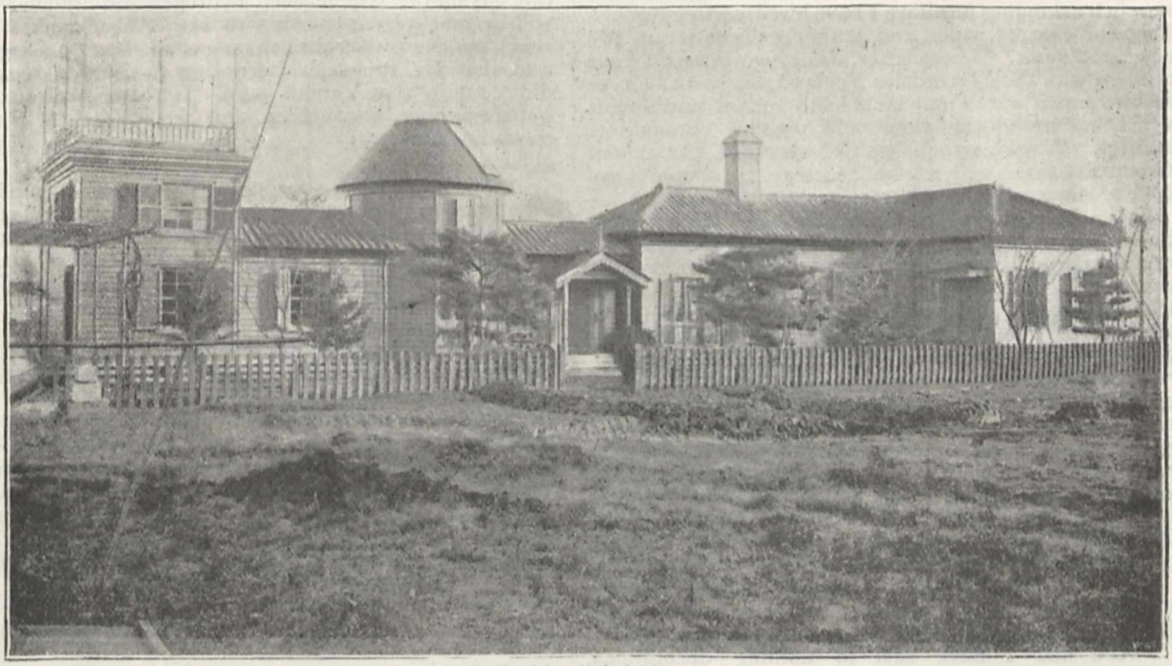
[1] The velocities of stars are probably much less than the possible velocities of smaller bodies.

snow-caps are seen to melt in summer. True, they may be solid carbonic acid, but I have recently read that the green colour of vegetation had been observed to appear and disappear regularly on the planet. If there is little water on the surface of Mars, I should imagine that this is rather due to its having soaked into the crust, which is probably colder underground than ours. Prof. Newcomb has evidently not thought of Mars in this connection, for elsewhere he says: "If there are any astronomers on Mars . . ." On this question I venture to quote Lord Kelvin, who said, in 1887 ("Pop. Lect.," vol. i. p. 376), that "the intensity of the solar radiation to the earth is $6\frac{1}{2}$ per cent. greater in January than in July; and neither at the equator nor in the northern or southern hemispheres has this difference been discovered by experience or general observation of any kind." It is difficult to imagine that if the effect of $6\frac{1}{2}$ per cent. cannot be detected, 25 per cent. should convert all the water to ice and destroy all life.

Even if a small diminution of the solar radiation produced a very cold climate on our present earth, we must remember that the earth's atmosphere may have been very different in the past; the earth may have been very greatly blanketed, and the surface may have been actually warmer, although there was much less solar radiation. That the atmosphere is far more important in this connection than the amount of solar radiation, is evident if we consider Langley's determination that in the tropics, if there were no atmosphere, the temperature of the surface of the earth would be $-200°$ C. Any addition to the quantity of air in our present atmosphere means an increase of the temperature of the rocky surface. But in the past, not only may there have been more atmosphere, but there may have been a very different kind of atmosphere. Again, we must consider a possible great amelioration of climate due to the earth's internal heat. It could not occur by mere conduction, but it is quite possible that for many millions of years there was great blanketing by clouds of watery vapour, and that underneath these blankets half the surface of the globe may have been a lake, or a number of lakes, of melted lava, which may have carried large amounts of heat convectively from considerable depths, this heat again being carried about convectively by the earth's atmosphere, keeping the solid parts of the earth's surface in a fit state for the existence of low forms of animal life. It is possible that at the present time the surface of Jupiter, which receives a very small intensity of solar radiation, may have solid parts surrounding watery lakes and oceans capable of supporting life because of the existence of many lakes of melted lava.

To sum up, we can find no published record of any lower maximum age of life on the earth as calculated by physicists (I leave out the estimates based upon the assumption of uniform density in the sun, and also that of Mr. Clarence King) than 400 million years. From the three physical arguments, Lord Kelvin's higher limits are 1000, 400, and 500 million years. I have shown that we have reasons for believing that the age, from all three, may be very considerably under-estimated. It is to be observed that if we exclude everything but the arguments from mere physics, the *probable* age of life on the earth is much less than any of the above estimates; but if the palæontologists have good reasons for demanding much greater times, I see nothing from the physicist's point of view which denies them four times the greatest of these estimates.

JOHN PERRY.



The Tokio Seismological Observatory.

THE SEISMOLOGICAL OBSERVATORY DESTROYED AT TOKIO.

THE destruction by fire of the Seismological Observatory and Library, at Tokio, Japan, has already been referred to in these columns (p. 533). The valuable work which Prof. Milne has accomplished during his long stay in Japan is well known to our readers; and it is to be hoped that means for its continuance will be fully provided. By the kindness of Japanese friends, Prof. Milne has been able to make observations in a temporary home since the fire, and it will not be for lack of enthusiasm and activity if a new observatory is not soon in working order. We print below extracts from Prof. Milne's optimistic account of the fire, and accompany it with a view of the Observatory buildings and house.

"It came at last. On Sunday morning, the 17th February, at 7.30 a.m., I was alarmed by the cry of fire, and at 8 o'clock I was looking at smoking ruins in the midst of my two armsful of salvage (which I took good care should include my last year's photographic records), receiving cards of condolence from high personages and presents from the neighbouring shop-keepers—like bottles of beer, boxes of eggs, and oranges—and all this while the fire engines were vigorously pumping. I saw that nothing more was to be saved, but before the flames were out, my colleague, Mr. C. D. West, set to work to take a series of snap-shots with his hand-camera. The results look like fogged plates, but they show amongst other things the effects of heat upon my big stone column. On Tuesday, I used some of the pieces as illustrations to a geological class as illustrative of the action of lava streams upon rocks they occasionally flow over. The President of the University, who very kindly had hurried from his house to see me and my ruins, had a new house ready for me before my own had done smoking, and I am now in it arranging furniture I have hired, sorting through heaps of charred paper, and getting ready to set up two new pendulums. These latter, if they work well, I hope to bring with their records to England, to show as a type of instrument which may be able to record movements which go round the globe and possibly through its interior. When earthquakes are recorded throughout Great Britain, it will not be necessary to always introduce a conversation with remarks about the weather.

"As nearly all the *Transactions* of the Seismological Society were packed up to go to Europe, a few that had middle places in the boxes may be saved, but I doubt if even out of 2500 copies I shall get more than two or three hundred. All my old earthquake books, some of which even dated from 1500 and 1600, but which were perhaps more curious than useful, seem to have gone.

"Instruments were fused or vapourised. Sixteen specially constructed clocks, which would turn drums once a day, once a week, or drive a band of paper for two years, together with seismographs and horizontal pendulums, self-recording thermometers and barometers, microscopes, and a museum of old and new contrivances, are now on the scrap heap. Until to-day, I felt that I had the observatory I intended to put up in England completely furnished, and I was proud of the furniture.

"The fire broke out in the midst of a pile of wood in an out-house, and this with a nice wind blowing on a Sunday morning when there was no one near to help.

"And now I have next to nothing—decorations, medals, diplomas, clothes, manuscripts, extending over twenty-five years, and everything else has gone to smoke; still it is not altogether a misfortune.

"Looked at in the right way, like an earthquake, a fire may after all be a blessing in disguise; but, of course, it is sometimes pretty well wrapped up.

"Dies iræ, dies illa,
Solvet saeclum in favilla."

TERRESTRIAL HELIUM?

I HAVE received the following letter and enclosure from Prof. Thorpe:—

"University of Glasgow, April 16.

"MY DEAR LOCKYER,—The enclosed extract from a letter just received from Cleve of Upsala may be of interest to you.

"Ever yours,
"T. E. THORPE."

"I have got from Mr. Crookes a letter in which he informs me that the gas in Cleveite contains the long-searched for helium.

"This letter arrived exactly the very day one of my pupils, Mr. Langlett, tried to get the gas of Cleveite in my laboratory. The gas given off from my mineral did not contain a trace of argon. The spectrum has been examined by Thalén, who found an exact coincidence of the line of the gas with the helium line and besides some others:—

Wave-length.	Intensity.
6677	half-strong
5875·9	strong: helium
5048	half-strong
5016	strong
4922	half-strong
4713·5	weaker

"I have sent a letter about it to Berthelot. If you like, you may communicate the result to the Chemical Society, Mr. Ramsay, Crookes, and other friends. . . . An experiment to determine the specific gravity did not give trustworthy results, but seems to indicate that it is a very light gas, still more heavy than hydrogen. Will this gas fill the gap between hydrogen and lithium? It will become very interesting to see. What makes me much curious is that our helium gas was free from argon, and that Mr. Ramsay's (according to *Comptes rendus*) did contain that curious stuff. Is there any relation between argon and helium, and are we facing a new epoch in chemistry?"

Although my results are not yet complete for publication, the foregoing communication makes it desirable that I should state at once that immediately on the publication of Prof. Ramsay's statement, by the kindness of Mr. L. Fletcher I was enabled to study the gases given off by Cleveite by heating in vacuo, a method I have used for metals and meteorites.

A very small quantity of Cleveite is all that is necessary to obtain a considerable volume of the new gas, which comes off associated with hydrogen.

I have now examined several tubes. I have found no argon lines; I have not found the lines, other than the yellow one, given by Crookes; but lines have been recorded near some of the wave-lengths given by Thalén, especially the one at 6677, near a line I discovered in the chromosphere in 1868. So far the sky has not been clear enough to enable me to determine by direct comparison with the chromosphere the position of the line in the yellow with great dispersion.

J. NORMAN LOCKYER.

NOTES.

IN honour of M. Berthelot, and as a demonstration of the power and progress of science in France, a banquet was held at Paris a few days ago. Nearly eight hundred guests were present, among them being M. Brisson, President of the Chamber of Deputies, and M. Poincaré, Minister of Public Instruction. Upon the invitation cards were printed the words: "Hommage à la science, source de l'affranchissement de la pensée." M. Poincaré made an eloquent speech in praise of the work done by the eminent Secretary of the Paris Academy of Sciences, and M. Berthelot, in his reply, dwelt, at some length, upon the beneficial influence of science on social and moral, as well as material, progress. Science, he said, had for its only guide the love of truth, and confidence in its final triumph. Proved under all circumstances, and strengthened every day by success, the scientific method had become the principal source of the moral and material progress of society. In fact, science was the source of all progress accomplished by the human race. Every one knew that, during this century science had conferred great benefits upon civilised peoples by the application of its results and laws to mechanical, chemical, and electrical industries. But M. Berthelot held that material progress was the least of the fruits of scientific work; he claimed for science the more extensive domains of the moral and social world, and vindicated the position taken up by him in his article on "Science et la Morale," which appeared in the *Revue de Paris*. The speech and the banquet may be taken as an effectual reply to those who question the benefits of scientific investigation in France.

ONLY last week, in announcing the publication of the fourth edition of Prof. James D. Dana's "Manual of Geology," we referred to the extraordinary activity of the author. We regret this week to have to record his death, at eighty-three years of age. Another eminent man of science who has just passed away is Prof. Lothar von Meyer, at the age of sixty-five.

REUTER'S correspondent at Toronto reports that the Provincial Legislative Assembly of Ontario has authorised a grant of 7500 dollars towards defraying the expenses of a meeting of the British Association for the Advancement of Science to be held in Toronto in 1897, should the Association accept the invitation to hold a meeting there.

MR. R. FITCH, whose name is especially well known among archæologists and geologists of Norfolk and Norwich, and who, three years ago, presented his collections to the Norwich Museum, and provided cases for them, has just died, at the advanced age of ninety-three.

JOHN ADAMS RYDER, Professor of Embryology at the University of Pennsylvania, died on March 26.

PROF. JAMES E. OLIVER, the mathematician, who was connected with the Cornell University faculty for twenty-five years, died at Ithaca, on March 27. He was born in Portland, Me., July 27, 1829. He graduated at Harvard in 1849, and received in the same year the appointment of assistant editor in the office of the American Nautical Almanac. He became in 1871 assistant professor of mathematics at Cornell University, and in 1873 was appointed to the chair as Professor. He was a member of the American Academy of Arts and Sciences, the American Philosophical Society, and the National Academy of Sciences.

