THURSDAY, MAY 31, 1883

HUMAN FACULTY AND ITS DEVELOPMENT

Inquiries into Human Faculty and its Development. By Francis Galton, F.R.S. (London : Macmillan and Co., 1883.)

AMONG all his anthropological brethren Mr. Francis Galton has no competitor in regard to the variety and versatility of his researches. So various and versatile, indeed, have these researches been, that, with the exception of "Hereditary Genius" and "English Men of Science, their Nature and Nurture," we have become accustomed to regard them as disconnected pieces of work, which from time to time were thrown off like sparks from the flame of an active mind. But in the present volume he has collected in one series most of the investigations which he has separately published during the last ten years, and this collection when read in the light of a considerable amount of additional matter, clearly shows that the sundry investigations which were separately published were not separately conceived, but have throughout been united by the bond of a common object. This object, as the title of the book indicates, is that of inquiry into Human Faculty and its Development. And it is evident, when this fundamental note is supplied, that it serves to join not only the researches contained in the present volume, but also those of its above-named predecessors, into one harmony or design.

But although there is one harmony pervading this work, the changes of theme are so numerous that we shall not be able to touch upon them all, and must therefore restrict ourselves to considering the more important.

The book begins with an essay on "Variety of Human Nature," as to features, bodily qualities, energy, sensitivity, special senses, &c. In the course of this chapter the leading results of the author's well-known investigations on composite portraiture are brought in, the audibility of high notes in different individuals, as well as in different species of animals, &c. Next there follows a chapter on "Anthropomorphic Registers," which is mainly directed to showing the desirability of keeping family records of the anthropometry of children until they are old enough to continue the records for themselves. To facilitate this process—which he deems to be one of much practical importance in view of all that is now known touching the potency of hereditary influences—Mr. Galton urges that anthropometric laboratories should be established where all the needful periodic portraiture and other observations on the life-history of children should be made and preserved on the payment of small fees by the parents. Without such systematic observation any one may pass through life without knowing that he presents so strongly marked a peculiarity as that of colour-blindness ; while the benefit to the race, a few generations hence, of a large mass of statistics of such consecutive anthropometry of numerous families would probably be of the utmost value. Indeed this suggestion as to anthropometric laboratories may be taken as the foundation of Mr. Galton's proposed science of "eugenics," to a tracing of the main principles of which his work on "Human Faculty" is chiefly concerned.

After a chapter on "Statistical Methods," we come to a consideration of "Character." So far as sex is concerned, "one notable peculiarity in the character of the woman is that she is capricious and coy, and has less straightforwardness than the man . . . and there can be little doubt as to the origin of the peculiarity. . . . The willy-nilly disposition of the female in matters of love is as apparent in the butterfly as in the man, and must have been continuously favoured from the earliest stages of animal evolution down to the present time. It is the factor in the great theory of sexual selection that corresponds to the insistence and directness of the male. Coyness and caprice have in consequence become a heritage of the sex, together with a cohort of allied weaknesses and petty deceits, that men have come to think venial and even amiable in women, but which they would not tolerate among themselves."

The type of character which leads to criminality is next discussed, and is shown by statistics to be strongly inherited. After a few pages on the allied topic of insanity, Mr. Galton passes on to consider the gregarious and slavish instincts, where he shows from first-hand observations on wild or but partly domesticated animals the immense utility of these instincts. We ourselves inherit from our savage ancestry instincts of the same kind, and thus it is that the less intellectually developed among us are so prone to submit ourselves, like sheep, to the guidance of a leader, and even to the tyranny of a despot.

Passing on to intellectual differences, a long and interesting account is given of mental imagery, the main points of which are already known to the readers of NATURE. It is remarkable that men of science, and of hard thinking generally, are for the most part totally deficient in this faculty. The discussion of mental imagery naturally leads to the resemblance which Mr. Galton has previously pointed out between his composite photographs and general ideas ; each alike are "generic images," and in many matters of detail the analogy, or, as we should prefer to call it, the illustration, holds good.

Next we come to a chapter on Psychometric Experiments, which is devoted to an account of interesting experiments on the association of ideas. The influence of early association and sentiment is shown by these experiments, and by considerations drawn from them, to be much greater than is generally supposed.

One of the most interesting chapters in the book is that which next follows on the History of Twins. It will be remembered that the main fact elicited by this inquiry is that nature counts for much more than nurture ; for it is shown that "instances exist of an apparently thorough similarity of nature, in which such difference of external circumstances as may be consistent with the ordinary conditions of the same social rank and country do not create dissimilarity. . . . The twins who closely resembled each other in childhood and early youth, and were reared under not very dissimilar conditions, either grow unlike through the development of natural characteristics which had lain dormant at first, or else they continue their lives, keeping time like two watches, hardly to be thrown out of accord except by some physical jar. . . . The effect of illness, as shown by these replies, is great, and well deserves further consideration. It appears that the constitution of youth is not so elastic as we are apt to think ; but that an attack, say of scarlet fever, leaves a permanent

mark, easily to be measured by the present method of comparison."

The essay which follows on the "Domestication of Animals" is not so interesting, because not so original, as the rest of the book; all its points are obvious to any one who has thought about the subject at all.

A consideration of the Possibilities of Theocratic Intervention next leads the way to a reappearance of the author's paper on the Objective Efficacy of Prayer. Here the logic is unexceptionable as far as it goes, but it is not such as to leave no loophole of escape for orthodox belief. The argument is that if prayer is of any avail in an objective sense, it ought to admit of being shown by the statistical method to be so. But, as the present writer pointed out nine years ago when considering this essay, the statistical method applied to such a case is of doubtful validity. To show this we may quote one paragraph from our previous criticism:—

"What, then, is the whole state of the case? To illustrate it most fairly, we shall take the strongest of the examples supplied by Mr. Galton, viz. that of the Clergy. As Mr. Galton truly observes, in no other class are we so likely to obtain men of Prayer. Suppose, then, for the sake of calculation, that one-half of the clergy are sufficiently prayerful to admit of their petitions influencing the course of physical phenomena. Next, let us suppose that one-half of their successful petitions for physical benefits are offered on behalf of individuals other than themselves: this is equivalent to reducing the number of the prayerful clergy to one-fourth. Here we ought to add that in whatever degree this section of successful prayers may influence the prayerless classes of the community, in that degree is the comparison still further vitiated. Neglecting this point, however, let us lastly suppose that one-half of the petitions for physical benefits offered on the petitioner's own behoof are answered by physical benefits of some other kind; . . . this is equivalent to reducing the original number to one-eighth. Now I do not think any of these suppositions are extravagant. Let us see the result of applying them to Mr. Galton's tables. According to these tables, the clergy as a class live, on an average, two years longer than men of any of the other classes quoted, notwithstanding we are repeatedly told that, as a class, they are the most poorly constitutioned of all. Now, neglecting the last-mentioned point, and also the fact that all clergymen do not pray for long lives; still, even on the above data, an average of two additional years over all the clergy allows, when concentrated into one-eighth of their number, an average of sixteen additional years of life to every pious divine. Of course this illustration is not adduced in order to prove that prayer has in this case been observably effectual. The greater length of life enjoyed by the clergy may be conceded due to the cause assigned by Mr. Galton—viz. the repose of a country life—or to any other cause, without in any way affecting the present argument. All we are engaged in showing is that the statistical method is not a trustworthy instrument wherewith to gauge the physical efficacy of prayer; and the above illustration has been adduced to show that even if the petitions of the pious clergy for lengthened days were somewhat more effectual than those of Hezekiah, statistics would still be so far unable to take cognisance of the fact that the observable average increase of two years over the entire body of the clergy might reasonably be attributed to other causes. Yet length of days is perhaps the most conspicuous, and therefore the most easily tabulated, of all physical benefits for which it is possible to pray."[1]

After some well considered remarks on Enthusiasm, or "to what degree the strong subjective views of the pious are trustworthy," the book begins to draw towards its final object, which is virtually that of marking out the lines of what may appropriately be called a new religion. We have of late had so many manufactures of this kind that the market is somewhat glutted, and therefore it is very doubtful how far this new supply will meet with an appropriate demand; but we can safely recommend Mr. Galton's wares to all who deal in such commodities as the best which have hitherto been turned out. They are the best because the materials of their composition are honesty and common sense, without admixture with folly or metaphor. He says: "We may not unreasonably profess faith in a common and mysterious whole, and of the laborious advance, under many restrictions, of that infinitely small part of it which falls under our observation, but which is in itself enormously large, and behind which lies the awful mystery of all existence." Having, then, this faith in the seen, and observing that, whatever the far-off divine event may be to which the whole creation moves, the whole creation is certainly moving in an upward course of evolution, Mr. Galton submits that man has now reached a level of intelligence which should enable him, not merely to know these things, but to do them. He ought to "awake to a fuller knowledge of his relatively great position," and begin to regard it as his high prerogative to cooperate with the unknown Worker in promoting the great work. He may infer the course that evolution is bound to pursue, and might therefore "devote his modicum of power, intelligence, and kindly feeling to render its future progress less slow and painful. Man has already furthered evolution very considerably, half unconsciously and for his own personal advantages; but he has not yet risen to the conviction that it is his religious duty to do so deliberately and systematically."

Several directions in which such assistance might be yielded are pointed out in the concluding pages of the book, especially in the way of "eugenics"; and there can be no question that, if the idea of promoting evolution could become generally, or even largely, invested with a feeling of obligation, the prospects of the race would be greatly brightened. The most important field of human activity under such circumstances would obviously be that of improving the race by selection, and Mr. Galton throws out several well considered suggestions as to the way in which this might be done without violating so precious a product of evolution as the moral sense, or seriously interfering in any other particular with the ordinary usages of civilised life.

We have said enough to show that in respect of its matter "Human Faculty" is an unusually interesting work; but we should not do it justice were we to conclude this brief notice without alluding also to its manner or style. There is a strand of humour woven through the serious texture of the whole, which, together with the ingenious cast of thought and the ingenuous cast of feeling, affords a most pleasing and instructive study, unconsciously presented, of the nature and nurture of an English man of science.

GEORGE J. ROMANES

[1] Burney Prize Essay on "Christian Prayer and General Laws," pp. 265-6 (Macmillan and Co., 1873), where other and more important considerations against this application of the statistical method are given. [I may observe that this essay was written on a thesis which was set by the Vice-Chancellor of Cambridge, and I still think that, upon its given basis of Christian belief, all the more important of its arguments hold, both as regards prayer and miracles.—G. J. R.]

THE GEOLOGICAL HISTORY OF BRITAIN

Contributions to the Physical History of the British Isles; with a Dissertation on the Origin of Western Europe and of the Atlantic Ocean. Illustrated by 27 Coloured Maps. By Edward Hull, F.R.S., &c. (London: Stanford, 1882.)

IF Geology may be correctly described as a history of the earth, then a geologist is in the first place and essentially a historian. His function is to trace back the gradual growth of the world, organic as well as inorganic, and to show through what successive stages the present conditions of geography and of life have been reached. His materials, like those of the historian of human progress, become fewer and less reliable in proportion to their antiquity. More and more as he pilots his way into the records of the remoter past is he driven to piece together their evidence with conjecture, until at last evidence of every kind fails him, and he is reduced to mere speculation. There is undoubtedly a strong temptation to minds of a particular order to indulge in wide excursions into the unknown realms of primeval cosmogony. The fewer the facts that may serve as guide-posts the greater the scope for the fancy. So long as the picture does not appear to outrage our established conceptions of physical law its enthusiastic limner considers himself within the safe limits of fact or, at least, of legitimate inference. He does not stop to consider whether his restoration may not in itself be flagrantly improbable, or whether enough may not be already known on the subject to show that it is quite untenable. In this way much harm has been done to the progress of sound geology.

The attempt to restore former aspects of the globe, or at least of different areas of its surface, may be made with fair measure of success up to a certain point. As the geologist goes beyond that point he leans more and more on conjecture. It is very desirable, for his own sake as well as for that of the subject, that the actual data on which he proceeds should be definitely stated. His readers ought to know exactly where ascertained fact ends and restoration begins. Yet he may be so convinced of the truth of his restoration that, until challenged to set down in definite form the amount of evidence actually at his command, he may honestly have come to regard some of his deductions as well-established truths. He cannot, however, be too careful to draw a clear and sharp line between what he knows and what he infers, when it is his object to write geological history.