A MONUMENT to the late Prof. Villemin, who added so much to the knowledge of the nature of tuberculosis, has just been unveiled at Val-de-Grâce. A monumental souvenir of the late Prof. G. Pouchet was also unveiled a few days ago, at Père-Lachaise.

THE Home Secretary, on the application of the East Riding County Council, has made an order prohibiting the taking or destroying of wild birds' eggs on the promontory of Spurn for a period of five years. Spurn Point is one of the chief places of deposit by sea birds of their eggs on the Yorkshire coast, and of late years there has been wanton destruction of both sea-gulls and their eggs.

A CORRESPONDENT informs us that a very bright meteor was seen at Tayport, N.B., at 10h. 4m. p.m., on Saturday, April 13. It started at a point a little to the east of Vega, and was moving almost directly towards Saturn. It was visible for about six seconds, and seemed to have a jerky motion. It was more brilliant than Venus, and was elongated in shape. The weather was very clear and quiet at the time of observation. The meteor's direction of flight was, approximately, from 270° +40° to 221° − 7°.

SIR JOSEPH FAYRER, Sir Guyer Hunter, and Mr. Jonathan Hutchinson, the adjudicators, have (says the *British Medical Journal*) awarded a prize of fifty guineas to each of the following essays, sent in in response to the invitation of the Leprosy Fund: "On the history of the decline and final extinction of leprosy as an endemic disease in the British Islands," by Dr. George Newman; "On the conditions under which leprosy has declined in Iceland and on the extent of its former and present prevalence," Dr. Edward Ehlers (Copenhagen); "On the facts as to the recent increase of leprosy at the Cape and its present prevalence in South Africa," Dr. S. P. Impey (Medical Superintendent, Robbin Island); "On the reputed recent increase of leprosy on the Australian Continent; its extent and possible causes," Dr. Ashburton Thompson; "On the conditions under which leprosy prevails in China, Cochin China, Batavia, and the Malay Peninsula," Dr. James Cantlie (Hong Kong). On some of the subjects for which prizes were offered no essays were sent in, and some of the essays sent in were held not to meet the terms of the competition. The successful essays will be printed and published at the expense of the Fund.

ABOUT 11.15 on Sunday night, shocks of earthquake of varying intensity were felt over a considerable portion of Austria-Hungary, extending from Vienna to the Adriatic coast and the adjoining islands in one direction, and from Salzburg to Agram in another. In Vienna, according to the *Times* correspondent, the shocks were slight. Clocks were stopped, however, in parts of the town, and windows were heard to rattle. The vibrations are reported to have proceeded from south to north. The earthquake would seem to have reached its greatest intensity within a triangle formed by Laibach, Trieste, and Fiume. At Laibach no fewer than twenty-five shocks were noticed, and they continued until seven o'clock on Monday morning. The collections in the Laibach Museum are said to have been almost entirely destroyed. At Trieste several vibrations were felt, one of which was of ten seconds duration. Many buildings have been more or less damaged. Tremors of still longer duration are reported from Krainburg. Shocks of less violence occurred at Klagenfurt, Leibnitz, Luttenberg, Agram, and elsewhere. Four distinct vibrations were felt at Venice: the first, which took place at 11.17 p.m., was very severe, and lasted twelve seconds. Severe shocks of earthquake were also felt in Italy, at Ferrara, Udine, Treviso, and Padua.

WE have on various occasions given short descriptions in these pages of experiments carried on by different investigators with a view to discovering a practicable method of telegraphing over a considerable distance without metallic wires connecting the two stations. Among the observers who have devoted a considerable time to this problem may be mentioned Mr. Preece, the chief engineer of the Telegraph Department of the Post Office; and it is to this gentleman we owe the first practical application of the method of telegraphing by induction. During last week the submarine cable connecting Oban with Auchnacraig broke down, and the telegraphic messages have since been passed between these two places (distant about six miles) by means of Mr. Preece's inductive method. A gutta-percha insulated wire, one and a half miles long, was laid along the ground from Morven, whilst on the Island of Mull use was made of the ordinary overhead line connecting Craignure with Aros. The distance between the two parallel wires was about 3½ miles, the Sound of Mull being here at its narrowest. Using a vibrator as transmitter, and a telephone as receiver, the usual messages were successfully dealt with until the cable was repaired, the whole experiment forming a very interesting event in the annals of telegraphy.

A paper which will have considerable interest for those who have to design and use electrical apparatus, where wood or slate is often used as the insulator, appears in the *Proceedings* of the American Academy. The subject of the paper is the electrical resistance of certain poor conductors, such as wood and stone, and is by Mr. B. O. Peirce. The author has examined a great number of samples of different kinds of woods and stones under different conditions as to dryness, a most important point, and although, as might be expected, the individual results differ somewhat widely, he considers the mean results give a very fair idea of what may be expected in practice. The author has also tried the effect of soaking the different materials in hot melted paraffin, and finds in every case, especially if the specimen has been previously well dried, that such treatment not only increases the specific resistance, but by preventing the absorption of moisture prevents the falling off in the resistance otherwise observed when such bodies as stone or wood are exposed in a damp place. The following are some of the results for the specific resistance obtained in megohms; the first number denotes in each case the lowest value observed, and the second number the mean value:—Mahogany 310, 610; hard pine 17, 1050; white pine 360, 1470; vulcanised fibre 3, 60; slate 184, 280; white marble 2000, 8800. The samples of wood were all well seasoned, and the resistance was measured in the direction of the grain, the resistance across the grain being generally from 20 to 50 per cent. higher. The samples of stone were dried in the sun for about three weeks before being tested.

A simplified phonograph is described by A. Költzow in the *Centralzeitung für Optik und Mechanik.* A conical tracing point is used for recording the sound waves. This has the advantage of durability, and if it should wear out on one side, it need only be turned round its axis. The tracing-point is attached to one arm of a lever, the second and longer arm being provided with a membrane. For some purposes the membrane is replaced by a stretched string. The cylinders consist of a very hard soap. They will stand several hundred renderings. After use they can be turned down by 0·02 mm., so that one cylinder will suffice for 200,000 words.

In the *Comptes rendus* of the Paris Academy of Sciences of the 1st inst., as briefly stated in our abstract last week (p. 576), Prof. Mascart presented a note by the Abbé Maze, stating that in a collection of astronomical documents at the National Observatory, a register had been found containing thermometrical and other observations made by the astronomer I. Boulliau, between 25 May, 1658, and 19 September, 1660. Up to the present time, it was not known that observations had been made at Paris prior to those of Lahire. These observations are of some interest, being among the earliest thermometrical readings on the continent, and they fill up a gap in the climatological history of Paris. It is also noteworthy that the thermometer used was one of the Academy del Cimento.

A recent number of *Modern Medicine and Bacteriological Review* contains a notice of Dr. Pictet's interesting experiments on the application of intense cold as a therapeutic measure. According to this investigator's observations, calorific radiations of a lower temperature than − 65° pass through all the ordinary conductors of heat; a fur overcoat or a wooden board offering no more resistance than a pane of glass or other transparent medium to the passage of a sunbeam. Dr. Pictet experimented upon himself in a frigorific well, in which a temperature of − 100° C. to 110° C. was maintained. He was wrapped in warm clothes and thick furs, and at the end of four minutes he stated that he experienced intense hunger, which increased; after eight applications of eight or ten minutes duration his appetite became normal, and his digestion greatly improved.

The last number of the *Bolletino* of the Italian Geographical Society criticises the proposed nomenclature of some of the rivers of East Africa, as given in a sketch map in the *Geographical Journal* illustrating the explorations of the American traveller, Dr. Donaldson Smith. The names to which the Italian Society objects are those of Dr. Smith himself and of his English companion, Mr. Gillett, which have been applied to the upper and middle course of the Webi Shebeli and the Web respectively. We are bound to admit that the criticism is well-founded, as it is contrary to authorised usage to apply European names when the native names can be ascertained. We have little doubt that Dr. Smith will, before the end of his explorations, discover some features still unknown which may fitly be called after him and perpetuate the memory of his excellent geographical work.

Mr. H. Rutgers Marshall, whose work on "Pain, Pleasure, and Æsthetics" was reviewed in Nature (vol. l. p. 3), contributes to the April number of *Mind* an article, "Emotions *versus* Pleasure-Pain," in further elucidation of his views. Mr. Marshall is fully alive to the importance of evolutionary development, and his treatment of the emotions is therefore of interest to students of comparative psychology. It is unfortunate, however, that the term "instinct" is used in an extended sense which will scarcely be acceptable to those who approach the subject from the biological side. The phrases, "imitation impulse," "art impulse," "benevolent impulse," would appeal to them as more satisfactory than "imitation instinct," "art nstinct," &c., since they have grown accustomed to the application of the term instinct to the manifestation of particular activities. But a consensus of opinion on psychological nomenclature seems at present impossible. And in any case, Mr. Marshall's views are well worthy of careful consideration.

An experiment of considerable interest in connection with the transmission of optical signals has been performed by M. Charles Henry at the Depôt des Phares. The question was whether rhythm in a succession of signals increases or diminishes their visibility? This was solved by means of a revolving drum, the surface of which contained sixty holes illuminated by a source of light placed at the axle. The drum was 3 feet across, and was driven by clockwork. By varying the speed of the drum and the brightness of the light, and by closing some of the holes at regular or irregular intervals, all the conditions of the experiment could be varied at pleasure. The chief difficulty in this, as in most physiological experiments, lay in bringing the eye back to its original state after each experiment. It was sometimes found impossible, even after keeping in the dark for close upon half an hour, to restore the observer's eye to its original state of sensibility. But it was conclusively shown that it is possible to increase the range through which an optical signal will carry by arranging the succession of flashes according to a sufficiently complex non-rhythmical law.

Probably no better example of an invention borrowed or adapted from nature could be found than is afforded by the sand-blast. As is well known, the invention, now a quarter of a century old, consists of a stream of sand or other abrasive powder, usually dry, but sometimes mixed with water, projected with more or less force and velocity to strike and pulverise the surfaces of glass, stone, metal, and other materials upon which it is directed. The many applications of this method of abrasion were recently described by Mr. J. J. Holtzapffel, at the Society of Arts. It appears that glass is almost immediately depolished by the blasts now in use, and but a comparatively short time is required to pierce and cut apertures through sheet and plate glass. Stone, marble, slate, and granite are just as amenable to the action of the sand-blast. Iron, steel, and other metals have their surfaces easily reduced, and smoothly or coarsely granulated, according to the force and abrasive powder used. It is remarkable that it is by no means necessary that the abrasive be harder than the material to which it is applied; thus, hardened steel and corundum are readily pierced with sand. The blast is not only in use for producing a uniform granulation on sheet glass; it is also employed for frosting the bubbles of incandescent lamps and the like; for the decoration of glass ware, and the labelling of graduated measures. In metal, the hard scale, so destructive to cutting tools, is removed from castings and forgings by the blast. On stone, slate, and granite the sand-blast is used for incised carving and inscriptions in intaglio or relief, and for delicate drawing for lithography. A print of a child's head, exhibited by Mr. Holtzapffel, was an astonishing example of the delicacy of treatment obtainable by the process. Among other purposes, the blast is employed for removing fur and deposits in tubes and tanks; for cleaning off accumulations of paint and dirt within iron ships; for decorating coat and other buttons; for piercing the apertures in glass ventilators; for marking pottery, and in the manufacture of ornamental tiles; for refacing grindstones, emery, and corundum wheels; for granulating celluloid films for photography; and on wood, to bring out the grain in relief, and, latterly, for blocks for printing. These many and various applications of sand-blast processes show that the art has developed in an extraordinary manner since it was introduced by Mr. Tilghmann in 1870.

DR. H. WILD has published, in the *Zapiski* of the St. Petersburg Academy of Sciences, a very important investigation, entitled "New Normal Air-Temperatures for the Russian Empire." In a former paper upon this subject, the data for the monthly, yearly, and five-yearly means were brought down to the year 1875, while in the present work observations have been included to the year 1890, and comprise materials from no less than 575 stations, of which 244 are new.

THE additions to the Zoological Society's Gardens during the past week include an Irish Stoat (*Putorius hibernicus*) from Ireland, presented by Viscount Powerscourt; a Grey Parrot (*Psittacus erithacus*) from West Africa, presented by Mr. A. A. Dowty; a Cape Viper (*Causus rhombeatus*) from South Africa, presented by Mr. J. E. Matcham; two Elephantine Tortoises (*Testudo elephantina*) from the Aldabra Islands, Seychelles; four Indian Pythons (*Python molurus*) from India, deposited; a Barbary Wild Sheep (*Ovis tragelaphus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

LUNAR RIVER BEDS AND VARIABLE SPOTS.—The highly favourable atmospheric conditions at Arequipa have enabled Prof. W. H. Pickering to make numerous observations which have a special bearing on the question of the existence of water on the moon (*Annals Harvard College Observatory*, vol. xxxii. part 1). In addition to the ordinary rills, Prof. Pickering has catalogued thirty-five narrower ones, which, from their resemblance to terrestrial watercourses, he does not hesitate to regard as "river beds." These are wider at one end than at the other, and the wide end always terminates in a pear-shaped craterlet. Most of them are only a few miles in length, and a few hundred feet in width at the widest part, and, except when very deep, they are very difficult objects. The largest and most readily observed has its origin in the Mount Hadley range n the Apennines; its course lies a little north of west, and its otal length is about sixty-five miles. There does not appear to be any reason to suppose that these formations actually contain water at the present time, but Prof. Pickering brings forward other evidence in favour of the presence of a small amount of moisture on the lunar surface.

Certain variable dark spots have been detected in different regions, many of them lying inside craters, others symmetrically surrounding craterlets, and others in the dark *maria*, or "seas." In the central craters, such as Alphonsus, the spots are darkest just after full moon, when shadows are geometrically impossible, and they are invisible when the shadows are strongest. "If called upon to offer an explanation of the phenomenon, we seem forced to call in the aid of water as an active agent." Still, the dark spots cannot be simply ponds, as one of the spots in Alphonsus for a portion of the time covers and darkens the slopes of a small hill near the crater-wall. "This seems to effectually overthrow the hypothesis of a free liquid surface, as well as the suggestion that the dark colouration may be due to frozen ground that has partially thawed. . . . Vegetation would undoubtedly explain away all our difficulties; but before we resort to such an extremity, it is evident that we need more facts upon which to base our theories."

The Mare Tranquilitatis is said to be almost covered by these variable spots, and Prof. Pickering states that the changes may be seen with the smallest telescope, or even with the naked eye; until past first quarter this area is lighter than the Mare Crisium; it then rapidly becomes the darker of the two until after full moon, when it again becomes lighter.

The changes in some of the spots are readily seen in the beautiful photographs which illustrate the memoir.

THE ULTRA-VIOLET SPECTRUM OF THE CORONA.—With spectroscopes in which the optical parts are made of glass, it is only possible to photograph the spectrum in the ultra-violet as far as wave-length 360; but when the spectroscopic train consists of quartz and Iceland spar, a more refrangible region is open to investigation. One of the spectroscopes employed in Africa by M. Deslandres during the total eclipse of the sun on April 16, 1893, was of the latter form, and a plate exposed for four minutes gives for the first time some information as to the coronal spectrum in the extreme ultra-violet (*Comptes rendus*, April 1). The slit of the spectroscope cut the image of the corona along the equatorial diameter, and to facilitate the reduction of wave-lengths, the spark-spectrum of iron was photographed on the same plate. The photograph shows the spectrum of the corona to consist of bright lines superposed on a continuous spectrum. In the blue, the continuous spectrum reaches a height equal to two-thirds the sun's diameter, but it diminishes both in height and intensity until about λ 300 it is almost reduced to zero. Forty lines are tabulated from λ 308 to λ 362; one at λ 3170·9 appears to reach a great height in the solar atmosphere, and others at λ 3164·5, 3189·5, and 3237·1, are comparable with the hydrogen lines H_δ, H_ϵ, and H_ζ. The remaining lines may belong either to the chromosphere or corona; but M. Deslandres considers the fact that they are shown with a small image on the slit, to be in favour of the view that they are coronal. Most of the lines cannot be identified with known substances, and they probably represent gases of low atomic weight.

STELLAR PARALLAXES.—A very suggestive investigation of stellar parallaxes in relation to magnitudes and proper motions, has been carried out by Mr. T. Lewis (*Observatory*, April). The parallaxes adopted are the means of the values obtained by various observers, and from these the velocities across the line of sight have been derived by dividing the proper motions by the parallaxes and reducing to miles per second. The conclusions suggested by the tables given are: "(1) Leaving out a few of the brightest stars, the parallaxes are constant down to 2·7 magnitude. (2) After 2·7 mag. is reached, the parallaxes are doubled and remain practically constant to 8·4 mag. (3) Up to the 3rd mag. the velocities are very small, averaging about 9 miles per second, while after the 3rd mag. the velocity is 38 miles per second." It seems probable that in our immediate neighbourhood there are a few stars of exceptional brilliancy (about 8) and a few smaller stars, of which nearly 40 are at present known; while stars of mag. 1 to 3 are as a class far outside this inner space, and have very small velocities. The investigation confirms the accepted idea that a measurable parallax accompanies a large proper motion, and shows, further, that this holds good whatever the magnitude of the star may be.

THE SUN'S PLACE IN NATURE.[1]

IV.

THE difference in the appearance of spectral lines and flutings having been explained, I now go on to state that the luminosity referred to, as seen in the meteorite experiment was not one of the lines in the spectrum of magnesium, but one of the flutings. I will next throw this on the screen (Fig. 17), and you will at once see the point. Here is the spectrum of magnesium obtained at the lowest temperature at which we can get any light from it at all. We have a fluting, which resembles closely the carbon fluting, but in a different part of the spectrum. We see that its brightest part is coincident with a certain part of the solar spectrum; and it so happens that the position of the line which Dr. Huggins had observed in the nebulæ lies very near the same position of the solar spectrum.

That, then, was one argument out of a great number in favour of the view that the luminosity to which the bright line of the nebula was due, might really be produced in the nebulæ by collisions of meteorites among themselves, rendering luminous the vapours of magnesium which we knew to be wherever there are meteorites.

Now, an additional argument for that view was found in the fact that almost every observer, including Dr. Huggins himself, had stated that as seen in the spectrum of a nebula the line did look somewhat different on one side to what it did on the other, and references were made to its being more degraded on the left-hand side than it was on the right. I had frequently

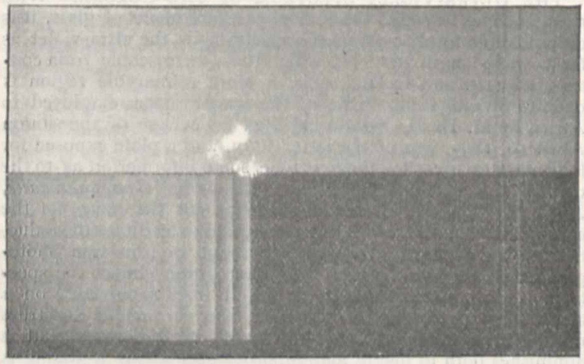


FIG. 17.—Spectra of burning magnesium compared with solar spectrum. (1) Sun. (2) Magnesium.

observed myself that the line representing the chief line of the nebulæ was degraded to the left, never to the right, over parts of the nebula of Orion which were more brilliant than the others; and at the same time that another line—about which more presently—instead of being degraded to the left, like this one, was equally eased off on both sides (Fig. 18), so that the argument was complete that the appearances presented by this line were not due to any instrumental defect, because, in that case, all the lines would have behaved in the same abnormal manner. Hence then I found myself justified in concluding and subsequently stating (1) that the position of the meteoritic dust-line was coincident with the line of the nebulæ in the apparatus which I used, and (2) that it resembled it in appearance.

What I had next to do in the matter was, of course, to carry the thing as far towards the truth as I could. We can never find out the whole truth, but it is better to have a part of the truth than none at all. Hence I started a new method of attack, which, you will note, differs very considerably from anything you have seen before. I have here a beautiful instrument invented by that eminent Frenchman Foucault, called a "siderostat." The essential part is a plane mirror (Fig. 19) which, when it is properly adjusted to the sun or moon or any star in any part of the sky, lays hold of a beam of light from it about twelve inches in diameter, and sends that beam in a horizontal direction due south, and keeps it there; so that the light falls fairly on the optical apparatus, and we can go on observing it for a long time. Next the instrument was adjusted to throw the light from the nebula of Orion on a powerful horizontal telescope placed in front of a large spectroscope, both rigidly fixed. In order to check the observations as far as possible, I placed in front of the object-glass of the telescope an arrangement by which the light from a magnesium wire might enter the slit of the spectroscope at the same time as the light of the nebula, so that if the light from the nebula and the light from the magnesium wire perfectly agreed in wave-length, we should get one line; if it differed, we should get two.

The slit of this spectroscope was exactly in the focus of the ten-inch object-glass, and then the light was passed through four dense prisms, so that we got a considerable amount of dispersion, and the exact position of the line, whether single or double, was observed. That of course was a very much more powerful dispersion than had been employed by Dr. Huggins in his first observations, and much more powerful than had been employed by myself in my first investigation. But what I wished to do in those first investigations was to understand and to clearly follow the observations which had been made previously by others; if therefore I had attempted to go over the ground with instruments ten times better, giving me ten times finer results than my predecessors had obtained, it would have been the worst possible way to go to work, because it was essential for me to make the necessary comparisons with the old observations while not exceeding the instrumental means which had been employed to obtain them.

The long and short of my various methods of observation was that they seemed entirely to confirm the idea which I got in the first instance from using telescopes and spectroscopes of very much smaller power. That, however, fortunately, for

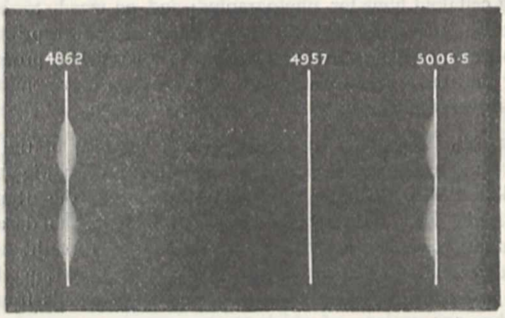


FIG. 18.—Appearance of principal lines in spectrum of Orion nebula, as observed at Westgate-on-Sea.

science, did not satisfy Dr. Huggins; he very wisely appealed to the American astronomers, and I am glad to say that the skilful astronomers of the Lick Observatory took up this work with interest, and employed instruments in the investigation more powerful than any I possessed, thus carrying matters a stage further. There were really two distinct bits of work to be done: first of all, one wanted the exact position of the line in the nebulæ, and after having got its right position, its origin could be thought out. We wanted also to see what the real physical appearance of the line was, *i.e.* whether it was most likely a line or a fluting. It is not a little curious to note that all the statements which had been made suggesting a fluted character of the line were at once withdrawn when I referred its origin to the magnesium fluting.

The Lick telescope is one of very considerable power indeed, and it is so solidly built that a very powerful spectroscope can be put on one end of it and used under almost the best possible conditions for determining the position of lines. Still the Lick telescope is not the best possible telescope to employ for any branch of work connected with nebulæ, if the work requires a great amount of light, because the longer the telescope, the larger the image which the object-glass gives; for instance, if you are dealing with a nebula one degree in diameter, if your one degree is written on a circle with a radius of sixty feet, it will be a very much bigger thing than if on a radius of ten feet, so you get a large image without increasing the light, and therefore are spreading your light over a very large area. As the slit of the spectroscope is a very small thing, all the light which is thrown outside the slit is of no use for your spectroscopic observation, so, whatever the size of the spectroscope may be, you want to deal with the smallest and

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 567.)

brightest possible image in order to get the best use out of your spectroscope, and that cannot be done with a long focus telescope. However, the important question for the American observers was to determine the exact position of the line; and we have lately been given some very interesting results.

Fig. 20 represents the way in which Dr. Keeler puts his last result. The upper part is a representation of the solar spectrum; the numbers represent wave-lengths on Rowland's scale. According to his latest value the wave-length of the nebular line is 5007·05. He also shows in relation to it the lines of nitrogen as well as the fluting of magnesium, and you see at once that, although according to this drawing the magnesium does not quite correspond with the line of the nebulæ, it is very much nearer to it than is either the line of lead or the lines of nitrogen. The publication of this result necessitated a fresh investigation, to see what the exact facts were when we no longer compared the nebula with magnesium, but compared the magnesium with the solar spectrum, and therefore sought the true position in which to place the magnesium line in relation to the solar spectrum.

Here is the result. You will see that there is a very small difference between the position of the magnesium fluting and the nebular line. In short, the more the work done on this question the more and more coincident have these lines become, and there are some considerations which have not yet been taken into account.

I have referred to this point somewhat at length, although the coincidence with the fluting of magnesium is not fundamental for the establishment of the view which even Dr. Huggins has now accepted, in the way I have already stated.

Now, what was the chemical constitution of the nebulæ stated to be as a result of the first spectroscopic observations? Dr. Huggins, in his paper of 1865, to which I have already referred, was of opinion that the chief line was due to nitrogen.

Here are two tubes, one containing hydrogen, the other nitrogen. You will see at once that these two gases, when I set them glowing by the passage of an electric current, are very different in colour; we get in one an excess of red light, in the other we have a purple tinge. When nitrogen is observed by means of a spectroscope, a double line is seen very nearly coincident with the line of the nebulæ. Dr. Huggins thought that one of the constituent lines was exactly coincident with it, and because there was apparently no line whatever corresponding with the other, he thought also that the nitrogen might not be nitrogen like that with which we are familiar, but an unknown form of it. There was no doubt from the beginning that another line was a line of hydrogen, although there was some slight doubt as to whether the hydrogen in the nebulæ behaved exactly like hydrogen on the earth.