One of the most attractive branches of this history is that which deals with the gradual growth of a country or continent. Many interesting and important memoirs on this subject have appeared, more especially in England, where it has long been a favourite study. Sketch-maps have been published indicating in a somewhat vague way what the authors believe to have been the probable distribution of sea and land at former geological periods. Among those who by their original researches have contributed materials towards the restoration of ancient geographical conditions in Britain, Prof. Hull, the Director of the Irish Geological Survey, deserves honourable mention. His papers upon the changes that occurred during Carboniferous, Permian, and Triassic times, and upon the south-eastward attenuation of the Jurassic series in this country are well known to geologists. He has now, however, attempted a much more ambitious task than any one has yet ventured upon in this department of science. He has published a series of maps representing what he conceives to have been the successive geographical phases through which the region of the British Islands has passed from the earliest geological times. Without discussing the question whether the information at the disposal of geologists is yet sufficiently ample and precise to warrant an attempt of this kind, one may at least demand that every care should have been taken to show precisely what is actually known fact and what is inference. But Mr. Hull gives us scanty guidance in this respect. There is not one of his restorations that does not prompt the question on what grounds its details have been put together. The position of former areas of sea is usually sufficiently definable, but it is by no means so easy to say what was land, and still more difficult to assign even the most conjectural outlines to the shores. The author doubtless thinks his geographical boundaries vague enough; we are inclined to regard them as a good deal more definite than the actual evidence in many cases warrants. To take as an illustration his map of Britain during the Upper Silurian and what he terms the "Devono-Silurian" periods; we should like to know on what grounds he makes Wales, the Lake Country, the north-west of Ireland, and much of the Highlands of Scotland elevated land at that time. The evidence, so far as we are aware, is rather in favour of these areas having been under the Upper Silurian sea; at least we know of no proof that they formed high lands, even after the plication and metamorphism he refers to. Nor is there any information as to why the author marks the area from the mouth of the Humber to the middle of Norfolk as part of his continental land. He mixes up in a curiously unintelligible way his "Devono-Silurian" and Lower and Middle Devonian formations, some of the estuaries or lacustrine areas being placed with the older group of strata, others with the younger, in accordance with certain theoretical ideas which he has already published.

According to Prof. Hull's maps, most of the high grounds of Britain have been elevated dry land since the Lower Silurian period. No one, however, who has seriously studied how the land is continuously denuded, can believe this representation to be even approximately true. Our mountains must have been many times, and probably for long intervals, under water. Even if no large amount of sedimentary material were laid down upon them, their submergence would at least protect them from the degradation which would otherwise have worn them down. How does Prof. Hull know that Ireland, which was almost if not entirely under water during the Carboniferous period, did not remain more or less in the same condition through several succeeding ages? The presence of Permian and Triassic deposits in the north-east of the island shows that considerable denudation of the Carboniferous rocks had taken place there before these red strata were laid down. But surely it is rather a large inference from these slender data that all the rest of the country was land, with high grounds where we see them still. How can he tell that Ireland was not entirely submerged beneath the Jurassic sea? Had it not been

for the protecting sheets of basalt in Antrim, probably no fragment of Lias or Oolite would now have been left in the island. Prof. Hull submerges his country a little more in the Cretaceous period, but still keeps the high grounds as islands. Can he produce any evidence that they were so? Has he sounded the Cretaceous Ocean about which he is so precise? The denudation of Ireland has been unquestionably enormous, but had the country been above water as long as the Director of its Geological Survey imagines, we fear that every geological formation would have been worn off its surface down to the very platform of its fundamental or Laurentian gneiss. In fact the continued survival of the country above water could only have been maintained by repeated uplifts that in some measure at least compensated for its superficial degradation.

The chapters accompanying the maps furnish the reader with some of the information he requires to be able to estimate the extent of the data on which the restorations have been constructed. But they do not give him nearly enough of it. Some of their statements moreover will provoke criticism not less than the maps themselves. The author asserts, for instance, as if it were an established fact, that what he regards as the "essentially oceanic" conditions under which the Chalk was formed prevail from Ireland to the shores of the Caspian, and from Belgium to North Africa. We can hardly suppose him to be ignorant of the fact that the Chalk is but a local development of calcareous matter confined to the western part of the European area. Yet the author not only spreads the Chalk across most of Europe and into Africa and Asia, but proceeds to infer from this asserted extension that "according to all the laws of terrestrial mechanics" the site of much of the North Atlantic must have been then dry land. In other words, he first infers a wide deep ocean, and then creates a continent to keep it company.

One of the chapters, with the sounding title of "The Genesis of the North Atlantic Ocean," will be read with amazement by those who have watched the progress of recent research on this question. The author begins it by the following oracular announcement: "I date the genesis of the North Atlantic Ocean, properly so called, from the close of the Carboniferous period; and, consequently, from the same period, that of the British Isles and Western Europe." One is disposed at once to ask what may be his "exquisite reason" for this extraordinary statement, and he frankly volunteers it. It appears to be somewhat as follows:—The Carboniferous rocks of Western Europe were much disturbed at the close of the Carboniferous period, being thrown into east-and-west ridges. Similar movements took place over the eastern States of North America, the direction of the ridges being there more nearly north and south. It may be concluded, therefore, that the formation of the basin of the Atlantic Ocean formed part of these terrestrial movements!

In his Preface the author tells us how he had long entertained the idea of preparing such a series of maps as he has now published, and how he was deterred by the cost of publication. At last, in what we venture to think was an evil hour for his reputation, the Royal Dublin Society generously agreed to bear the expense. The maps were therefore prepared and published in the Society's *Transactions*, and a fresh impression has been printed off from the plates for the volume just issued. Fortune would have been kinder to one whose long services entitled him to gentle treatment at her hands had she induced him still to keep his restorations in the privacy of his own portfolio, at least for some years to come, or, if they must be published, had she insisted on greater accuracy in the statement of what is known and greater precision in the expression of what is conjectured.

OUR BOOK SHELF

Die Verwandlungen der Tiere. Von Dr. Otto Taschenberg, Privat-dozent in Halle. Pp. 268, with 88 Illustrations. Small 8vo. (Leipzig: G. Freytag, 1882.)

THIS forms the seventh volume of the series known as "Das Wissen der Gegenwart," the object of which is to give, in an attractive and popular form an outline of the "science of the day." Metamorphosis and development are always interesting subjects, and we are of opinion that Dr. Taschenberg has contrived to place them before his readers in a specially clear manner by choosing a few types in each class of the animal kingdom upon which to dilate, leaving the blanks to be filled in by more advanced students than those for whose instruction this elementary treatise is intended. The author goes in this manner through the entire animal kingdom, and so far as we can see he is well posted up in most of the latest discoveries and theories bearing upon his subject; we miss, however, any allusion to the disputed position of *Limulus*, although the metamorphoses of that remarkable animal are not entirely overlooked. A work such as this is naturally to a large extent a compilation, and in all such works the good or bad influence exercised depends upon the acumen of the author in his choice of subjects and authorities. In the present instance our author seems usually to have consulted the best and most modern authorities. The numerous illustrations are mostly very good; some of them are superlatively so. In these, as in the text, various works have been laid under contribution; and probably to no work is the author under greater obligation than the text-book on embryology by the lamented F. M. Balfour, but due acknowledgment is always made.

The concluding chapter is devoted to a sketch of the "evolution of species," in which, in a few pages, the author has contrived to give succinct historical information, winding up with a definition of "protoplasm," in connection with which a German translation from well-known English lines is given, and perhaps the definition was so modelled as to fit the lines. We reproduce them, just to show what latitude may be allowable in translation:—

"Der grosse Cäsar tot und Lehm geworden
Verstopft ein Loch wohl vor dem rauhen Norden,
O dass die Erde, der die Welt gebebt,
Vor Wind und Wetter eine Wand verklebt."

LETTERS TO THE EDITOR

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

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

Natural Selection and Natural Theology

I AM very glad to find from Prof. Asa Gray's last communication (NATURE, vol. xxviii. p. 78) that the result of our "amicable discussion" has been that of coming to an agreement on all

points save one, which, as he truly observes, is "seemingly capable of settlement by scientific inquiry." This point simply is as to whether variation in plants and animals is promiscuous (not "lawless") or is restricted to beneficial lines.

Now with reference to this point, I observed in my first letter (NATURE, vol. xxvii. p. 362) that if variation is promiscuous it is only the favourable variations that are able to survive, and hence the sole ground of entertaining natural selection as an agency in the process of evolution; but that, on the other hand, if it could be shown that variations always take place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of natural selection, then the evidence of design as deduced from the theory of evolution would become comparable with that evidence as deduced from the theory of special creation. But I also pointed out that "the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance." And now I understand Prof. Gray accepts this as a correct statement of the case, observing in his last letter that, if variation is promiscuous, "then no doubt the theory of natural selection may be 'the substitute of the theory of special design,' so as to efface that evidence of underlying intelligence which innumerable and otherwise inexplicable adaptations of means to ends in nature was thought to furnish. If it is not so, then the substitute utterly fails."

It is most satisfactory to me that the issue has thus been clearly reduced to a simple matter of scientific observation, and I may add that I am much interested to find that a naturalist of such high standing as Prof. Gray still holds to the view that, "so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation." Of course, if I had not believed in "aimless variation" as of universal occurrence in organic nature, I should never have supposed that the theory of evolution by natural selection could in any way touch the theory of special design; but finding that my fundamental belief in this matter is still open to question by so esteemed an authority as Prof. Gray, and observing that we are here upon the ground of a purely scientific question, I should like to say a few words in justification of this belief.

No one has attended to the subject of variation with a tenth part of the care that was bestowed upon it by Mr. Darwin, and no one has been gifted with a better judgment in matters of this kind. I shall therefore restrict myself to giving a brief outline of his matured opinion upon the subject.

Everywhere he speaks of variation as promiscuous or aimless, but never as "lawless," and only under a carefully guarded meaning as accidental. That is to say, he has no doubt that every variation is due to causes, though not of a teleological kind. Of these causes he regards changes of environment as highly important; but nevertheless he is inclined to lay less weight on these "than on a tendency to vary due to causes of which we are quite ignorant."[1] But with reference to variations not taking place exclusively in beneficial lines he says: "As man has domesticated so many animals and plants belonging to widely different classes, and as he certainly did not choose with prophetic instinct those species which would vary most, we may infer that all natural species, if exposed to analogous conditions, would, on an average, vary to the same degree. . . . We have abundant evidence of the constant occurrence under nature of slight individual differences of the most diversified kinds; and we are thus led to conclude that species have generally originated by the natural selection of extremely slight differences; . . . although each modification must have its own exciting cause, and though each is subjected to law, yet we can so rarely trace the precise relation between cause and effect, that we are tempted to speak of variations as if they arose spontaneously. We may even call them accidental, but this must be only in the sense in which we say that a fragment of rock dropped from a height owes its shape to accident. . . . If an architect were to rear a noble and commodious edifice without the use of cut stone, by selecting from the fragments at the base of a precipice wedge-formed stones for his arches, elongated stones for his lintels, and flat stones for his roof, we should admire his skill and regard him as the paramount power. Now the fragments of stone, though indispensable to the architect, bear to the edifice the same relation which the fluctuating variations of organic beings bear to the varied and admirable structures ultimately acquired by their modified descendants. . . . The shape of the fragments at the base of our precipice may be called accidental, but this is not strictly correct; for the shape of each depends on a long sequence of events, all obeying natural laws; . . . but in regard to the use to which the fragments may be put, their shape may be strictly said to be accidental. . . . Can it be reasonably maintained that the Creator intentionally ordered, if we use the word in any ordinary sense, that certain fragments of rock should assume certain shapes, so that the builder might erect his edifice? If the various laws which have determined the shape of each fragment were not predetermined for the builder's sake, can it be maintained with any greater probability that He specially ordained for the sake of the breeder each of the innumerable variations in our domestic animals and plants;—many of these variations being of no service to man, and not beneficial, far more often injurious, to the creatures themselves? Did He ordain that the crop and tail-feathers of the pigeon should vary in order that the fancier might make his grotesque pouter and fantail breeds? Did He cause the frame and mental qualities of the dog to vary in order that a breed might be formed of indomitable ferocity, with jaws fitted to pin down the bull for man's brutal sport? But if we give up the principle in one case,—if we do not admit that the variations of the primeval dog were intentionally guided in order that the greyhound, for instance, that perfect image of symmetry and vigour, might be formed,—no shadow of reason can be assigned for the belief that variations, alike in nature and the result of the same general laws, which have been the groundwork through natural selection of the formation of the most perfectly adapted animals in the world, man included, were intentionally and specially designed. However much we may wish it, we can hardly follow Prof. Asa Gray in his belief 'that variation has been led along certain beneficial lines,' like a stream 'along definite and useful lines of irrigation.'"[1]

I could give a number of other quotations to the same general effect from the writings of Mr. Darwin, but I think these are enough to show, as I have said, that if there is any evidence of variations being determined in special and beneficial lines, it now lies with the teleologist to adduce such evidence. If this could be done it would be a matter of immense importance, both from a scientific and a speculative point of view, seeing that on the scientific side it would be subversive of the whole theory of natural selection, and on the speculative side would therefore leave us where we were before the publication of the "Origin of Species." But at present the whole weight of such scientific evidence as we have appears to me unquestionably opposed to Prof. Gray's statement that, "so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation."

GEORGE J. ROMANES

[1] 'Origin of Species," 6th edition, p. 107.

[1] "Variations of Animals and Plants under Domestication." Second edition, vol. ii. pp. 401-2, 410, 416, 426-8.

Carson Footprints

IN NATURE (vol. xxvii. p. 578) which I have just seen, the Duke of Argyll calls your attention to the so-called human footprints uncovered in the prison yard at Carson, Nevada. I have carefully examined these tracks, and read a paper on the subject before the California Academy of Science, August 27, 1882. Unfortunately the *Proceedings* of the Academy have not yet been published, though copies of the several papers on this subject have been printed and privately distributed. Perhaps a brief account of these tracks will be interesting to your readers.