Nobody believes in the nitrogen constituent of the nebulæ now; and I presume Dr. Huggins has withdrawn in fact, if not in words, his statement concerning the coincidence, for in his address as President of the British Association, in which, as I have already stated, he withdrew his published statement as to the position of the nebulæ among the various bodies that people space, he remarks, "the progress of science has been greatly retarded by resting important conclusions upon the apparent coincidence of single lines in spectroscopes of very small resolving power," an *apologia* of which every one will see the propriety, for you will gather from Dr. Keeler's diagram that the nearest nitrogen line is three times further removed from his position of the nebula line than is the magnesium fluting. I trust I shall not be thought to be exceeding the bounds of decorous criticism when I remark that while Dr. Huggins has referred to the inaccuracy of my work in relation to this line, which is apparently indicated by Dr. Keeler's results, he has never pointed out the three times greater inaccuracy of his own.

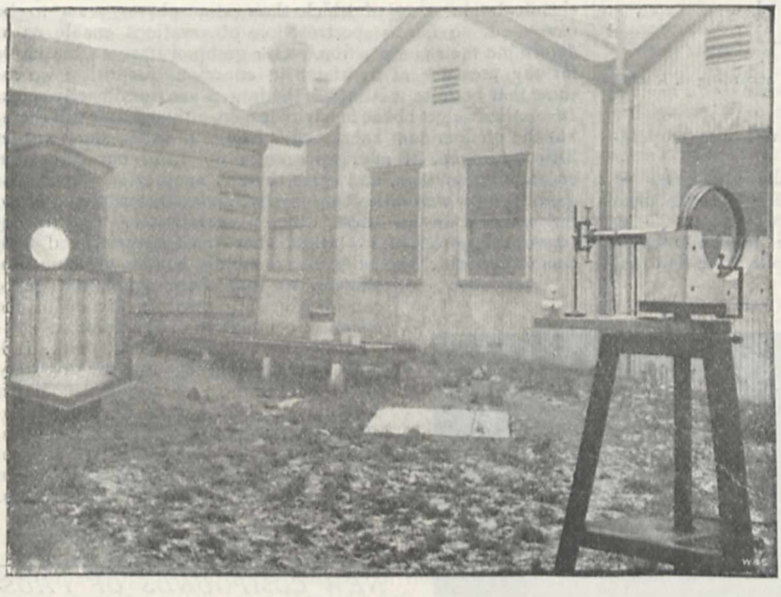


FIG. 19.—Showing object-glass of horizontal telescope used with siderostat.

In order to give you an idea of the relative accuracy which all these references to wave-length indicate, let us suppose that we are trying to define the position of a place in London on an E. and W. line running through Charing Cross, and then you will see exactly how matters stand. Assuming Dr. Keeler's value to be absolutely true—and I expect it is as near the truth as we are likely to get for some time—we will suppose that it represents the nebular line situated on the statue of King Charles at Charing Cross. When Mr. Huggins first measured it, he brought it to the East India Docks; his next attempt brought it to Hammersmith. Dr. Keeler's first observation brought it to Albert Gate; his next, in 1891, brought it to St. James's Palace. Subsequent work at Kensington, not yet completed, has brought it nearer still.

There is another argument in favour of the now accepted view which may be gathered from a careful examination of the forms of these different nebulæ, and by endeavouring to reason out from the form what the actual conditions at work may be. One of the most wonderful spiral nebulæ in the heavens is that in the constellation of Canes Vanatici, which has been photographed by Dr. Roberts (Fig. 7, p. 397). This is a nebula which we look down upon; we see it in plan; we are, so to speak, at the pole of the system, so that it is not foreshortened.

There is no question about the wonderful spirals being connected with the central condensation and stretching away from it, and the point which I made with regard to the one in Ursa Major is even more decided here, when I call your attention to these points of condensation right along one of the spiral branches, and when you get the possible intrusion of two spirals one on the other you see a confused mass of light. Now, if we imagine ourselves dealing there with a mass of pure gas, whether it is hydrogen or nitrogen or ammonia—that is, a combination of both—or any other, it would be extremely difficult to see why there should be any change in temperature in different parts of that mass; but the moment you assume that you are dealing with cool materials—meteoritic dust—you will see that such a picture as this is important to us, for the reason not that it shows us what is there, but because it shows us what is going on there. These bright spots do not represent the presence of matter, and the dark ones the absence of matter; but these brighter portions represent the stream lines where collision is possible—the intervals those regions where collisions are less likely, and you will see from the very configuration of this system, that if all the dust, or meteorites, or conglomerations of particles, whatever they may be, are going the same way, there will be a condition in which we shall get a minimum of collisions, and therefore a minimum of temperature.

The probability, therefore, is that we are not dealing with gas, but with masses of matter in certain regions of which, in consequence of general action, there is greater luminosity given off by the particles of which the nebulæ are composed; in

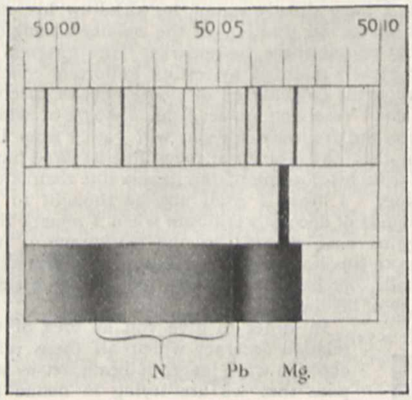


Fig. 20.—Normal position of the chief nebular line, according to Keeler.

other regions where there is less action, we have lower temperature and less light.

If, as was at first imagined, these nebulæ are gases at enormous temperatures, it would be a question of seeing them or not seeing them; there would be no special parts to be picked out at all. But, in the case of those nebulæ to which modern photographic methods have been applied, we find that

Fig. 21.—Orion nebula photographed with short and long exposures.

the nebula which we see ordinarily in our telescopes is only a very, very small fraction of the real nebula as it really exists, when we can get at it under the best possible observing conditions. Many of you, I hope, have seen the nebula of Orion in an ordinary telescope. Here it is as it has been photographed by means of a telescope powerful enough to give us the brighter portions. Here is another photograph of the nebula exactly on the same scale, in which the nebula that we usually see occupies only a very small portion; the only difference between the two photographs is that one has been exposed for a very long time to enable us to fix and to study the very dim reproduction of certain parts of it, whilst the first one was exposed only for a short time, in order that we might dwell effectively on that part only of the nebula which is generally visible to the human eye with an ordinary telescope. (Fig. 2.) If we were dealing with incandescent gas, the incandescent gas ought to leave off suddenly; but all round this nebula, where there appears to be nothing at all, the longer exposure brings before us other portions of the nebula just as rich in details, just as exquisite in their variety and tone as those ordinarily seen with the naked eye. Such a condition as that cannot be brought about by a mere homogeneous mass of gas at high temperature, but we can explain it quite easily by assuming that in such a nebula as that we are dealing with, the luminosity is brought about by disturbances, these disturbances giving rise to collisions among the particles which are apt to collide and give out luminosity. The nearer they are to the centre of gravity of the swarm, the greater will be their chance of collision, and the greater will be the luminosity of their central portion.

Still another consideration. Astronomers, since the time of Rutherfurd, who was the first to begin stellar spectroscopic work in the United States, between 1860 and 1870, have established many different classes of stars as defined by the chemical substances of which their atmospheres seem to be composed, so far as spectroscopic observations enable us to determine their composition. One group of stars is remarkable for the presence of hydrogen in enormous quantities; we assume that because the lines of hydrogen are inordinately thick. In another we get not so much hydrogen, although it is still there, but the predominant substance is iron. In other stars we get little hydrogen, if any, apparently no iron, but carbon in enormous quantities, and again there is another substance, the quantity of which varies enormously, and that is calcium. Now, if stars contain all these different substances, and if they represent epochs of evolution, they must be produced from something which actually or potentially contained these substances, so that there again you get a considerable argument in favour of the chemical complexity of the nebulæ.

Finally, we reach the second point. It is now generally conceded that the first stage in the development of cosmical bodies is not a hot gas, but a swarm of cold meteorites. From the point of view of evolution, keeping well in touch with the laws of thermodynamics, the nebulæ must begin cool if they are to develop into hot stars.

J. Norman Lockyer.

(*To be continued*).

NEW COMPOUNDS OF PHOSPHORUS, NITROGEN, AND CHLORINE.

A series of new compounds of phosphorus, nitrogen, and chlorine, and likewise a series of acids derived from them, have been discovered by Mr. H. N. Stokes, and an account of them is contributed to the *American Chemical Journal*. A familiar compound of the three elements in question, the chlorophosphuret of nitrogen, discovered by Liebig in 1832, has been the subject of frequent study, and its nature has comparatively recently been very fully demonstrated by Dr. Gladstone. It has been shown, from vapour density determinations, that this remarkably stable compound, which may be distilled in steam and boiled with acids and alkalies without appreciable change, possesses the molecular composition $P_3N_3Cl_6$. Mr. Stokes now shows that this substance is only one of a homologous series of compounds having the general formula $(PNCl_2)n$, and that these are the chlorides of a series of acids $(PNO_2H_2)n$, which he terms metaphosphimic acids. The second member of the series, $(PNCl_2)_4$, has been isolated from the product of Dr Gladstone's reaction for the preparation of $(PNCl_2)_3$, that be tween phosphorus pentachloride and ammonium chloride; it is a substance almost as stable as the triple compound, and yields on saponification the acid $(PNO_2H_2)_4$, which is likewise a stable body. Moreover, the acid corresponding to the triple compound has been isolated, and also a higher chloride $(PNCl_2)x$ of an oily character, and whose molecular weight has not yet been ascertained. In describing his repetition of Dr. Gladstone's work, Mr. Stokes incidentally mentions the interesting circumstance that the triple compound readily forms enormous crystals, well-developed prisms several inches long and of considerable thickness being frequently deposited from benzene, and indeed their size appears only to be limited by that of the containing vessel and the bulk of solution. These crystals melt at 114°. The quadruple compound melts at 123°·5, and boils at the normal pressure at 256°·5. It crystallises well in colourless prisms, which are usually much smaller than those of the triple compound, and exhibit great tendency to develop an acicular character. It is less soluble in alcohol and benzene than the latter compound; it may be recrystallised from glacial acetic acid, but it exhibits a great aversion for water, not being wet by it, and consequently the crystals float on water. It dissolves in hot concentrated oil of vitriol, but upon boiling most of it sublimes unchanged, an evidence of its great stability. Its vapour is endowed with a pleasant aromatic odour, but inhalation of more than traces is followed in two or three hours by alarming difficulty in breathing and persistent irritation of the air passages. Its vapour density was determined in an atmosphere of hydrogen, and indicated the quadruple formula. Even boiling water exerts only a very slight action upon it; but a smooth decomposition is effected by dissolving in ether, and repeatedly agitating with water. The acid produced is deposited from the water in crystals having the composition $(PNO_2H_2)4 + 2H_2O$. This interesting acid readily decomposes soluble chlorides, nitrates, and sulphates, forming three series of salts, in which respectively one-fourth, one-half, and all the hydrogen is replaced by the metal. The free acid is so highly stable that it may be boiled for hours with nitric acid or *aqua regia* without decomposition. Further details concerning it, and the other compounds isolated, will shortly be published.

BIOLOGICAL WORK ON THE ILLINOIS RIVER.

ILLINOIS possesses a good Laboratory of Natural History, in which Prof. S. A. Forbes, with a number of assistant entomologists and zoologists, carry on investigations of value to science and the State. A report on the work of the Laboratory during the past two years has recently been issue d. To us the points of special interest with which it deals are (1) the establishment, in 1894, of a biological station for the continuous investigation of the aquatic life of the Illinois River, and its dependent waters, near Havana; and (2) an elaborate experimental research carried on during the past year to determine means for the destruction of the chinch bug, and especially for the dissemination of the contagious diseases of that insect. This investigation was undertaken by the Laboratory staff, with the co-operation of the State Agricultural Experiment Station.

We have already noted the establishment of the biological station (NATURE, vol. l. p. 275, 1894), but as it is the first inland aquatic biological station in America, manned and equipped for continuous investigation, the following further details are interesting. By the kindness of Prof. Forbes, we are able to illustrate the description with two of the fifteen fine process plates contained in his report.

The Station was opened just a year ago. Its general objects are to provide additional facilities and resources for the study of the natural history survey of the State, now being carried on by the State Laboratory of Natural History; to contribute to a thorough scientific knowledge of the whole system of life existing in the waters of this State, with a view to economic as well as educational applications, and especially with reference to the improvement of fish culture and to the prevention of a progressive pollution of Illinois streams and lakes; to occupy a rich and promising field of original biological investigation hitherto largely overlooked or neglected, not only in America but throughout the world; and to increase the resources of the zoological and botanical departments of the University of Illinois, by providing means and facilities for special lines of both graduate and undergraduate work and study, for those taking major courses in these departments.

The Station differs from most of the small number of similar stations thus far established in the United States, in the fact that its main object is investigation instead of instruction, the latter being a secondary, and at present an incidental, object only. It has for its field the entire system of life in the Illinois River and connected lakes and other adjacent waters, and it is Prof. Forbes' intention to extend the work as rapidly as possible to the Mississippi River system, thus making a beginning on a comprehensive work in the general field of the aquatic life of the Mississippi Valley, in all its relations, scientific and economic.

The special subject which Prof. Forbes fixed upon as the point of direction towards which all the studies shall tend, is the effect, on the aquatic plant and animal life of a region, produced by the periodical overflow and gradual recession of the waters of great rivers, phenomena of which the Illinois and Mississippi rivers afford excellent and strongly marked examples. This field of research is entirely fresh, and at the same time is highly interesting and important.

As an incidental, but by no means unimportant, result of the work, material will be accumulated for a comparison of the chemical and biological conditions of the waters of the Illinois River, at the present time, and after the opening of the Chicago drainage canal.

The practical importance of the undertaking, as affording the only sound basis for a scientific fish culture, will be fully recognised by all who are seeking to apply scientific methods of investigation to economic problems.

It is pointed out that the Station will also serve as a centre of interest and activity for University students engaged on zoological and botanical subjects. Not many years ago biological instruction in American colleges was mostly derived from books; of late it has been largely obtained in laboratories instead; but Prof. Forbes believes that the mere book-worm is hardly narrower and more mechanical than the mere laboratory grub. Both have suffered, and almost equally, from a lack of opportunity to study nature alive. One knows about as much as the other of the real aspect of living nature, and of the ways in which living things limit and determine each others' activities and characters, or in which all are determined by the inorganic environment.

Means are provided by which students, and intending teachers of biology, may be given a wider knowledge of their subject, and where they may investigate experimentally the problems of mutual influences and relationship in the living world.

Possibly still more important is the opportunity which the Station will offer, when permanently established and fairly well developed, to the independent student and investigator, zoological or botanical, who may desire to pursue his studies in the field covered by the operations. It is a part of the plan of organisation and equipment to receive and assist in every practicable way advanced students and investigators of this description, from whatever place they may come.

Havana was selected as the site of the Station because of several unique advantages offered by that locality. Streams and lakes illustrating practically all the typical Illinois River situations are to be found there, convenient of access from a central point, and from each other. An extensive sandy bluff, commonly well shaded and oozing spring water at its foot, borders the river bottom on the east, and introduces several unusual features of interest to the œcologist, besides affording a clean and hard shore to work from, dry, shady, and well-drained camping ground, and an abundance of very pure cold water at all times of the year.

A "cabin boat" was used as a field headquarters, and stationed on Quiver Lake, two and a half miles above Havana. It carried the seines, sounding lines, aërial and aquatic thermometers, dredges, surface nets, Birge nets, insect nets, plankton apparatus, and other collecting equipment, together with microscopes, reagents, a small working library, a large number of special breeding cages for rearing aquatic insects, and a few small aquaria. This boat was provided with sleeping accommodation for four men, and with a well-furnished kitchen.

The greater part of the field work was done on seven regular stations, visited periodically throughout the year; two on the

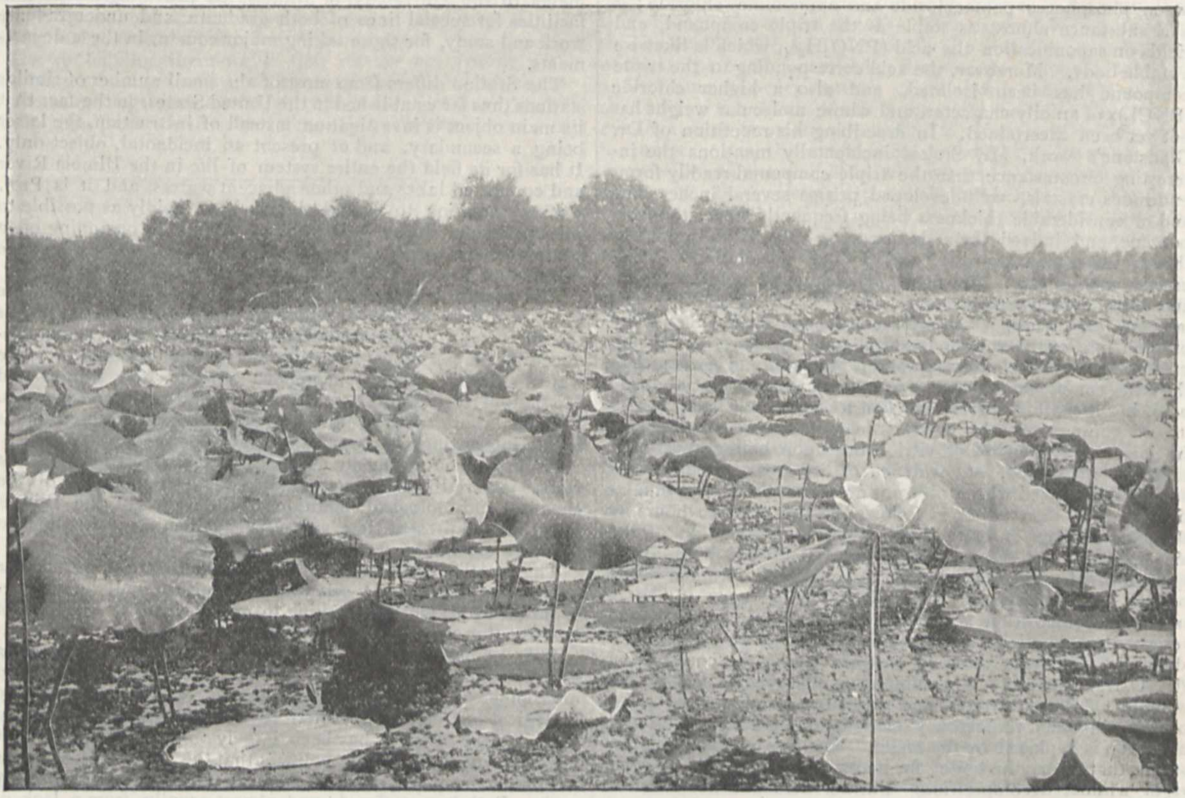


FIG. 1.—Lotus Bed, near Head of Quiver Lake, West Side.

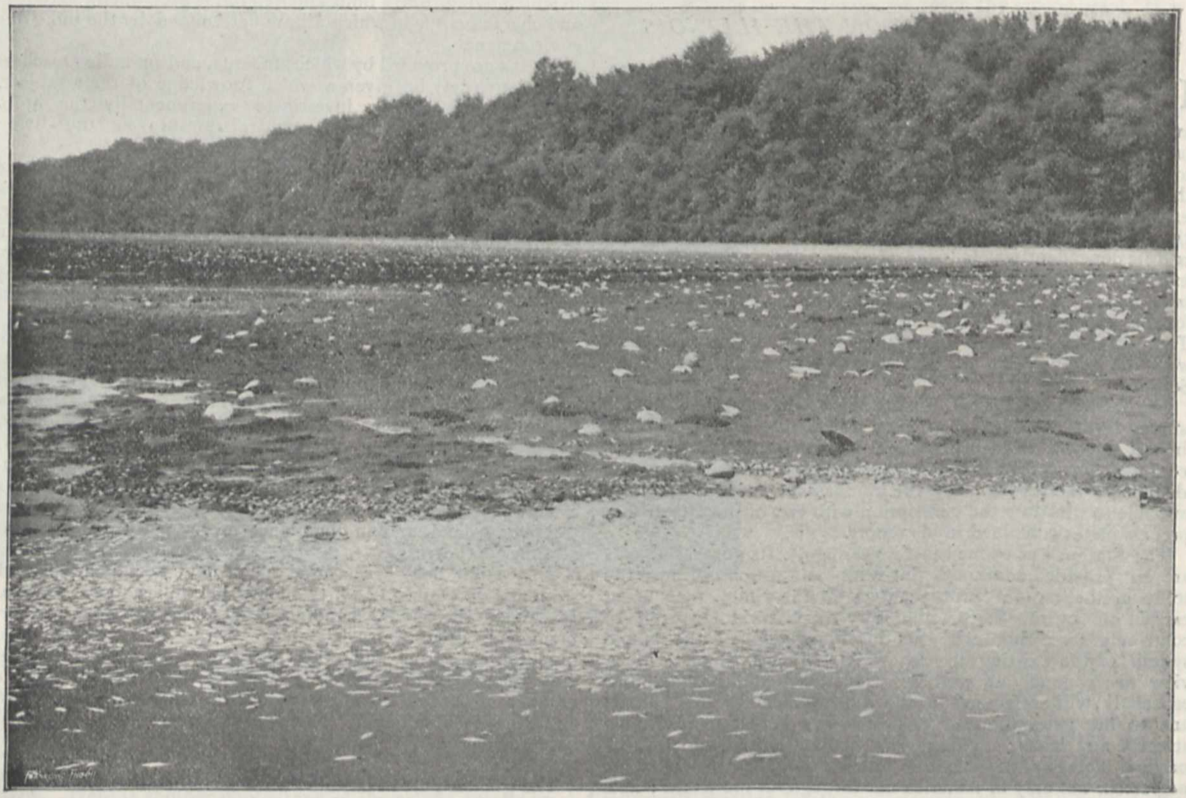


FIG. 2.—Dead Fish and Mussels. Bed of Phelps Lake.

Illinois River, three on Quiver Lake, and one each on Phelps and Thompson's Lakes.

Quiver Lake (Fig. 1), in which the headquarters' boat was placed, is an abandoned portion of the old bed of the river. It varies in length (when the water is low enough to define it clearly) from one and a half to two and a half miles, and has a usual width of about five hundred feet at low-water mark. It lies nearly parallel with the main river, into which it opens, even in the lowest stage of water, at its lower or southern end, by about half its greatest width.

Thomson's Lake lies wholly within the bottom lands of the main river, and its banks are consequently everywhere low and flat. It is five miles in length by about half a mile in width at an average midsummer stage. Neither this nor Quiver Lake ever goes dry, the water in the deepest places being not less than three and a half or four feet during the dryest seasons. Phelps Lake (Fig. 2), on the other hand, is a pond about half a mile long by a fourth as wide, having neither inlet nor outlet after the overflow has receded, rarely drying up entirely, but not infrequently being reduced to a few shallow pools. It is completely surrounded by a bottom-land forest, and its bed is a mere shallow depression in the mud.

The results of the first season's work are, of course, but just beginning to appear. Indeed, the problems to be solved in such situations have scarcely more than dimly shown themselves as yet, but the promise is nevertheless already very interesting. Notable contrasts in kind and number appear between animals of the springy shore of river or lake, and those of the muddy bottom, only a few rods away on the other side; between river and lake; between Quiver and Thompson's Lakes; between each of these and Matanzas Lake; and between all the other lakes and the temporary pond distinguished locally as Phelps Lake—contrasts sometimes easily comprehensible; and sometimes peculiarly puzzling, like that between Quiver Lake on the one hand, the waters of which are choked in midsummer with a dense growth of aquatic vegetation, but contain fewer of the smaller animal forms (Entomostraca, and the like) than the open current of the river itself, and Thompson's Lake, on the other hand, where the water is relatively clear of aquatic plants but abounds in rotifers and Entomostraca. Still more curious is the contrast between the similarly situated and very similar lakes, Quiver and Matanzas, the waters of one loaded and clogged with plants, and swarming with small molluscs and insect larvæ, and those of the other with scarcely a trace of even microscopic vegetation, and with a correspondingly insignificant quantity of animal life.

One surprising result is the abundance of minute life in the main stream, which sometimes contains a greater abundance of animal forms than most of the lakes connected with it; and another is the relatively small difference between the animals frequenting widely unlike situations in the same body of water. A large number of new forms were found, especially among rotifers, worms, and insect larvæ. The collections of the season, preserved for detailed study, are included under nine hundred and fifty-eight collection numbers, representing as many different lots of specimens. In connection with these, Prof. Forbes and his assistants are now engaged upon determination work and other laboratory studies, and the preparation of reports. The papers and reports embodying these studies will be printed in the *Bulletin* of the Illinois State Laboratory of Natural History. So far as possible, each general taxonomic paper will be preceded by a thoroughly practical synopsis of genera and species, illustrated by figures of typical forms, and intended to open up to the student and teacher of natural history in Illinois many interesting and important parts of the local zoology.

THE VARIETIES OF THE HUMAN SPECIES.[1]

IN man, as in other animals, we find physical characteristics of two kinds, external and internal. The first are principally those pertaining to the cutis and certain cutaneous appendages, and include the colouring of the skin and hair, the structure and form of the hair, and also the colouring of the eyes. The chief internal characteristics are the bones from which the form and figure of all the members are taken, as well as those of the separate parts of the body clothed with soft tissues, such as muscles and fat. The cranium is the most important and most characteristic part of the entire human skeleton.