The nearly horizontal strata in which they occur consist of beds of sandstone with thin layers of fine shale. The track layer, which is one of these latter, has been uncovered over an area of nearly two acres, and forms the floor of the prison yard, while the stone removed has been used to build the prison. In the course of the excavation a number of fossils have been found, among which the most important are the jaws and teeth of an elephant, probably *E. Americanus*, and two species of horse, *Equus Pacificus* and *occidentalis*; some freshwater shells, all of recent species, have also been found. The age of the deposit seems to be that of the "Equus beds" of American geologists, which by some are put in the uppermost Pliocene, and by others in the lowest Quaternary. It is probably a transition between the two.

The whole surface of the shale exposed in the prison yard is literally covered with tracks of many kinds, but the mud was so soft when the tracks were made that the nature of many of them can only be guessed. Some were probably those of a horse; some probably of a wolf; some certainly of a deer;

many were those of long-legged wading birds. But the most interesting are those of the Mammoth and the problematical so-called human tracks. About the Mammoth tracks there can be no doubt. Some of these were uncovered by blasting in my presence; round basin-shaped impressions, 5 inches deep and 22 inches across, and occurring in regular alternating series, the hind-foot tracking almost perfectly with the fore-foot. The nature of the so-called human tracks, however, is far more doubtful. These occur in several regular alternating series of 15-20. In size they are 18–20 inches long, and 8 inches wide. In shape they are many of them far more curved than the human track, especially in soft mud. The stride is 2½ to 3 feet, and even more. The outward turn of the track is in many cases greater than in human tracks, especially in soft mud. But the most remarkable thing about them on the human theory is the straddle, *i.e.* the distance between the right and left series. This I found to be 18 and even 19 inches, which was fully as great as that of the mammoth tracks. This is probably the greatest objection to the human theory. On the other hand, the great objection to the quadrupedal theory is the apparent singleness of the tracks, and the absence of claw-marks. But it must be remembered that the tracks are deep, and the outlines somewhat obscure, and also that the mammoth tracks, on account of tracking of hind with fore-foot, are in most cases, though not always, single.

After careful examination for several days, the conclusion I came to was that the tracks were probably made by a large plantigrade quadruped, most likely a gigantic ground-sloth, such as the Mylodon, which is found in the Quaternary, or the Morothenium, which is found in the upper Pliocene of Nevada. The apparent singleness, the singular shape, and the large outward turn of the tracks I attribute to the imperfect tracking of hind and fore-foot on the same side, while the absence of claw-marks was the result of the clogging of the feet with mud.

This view seems to me most probable,[1] but many who have seen the tracks think them human, and I freely admit that there is abundant room for honest difference of opinion. On any theory the tracks are well worthy of scientific attention.

Berkeley, California, May 12 JOSEPH LE CONTE

Cloudiness of Aquarium

CAN you tell me the reason why the water in my fresh water aquarium will not remain clear, but becomes cloudy throughout in a few days after filling.

The aquarium in question holds about twelve gallons of water. It stands in a window facing north. I have in the water two or three water-plants, among them a water-aloe. At the bottom are small gravel stones, which have been thoroughly washed before using. Floating on the surface for the benefit of a few newts is a piece of virgin cork, on which is placed some carpet moss. I had a dozen minnows and four newts to begin with, but nine of the minnows and two of the newts have died, manifestly from the fouling of the water.

The framework of the aquarium is iron, with a slate floor. The glass sides are fixed with red lead. There is a copper tube for overflow purposes, which was inserted when a fountain was used in the centre. This has now been removed and the water is stagnant.

It is now some years since I have kept an aquarium, and I cannot divine the reason for the above-mentioned cloudiness of the water. I shall be much obliged if you or some of your correspondents will help me. X.

May 9

So far as I can judge from "X's" description, the cloudiness of the water in his aquarium is due to the abnormal development of some unicellular algal (Palmellaceæ) or to the prolific spore-production within it of one of the filamentous forms (Confervaceæ). This may be obviated by screening the back of the tank from the access of light. Possibly "X" may find on examination that the cistern whence he obtains his supply has been left uncovered, and that the intruding algal has established itself and entered upon the reproductive process in that position. In that case he should either isolate the water he requires in a dark place for a week or so, when the spores will die, or obtain his supply from a purer source. An investigation with a high power of the microscope of the turbid water complained of will speedily determine whether the explanation here suggested is the correct one. By way of illustration, I may mention that the water of the ornamental pond in the centre of the Horticultural Gardens, supplied clear and bright shortly before the opening of the Fisheries Exhibition, had assumed within a few days and still retains the colour and consistency of green-pea soup through the rapid development, under the action of light, of a unicellular cryptogam in the manner above described. W. SAVILLE KENT

[1] Views similar to my own have recently been expressed by Prof. Marsh and by G. K. Gilbert.

Singing, Speaking, and Stammering

REFERRING to the letters in NATURE (vol. xxvii. p. 580) on my classification of vowel sounds, allow me to explain:—

The classification given in the "Principles of Elocution" (4th ed., 1878) was retained from the earlier editions of that work, because of the difficulty, or impossibility, of exhibiting the complete vowel system of visible speech without V.S. symbols. For the purposes of the book on Elocution, the latter were not required; but a *note* (on p. 36) immediately preceding the "General Vowel Scheme" explains the basis of the complete classification developed in visible speech.

As you have given an abstract of my classification, quoted by Dr. Stone from "Principles of Elocution," I shall be glad if you will show your readers the following abstract of the visible speech classification:—

Classification of Vowels in Visible Speech

Nine Lingual positions yield 9 Primary vowels ... ...
Each Primary vowel yields a "Wide" variety by faucal expansion = 9 Wide vowels ... ... = 18 Lingual vowels.
Each Lingual vowel yields a "Round" variety by labial contraction ... ... = 18 Labio-lingual vowels.
= 36 Normal vowels.

Each Normal vowel yields a possible variety by *higher*, *lower*, *broader*, or *narrower* formation = 36 + 144 = a total of 180 vowels.

The mutual relations of the different sounds may be exhibited in this way:—

LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

LABIO-LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

In this arrangement, each No. 1, No. 2, No. 3, &c., in the four sets is formed from one and the same lingual position. These relations are plainly exhibited in the symbols of visible speech. They cannot be shown by ordinary letters, but the use of *numbers*, as above, may make the arrangement clear to those who are not acquainted with visible speech.

Washington, D.C., May 12 ALEX. MELVILLE BELL

On the Cold in March, and Absence of Sunspots

I WAS travelling when Dr. Woeikof's letter appeared in NATURE (vol. xxviii. p. 53), and could not sooner reply to his criticisms on my communication (vol. xxvii. p. 551), "Unprecedented Cold in the Riviera—Absence of Sunspots." Let me first remark that I do not go so far as to "ascribe (as Dr. Woeikof says that I do) the great cold of March, 1883, at the

Riviera, to the absence of sunspots." My observations prove only the *coincidence* of a sudden and unprecedented visitation of cold, with an absence of sunspots (the more remarkable as occurring during a *maximum* sunspot period); and the further *coincidence* of a progressive rise in temperature with the return of the sunspots; but I add, "These observations are too few and too imperfect to warrant any decided conclusions; but they add to those already made in evidence of the connection between the absence of sunspots and the diminution of terrestrial heat; and I trust they may be followed by further and more exact investigations, to determine the influence of our great luminary on the weather and climate of the world."

It does not appear to me that Dr. Woeikof has succeeded in establishing a parallel between Cannes and Suchum-Kale on the Black Sea; which, however sheltered locally, must, far more than Cannes, be liable to chilling influences in the cold winds from the lofty mountains and vast elevated steppes to the north, extending even to the Arctic regions. Therefore the fall of 31° below average in March, 1874, might not be extraordinary, even in a *year* with a considerable number of sunspots. It is not stated that the spots continued in this particular month.

The case of Cannes may be thus stated: With a climate usually so mild in winter that frost and snow are of rare occurrence; and this winter, with slight frost only three times before February, and none at all in that month, the average minimum being 44°,—on March 7 minimum fell to 36°, with a heavy fall of snow; and on the 8th, 10th, 11th, and 12th, the minimum fell further to 27°·7, 27°, 24°·1, 25°·7. The sunspots, which had been observed by my friend, Mr. Campbell, of Islay, to be large and active until February 26, suddenly disappeared, and on February 28 and March 3 I found no spots; on the 10th and 11th only one or two small spots. On the 12th they began to appear in numbers, with a large oval facula. From that day they continued to increase, and the temperature gradually rose to the ordinary average.

I will not occupy space with further arguments, but I will merely state some more facts with regard to the extraordinary intensity and universality of this invasion of cold, and my further observations of the sunspots. At my villa at Cannes, which is favourably placed in position and shelter, the register did not fall so low as in other parts. At Dr. Frank's villa, Grand Bois, more open to the north (thermometer in louvred box, a metre above ground), the minima were: March 7, 27°; 9, 25°·2; 10, 21°; 11, 21°; 12, 20°; 13, 25°. At Villa Beaulieu, more sheltered (therm. also in louvred box), minima were: March 7, 29°; 9, 27°; 10, 25°; 11, 26°; 12, 28°. Dr. de Valcourt's minima are somewhat higher; but he adds this note: "La période de froid du 7 au 14 Mars, 1883, a été très remarquable; elle est unique, depuis que les observations regulières ont été recueillis à Cannes." Where instrumental records are wanting, we refer to the report of the "oldest inhabitants," and learn that there has not been a cold so severe or destructive to oranges and olives since the year 1820.

Extraordinary and intense as was this invasion of cold, it might have been supposed due to local or regional causes only, had it been confined to Cannes and its neighbourhood. In my former paper I stated that I was not informed how far the cold had extended to other countries and latitudes. We still need further exact information on this point, but what has already reached us goes far to prove that the cold was universal, and not limited to a region. In England, Mr. Thomas Plant writes to the *Times* from Moseley, Birmingham:—"After one of the mildest winters registered in the Midland Counties, the month of March, which is generally expected to be the beginning of spring, has been colder this year than any corresponding month for 38 years." "When we consider the power of the sun in March, as compared with December, January, and February, then we can realise some idea of the prolonged and most abnormal cold of the month now ended." By private information I learn that at the same time, in Stockholm, Centigrade's thermometer fell 13°, and at St. Petersburg 18°, below freezing. Unusually intense cold in March is also reported from Canada. In the south we hear of snow and frost in South Italy, Sicily, Algeria, Egypt, and even Nubia. Later still there have been reports of snow on the mountains of Madeira and California, where it had never been seen before.

Since March 19, the date of my former letter, I have been able to make sketches of the sun's position on 49 days.[1] Of these observations the following summary may suffice. In number the spots varied from 3 to 18; the larger showing, more or less, holes or clefts of central umbra, with fringe of penumbra. Faculæ, or clouds of whiteness, were often seen around the larger spots. The spots varied in number and form from day to day; and although the same large spots and even groups could be traced for several successive days, they never retained the same aspect during the whole period of the sun's semi-rotation. On April 17 the spots were at their maximum; in number 18, in three groups. During this period, from March 19 to April 19—thirty-one days—the mean minimum temperature was 46°·2, mean maximum 57°·9.

From April 20 to May 7 there was considerable diminution of the spots; numbers not exceeding 8; and on May 7 there was only one large spot, with surrounding facula. The mean temperature of these seventeen days was—minimum 49°·8, maximum 60.

From May 8 to 16 spots were few, from 2 to 8; but two of them were very large, with umbra and penumbra and sometimes adjoining faculæ. The mean temperature of these nine days was—minimum 52°·7, maximum 63°·8.

Here my observations terminate, as I left Cannes on the 16th, and have no means of observing in London, even if the atmosphere permitted. But I conclude by strongly commending the attentive study of the sun not only to astronomers and physicists, but also to practical meteorologists, as an interesting and not difficult addition to their work of observation, and one likely to supply information concerning the most important factor in the problems of weather and climate.

C. J. B. WILLIAMS

47, Upper Brook Street, May 25

[1] I use only a modest achromatic of 32 inches focus, and 2¼ inches aperture, which, projecting the solar image on a white card, exhibits the spots with umbra and penumbra, and the faculæ, sufficiently for this purpose. Of course a more powerful instrument would show a great deal more, both in number and in construction of the spots. For instance, on April 17, when I made out 18 spots, Mr. Campbell's solar image exhibited 104, with a marvellous variety in the larger spots, and in the dome-like expansion of the adjoining faculæ. But these details, so deeply interesting in heliography, are not wanted for meteorological purposes.

The Soaring of Birds

MY thanks are due to Mr. R. Courtenay for the notice he has taken (NATURE, vol. xxviii. p. 28) of my letter on the Soaring of Birds (vol. xxvii. p. 592). It is a great satisfaction to me to find my general conclusion supported by his observations. As to the possibility of a soaring bird utilising a downward current of air, I stand corrected. There is no difficulty in agreeing with Mr. Courtenay that the bird, finding itself in a downward current "will descend swiftly o as to acquire the necessary impetus for a rapid escape;"—that is to say, it will seek to make the best of a bad bargain. But it is not so easy to see that the bird, in a current approaching the perpendicular, will "acquire an impetus much more than compensating for the slight loss of elevation;"—that is, will actually make a profit out of a seemingly adverse condition.

This paradox, however, becomes more acceptable by the aid of an illustration:—A marble held lightly just within the rim of a hemispherical bowl, if let drop, will barely reach the opposite rim, but, if struck sharply downward, will run up the opposite side and leap up above the opposite rim. In like manner a bird, struck by a downward current as by a hammer-stroke, may speedily acquire a downward velocity greater than that due (under gravity) to the height through which it has descended; and may therefore rise, if it can escape from the downward current into a horizontal (or *a fortiori* into an upward) current, to a greater height than if it had fallen from the same starting-point through still or horizontally-moving air.

I am very much obliged to Mr. Courtenay for pointing out this interesting result. It gives completeness to the theorem, which now stands thus: that any alternations in the strength or direction of air-currents can be so utilised by birds as to enable them to soar.

HUBERT AIRY

Woodbridge, May 25

The Zodiacal Light

THE phenomenon to which your correspondents allude, under the head of zodiacal light, was seen by me in the month of April, 1852. At the time I wrote a letter to the *Times*, in which I suggested it might be caused by the reflection of the sunlight at the surface of two masses of air of different densities, however irregular the bounding surface might be, in the same manner as the line of light seen reflected between the observer

and the sun across the sea. One of your correspondents has suggested a more probable origin, viz. particles of ice in the air. From other correspondents it seems that the sun column is not always vertical, which might be the result of the general flame of the reflecting surface not being parallel with the earth's surface.

In the June number of the *Philosophical Magazine* there was a notice of a sun column as seen at Orkney by the Rev. C. Clouston, who at that period made meteorological observations for that publication. He says that in the month of April of that year the drought was unprecedented, the atmospheric pressure great, and the temperature high. I believe two of these characteristics belonged to the recent month of April if not the third, the high temperature. He says it was seen six times, and once or twice before he noted the date, and also before sunrise.

Saltburn, May 21 E. R. TURNER

Sheet Lightning

MAY not this be an auroral phenomenon, at times, at least, and hence the differences of opinion as to its nature? Reading Wilkes's "Narrative of the U.S. Expedition," I find the following:—"On the 7th February (1840) the weather had become less boisterous, and having reached latitude 49° S., longitude 155°·23 E., the aurora Australis again appeared. It was first seen in the north, and gradually spread its coruscations over the whole heavens; the rays and beams of light radiating from nearly all points of the horizon to the zenith, when their distinctive outlines were lost in a bright glow of light, which was *encircled by successive flashes, resembling those of heat lightning on a summer's night*. These formed a luminous arc in the southern sky, about 20° in altitude, from the upper part of which rays were continually flashing towards the zenith. Light showers of rain finally shut it out from view." FRED. PRATT

Clapton Park, May 25

Pocky Clouds

FOR twenty years I was constantly observing the forms and appearances of the clouds, as clues to the weather and its changes. I observed this form on a very great number of occasions, and from experience always came to the conclusion, "no rain today," and I can only remember two occasions on which the conclusion was not justified. I saw it again a few days ago, with the same result of good weather.

I always termed it the "bubble" cloud till I saw Dr. Clouston's work. It seems to me to be a body of vapour the upper surface of which is being acted upon by an upper current of very dry and rarefied air, causing a great and rapid evaporation, and thence a gradual and unequal cooling and shrinkage of the under surface in the detached globules from which it takes its name. I have seen a very simple illustration while passing through the laundry, and observing a neglected trough of soapsuds cooling down and nucleating in the exact form presented by the pocky cloud, and with the same gradations of tint.

This kind of cloud is generally observable at periods most probable for storms and electric condensations, the which, acting at a distance, would influence outlying areas of upper atmosphere and cause this form of cloud condensation in the way explained. In my observations I have generally found the cloud revert to uniform sheet stratus rather than to disappear in cloudlets in the upper air. FRED. PRATT

Clapton Park, May 25

Clerk Maxwell's "Devil on Two Sticks"

IN the very interesting life of Clerk Maxwell which has lately appeared there are frequent references to a philosophical toy, from which he seemed to derive endless amusement. He calls it the "devil on two sticks." Can you give your readers any account of it? The editors take it for granted that the apparatus is well known, but I cannot find any one here who can tell me what it is. DENNY LANE

72, South Mall, Cork

The Centres of a Triangle

CONTINUING my suggestion in your number of May 3 (p. 7), I propose not only to call the circle circumscribing a triangle the *circumcircle*, but also to call its centre the *circumcentre*, and in the same way to speak of the *incentre*, the three *excentres* (namely, the *a*-excentre, the *b*-excentre, and the *c*-excentre), and the *midcentre*.

The line joining the circumcentre to the orthocentre, on which the masscentre and the midcentre lie, may be appropriately called the *central line* of the triangle.

Similar abbreviations would apply to the radii of these circles; they might be spoken of as the *circumradius*, the *inradius*, the a-*exradius*, the b-*exradius*, the c *exradius*, and the *midradius*.

May 25 W. H. H. H.

THE ROYAL GEOGRAPHICAL SOCIETY

THE annual meeting of the Royal Geographical Society on Monday was of rather more than usual scientific interest. Sir Joseph Hooker was presented with the Royal Medal which the Society has awarded him, Mr. Colborne Baber being the recipient of the Patron's Medal; while among the speakers at the dinner, besides Sir Joseph Hooker, were Mr. Spottiswoode and Prof. Huxley. From the address of the President, Lord Aberdare, it is evident that geographical research, and especially exploration, has been as active as ever during the past year, yet, as the speakers we have named pointed out, the discovery of new countries must have a limit, and in time must come to an end. Still there will be plenty of work for geographers to do in the wider acceptation of the term geography, implied in the presentation of the Royal Medal to so distinguished a botanist as Sir Joseph Hooker. In the words of Mr. Spottiswoode, and as we have frequently pointed out in these pages, geography in its modern acceptation includes "an accurate delineation of the earth's surface, and an exact account of its inhabitants and of their habits, of the animal and vegetable life, and its distribution over the face of the globe." In this direction the Society has a long and brilliant career before it. But as Prof. Huxley humorously pointed out in replying for the "other societies," these societies "were all growing a little dull. He did not say this in the way of reproach. The progress made in research and accuracy in methods of procedure involved that consequence. So long as there were large regions of knowledge which the methods of modern science had not penetrated, so long was it possible to go to meetings of societies, and to hold brilliant discussions. Looking at the means which now existed for the diffusion of information, he had been led to think that in many cases where the field of knowledge had been extensively explored the utility of societies was constantly diminishing, and that sooner or later it would be necessary to devise other means of effecting the results now attained by meetings of societies. But there was one thing which would not be reached at any period of time by any other organisation than that of societies, and that was the stimulus which was given by their meetings to investigators; and the reward they found for their toils and sacrifices in such a welcome as had been given that night to his long-tried friend Sir J. Hooker."

The prosperity of the society continues to be maintained. Mr. Clements Markham read the annual report, which showed that during the year the number of Fellows elected was 163, besides three honorary corresponding members, and the total number of Fellows on the list (exclusive of honorary members) was 3392. The total net income for the financial year ending December 31, 1882 (exclusive of balance in hand and 1005*l.* sale of Exchequer Bills) was 7937*l.*, of which 5652*l.* consisted of entrance fees and subscriptions. The net expenditure during the past year was 8779*l.*, including 1135*l.* spent on expeditions. The sale of 1000*l.* of Exchequer Bills was rendered necessary to meet the Society's contribution to the Eira Relief Expedition, but this sum had since been generously presented to the Society by Mr. Leigh Smith. The investments and assets of the Society on December 31, 1882, exclusive of the map collection and library, amounted to 39,831*l.*

THE TRUE ORBIT OF THE AURORAL METEOROID OF NOVEMBER 17, 1882

AFTER many fruitless efforts to conciliate the apparently widely diverging data, given by the numerous observations of this most interesting phenomenon; and after having been many times on the same point as Mr. H. D. Taylor (vol. xxvii. p. 434), who has given the first approximate calculations of this orbit, namely, "to give up the reconciling of such contradictory evidence," I have devoted my Easter holidays to new research on the true orbit. Besides the encouraging letters received from some of the English observers, I found still another motive in the observation of Mr. Julius Dupire at Laon (France, $\beta = 49° 34'$), who had the kindness to give me ample information, for which I offer him my sincere thanks, and in the communication of the following citation, kindly given me by Prof. Ch. Montigny, of Brussels, taken from the *Bulletin de l'Observatoire de Bruxelles*, November 18, 1882: "À 6h. 23m. un énorme rayon d'un blanc vif s'éléva à l'horizon E.N.E.; il traversa le ciel en passant le zénith et alla s'éteindre à l'horizon O.S.O." A similar phenomenon has been observed by Dr. F. Terby at Louvain. The great attraction of the Laon observation consisted in the fact that the meteor's apparent path was there seen at the north side of the zenith, this being in harmony with the Brussels zenith observation, and promising a good determination of the sought orbit.

In the first place I took the following apparent orbits from the numerous given observations. They can or must be taken as great circles, and must, in this case, fulfil the condition of intersecting one another in two opposite points of the sphere. In fact their intersections are contained within a small space and gave me an approximate position to one of these two points, $\alpha = 70° 30'$, $\delta = +14° 30'$.

These five apparent orbits, the only ones given completely, are the following:—

No.	Place of observation.	Pages of NATURE, vol. xxvii.	Data of observation.	Deduced horizontal direction.	Local time of max. elevation.	Observer.
1	York, $\beta = 53° 58'$.	87, 140, 434	The centre was 6° or 7° below the moon's centre (given not directly after observation); 30° elevation in meridian (probably a mistake or a printer's error, being in contradiction with the other data).	E. 20° N.–W. 20° S. (nearly); deduced by the observer.	6h. 4 or 5m.	H. D. Taylor.
2	Clifton (Bristol), $\beta = 51° 28'$.	85	8° from Saturn, to the right, in a line inclined 45° to the horizon.	E. 20° N.–W. 20° S.	6h. 4m.	A. M. Worthington.
3	Old Windsor, $\beta = 51° 30'$.	87	First seen a little S. of Aldebaran; moves across the moon's disk.	E. 20° N.–W. 20° S.	6h. 6m.	John L. Dobson.
4	Utrecht, $\beta = 52° 5'$.	296	Aldebaran and two points in the equator at 110° and 290° R.A.	E. 20° N.–W. 20° S.	6h. 24m.	Prof. J. A. C. Oudemans.
5	Zonnemaire (near Zierickzee), $\beta = 51° 42'$.	296	Aldebaran and β Pegasi (α Pegasi on p. 296 was a printer's error).	E. 20° N.–W. 20° S.	6h. 21m.	P. Zeeman.

Tracing these five apparent orbits on a celestial globe they gave the intersection point above mentioned. It is clear that this point, joined with the eye of the observer, gives the direction of the true path. This point lying further, the globe being placed on the different latitudes and hours, not far from the point E. 20° N. of the eastern horizon (at Utrecht 7° above the horizon), it is evident that the lines of intersection, formed by the plane of the mean horizon with the planes of the apparent orbits must be nearly parallel to this direction. That these lines of intersection cannot be true parallels follows from the observations of the four students at Cooper's Hill (p. 97), from that of Mr. Joseph Clark at Street, communicated by Mr. J. E. Clark, at York; and from that of Mr. A. S. P. at Cambridge (p. 87), who saw the phenomenon disappear in the S.W., S.W. and S.S.W. Further the *Revue Mensuelle* of M. C. Flammarion (2me Année, p. 72), containing a short report of Mr. Dupire's observation, mentioned above, gives also an observation made at Ploërmel ($\beta = 47° 55'$, λ 2° 23' W. Greenwich), where the phenomenon disappeared in the west.

Now I have drawn a stereographic map on a large scale, and brought the intersection of the vertical plane through Brussels, with the bearing E. 20° N. It is clear that the true orbit must lie in the vertical plane. Further I have constructed the angles formed by the planes of the apparent orbits with the respective horizons, correcting, if necessary, for the curvature of the earth, and after much trouble found the following path, being a straight line having the properties given in the table on p. 106, that enables us at the same time to compare the results of my construction with the data of the different observations given in order from E. to W.