The cranium is a bony case which encloses a viscus of the first order, the brain, which in man is, in relation to the animal series, better developed, both in its forms and functions. It is known that the brain and cranium, from the embryological to the adult state, are in a parallel manner and gradually connected in evolution, and the external form of the one corresponds to that of the other. Most certainly it is not the cranium which gives form to the brain of man; it is more probable that it is the brain which moulds its organ of protection. Given hereditary conditions, we may affirm that the form of the cranium is correlative to that of the brain. If we could discover why the brain takes or has taken different forms, we would possibly understand better its correspondence with the exterior structure of the cranium by which it is surrounded. We might be able to learn also what functional and especially what psychological characteristics are united to the cerebral forms which are revealed by cranial forms. All that is obscure for us, and also unexplored, because unsuspected; for in place of that, and in an inexact manner, the volume has been taken into account, and therefore the weight of the brain, as being the only means of making an anthropological diagnosis of its functional value, the volume and weight corresponding to the capacity of the cranium.

But besides the cranium commonly called cerebral, there is the face, which, from the morphologic point of view, is not less important. The face has generally given more positive means for distinguishing human groups, not only on account of the colouring of the skin, but on account of the form and disposition of its parts, of the nose, of the cheeks, of the molar teeth, and on account of other characteristics which, when considered together, disclose differences not immediately revealed by the cerebral cranium.

The other parts of the skeleton also have differences more or less profound in the different ethnic groups, the stature, the length of the extremities both absolutely and relatively to the stature and to the trunk; the thoracic form, and so on. But such differences are but slightly characteristic in comparison to those presented by the cranium and the face; until now, moreover, they have had but slight value, the reason being that they are derived from characteristics which are merely secondary.

We are ignorant what may have been the primitive type or the primitive human types, considered in all their internal and external characteristics; that is, what skeletal forms certain ethnic groups of differently coloured skin possessed; or, on the other hand, what colour of skin and hair belonged to certain skeletal forms. That difficulty is caused by a fact easy to understand, by the mingling of different types among each other, and by the hybrid forms from which man is derived. It is true, however, that certain hybrid results seem to be limited to certain regions and to a few human groups; and that, on account of this, the elements which have furnished such products may be learned up to a certain point; but in the beginning, at least, it will be necessary to learn the structures of the parts from which hybrids are derived.

It is impossible not to admit human hybridism, since it is demonstrated clearly by all anthropologists; in this direction America alone shows us a perfect example of experimental anthropology. It has been determined from observations that human hybridism is multiform among all peoples; but what we learn from the facts relates to the exchange of external characteristics and their mixture with those internal, that is, the union of the external characteristics of one ethnic type with the internal characteristics of another type. Thus, one may observe the colour of the skin and hair with its special form united to characteristics of skeletons which do not generally belong to types of that colour, and *vice versâ*. That may be observed concerning certain characteristics, and not of all; such as the stature, or the face, with its soft covering, or the form of the cranium only.

If we study our European populations which are called white, but which have many gradations of whiteness, we may note the great mixture of characteristics, a mixture which is changeable, from which results a great variety of forms of individual types, constituted of characteristics differing from each other. An analysis must be very accurate and very minute to discriminate these different elements which exist in the composition of the ethnic characteristics of individuals

[1] Extracted from a translation of Prof. Giuseppe Sergi's "Le Varietà Umane," published by the Smithsonian Institution.

and peoples. These mixtures and these combinations of characteristics differ according to the character and number of elements existing in the various nations of the south, the centre, or the north of Europe. They arise from different relations with mixed peoples.

What is most important in this human hybridism, so various and so complex, is the lack of the blending of the external and internal characteristics from which new human varieties may be had. Among the different ethnic elements there exists only a relation of position, called syncretism, or propinquity of characteristics, and therefore a facility for forming small groups. Such a phenomenon has already been recognised in America, and it is evident in Europe among peoples who appear little homogeneous, if a careful observation separates the characteristics constituting ethnic types and those of individuals in a mixed population.

If there were no other cause for such an absence of blending among the characteristics of human hybridism, this cause would exist, that the relations which produce the mixtures are not equal and constant, but are varied and inconstant. If there should be the union of two pure ethnic types only, for several generations, we should be able to derive a hybrid product constant and fixed, as among animals and plants; but a third element, either pure or mixed, arrives in the second or third generation of man, and so on indefinitely. Thus it is easy to understand how unstable must be the characteristics of the hybrid, for they can scarcely survive in one individual for a generation. The hybrids which follow may have characteristics of different types, with the tendency each time to have these reappear by heredity, although not blended and not fixed in the individual.

To this should be added another fact, that of individual variation, which is present in man, as in other animals, increased by his constant interminglings, which may be considered stimulants of this phenomenon, as has been suggested by Darwin and Wallace.

Hence, I conclude from my observations, that human hybridism is a syncretism of characteristics belonging to many varieties, and that these do not modify the skeletal forms as do individual variations, and that hybridism may affect different parts of the skeleton, constituting characteristics in themselves distinct. The stature, the thoracic form, the proportion of the long bones, may be united with external characteristics differing from each other, as well as from different cranial structures. The cranial form may be associated with different facial forms, and inversely. It happens, however, that the structures taken separately remain in part unvaried in the hybrid constitution. The face preserves its own characteristics in spite of the union of different cranial forms; so also the cranium preserves its structures, associating them with different facial forms. The stature preserves its own proportions in spite of its associations with different cranial and facial types, and in spite of the different colouration of the skin and the form and colour of the hair. All this may be affirmed, particularly of much larger human groups which, according to external characteristics, may be considered much nearer than they really are in geographical position, as the so-called white races in Europe, the negroes in Africa, in Melanesia, and so on.

Now, granting that all peoples exhibit the characteristics of hybridism in the manner just described, it will be necessary to learn how races, groups and human families may be classified. Let us observe for a moment the classification by means of external characteristics, most common among anthropologists from Linnæus to Quatrefages and Flower, and we shall see:

(1) That the colour of the human skin in one great group of a type, such as yellow, black, or white, is of different gradations, and not uniform.

(2) Since, as above stated, all peoples, at least in a great measure, are composed of hybrid elements, it happens that different elements are united under one category, which is, in this instance, the colour of the skin.

(3) We must not forget that the external characteristics are more easily lost, and much easier to acquire, by intermixture and heredity.

A curious example of what I state is found in human classification according to Quatrefages, which perhaps is now the most complete, considered only as a classification by external characteristics. He places the Abyssinians within the white race notwithstanding that they have the negro colouring, and he does so because he believes that the characteristic form of the skeleton or internal characteristics of the Abyssinians are those of the white race. This is without doubt inconsistent when the principle of classification by colour is accepted. This inconsistency itself shows the defect of the method and of the principles mentioned as applied to human characteristics and their combination.

(4) Finally, as we perceive, the theory is not justified that man be classified as a single species with three, five, or more variations.

If the characteristics which present greater stability are internal or skeletal, they should serve for human classification:

(*a*) Because, notwithstanding amalgamation and consequent hybridism, the characteristics originating in the skeleton are persistent.

(*b*) Because they may be taken as fixed points with which other characteristics may be associated, and may be also external, as I shall demonstrate.

(*c*) Because, finally, the internal characteristics can demonstrate the full number of divisions and subdivisions in classifying ethnic groups, and in analysing peoples which are a combination of a great number of hybrids.

It remains to determine which internal characteristics should have the preference in deciding the value of types for classification. If we consider the human skeleton, with that object in view, we find three parts which may serve for that purpose, the cerebral cranium, the face, and the stature, with the long bones.

Stature.—The stature is a good, but an insufficient characteristic, because it gives only linear differences, and in its value resembles greatly other external characteristics, and is associated with all the most dissimilar derived from the skeleton.

Face.—The face offers very important characteristics for classification, because it shows typical differences in the ethnic groups. The face has given more points for the distinction of human types than the other parts of the human body, and would appear better adapted for that purpose than the cerebral cranium. But the face is more disposed to individual variations than any other part, because it is very complex, being composed of numerous small bones, clothed with muscles which have continuous and important functions relating to the physiognomy, to the expression of psychical conditions, and to the nutritive functions. These facts render its typical form less constant, and are, or may be, the cause of a multiplication of types.

Cranium.—The cerebral cranium is itself also liable to variations. More than any other organ, it exhibits a phenomenon often observed and clearly demonstrated by me, that is, the persistence of forms from immemorial epochs, and their reproduction through numerous generations notwithstanding amalgamation with other types. I have demonstrated such a persistence of cranial forms in the varieties of the Mediterranean from the Neolithic and from the most ancient Egyptian epochs; other anthropologists have recognised such persistence in European types of the Quatenary epoch, and in others, very ancient, from America. This cannot be said of the structure of the face.

Therefore if the human cranium is accepted as a basis for the classification of human groups, positive results may be had:

(*a*) In groups which have been subjected to mixture in whatever epoch or however many times, the distinctive ethnic elements may be discerned by examining the cerebral cranium only, which, remaining unaltered in type, may be found united by hybridism with other internal and external characteristics. For the cranium is the point about which revolve all other variations of form, either in hybridism or in the human form itself.

(*b*) Knowing the cranial types of a people who seem more or less homogeneous, we are sure of learning of what and how many ethnic elements it is composed, notwithstanding the hybridism present.

(*c*) Having classified all the cranial types in different regions and among different peoples, we may learn by their geographical distribution the numerical extension of types and also their geographical origin; that is, the place of departure and the course of emigration and dispersion of such forms.

(*d*) Then it will be easy to learn what cranial characteristics are found among populations which already have ethnic names, ancient and modern, and to discover among them points of similarity and difference.

Being, therefore, obliged on account of universal human hybridism to select as a guide to classification the most important and the most useful of the internal characteristics, we find greater advantages in choosing the human cranium, about which all the other characteristics, internal and external, are grouped. If we select one characteristic, or a number of variable characteristics, we shall find ourselves in the same position as other anthropologists who classify by external or accessory traits. It follows that, accepting the cranium as the principal internal characteristic, we impliedly accept the brain in its various forms, and the brain is the most important of human organs.

The classification of man by means of the cranium alone is by no means new. It will be well to consider these schemes, from that of Retzius down to the last, that of Kollmann. Nor, indeed, is the conception of the importance and superiority of the cranium for distinguishing ethnic groups by any means recent. To show that, we have but to refer to the enormous work which has been done, from Morton to Davis and Thurman, from Broca to G. Retzius, to De Quatrefages, to von Holder, to Ecker, to His and Rutimeyer, to Virchow, to Ranke, to others still more numerous, in Italy, from Nicolucci to Mantegazza.

Notwithstanding so much labour expended on the human cranium, satisfactory results were not reached, nor, indeed, I may affirm, have we yet reached them, at least not in the signification which I intend these results to have. The fault lies in the nature of the method of studying the human cranium, and in the value attributed to craniometry.

The classification of Retzius is based upon a single characteristic of the cranium, which, however, is merely the numerical expression of the *norma verticalis* of Blumenbach, that is, the cephalic index.

According to Retzius we have only two forms of crania, the long and short; though, in fact, many forms of short and long crania are found differing very much from each other.

When craniometry was developed in a systematic manner, following principally the work of Broca, it appeared the key of anthropology, and took the first place among means of investigations, as being the most effectual method for distinguishing human races. The French exaggerated its value; the Italians followed with zeal, in spite of the scepticism of Mantegazza, the head of the Florentine school of anthropology; the Germans have been more rational, and with them the Swiss, represented by His and Rutimeyer. At the head of them I would place Blumenbach, who based his small but valuable book upon a rational foundation.[1] The Germans try to establish cranial type almost or entirely independent of the cephalic index; as one may see from the works of von Holder, of Ecker, of His and Rutimeyer, of Virchow, of Kollmann, of Ranke and others. In my opinion the German method is an approximation to the truth, but unfortunately the conception of type is undeveloped and, I should say, has remained rudimental, because craniometry, like a pernicious weed among the grain, injures the harvest. Virchow, the most pronounced scholar in anthropology, and the man who has studied more than all others the crania of all peoples, believes that the germ of a sound anthropology should develop from it, and concedes only a secondary value to craniometry.

According to my observations upon craniometry, which has now become cabalistic, especially in France, on account of the abuse of measures and numerical ciphers, the indices of the cranium and face are taken as a means of distinguishing races, human groups, as we might call them, and other measures are either omitted or applied only to individuals. In order to be convinced we should carefully and conscientiously study the craniometrical works of Dr. Danielli, of Florence, upon the Nias and Bengalese. The author has not been able to find satisfactory results after persevering researches, but whoever would seek evidence of individual variations will find more than enough. It seems to me, therefore, that the method by measurement may serve this purpose, that is, to discover numerically individual differences, but never those typical of a race. But such a discovery is useless, since we are all convinced of the existence of individual differences. I will therefore add that such differences, to be valuable, must be sought, not among forms differing from each other, but among individuals of the same type. That implies, therefore, necessarily and always, the search for types and their distinction, which is not possible by means of the craniometrical method.

If it were true, and there were no doubt respecting the value of the celebrated cephalic index in determining cranial forms, it would follow that all human crania of whatever type and volume should be placed in the three categories of dolicho-, meso-, and brachycephalic, or of hypsi-, ortho-, and chamæcephalic. Thus all the populations of the earth, either of white, yellow, black or red skin, would have crania belonging to the three categories. A classification solely according to the cephalic index is therefore an absurdity. It is incoherent and without meaning, as are those of Retzius and Kollmann.