I hope that the observers will be content with the degree of harmony between their observations and my results. I believe that a small change in the direction of the orbit's plane will give still more harmony between calculation and observation, but the orbit found satisfies the chief observed facts, and gives the greatest divergence, where the observations have the smallest sharpness. I believe I have proved by this research that there existed, with the aurora of November 17, 1882, cosmic dust, passing through the upper strata of our atmosphere with great velocity, and giving, according to the most interesting observation of Mr. Rand Capron (p. 84), "the usual green line" of the aurora spectrum. Thus nature itself has been so kind as to give an experiment that till now, and perhaps for ever, is beyond human

Places of observation.	Height of path in kilos.; meteoroid at max. alt. above horizon.	Max. altitude of the apparent path		Comparison of the position with regard to the moon.		Horizontal bearings of the apparent path.		Observers.	Remarks.
		Deduced from the found path.	Deduced from observation	Constructed.	Observed.	Constructed.	Observed.		
I. Utrecht.	159·5	° 54	° 54	—	—	E. 22° N.	E. 20° N.	Prof. J. A. C. Oudemans.	The inclination on the horizon, from observation 6° to 7°, from construction 7°.
II. Zonnemaire (near Zierickzee).	150·6	56	62	—	—	E. 22° N.	E. 20° N.	P. Zeeman.	The altitude 62° uncertain, according to the observer's letter.
III. Brussels.	149·1	90	90	—	—	E. 20° N.	E. 22° 30′ N.	(?)	From the *Bulletin* of the Observatory. The horizontal bearing was given E.N.E.
IV. Laon.	140·9	51½	60 to 65	—	—	W. 17° S.	W.	Jul. Dupire.	The maximum altitude seems to have been determined by estimation.
V. Ipswich.	135·1	—	—	The centre 1° above the moon's centre.	Exactly across the moon.	E. 21° N.	E. 10° N. (?)	Hub. Airy.	The eastern horizon cloudy, the width of the meteoroid 5°.
VI. York.	134·3	—	—	The centre 10° below the moon's centre when nearest.	The centre 6° to 7° below the moon's centre when nearest.	E. 37° N.	E. 20° N. (p. 434) given by the observer.	H. D. Taylor.	The angle below the moon's centre is given from memory (p. 140), and was difficult to estimate. I find the horizontal bearing of the apparent path, given by the observer, E. 25° N. The small inclination to horizon makes it very uncertain.
VII. Cambridge.	133·5	—	—	—	—	W. 51° S.	S.S.W., or W. 67° S.	A. S. P.	It disappeared below the horizon in the S.S.W. (p. 87).
VIII. Greenwich.	132·1	—	—	The centre 5° above the moon's centre.	A little above the moon (p. 83).	About E.N.E., W.S.W.	E.N.E., W.	W. H. M. Christie.	P. 83.
IX. Lincoln's Inn Fields (London).	132	—	—	Across the moon's disk.	Across the face of the moon.	—	—	Edw. Pollock.	P. 141.
X. Cooper's Hill (near Windsor).	130·6	—	—	Nearly above the moon, the centre about 2° above the moon's centre.	Just below the moon.	S. 58° W.	S. 45° W.	Messrs. Sykes, Wildeblood, Thornhill, and Wackrill.	P. 99. Communicated by Mr. Herb. McLeod. The meteoroid being seen from Windsor and from Lincoln's Inn Fields, to the north of Cooper's Hill *before* the moon, it could not *possibly* be *below* the moon, when seen from Cooper's Hill. There *must* be a mistake in the communication or in the observation. The being *before* the moon is naturally a *fact*, where a mistake is *impossible*.
XI. Old Windsor.	130·5	33	30½	Across the face of the moon.	Across the face of the moon.	—	—	John L. Dobson.	P. 87. The apparent path not being very sharply given, the difference in the observed and constructed max. altitude is very small.
XII. Ramsbury (near Hungerford).	127·9	—	—	Across the moon. The centre 2° below the moon's centre.	It seemed to pass directly over the moon.	—	—	Alfred Batson.	P. 100. The given position of the moon (p. 141) seems to be geocentric. I find an altitude of the moon of about 27° at Greenwich.
XIII. Clifton (Bristol).	126·2	28½	25	The centre 6° beneath the moon's lower cusp (measured vertically). The upper boundary of the meteor 4° beneath the lower cusp.	Passing close under the moon, the centre 1½° beneath the moon's lower cusp.	E. 32° N.	E. 20° N.	A. M. Worthington.	The apparent path having but little inclination on the horizon, and its eastern extremity being not without some doubt, the intersection point is very uncertain.
XIV. Street.	126·2	—	—	—	—	S.W. by W.	And went right across the heavens to the S.W.	Joseph Clark.	P. 84. Communicated by Mr. J. Edm. Clark at York.
XV. Clevedon.	126·2	—	—	—	—	W. 32° S.	W. 20° S. (?).	Stephen Saxby and another careful observer (p. 86).	Pp. 86 and 100. The western horizon covered by trees. The altitude in the meridian, given by two observers, is 22°. The true path gives 22½°.
XVI. Ploërmel (Bretagne).	123·9	—	—	—	—	W. by S.	W.	—	"Elle s'avance rapidement vers l'Ouest." Given by the *Revue Mensuelle* of Mr. C. Flammarion.

power, for our means are not sufficient to throw projectiles with several thousand metres velocity ; and it is very remarkable that this experiment comes at the same time as the interesting experiment of Prof. Lemström, showing that electric currents are able to give a development of light in our atmosphere, possessing the same number of undulations in a second as the auroral light. Now our meteoroid being a part of an aurora, it gives a stronger proof of the origin of that phenomenon than Prof. Lemström's experiment, the greatest attraction of which is that we are able to repeat it arbitrarily and with our own means. Further, I have always maintained that electricity, excited easily by friction, must be one of the causes of the auroral light ("Théorie Cosmique de l'Aurore Polaire," *Journal des Spectroscopistes Italiens*, 1878, vol. vii. chap. ii.), and it seems to me very plausible that cosmic matter, approaching the earth, induces electric currents through the air. Therefore I think that the results of Prof. Lemström are in full harmony with the idea of a cosmic origin of auroræ.

The orbit found does not reach the surface of the earth, being at its nearest approach still 123·9 kilometres (1 mile = 1609·3 metres ; 1 German geogr. mile = 7420·4 metres) or 16·7 geogr miles from that surface. The length of the orbit from the Utrecht perpendicular line to the Utrecht horizon is 1,483,070 metres, and this line being run over in 60 seconds,[1] the mean relative velocity was 24,673 metres, 15·3 miles, or more than 3 German geogr. miles.

The dimensions of the "cosmic cloud" (length 40°, width 5°, as seen from Ipswich) are: length = 182,594, width = 21921 metres. By these dimensions, probably too great from irradiation, it must show at Utrecht an apparent length of 50° ; but the extremities were tapered and therefore the length strongly influenced by the transparency of the air. It is therefore not strange that the apparent length at Utrecht was during some few seconds 90 degrees.

To conclude, I will remark that the proved existence of a cosmic cloud, preserving its pretty sharp sides during so long a path as that from Sweden to the Atlantic Ocean, notwithstanding its velocity of 247 kilometres, proves its particles to be nearly spherical. Otherwise these particles should necessarily have diverged sideways from the orbit and spread into space. In connection with the fact observed by Mr. P. Zeeman (p. 297), that auroral clouds gave interference-phenomena, when coming before the moon's disk, and these latter phenomena requiring (Dagnin, "Traité de Phys.," iv. p. 446) the presence of nearly equal particles of dusty matter, Mr. Zeeman's observation proves the same property in the particles of the auroral cloud. Being nearly equal, but not perfectly, the tangential atmospheric resistance must throw the smallest particles backward, and this explains the oblong shape of the cloud.

In presenting my results and reasonings to the readers of this journal, I hope that they will remember that this paper has been written in a shorter time than the author had wished.

H. J. H. GRONEMAN

Groningen (Netherlands), April 7

THE AURORA BOREALIS[2]

II.

THE Aurora Borealis at Sodankylä.—Although the aurora borealis often appeared with considerable intensity, it did not boast many varieties. It began generally with a faint arc in the northern sky, which soon developed into a sharp arc, with streamers and a kind of luminous "drapery" spreading from east to west. The colour of this luminous drapery was not very changeable, so that the spectroscope only returned the usual yellow-green line. Generally it was of a yellow-whitish colour, with a slight shade of green.

There was, however, an observation made of far greater interest, viz. that the spectroscopic "reaction,"[1] *i.e.* $\lambda = 5569$, on several occasions was returned from every quarter of the horizontal plane, even from the zenith, *without any aurora being visible.* As this reaction was obtained while the ground was still bare, there can be no question of its being a reflection, but that this place was at the moment within the sphere of an auroral discharge, but of such a weak character that it did not appear in the form of aurora borealis. This observation was therefore precisely similar to the one made in 1871 in Lapland, described above.

These observations were chiefly made by my assistant, Herr Biese, who made another remarkable discovery. Nearly due south-east from the Observatory, he received on several occasions a spectral reaction from a narrow belt of the sky, *although no aurora was visible.* This observation, which was very difficult to effect, as the eye had to be kept entirely away from all light for fully five minutes before the reaction could be traced, I had myself several opportunities of corroborating. In this direction were situated some mountains 300 metres high, about 30 kilometres distant, and in my opinion the reaction was due to the above-described phosphorescent flames, which were seen around the mountain-tops in Lapland and Spitzbergen. All observations were of course made after *every trace* of daylight had disappeared.

The Phosphorescent Luminosity.—On several occasions the attention of travellers in the Arctic regions has been attracted to a peculiar soft light or "shine" during the night. But, as the change from day to night is very gradual in the Polar regions, as compared with that of southern climes, a certain amount of exertion of the mind is required in order to take cognisance and retain the features of this phenomenon. As, however, attention has been once drawn to the same, it will always be observed. Already in October I noticed it at Sodankylä, and directed the attention of my assistants to it. I give subjoined some extracts from my diary concerning this phenomenon :—

December 9, 1882.—The Polar night shows sometimes a peculiar phosphorescent "shine" or diffused luminosity, which possesses several phases, but the general character of which is a luminosity of a yellow-white colour, which renders the night as light as the moon with a thick hazy air. I take here the appearance and disappearance of the light on two nights when its intensity was greatest.

On December 6 I was on a journey between Orajärvi and Sodankylä. The phenomenon became then apparent at 7 o'clock p.m. When daylight had completely disappeared, there seemed to remain a faint light in which the outlines of objects around could only with difficulty be discerned. At 7.40 this increased, however, so rapidly that in a few moments every object around stood out clearly in a yellow-white hazy phosphorescent luminosity of quickly-shifting intensity. I had unfortunately no photometer by me by which I could determine the same. It lasted in this form until about 10 o'clock.

December 8, at 5 p.m., I walked from the Observatory to the church near it, in order to observe from its steeple some fire-signals from Oratunturi. On the way, I noticed that a yellow-white luminosity of shifting intensity filled the entire horizon, while twenty minutes after it had increased greatly in intensity, and was now strongest in the north, whence it gradually faded to the south, where it had least intensity. Near the horizon it was difficult to discern the stars. Higher up it was, however, easier, and from 60° to the zenith the sky was clear, of a mauve colour. It was exceedingly interesting to compare the light with the Milky Way. The yellow-white light

[1] This number is stated also by the sharp determination given by the Astronomer Royal, Prof. Christie.

[2] Continued from p. 63.

[1] [By this term Prof. Lemström of course refers to the *characteristic line in the spectrum* of the aurora. The term might be justified by analogy with the "reactions" characteristic of the presence of the various chemical elements.—ED.

contrasted sharply with that of the latter, particularly where the Milky Way stood out of the same. In the yellow-white light it was difficult to make out the Milky Way. This phenomenon lasted far into the night. Later in the evening, between seven and nine, there appeared an aurora of great intensity, of which I shall speak below. This luminosity gave no reaction in the spectroscope at our disposal, but no doubt it would have been obtained had this been less absorbing. Thus, for instance, the larger Wrede's spectroscope (four prisms) did not give the reaction of the auroral phenomenon at Oratunturi, whereas the smaller, as stated above, really showed the line.

There is not the least reason for assuming that this luminosity is of any but an auroral nature, and the result of these observations is *that the whole of northern Lapland is during most winter nights illuminated by a phosphorescent luminosity*, whose intensity varies greatly according to period and place, *but which is undoubtedly of an auroral nature.*

On the same day, viz. December 8, the expedition was enabled to make the first measurement in the magnetic meridian of the elevation of the auroral arc. The wire, which was laid out north and south for the study of the terrestrial current, was used as a t lephone line, and the observations thus made by signals. Two theodolites with the necessary instruments were employed, viz. one at Sodankylä and the other about 4·5 kilometres distant to the north, near the mouth of Kälujoki. The observations at the observato y were made by Herr Biese, and at the northern end of the telephone line by Herr Petrelius. The auroral arc appeared in the north and shone with a quiet, subdued light, while a streamer now and then shot forth into the sky. Six measurements were made with the following result :—At the northern station the line of sight formed, with the under rim of the arc and the horizontal plane, an angle of 9°, and at the southern station one of 12°, *i.e.* an angle 3° larger at the southern than at the northern station! Even assuming that both observers saw *the same* arc, the result is absurd, as however great the distance between the two might be, the difference of the angle would be very small indeed, and, if a difference at all, the angle of the *northern* station should have been the greatest. As, however, the reverse was the case, I have come to the conclusion *that the two observers did not see the same aurora.* A corroboration of this opinion is that on one occasion Herr Biese telephoned, "Turn the instrument to where the *red* column is," while at the northern station no such colour could be traced. This was proved still further during the return journey from Kultula to Sodankylä by the following circumstance. At Köngäs, 60 kilometres north of Sodankylä, on January 3, 1883, at 4 p.m., the whole horizon was flooded with a yellow-white luminosity of great intensity. At the same time an auroral arc formed in the south about 25° over the horizon, and a similar one was at the same moment observed at the same elevation in the north from Sodankylä. The departure from Köngäs took place just after 4 p.m., and during the journey this arc gradually disappeared, while the luminosity and the arc seen at Sodankylä were seen all through the evening. Here there was an opportunity of measuring the elevation of the auroral arc, but as I was convinced that the two phenomena were not the same, I did not attempt it.

It was clear that we were within an auroral discharge which extended considerably east and west, but the main strike of which was north and south. It is very probable that the electric current which caused this light some thousand metres above the surface of the earth also produced the above-described intense luminosity in a layer some 20 metres in depth, running parallel with the earth. It was this layer which was projected from both points into the sky in the shape of an arc. But it is clear that the auroral "drapery" did not penetrate far into the horizontal plane, but as it is generally produced in the centre of a weak discharge of great penetration its appearance from various places in the line north and south would be very variable according as the layer lends its light to the drapery.

The measurements and results described above exactly correspond with those of Mr. Fritz in Greenland (*Bulletin de la Commission polaire Internationale; Mittheil. der Internationalen Polarcommission*, Heft 3), where he obtained an auroral drapery of 650 feet, 1700 feet distant from the observer, and another one of 170 feet, 350 feet distant.

Without further discussing this question here I must state that I consider that *all* measurements of the height of the aurora, calculated on those with a long base north and south, are always erroneous, as the two observers *never* see the same aurora. And even those calculations which are based on the measurments of the height and length of an arc from *one* point, and the hypothesis that the arc extends around the magnetic pole, must be considered very unreliable, as no satisfactory answer can be given as to what results would have been obtained a little further north or south. This is also the case with auroræ with long bases east and west, as only on a shorter distance is it possible to say if it is the same phenomenon which is seen.

That the height of the aurora borealis is very variable I fully admit, but in my opinion it has been greatly over-estimated.

Researches with the Terrestrial Current.—During my expedition to Lapland in 1871, we examined the terrestrial current in two places, viz. Kittilä, lat. 67° 40′, and Enare Vicarage, lat. 68° 55′, with wires 1½ kilometres long—east and west, north and south—of copper 0·4 mm. in diameter, and finishing in platina disks 10 cm. by 5 cm., buried in the earth at a depth of 0·7 to 0·9 metre. The deflexion was measured by a galvanometer with astatic needles with telescope and scale (Weber's magnetometer, of Edlund's improved construction). The remarkable result obtained here was that the galvanometer at Kittilä, with the current east and west, gave a deflexion equal to 60 to 100 parts of the meter scale, whereas the current at Enare only gave *a fraction of one part of the meter.* With the current north and south, the difference was not so great, although even here the deflexions were smaller at Enare. It was unfortunately impossible to ascertain if this remarkable phenomenon was due to *latitude* or *season*, the researches at Kittilä being made in October, and those at Enare in the latter half of November, while on the former occasion the ground was not frozen, which it was on the latter.

The Finnish expedition this year to Sodankylä has also examined the terrestrial current, viz. during certain periods of the phenomenon every five minutes, at other times once every hour, with a wire 5 kilometres long, terminating in small platina disks in the earth. During my visit to Kuttala—Decem er 22 to January 4—I also tested the terrestrial current, but with a wire only 1 kilometre in length, running east and west. Here, too, no deflexion was shown, while in Sodankylä the current was just as strong as ever. At Kuttala the galvanometer was certainly not so sensitive as at Sodankylä, still, the experiments of 1871 are even in this respect not without importance.

I have, therefore, from these researches drawn the inference that, while the condition of the ground is of some influence, *the terrestrial current ceases at a certain latitude.* In 1871 already I maintained that the terrestrial current was caused chiefly by the electricity which descends from the atmosphere in the belt around the Pole, in which the aurora borealis attains its maximum, and my recent researches at Sodankylä have greatly confirmed this theory.

I now intend to discuss the conclusions I have come to from the above detailed researches.

Although the general belief as to the nature of the aurora borealis certainly is that it is of electric origin,

other theories have been advanced, as for instance by Grönemann, *Astr. Nachr.*, 1874-75, and the reason of this is, I believe, that hitherto no direct proof had been obtained demonstrating its true nature.

But the experiments at Luosmavaara in 1871, *and at Oratunturi and Pietarintunturi in* 1882, *clearly and undeniably prove that the aurora borealis is an electric phenomenon.*

The science of the physical conditions of the globe has hitherto, particularly as regards the electric and magnetic ones, simply advanced by observing the effects of these great forces of nature, without however any successful attempt having ever been made to influence or call them forth either directly or indirectly. My experiments now, however, prove that *aurora borealis may be produced in nature* by a simple contrivance assisting the electric current flowing from the atmosphere to the earth. And although the efforts of man must always be limited in comparison with the grand products of nature, the conclusions which may be drawn from the same are not the less instructive.

In a question wherein the theoretical deductions, supported only by a few indirect proofs, have but slowly advanced, *absolute certainty* has now been obtained, and this result should induce future students of the aurora borealis not to devote attention to the "light" phenomenon itself, but to the investigation of those wonderful forces of nature the existence of which it so "lucidly" demonstrates. We have, of course, much to learn from the light also, but far more, I believe, from the electric forces which create it.

It is, however, far from my intention to insist that the apparatus invented by me is the best or that the method followed may not be improved on; still it has certainly one advantage, viz. that of being effective. It is, of course, evident that the drawbacks under which the experiments suffered—as, for instance, weak wires and defective insulators—must be remedied, and it appears to me that the theory which is the basis of M. Mascart's insulator would be particularly suited to the apparatus. The galvanometer should also be altered so as to consist of a *great* number of well insulated coils, in order better to regulate the deflexions, and the experiments should be made in a warm room. As the electrometrical method hitherto used gives only the electric tension at a certain point, it would, it appears to me, form a good meter for measuring the electric state of the surrounding atmosphere. The galvanometer deflexions depend certainly on the electric potential, as well as on the variable conducting power of the air; but it can, as will be seen from my experiments, be measured and even divided by using a constant galvanic element. The electric condition thus measured will give us *an idea* of the strength of the electric current, which in a certain place descends to the earth, and of the electric changes which take place in the atmosphere.

From the experiments with the terrestrial current described above it seems very probable that the current is closely related to the electricity in the auroral belt. The terrestrial current is, as is generally known, related to the magnetic variations, which is most conclusively shown by Mr. Airy's curves (*Phil. Trans.*, vol. cxxxviii. p. 465). In Sodankylä disturbances of the terrestrial current were always followed by a magnetic one. The exact result has of course not yet been calculated, but a glance at the figures returned is sufficient to show this. Mr. Airy's researches have caused these questions: (1) Are the variations in the terrestrial currents more numerous than the corresponding magnetic ones? (2) Do the terrestrial variations occur about half an hour from the corresponding magnetic disturbances?

We have from the experience gained attempted to explain these peculiarities, viz. by the hypothesis that the earth forms, so to say, the core in a flexible bobbin, represented by the terrestrial current circulating around her. In the first place, many of the changes to which the terrestrial current is subject could not affect the magnetic moment of the core, *i.e.* the earth; and, in the second place, the current acts directly on the instruments whereby the magnetic variations are measured; and in these circumstances we must find the explanation of the first-named peculiarity. With regard to the very remarkable difference in time of about *half an hour*, this is the exact time elapsing before the variations of the terrestrial current can affect the magnetic moment of the earth. It is, by the bye, only necessary to compare the duration of induction currents produced in bobbins with different iron cores, to observe that *half an hour* might well pass before the current became perceptible, *if the earth constituted the core.* In Polar regions the electric current descending from the atmosphere to the earth may also contribute to the variations which are measured by our instruments.

In accordance with this theory, therefore, the *electricity* which descends into the auroral belt is the *primary* cause of the greatest part of the terrestrial current, and, through this, of the many variations of the magnetic elements. There are also others, as the diurnal changes in the temperature on the earth's surface, but *the chief* cause is, in my opinion, the electric current from the atmosphere.

In my belief, therefore, the possibility of explaining the peculiarities of this phenomenon lies in a thorough and complete knowledge of the current from the atmosphere.

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(*To be continued.*)

THE FLORA OF ANCIENT EGYPT[1]

THE discovery made by Emil Brugsch Bey on July 6, 1881, in the vault of a king of the twentieth dynasty is of the greatest importance to botany in consequence of the large number of species of plants contained in the offerings and funeral repasts and in the wreaths which adorned the illustrious dead. Among them are several which were not known to belong to ancient Egypt. I have begun the study of the remains of these plants taken from the breasts of the most celebrated kings of Egypt and of such inestimable value to science. Deputed by Mr. Maspero to arrange these relics for the Egyptological Museum of Boulak, I have classified them according to the high personages for whom they were intended. On the eight cardboards which I have the honour to send you in the name of Mr. Maspero, you have a part of the funeral wreaths belonging to Ramses II., Amenhotep I., and Aahmes I.

The wreaths of Ramses II. were renewed towards the end of the twentieth dynasty (1100 or 1200 B.C.), or at the time of the twenty-first dynasty (1000 B.C.). The king of that period, according to records inscribed on the coffins and translated by Mr. Maspero, caused a new coffin to be made for the great Ramses, the one in which he had first been placed having been accidentally destroyed. In this new coffin were several yards of wreaths, which Mr. Maspero handed to me. I have examined them all and ascertained their composition.

The wreaths of Ramses II. are formed of the leaves of *Mimusops Schimperi*, Hochst., either folded or torn in

[1] "Memoir on the Discovery at Deir-el-Bahari in Relation to the Ancient Flora of Egypt," by G. Schweinfurth. [This article, written in French, was communicated to Sir Joseph D. Hooker by Dr. Schweinfurth, together with a set of the wreaths, flowers, &c., described therein. These objects were exhibited at the annual *soirée* of the Royal Society on the 25th ult., and are now on view in No. 3 Museum, Royal Gardens, Kew. With regard to the orthography of the names of the Egyptian kings, that employed by some of the leading Egyptologists of this country has been adopted in this translation. Thus Amenhotep has been substituted for what looks like Amenhotpan in Dr. Schweinfurth's manuscript.—W. B. HEMSLEY.]

See "La Trouvaille de Deir-el-Bahari." 20 photogr. par M. E Brugsch. Texte par G. Maspero. (Le Caire: chez F. Maurès et Cie. 1881.)

two and stitched together, and serving as clasps for the sepals and petals of *Nymphæa cærulea*, Savi, and *Nymphæa Lotus*, Hook., the whole strung on strips of the leaves of the date palm. Besides the wreaths, there were in the coffin at the side of the body, and fastened between the bands encircling the mummy, whole flowers of *Nymphæa cærulea* on stalks eighteen or twenty inches long. The water-lilies thus scattered separately on the mummy were all of the blue-flowered species. An examination of these entire flowers and the sepals and petals in the wreaths, whether of the white or of the blue-flowered species, leaves no doubt whatever respecting their identity with the living plants so common in ditches at the present day, especially in Lower Egypt, where they blossom from July to November.

The *Nymphæa cærulea*, Savi, which figures on all the ancient monuments of Egypt and among the offerings painted on the walls of the temples is often recognisable from the blue colour of its petals. In the temple of Ramses II. at Abydos the colour is remarkably well preserved, and besides there is always a leaf associated with each cluster of flowers, clearly demonstrating by its entire (not toothed) margin that the species represented is *N. cærulea* and not *N. Lotus*. The latter, whose sepals and petals occur abundantly in the wreaths taken from the coffins of Ramses II. and Amenhotep I., has not been found by me on the ancient monuments, though Unger records an instance at Beni Hassan where the white flower could be recognised. With regard to the question to which of the species the old name Lotus properly belongs, I have been able to ascertain the following facts. No design on the ancient monuments is referable to *Nelumbium*; neither the fruits nor the leaves, so easily characterised, are recognisable. Further, no remains of *Nelumbium* have been found either in the coffins or among the offerings and funeral repasts deposited in the vaults of the Pharaohs. The Lotus was not referred to *Nelumbium* until a very much later epoch. This plant has not been found among the wild plants of any part of Africa. It is eminently Asiatic, and was perhaps not introduced into Egypt before the Persian invasion. At the time of Ramadus it was probably cultivated every-

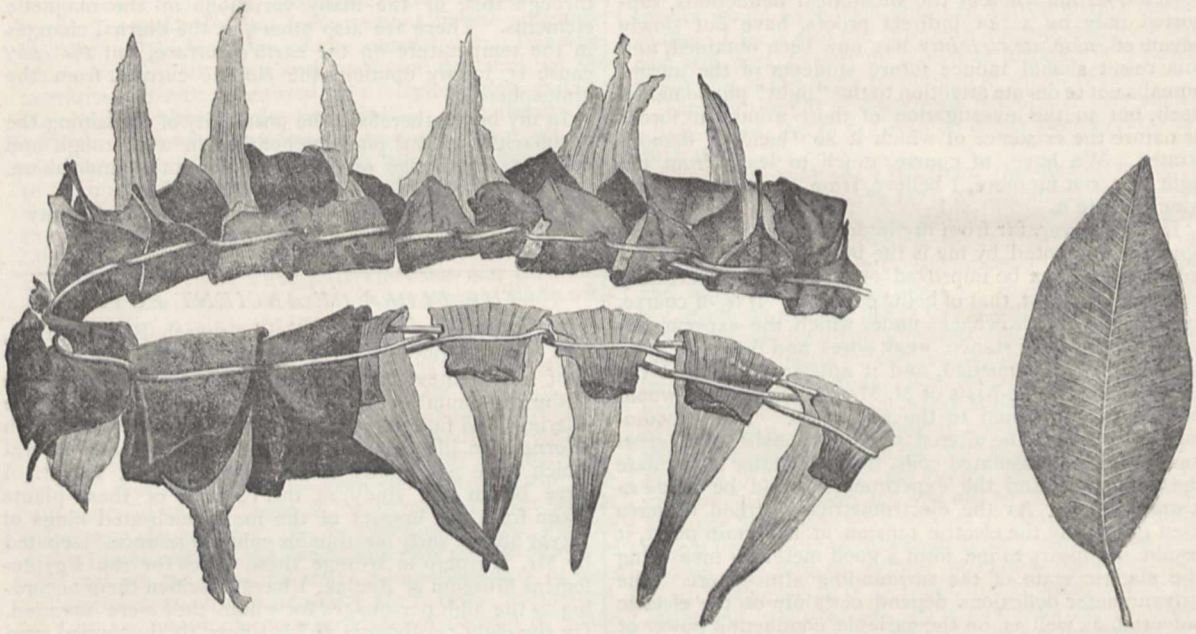


FIG. 1.—Portion of a Funeral Wreath from the tomb of Ramses II. (1000 to 1200 B.C.), composed of the folded leaves of *Mimusops Schimperi* and the petals of *Nymphæa cærulea*, Savi, stitched together with strips of the leaves of the Date Palm. A separate leaf of *Mimusops Schimperi*.

where in Egypt, for we often find it in the mosaics, sculptures, &c., of that period, associated with papyrus and animals characteristic of the Nile, and easily recognised by its fruit.