This conclusion is so true that such anthropologists are obliged to add descriptions to the forms of each part of the cranium, in order to distinguish it, recognising the insufficiency of cranial data. Such descriptions can, to a certain degree only, supply the defect of the method, but they always remain incomplete, and leave the forms or types of the human cranium of various populations and regions indefinite. The French school has gone still farther, and has supplied the deficiency with an infinite number of measurements, which only increase the obscurity, leaving the conception of the form more uncertain, and fatiguing the most patient student, who becomes convinced of never reaching any satisfactory result from such a confused accumulation of numbers.

In order to render classification more definite, or for the sake of finding a second characteristic which might be associated with the cephalic index, Retzius turned his attention to the prognathism and the orthognathism of the molar teeth; Kollmann to the facial index. Use could be made of the nasal index instead of the facial, or the orbital index, or any isolated characteristic, and we should have the same results. The combinations given by Retzius and Kollmann are possible, but cannot indicate races or varieties, from the fact that they are hybrid associations.

I need not make a longer demonstration of what I have affirmed, that classifications of human groups have been attempted by means of the cerebral cranium, but have not been successful on account of deficiency of method; and that the craniometrical method, still so undeveloped, has not yet, nor cannot, give those results while there is an exaggeration of an exact principle, that of expressing numerically facts relating to the cranium. It seems to me, after several years of study, and after having adopted the accepted form of craniometry, for want of a better, that it is time to establish for our use and for the study of the variations of man, a natural method, resembling that which is used in zoology and botany, and of which I laid the foundation about two years ago.

With the observations and the methods which I propose, I believe that many errors will be eliminated from anthropology. Those errors have been accepted because we have never possessed natural scientific methods for the study of human classification, such as we have in zoology. Blumenbach, in a valuable little book, attempts to apply the zoological method to man, not only for classification, but for the explanation of the causes of animal and human varieties. De Quatrefages, in his last work, employs the same method and the same scientific freedom. Unfortunately the followers or successors of both have only followed their masters in form, but not in method. Blumenbach, who, after various researches, reduces the human species to five varieties, finds, however, that human variations are infinite in number. If his method had been followed strictly, the number of human varieties would long ago have been increased, both in respect to the structure and the cranial forms.

The neglect of such methods and the failure to distinguish human varieties by means of the cranium has caused a curious error, that of regarding certain forms which are typically normal, as pathological, as I shall have occasion to demonstrate in the future when I speak of classified forms. This is apt to happen when new and unrecognised forms are placed before the observer.

One of the important characteristics in classifying the cranial varieties of man is the *cranial capacity*, which has a direct relation to the volume and weight of the brain; hence classification by crania means the classification of brains estimated by their form and external configuration. Its importance is for us increased by the fact that that which we find among races of animals occurs also in man; that there are races of small and large animals, races differing in size. This is also repeated in man, and we therefore have large, medium and small varieties, as measured by stature. The origin of such varieties is perfectly analogous to that in other animals. Nor is it an accidental phenomenon, because it is confirmed by heredity, through numerous and indefinite generations.

I have concluded, in studying cranial varieties morphologic-

[1] "De generis humani varietate nativa." IIIa edit. (Göttingen, 1795.)

ally as human varieties, that is, by their characteristic structures, that the volume has a direct relation to the form, in other words, many forms have limited and definite capacities, while other forms have sub-varieties differing in capacity. Such varieties are analogous to the stature of the large and small varieties of animals. The cranial capacity, therefore, while it is one of the integral characteristics of the cranium in regard to its classification, is also the indication of different varieties according to size. I discovered this fact when I classified for the first time the crania of Melanesia, and subsequently I defined it more accurately when I examined and classified thousands of other human crania.

This fact points to a correction of the value of cranial capacity and, therefore, of the weight of the brain, until now calculated by the average without distinction among different varieties. The cranial capacity of man varies from 1000 cc. to about 2000 cc. in the masculine sex; this enormous difference is admitted as individual variation, and it is thus conceded that there may be a least limit of normality possible which can be ascribed to the function of the brain, crania which descend to 1150 cc. being considered as pathological microcephali, according to Broca, and more or less according to other anthropologists; giving, on the other hand, a great value to a large capacity. Both conclusions are contrary to the real significance of the facts. I have found normal masculine capacities of 1000 cc. and a little greater, representing small human varieties, not being sporadic and individual phenomena; and, on the other hand, anthropologists have registered for eminent men, like Dante, Gauss, and others, very mediocre capacities, even very low, while for ordinary men they have recorded a much higher capacity. I have found in Melanesia normally constituted heads absolutely microcephalic, together with megalocephalic heads, belonging to varieties which have the same social value; they are both inferior, some anthropophagous, and live mixed together as one people. That which I have asserted concerning Melanesia may be said of the ancient and modern populations of the Mediterranean, among which are the Sicilians, the Sardinians, and the inhabitants of Central and Southern Italy; and I do not believe it can be said that there are no signs of human superiority in those regions. There are not, therefore, individual differences so great as from 1000 to 1500 cc., and from 1500 to 2000 cc., but characteristic differences of variety in human forms. The general average I therefore maintain is inexact and also arbitrary, because it is the average of incommensurate quantities. The exact average is that between individuals of the same variety, and the difference is the true individual variation.

But there is another error to correct, due to the signification which I am able to give to varieties distinguished by means of my method. It is considered by some a demonstrated fact that the cranial capacity has been increased in the course of social evolution from prehistoric epochs to historic times. Eminent men have affirmed it, but I have already placed their conclusions in doubt, because the facts do not appear to me evident and affirmative. I wrote some years ago:[1] "The most important physical evolution of man would be that which related to the organ of the mental functions, the brain. But the facts are still very doubtful and very obscure which relate to the weight and volume of the brain, and consequently to the cranial capacity. In a recent work of Prof. Schmidt, I find that the cranial capacity of the ancient pure Egyptians is 1394 cc. in the masculine, and 1257 in the feminine sex; in the pure modern Egyptians it is 1421 in the males, 1206 in the females. According to these figures there would be an increase of the cranial capacity of the modern over the ancient males, but a decrease in the females. The reverse would be true of the Egypto-Nubian cranium, which is 1335 in the modern males, and 1205·8 in the females. Broca found that the Egyptians of the IV. Dynasty had, males 1534, females 1397 cc.; those of the XI., males 1443, females 1328; and, finally, those of the XXIII., the most recent, males 1464, females 1322. There would be in such a case no increase, but decrease, but that is not possible; the cause of these facts lies in the mixtures of races at different times and in different proportions."

Now I conclude from my recent studies upon the Egyptians of different dynasties, from the most ancient to the present, that according to my method of classification there are capacities of 1260 cc., of 1390, of 1480, of 1550, of 1710, and still other capacities differing according to the varieties determined.[2] As is easily understood, a general average necessarily alters the facts, according to the number of varieties which enter as components of the average in the different series in anthropological museums; hence the curious results above indicated.

Another important point is as follows:

"But the fact which surprises us is the high figure of the capacity given by prehistoric crania. The masculine crania of Lozère have given 1606 cc., the feminine 1507; also of Lozère, masculine 1578, feminine 1473; crania from the *pietra levigata*, masculine 1531, feminine 1320; the contemporaneous Parisians, masculine 1559, feminine 1337. The approximate average of crania from the *pietra levigata* is 1560, equal to that of modern Europeans, as is related by Topinard."[1]

In another of my recent works, I have demonstrated that of the crania of the neolithic age[2] the *Isobathyplatycephalus* has a capacity from 1230 to 1405 in the feminine, and the *Eucampylos* varies from 1470 to 1564 in the masculine. The two varieties, still persistent in Sicily, do not vary in capacity in the modern series, and at the same time show that in the neolithic epochs, as among modern populations, large and small varieties are found, just as the same types are now found through persistence of forms.

From this it is evident how much there is to reform in anthropology when we study by natural methods facts until the present misinterpreted, respecting the classification as well as the physical and psychological characteristics of man in time and space. Perhaps in the future, when we know all cranial forms by natural classification, it will be possible to find a correspondence of psychological characteristics in populations according to the predominance or superiority of types, a fact which has until now escaped research, because the capacity of the cranium in its absolute sense is not in correlation to the development of the mental functions, notwithstanding what is commonly affirmed.

The following are the varieties into which Dr. Sergi classifies the forms of skulls in the *norma verticalis* of Blumenbach:— (1) Ellipsoid (*ellipsoides*); (2) Pentagonoid (*pentagonoides*); (3) Rhomboid (*rhomboides*); (4) Ovoid (*ovoides*); (5) Sphenoid (*sphenoides*); (6) Spheroid (*sphæroides*); (7) Byrsoid (*byrsoides*); (8) Parallelepipedoid (*parallelepipedoides*); (9) Cylindroid (*cylindroides*); (10) Cuboid (*cuboides*); (11) Trapezoid (*trapezoides*); (12) Acmonoid (*acmonoides*); (13) Lophocephalic (*lophocephalus*); (14) Chomatocephalus (*chomatocephalus*); (15) Platycephalic (*platycephalus*); (16) Skopeloid (*skopeloides*).

[1] "Human Evolution." (Review of Scientific Philosophy, 1888, Milan.)

[2] "Concerning the Primitive Inhabitants of the Mediterranean." (Archives of Anthropology, Florence, 1892, vol. xxii.)

[1] See "Human Evolution."

[2] "Crania of the Neolithic Age." (Boll. Paletnol. Italiana, Parma, 1892.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

We have received a verbatim report of the interview which a deputation from the Association of Technical Institutions recently had with Mr. Acland. Several suggestions were made, some of which have already received the attention of the Science and Art Department. Prof. Wertheimer pleaded for an advisory voice in the construction of the Department's schemes before they were finally adopted, in a manner similar to that by which the Education Department allowed the managers of public elementary schools to express their views on the Code under which they had to work before it was finally adopted. Mr. Acland, in the course of his reply, said it was the intention of the Department not to publish near the summer months anything which will be in the nature of an important change. The recent form dealing with organised science schools had been issued early, with a view to embodying it in the Directory next autumn, the Department in the meantime being open to suggestions. During the course of the Vice-President's remarks, the question of the publication of the dates of the May examinations was raised, and, in reply to an inquiry, Sir John Donnelly said he saw no difficulty, if the schools wanted it, in publishing in May the dates of the subsequent May examinations. As to the question of the proper basis for the calculation of the Government grant, Mr. Acland expressed the hope that some day a part of the principle, which is shortly to be applied to organised science schools, will also be applied to evening classes; that is to say, there is every prospect that the grants will in a year or two be awarded more on the Inspector's reports as to the soundness of the teaching than on the results of examination.

There are 119 Universities in the world, says the *Oxford University Extension Gazette*. Dr. Kukula in his list names

114, but he omits the Universities of London, of Paris, of the State of New York and of Wales, and the New University of Brussels. Excluding the first three, which, being of the Napoleonic type, have no resident students, the undergraduate population of the Universities of the world is estimated by this academic statistician as amounting to 157,513 persons. Berlin is the most populous University, Urbino the smallest. The first has 7771 students, the latter only 74. In point of numbers Oxford comes tenth on the list; Cambridge, twelfth; Victoria, sixty-fourth, and Durham ninety-eighth.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 21.—"On the Development of the Branches of the Fifth Cranial Nerve in Man." By A. Francis Dixon.

In this paper "detailed descriptions of the fifth nerve branches are given for five different stages of the human embryo, beginning with an embryo of four weeks, at which time merely the three main divisions of the nerve are represented, and ending with one of the eighth week. The observations on the human embryo have been checked by further observations on rat embryos, and an almost complete correspondence between the two has been made out." In mammals, the three divisions of the fifth nerve are found to rise independently from the Gasserian ganglion, and the nasal nerve is found to be the first representative of the ophthalmic division, the frontal being formed later. In like manner, the inferior dental nerve represents the first formed inferior maxillary nerve, the lingual branch appearing later. No special ganglion is present either for the nasal or for the ophthalmic nerve in mammals in the sense of a ganglion of a posterior nerve root. The ciliary ganglion does not represent such a ganglion, and when first found is more closely connected with the fourth and frontal than with the third and nasal nerves. The fourth and frontal nerves from an early period are closely connected. At the beginning of the sixth week nearly all the named branches of the fifth nerve of the adult are represented in the embryo; also at this time the accessory ganglia of the fifth nerve are recognisable. No evidence was found to show that the cells of these smaller ganglia are derived directly from those of the Gasserian. None of the different nerves which in the adult connect the fifth with the other cranial nerves are to be considered branches of the fifth nerve; thus the chorda tympani and the Vidian are found to be derived from the facial, and the nerve of Jacobson from the Glossopharyngeal.

"On the Conditions affecting Bacterial Life in Thames Water." By Dr. E. Frankland, F.R.S.

Since May, 1892, the author has been making monthly determinations of the number of bacteria capable of development on a peptone-gelatine plate in a given volume of Thames water collected at the intakes of the metropolitan water companies at Hampton. The number of microbes per cubic centimetre of water varied during this time between 631 and 56,630, the highest numbers having, as a rule, been found in winter or when the temperature of the water was low, and the lowest in summer or when the temperature was high.