The most ancient writer who treats of the Egyptian Lotus in such a way as to leave no doubt that he meant the *Nelumbium*, and not a species of *Nymphæa*, is Herodotus (lib. ii. cap. 92); after him Theophrastus ("Hist. Plant." lib. iv.), and then Strabo, while Pliny (lib. xiii.) clearly alludes to a *Nymphæa* in a comparison of the fruit with the capsule of a poppy.

The *Mimusops* was evidently a sacred tree to the ancient Egyptians. The fruits, or the stones of the fruits, which had been eaten, are often found in the funeral repasts in the vaults; and the leaves not only occur in the wreaths of the ancient empire but likewise in those of later times, even down to the Græco-Roman epoch, as specimens in the Leyden Museum testify.

The fruit of *Mimusops* found in Egyptian tombs[1] exactly resembles—except that the stones are a little thicker—that of *M. Kummel*, Bruce, a species spread throughout Abyssinia and the region of the Upper Nile; yet no species of the genus is found wild in Egypt. The leaves forming the wreaths in question should belong to the same species as the fruits found in the tombs. Nevertheless, in comparing them with numerous specimens of *Mimusops Kummel*, I did not meet with the perfect identity one would have expected from the resemblance of the fruits. In Central Africa, and especially in Abyssinia, an allied species, *M. Schimperi*, exists, the leaves of which are much more like those of the wreaths. A longer, and especially a slenderer, weaker petiole, and a more acute, less abruptly acuminate blade characterise these leaves. With regard to the fruit of *M. Schimperi*, I have not had an opportunity of studying it. Moreover the two species under consideration are not sufficiently established as distinct species. But an anatomical character came to my aid. Dr. Westermaier of Berlin has ascertained that the leaves of *Mimusops Schimperi* and of *M. Elengi*, L., have a double layer of epidermal cells, a character they possess in common with the leaves from the ancient tombs; whereas in the leaves of *M. Kummel* there is only a single epidermal layer of cells.

[1] The ancient fruits, however, have usually a thicker stone, the three angles of which appear to be more prominent than in that of *M. Kummel*, Bruce.

Should this distinctive character be constant in the two African species, there is a double reason for naming the ancient *Mimusops M. Schimperi*. The fruit of *M. Elengi* is very distinct from that found in the tombs. I think it very likely that this species, of which we so often find the fruits and leaves in the tombs of the ancient Egyptians, may be the *Persea* of the old authors, which modern botanists have erroneously referred to *Balanites* and *Diospyros mespiliformis*.[1] The latter has not hitherto been found in the ancient tombs; neither does it occur depicted on the monuments. Diodorus (i. p. 34) has transmitted to us a valuable tradition concerning the *Persea*. He states that it was introduced into Egypt with the first colonists coming from Ethiopia, which clearly implies that the ancient authors regarded it as having been introduced from the regions of the Upper Nile and not as belonging to the indigenous flora. *Balanites*, however, grows wild in the valleys of the Eastern Thebaid and on the borders of the Red Sea, and in Nubia this shrub is of general dispersion. True its fruit has been found in the funeral repasts in the tombs, yet that of the *Mimusops* has been found much more frequently, and, in support of my hypothesis, the thick leaves of the *Balanites* are always wanting in the wreaths.

According to Theophrastus, the *Persea* had a black wood, and he compares the flowers with those of the apple-tree. I do not know the wood of the *Mimusops* sufficiently, but with regard to the flowers it must be admitted that no ancient authors ever made a more unmistakable comparison, while the flowers of the *Balanites* have nothing in common with those of the apple. Pliny (lib. xiii. p. 9) does not speak of the *Persea*, but of the *Persica*, and the only surprising thing in it is that he treats it as indigenous in Egypt. He mentions, too, the peculiarity of the Egyptian variety of the peach-tree, which consists in its persistent foliage. Even now in the middle of winter we see the peach-trees in blossom while still carrying their leaves. The same author (lib. xv. p. 13) expressly points out the difference between the *Persica* and the *Persea*. On Egyptian monuments we often see a tree diagrammatically represented, though the distichous, elliptical, acute leaves are evident. This tree, sacred to Hathor or Isis, and often drawn with these divinities, probably represent the *Mimusops* in question. The fruit of *Mimusops Kummel*, of Central Africa, resembles in appearance as well as in taste that of the wild rose; and it may be that under cultivation a still more palatable fruit could be obtained. Indeed, the fruit of specimens of this species collected in Abyssinia appears to be much more pulpy.

All the wreaths of the find at Deir-el-Bahari are of one and the same pattern. The leaves are folded lengthwise in the middle,[1] then folded again in the contrary direction over a string or strip about $\frac{1}{8}$ in. wide, of a leaf of the date-palm. In the fold of each leaf, single flowers, or parts of flowers (sepals and petals), are inserted in

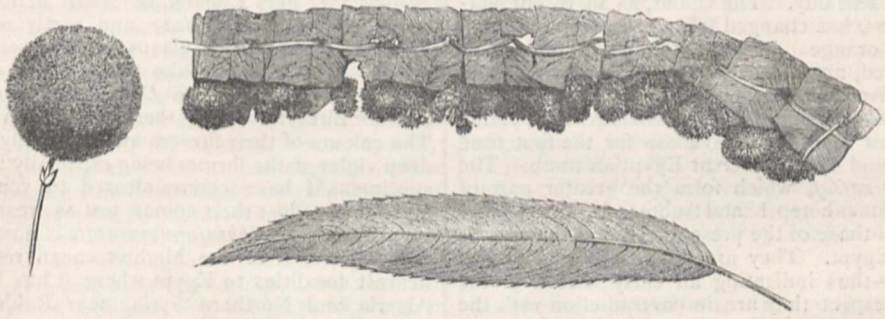


FIG. 2.—Portion of a Funeral Wreath from the tomb of Amenhotep I. (1300 to 1700 B.C.), composed of the folded leaves of *Salix safsaf* and the flower-heads of *Acacia Nilotica* strung together with strips of the leaves of the Date Palm. A separate leaf of the *Salix* (the teeth represented too sharp) and a flower-head of the *Acacia*.

such a manner that they are fixed in the leaf as in a pair of pincers. Then with a finer strip of the date-leaf than the central one, they are stitched through and securely fastened together in long rows side by side, and all pointing in the same direction. These wreaths are arranged in semicircles on the breast of the mummy, so that their disposition is like one sees in the necklaces of the present day. Their thinness rendered them suitable for using in large numbers, and sometimes they occur in several layers one above the other, filling up the limited space between the mummy and the lid of the coffin.

It is probable that it is to this kind of wreath that Pliny alludes (lib. xxi. p. 2) as the "so-called Egyptian wreaths," of which Plutarch and Athenius praised the beauty. Unfortunately these wreaths, which, with ordinary care, might have been removed entire from the mummy when the coffin was first opened, were broken and reduced to powder in several places. The specimens I send you attached to cardboard are the most perfect that I could procure after those selected for the Museum of Boulak. On placing them in boiling or cold water, according to the species, the leaves, &c., recover their original flexibility, especially in *Nymphæa cærulea*; and with proper precaution one succeeds in spreading them out and drying them again effectually. The fragility of these objects is only due to the extreme state of dryness they have reached during the thirty to thirty-five centuries they have lain in the tombs. It is at the same time the principal factor in their wonderful preservation.

The wreaths of the other kings of this vault I have at present only partially examined. From their general appearance, however, as well as from the flowers and leaves of which they are composed, which also indicate a different season[2] of the year, one would be justified in attributing them to a different period from that during which the wreaths of Ramses II. were renewed. If they really date from the time when the bodies of the kings of the eighteenth dynasty were first deposited in the vault, we have here to do with specimens four or five centuries older than the wreaths of Ramses II. In any case these objects are at least contemporaneous with the time commonly assigned to the Trojan war, if not several centuries more ancient.

The wreaths of Amenhotep I. (who was found during

[1] Kunth took the stones of *Mimusops* found by Passalacqua to be this plant.

[It may be mentioned that Kunth published his determinations of the relics found by Passalacqua in the *Annales des Sciences Naturelles*, viii. (1826) p. 418. Unfortunately it is not known to what period they belonged. Among them were seeds of a palm, *Areca* (?) *Passalacquæ*, Kunth, which was subsequently identified by Unger with *Hyphæne Argun*, Mart., a palm which inhabits some of the valleys of the Nubian desert in the bend of the Nile between Korosko and Abou Hammed.—W. B. H.]

[1] Or when they were too large they were torn in two.

[2] The records to which I have alluded indicate the day and the month; and these flowers will one day serve to fix the season with which the month of that epoch coincides. The Carthamus could only be had from the end of March to the middle of May; the Water-lilies from July to November; while the young leaves of Salix indicate the spring. The Acacia and Sesbania flower at all seasons.

the twentieth dynasty still intact in his coffin, and who, according to Brugsch, preceded Ramses II. by three centuries) are more varied. Among them are some composed, like those of Ramses II., of the leaves of *Mimusops* and the sepals and petals of the two species of *Nymphæa;* while others are formed of the leaves of *Salix safsaf*, Forsk., which serve as clasps for the little balls of flowers of *Acacia Nilotica*, Del., portions of the heads of flowers of *Carthamus tinctorius*, L., or the separate petals of *Alcea ficifolia*, Cav.

Nobody could recognise either the *Salix* or the *Alcea* among the hundred Egyptian species of plants enumerated by Pliny, or in the writings of other ancient authors; whereas the *Acacia* and the *Carthamus* occur under the names of *Acanthos* and *Cnicus*. Concerning the former, Pliny (lib. xiii. p. 19) mentions the employment of its wood in boat-building, the use of its gum, of its pods in tanning; he speaks of the spines, even, which are found on the leaves; in short he indicates the distinctive feature of the species, adding that the flowers are effective in wreaths. Several of the old authors treat of this tree. With regard to the *Cnicus* or *Knekos* (Pliny, xxi. p. 53) it is only recognisable by the indication that it is spiny, that its large white seeds yield an oil, and that there are in Egypt both wild and cultivated species, which is true. The flowers of *Carthamus* found in the wreaths of Amenhotep I. have retained their red colour, and resemble those of the species cultivated everywhere in Egypt at the present day. The colour, as in recent herbarium specimens, has changed from cadmium red to a brownish red or orange. In water the colouring matter is rapidly excreted, and we behold these flowers of some thirty to thirty-five centuries ago intensely colouring the liquid in the phial containing them.[1] All four of the plants which I have just mentioned have now for the first time been actually found in an ancient Egyptian tomb. The leaves of *Salix safsaf*, which form the greater part of the wreaths of Amenhotep I. and Aahmes I., do not differ in the least from those of the present day, and the species is common in Egypt. They are young—that is to say small and pale—thus indicating an early season of the year. In this respect they are in contradiction with the blue and white petals of *Nymphæa* found in the same coffin, though not, it should be stated, in the same wreaths as the *Salix*, but in the wreaths with leaves of *Mimusops*. The latter very closely resemble those found on the mummy of Ramses II. Perhaps at the time of the removal of the kings of the eighteenth and nineteenth dynasties from one vault to another, and finally to the place of concealment at Deir-el-Bahari, when a new coffin was made for Ramses II.—perhaps, I say, they renewed a part of the wreaths of the other kings, or having ascertained the condition of the mummies (whether under the twentieth or under the twenty-first dynasty), they added some new wreaths to the original ones. This would explain the presence in the same coffin of flowers belonging to different seasons of the year.