The complete observations demonstrate that the number of microbes in Thames water depends upon the rate of flow of the river or, in other words, upon the rainfall, and but slightly, if at all, upon either the presence or absence of sunshine or a high or low temperature.

With regard to the effect of sunshine upon bacterial life, the author remarks that the interesting researches of Dr. Marshall Ward leave no doubt that sunlight is a powerful germicide; but it is probable that its potency, in this respect, is greatly diminished, if not entirely annulled, when the solar rays have to pass through a stratum of water even of comparatively small thickness before they reach the living organisms. If this be the case, it is held to be no matter for surprise that the effect of sunshine upon bacterial life in the great mass of Thames water should be nearly, if not quite, imperceptible.

Geological Society, April 3.—Dr. Henry Woodward, F.R.S., President, in the chair.—Dr. K. de Kroustchoff, St. Petersburg, was elected a Foreign Correspondent of the Society. —Physical features and geology of Mauritius, by Major H. de Haga Haig, R.E. The author gave full details of the physical geography of the island, including the nature and composition of the mountain ranges, the depth of the ravines, the occurrence of caverns in the lavas, and the character of the coral reef surrounding the island. Information was furnished concerning the neighbouring islands, and reference made to the possible former existence of an extensive tract of land at no great distance from Mauritius.—On a comparison of the Permian freshwater Lamellibranchiata from Russia with those from the Karoo formation of Africa, by Dr. Wladimir Amalitsky, Professor of Geology in Warsaw University. The freshwater shells from the Russian Permian deposits belonging to the genus *Palæomutela* are also known from the Karoo beds of South and Central Africa, as pointed out by the author in 1892. He had recently had the opportunity of studying the actual specimens from the Karoo beds, and found in them species of the groups *Palæomutela Inostranzewi*, *P. Keyserlingi*, *P. Verneuilii*, and *P. Murchisoni*; also of a new genus, the forms of which he had previously referred to *Naiadites*, Dawson. All these groups are found also in Russia, and a list was given of species found in the upper horizons (A, B, and C) of the Permian beds of Russia and in the Karoo beds. These upper beds of Russia have been determined by the author as the freshwater equivalents of the Zechstein; consequently the Beaufort beds of the Karoo series, if considered as the homotaxial equivalent of the European strata referred to above, should be regarded as Upper Permian. The Upper Permian group of freshwater lamellibranchiata of Russia, which bears traces of genetic relationship with the Carboniferous Anthracosidæ, and which was already well represented in Permo-Carboniferous and Lower Permian times, is, according to the author, much older than the African fauna of the Beaufort beds. These may be concluded to have migrated from Russia, the Gondwana beds of India having probably been the connecting-link between all these deposits. The author gave a description of the fossils of the Karoo series which he had examined, including a diagnosis of the new genus in which he placed the fossils already alluded to as having been previously referred to the genus *Naiadites*.

PARIS.

Academy of Sciences, April 8.—M. Marey in the chair.—On the fluted spectrum, by M. H. Poincaré. A mathematical paper in which it is shown that a complete analysis of the phenomena of Fizeau and Foucault's experiment confirms Fizeau's deduction concerning the permanence of luminous movement during a large number of oscillations.—Official plans and reports relating to the removal of the capital of Brazil to a new site, by M. H. Faye. A series of reports printed in Portuguese and French. The district in which the proposed new site for a Brazilian capital is situated lies between the parallels 15° 40′ and 16° 8′ and the meridians 3° 18′ and 3° 24′ at an altitude of above 1000 metres.—Structure of the hymen in a species of *Marasmius*. An abstract of a memoir by M. J. de Seynes.—On substitutions, by M. Zochios. An algebraical paper.—Removal of the Brazilian capital. A letter to M. Faye, by M. Cruls. A short account of the main features of the survey work undertaken on the new site.—On geodetic work in the basin of the Amour, by M. Venukoff.—On the determination of the mass of the cubic decimetre of distilled water at 4°, by M. J. Macé de Lepinay. This datum is yet imperfectly determined. Shuckburg and Kater give 1000·480 grams, whereas Stampfer finds the value 999·653 grams. The author proposes a new method of determination by which he expects to determine this constant within 6 mgm. The proposed method includes (1) the study of the geometrical form and dimensions of a certain solid as related to the standard metre, (2) the measurement of the loss of weight of this solid immersed in pure air-free water at its temperature of maximum density in terms of the standard kilogram. The solid taken is a parallelopipedon formed of transparent quartz. Its thickness in different directions will be examined optically by means of Talbot's fringes.—New apparatus for the measurement of the specific inductive power of solids and liquids, by M. H. Pellat.—On a new form of spectroscope termed the "héma-spectroscope comparateur," by M. M. de Thierry.—On a simple experiment demonstrating the presence of argon in atmospheric nitrogen, by M. Guntz. The author obtains argon by replacing magnesium by electrolytic lithium. Owing to the lower temperature at which lithium completely absorbs nitrogen, it is possible to pass atmospheric nitrogen over several heated iron boats containing lithium, and collect argon over mercury at the exit end of the apparatus.—On the spectra of selenium and some natural selenides, by M. A. de Gramont. The minerals examined are: Berzéline Cu_2Se, Zorgite $(PbCu_2)Se$, Clausthalite PbSe, Eucaïrite $Cu_2Se.Ag_2Se$, Guanajuatite or Frenzélite Bi_2Se_3.—On the estimation of thiophene in benzene, by M. G. Denigès. Two methods are given, both depending on the use of the mercury reagent previously described. In aqueous solution the reagent precipitates the compound $(SO_4HgOHg)_2.C_4H_4S$ when heated with the impure benzene in a closed flask at 100° for about fifteen minutes with frequent shaking. In methyl alcohol solution the precipitate $SO_4(HgO)_2Hg.C_4H_4S$ is produced; in this case the benzene is miscible with the reagent, and hence the reaction is much facilitated.—On the action of potassium permanganate on various organic substances, by M. E. Maumené.—On the calcium phosphate of milk, by M. L. Vaudin. The conclusions are drawn that: (1) Milk contains citric acid as alkaline citrate, which aids in keeping its calcium phosphate in solution. (2) This solution occurs owing to the effect of lactose in preventing the precipitation of calcium citrate from solution. (3) Every influence modifying or destroying the molecular equilibrium of the salts dissolved in milk, tends to precipitate tricalcic phosphate together with calcium citrate.—The sandstone of Taveyannaz and its relationships with the "flysch," by MM. L. Duparc and E. Ritter.—On the calcium carbonate of lake-waters, by M. André Delebecque.—On the connection of latitudinal displacements of lines of barometric maxima with the movements in declination of the moon, by M. A. Poincaré. The mean atmospheric conditions are powerfully and regularly influenced by the moon at each tropical revolution, and at each revolution of the node.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, April 18.

Linnean Society, at 8.—Observations on the Loranthaceæ of Ceylon: F. W. Keeble.

FRIDAY, April 19.

Quekett Microscopic Club, at 8.
Malacological Society, at 8.

SATURDAY, April 20.

Geologists' Association (Cannon Street Station, at 2.30.—Excursion to Charlton. Director: T. V. Holmes.

MONDAY, April 22.

Medical Society, at 8.30.

TUESDAY, April 23.

Royal Institution, at 3.—Alternating and Interrupted Electric Currents: Prof. G. Forbes, F.R.S.
Institution of Civil Engineers, at 8.
Royal Horticultural Society, at 1.—Conference on Primulas.
Royal Statistical Society (Royal United Service Institution), at 5.—Progress of Friendly Societies and similar Institutions during the Ten Years 1884-94: E. W. Brabrook.—Some Illustrations of Friendly Society Finance: Rev. J. Frome Wilkinson.
Royal Medical and Chirurgical Society, at 8.30.
Royal Photographic Society, at 8.
Society of Antiquaries, at 2.

WEDNESDAY, April 24.

Institution of Mechanical Engineers (Royal United Service Institution), at 7.30.—Discussion of the Governing of Steam-Engines by Throttling and by Variable Expansion: Captain H. R. Sankey.—Third Report to the Alloys Research Committee: Prof. W. C. Roberts-Austen, C.B., F.R.S.
Geological Society, at 8.—On the Shingle Beds of Eastern East Anglia: Sir Henry H. Howorth, M.P., F.R.S.—An Experiment to illustrate the Mode of Flow of a Viscous Fluid: Prof. W. J. Sollas, F.R.S.—Supplementary Notes on the Systematic Position of the Trilobites: H. M. Bernard.
British Astronomical Association, at 5.
Society of Arts, at 8.

THURSDAY, April 25.

Royal Society, at 4.30.
Royal Institution, at 3.—The Liquefaction of Gases: Prof. J. Dewar, F.R.S.
Camera Club, at 8.15.—Photo-etching Printing: Leon Warnerke.
Numismatic Society, at 7.
Chemical Society, at 8.—The Action of Nitrosyl Chloride on Amides: Prof. Tilden, F.R.S., and Dr. M. O. Forster.—The Action of Nitrosyl Chloride on Asparagine and Aspartic Acid; Lævo-rotatory Chlorosuccinic Acid: Prof. Tilden, F.R.S., and H. J. Marshall.—On a Property of the Non-luminous Atmospheric Coal Gas Flame: L. T. Wright.—A Constituent of Persian Berries: A. G. Perkin and J. Geldard.—Potassium Nitrosulphate: E. Divers, F.R.S., and T. Haga.—Diortho-substituted Benzoic Acids: Dr. J. J. Sudborough.—Hydrolysis of Aromatic Nitriles and Acid-amides: Dr. J. J. Sudborough.—Action of Sodium Ethylate on Desoxybenzoïn: Dr. J. J Sudborough.
Institution of Electrical Engineers (the Society of Arts), at 8.—A Magnetic Tester for Measuring Hysteresis in Sheet Iron: Prof. J. A. Ewing, F.R.S.

FRIDAY, April 26.

Royal Institution, at 9.—The Effects of Electric Currents in Iron on its Magnetisation: Dr. John Hopkinson, F.R.S.
Physical Society, at 5.—A Theory of the Synchronous Motor: W. G. Rhodwell.—Note on a Simple Graphic Interpretation of the Determinantal Relation of Dynamics: G. H. Bryan.
Clinical Society, at 8.30.
Institution of Civil Engineers, at 8.—Brine Pumping: Bernard Godfrey.
Institution of Mechanical Engineers (Royal United Service Institution), at 7.30.
Epidemiological Society, at 8.—Immunity: Dr. Washbourn.

SATURDAY, April 27.

Royal Institution, at 3.—English Music and Musical Instruments of the Sixteenth, Seventeenth, and Eighteenth Centuries: Arnold Dolmetsch.
Geologists' Association (St. Pancras Station), at 9 a.m.—Excursion to Brigstock, Geddington, &c. Directors: B. Thompson and W. D. Crick.
Royal Botanic Society, at 3.45.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Field-Path Rambles: W. Miles, series 8 (Taylor).—Alembic Club Reprints—No. 11: Essays of Jean Rey (Edinburgh, Clay).—Results of Rain, River, and Evaporation Observations made in New South Wales during 1893: H. C. Russell (Sydney, Potter).—Progressive Revelation: E. M. Caillard (Murray).—The Schott Methods of the Treatment of Chronic Diseases of the Heart: Dr. W. B. Thorne (Churchill).—Wayside and Woodland Blossoms: E. Step (Warne).—Economic Classics: T. R. Malthus (Macmillan).—Problems and Solutions in Elementary Electricity and Magnetism: W. Slingo and A. Brooker (Longmans).—Lepidoptera of the British Isles: C. G. Barrett, Vol. 2 (L. Reeve).—Hydraulic Motors: G. R. Bodmer, 2nd edition (Whittaker).—Queen's College, Galway, Calendar for 1894-95 (Dublin, Ponsonby).—Stephen's Catechism of Practical Agriculture, new edition (Blackwood).—A Handbook to the Carnivora. Part 1: Cats, Civets, and Mungooses: R. Lydekker (Allen).—Science Readers: V. T. Murché, Books 1, 2, 3 (Macmillan).

Pamphlets.—Bacteriological Test of the Purity of Water: E. H. Hankin (Agra).—The Early Relations between Maryland and Virginia: J. H. Latané (Baltimore).—Report of the Rugby School Natural History Society, 1894 (Rugby).—Report of the Manchester Museum, Owens College, 1894 (Manchester).

Serials.—Engineering Magazine, April (Tucker).—Journal of the Royal Statistical Society, March (Stanford).—Journal of the Chemical Society, April (Gurney).—Journal of the Federated Institutes of Brewing, Nos. 1 and 2 (Harrison).—American Journal of Science, April (New Haven).—Journal of the Sanitary Institute, April (Stanford).—Science Progress, April (Scientific Press, Ltd.)—Bulletins de la Société d'Anthropologie de Paris, No. 9, 1894 (Paris, Masson).—Astrophysical Journal, April (Chicago).

CONTENTS