Salix safsaf, which occurs in a wild state on the banks of the Nile in Nubia, is in Egypt proper only a riverine fugitive, like many other plants, whose real home is in the south. Away from the river it only exists on sufferance, chiefly near wells and canals. To my mind it is an example of the wild flora which agriculture has caused to disappear. *Alcea ficifolia*, Cav., is now found in Egypt only in the ancient Arabian gardens of Cairo and other towns—that is to say, in gardens dating before the introduction of European horticulture by Barillet in 1869, where it grows almost wild as a weed. I have found it in a wild state in Syria and the Lebanon. Boissier, in his "Flora Orientalis," has not clearly defined it, and gives one or two other forms (*A. lavateræfolia*) as distinct species, which they are not. The petals of the *Alcea* contained in the wreaths of Amenhotep I. leave no doubt that they belong to the species named. Their shape, the distribution of the veins, and especially the hairy callosity on the inner surface of the claw, as well as the size even, confirm the identity of the species. Moreover one perceives in the petals of the ancient wreaths traces of a purplish tint corresponding to the crimson of the living plant. The ancients probably esteemed this plant alike for its beauty and its medicinal properties.

I have examined a head of flowers of *Acacia Nilotica* coming from one of the wreaths, and I found that the flowers agreed in the minutest details with fresh ones, with the characters of which I am sufficiently familiar. The proportions of the peduncle, the position of the annular bract, the shape of the bracteoles, the calyx, the petals, and stamens of each flower do not exhibit the slightest differences. This tree, which is planted or tolerated by man all over Egypt, is nowhere completely wild except on the White Nile between 11° and 12° N. lat., where it constitutes large riverine forests.

The wreaths which were found in the coffin of Aahmes I., the great founder of the eighteenth dynasty (1700 B.C., according to Brugsch), are the most varied, and astonish the eyes with the bright colours they have retained. They are partly composed of leaves of the Egyptian willow (*Salix safsaf*), containing separate flowers of *Delphinium orientale*, Gay, of *Sesbania Ægyptiaca*, Pers., petals of *Alcea ficifolia*, or flower-heads of *Acacia Nilotica;* and partly of the leaves of *Mimusops*, serving as clasps for the petals of the two species of *Nymphæa*, like the wreaths of Ramses II. and Amenhotep I. The *Delphinium* and the *Sesbania* had not hitherto been authenticated from ancient Egypt. The colours of their flowers are admirably preserved, the deep violet of the former being especially striking, but the specimens I have communicated to you in a phial of alcohol have lost their colour, just as fresh flowers of our time would. *Delphinium orientale* is now spread over a very wide area of the Mediterranean region. The two nearest localities to Egypt where it has been found are Algeria and Northern Syria, near Raldoun. It is not impossible that it still occurs in some parts of Egypt, while it is equally possible that it was cultivated by the ancient Egyptians as an ornamental plant. In the event of our being able to prove that some of the wreaths of Aahmes I. and Amenhotep I. were removed at the time of the twentieth dynasty, together with those of Ramses II., we should be justified in the assumption that this plant and *Alcea ficifolia* were introduced through the conquest of Syria. A minute analysis of the flowers, and comparison with those from various localities, leaves no doubt that they are of the species mentioned; and if I had had access to a larger number of flowers of the plant of the present period, I am certain that I should have been able to have exactly matched the ancient ones. The differences that I was able to detect between the ancient flowers and recent ones from Algeria, the Caucasus, Phrygia, and Lycia, kindly supplied by Mr. E. Boissier, may be set forth in a few words. In the first place there are two narrow linear bracteoles exceeding the peduncle in length, and reflexed; then the ovary is less pubescent, and the sepals are narrower and less acute. With regard to the bract, the thickened peduncle, the shape, number, and disposition of the stamens, the stigma, and especially the single petals, I have seen recent flowers in which these organs are absolutely identical. It will be seen that the characters in which they differ are only of individual value. Further, the species in question, commonly cultivated at the present time, comprises a considerable range of forms. Thus there are varieties in which the single petal is merely three-lobed, whilst in others the intermediate lobe is again divided. Both conditions occur in the ancient flowers. These flowers are so well

[1] Unger ("Botanische Streifzüge," p. 113) mentions that a chemist named Thomson had proved that the red dye in the mummy bandages was derived from *Carthamus*.

preserved that under the influence of boiling water the spur of the posterior sepal is easily separated from that of the petal projecting into it. That is to say, the latter may be extracted without injury. The numerous details of the petal, its intricate venation, the coloured glands on the margins, the claw with two lateral folds—all correspond to recent specimens. The colour of the ancient flowers is rather a deep bluish violet than a reddish violet, as in the plant of our time.

I have also carefully analysed the flowers of *Sesbania Ægyptiaca*, from the wreaths of Aahmes I. They belong to the typical form of the shrub, which still springs up on the borders of cultivated fields and on roadsides in Egypt, though it is not really spontaneous below the Soudan. The flowers are so perfectly preserved that the minutest detail did not escape my scrutiny. Submitted to the action of boiling water they scarcely differed from flowers taken from my herbarium. One circumstance shows how hurriedly these funeral wreaths were made. The flower torn from its pedicel and pinched with the finger nails always retains only a part of the calyx cut through the middle.

In the find at Deir-el-Bahari other objects besides the wreaths were found for the first time. Thus in the coffin of the priest Nibsoni, of the twentieth dynasty, the leaves of *Citrullus vulgaris* were scattered between the body of the mummy and the sides of the coffin; and flowers of *Nymphæa cærulea* were found fixed beneath the outer bandages of the same mummy. The Egyptian Museum of Berlin already possessed seeds of this *Citrullus* in the collection of Passalacqua, though the epoch to which the collection belongs is unknown. *Citrullus vulgaris* is found wild in the greater part of Central Africa,[1] and its fruit is smaller than that of the cultivated race, and less palatable, though otherwise like it. Among the broken remains in question I found one whole leaf, which enabled me to fully study its specific characters. Placed in cold water it recovered its original flexibility, so that it could be spread out flat and dried again. The chlorophyll was perfectly preserved, and what was curious, it was absorbed by the water to such a degree, that the glass of water in which the leaf and portions of leaves were placed became of an intense green colour. The problem to solve was whether the leaves were those of the water-melon or those of the colocynth, a species spread over the whole desert region, and only differing from the former, which has long hairs on the young fruit, by the complete nudity and spongy nature of its bitter fruit with a hard rind, and by the seeds. The leaves of the water-melon often very closely resemble those of the colocynth, especially in the variety called *Gjurma* (*Gyurma*) in Egypt, which bears fruit no larger than that of the colocynth, though it is always sweet. Nevertheless the large leaves of elongated outline and having less numerous lobes, are rare in the colocynth, and only in places well watered by rains. There is an association of characters in the leaves from the mummy of Nibsoni, that enable one to refer them to varieties of the cultivated water-melon, rather than to the wild colocynth. I have compared them with a long series of specimens of the water-melon from all parts of the Nilotic region, and with a no less numerous series of specimens of the colocynth; and I have come to the conclusion that they may be regarded as belonging to the former species. The uses of the two species would render them equally admissible in a coffin of ancient Egypt. As a funeral offering an alimentary plant might serve as well as a medicinal one. Still the fact that there are seeds of the water-melon in the Berlin Museum from an ancient tomb supports my first supposition. The leaves found on Nibsoni are about a palm long, and of a pinnatisect form, with obtuse lobes. If these leaves were distinctly hairy there would be no doubt of their belonging to the water-melon. Yet, as already mentioned, there is a variety widely spread in Egypt which has not the long and numerous hairs attached to the tubercles with which the leaves are covered, but merely short bristles, which is also the case in the colocynth.

This variety of water melon, which I have named *colocynthoides*, is the *Gyurma* of the Egyptians, and is cultivated in dry neglected ground in Upper Egypt. It is probably the primitive condition of the species before it had reached its present state of perfection. The leaves of the *Gyurma* are sometimes hairy, as in the water-melon, sometimes only provided with short deciduous bristles, as in the colocynth. The leaves from the coffin of Nibsoni exhibit only the latter condition. It may be that they have lost a great part of these deciduous hairs during the long period that has elapsed. I found one character, however, that the *Gyurma* has in common with those in question. There are on the petiole, and especially on the under surface of the leaf in the middle, among the round tubercles with which it is beset, other tubercles or callosities of an elongated linear form and arranged in rows corresponding to the secondary veins. On these leaves, as well as on those of the *Gyurma*, these elongated tubercles are much more prominent than they are in the colocynth. Moreover the numerous specimens that I have compared of the last have all of them leaves more densely furnished with the round tubercles than is the case with those of the water-melon, of the *Gyurma*, and the ancient leaves.

The secret vault of Deir-el-Bahari, besides the coffins of so many illustrious kings, also contained numerous funeral offerings deposited there by the later kings of the twenty-first dynasty who used this collective tomb, so well concealed by the topographical conditions. Among these offerings I was able to recognise dates, raisins, and pomegranates. There was also a basket filled with a lichen (*Parmelia furfuracea*, Ach.) which at the present day is sold in the bazaar of drugs in every town of Egypt. It is now called "Chèba" (Sheba), and is used to leaven and flavour the Arabian bread. Medicinally, also, it is in great request. The presence of a lichen of solely Greek origin, mixed with the species named, and which also occurs in the modern drug, excludes all doubt as to its being a commercial product. *Ramalina Græca*, Muell., Arg., which was mixed with the *Parmelia*, has only been found in the islands of the Greek Archipelago, and the Arab merchants regard that country as the source of their drug. As there is no locality in Egypt where *Parmelia furfuracea* could grow, the only explanation of its presence in the offerings of the twenty-first dynasty (1000 B.C.) is that it was derived from Abyssinia or Greece. In the latter case the find at Deir-el-Bahari would prove the existence of commercial intercourse with Greece at about the time of the Trojan war. Among the *Parmelia* (which was perhaps the *Sphagnos* of Pliny) were fragments of *Usnea plicata*,[1] Hoffmg, and the straw of a grass (*Gymnanthelis lonigera*, Anders.) of Nubia, which at the present day is used by the natives as a remedy against affections of the chest and stomach. On searching through the copious remains of this plant I succeeded in finding a few well-preserved flower-spikes, which I carefully examined and determined beyond doubt to belong to the species mentioned. In Arabic it is called "*mâhareb*." The odour even of this grass was preserved to a certain extent in the mixture of the offering. The fragrant secretion is of the same nature as that of the allied section *Schœnanthus* of *Andropogon* of India. Besides the lichens and the grass, this offering contained the hairy buds of some *Composita*, probably an *Artemisia*, with pinnatisect leaves; tendrils of some *Cucurbitacea*; seeds of the coriander; and numerous berries and seeds of the eastern Juniper (*Juniperus Phœnecia*). Inasmuch as we have here to do with plants coming from opposite regions of Africa and from Europe or Asia, it was

[1] I have gathered it in that state in the islands of the White Nile.

[1] Dr. J. Mueller of Geneva undertook the naming of the lichens.

not an easy matter to pronounce an opinion on the *Cucurbitacea* and the *Composita* mentioned. The coriander is a plant of early cultivation in Egypt, being mentioned by Pliny as one of the best products of the country. The berries and seeds of the juniper (the latter free in consequence of the decomposition of the former) could only have been derived from Syria or the Greek Islands. I carefully compared them with the allied species, including the Abyssinian *Juniperus excelsa* (which has larger berries and much thicker seeds, to the number of six), and there can be no doubt that they belong to *J. Phœnicea*, L. Kunth had previously determined this species in the collection of Passalacqua.

Among the fragments of the offerings and repasts found scattered on the floor of the vault of Deir-el-Bahari when it was first inspected by Brugsch Bey (some of the objects had already been disturbed by Arab robbers) was a tuber of *Cyperus esculentus*, L., some specimens of which from ancient Egypt are also preserved in the Berlin Museum. It is common in a wild state, and generally cultivated in the country.

In bringing this enumeration to a close I have only to mention the finding of a bundle of the grass called *Halfa* by the Egyptians (not the *Halfa* of Tripoli and Algeria), *Septochloa bipinnata*, Hochst., syn. *Eragrostis cynosuroides*, Retz. This bundle probably formed part of an offering representing the productions of the black and fertile soil of the valley of the Nile, of which this grass was a good sample.

ON THE CHEMICAL CHARACTERS OF THE VENOM OF SERPENTS

DRS. WEIR MITCHELL and E. T. Reichart, of Philadelphia, are now engaged in an inquiry into the chemical composition and characters of snake poison, which promises to yield important results and to supply information long wanted on an aspect of the subject which has made little progress since Prince Louis Lucien Bonaparte published his discovery of an active principle in viper venom, which he considered to be the sole cause of its toxic properties, and to which he gave the name of Echidnine or Viperine. He described the mode of separation of this principle in a paper read before the "Unione degli Scienziate Italiani" at Lucca in the year 1843.

The investigations of Drs. W. Mitchell and Reichart relate chiefly to crotaline snake poison, but include a partial analysis of some dried cobra (colubrine) poison sent to them by Mr. V. Richards from India.

Difference in the mode of action of the colubrine and viperine virus was pointed out by me many years ago in India, when I observed that viperine poison destroys the coagulability of the blood in animals, causes hæmorrhage, and has peculiar effects on the nervous system differing from the cobra's (colubrine) venom, which does not destroy the coagulability of the blood, nor cause so much hæmorrhage.

Dr. Wall of the Bengal Medical Service has added much to our information on the subject, and has defined the different modes of action of the venom of the principal Indian poisonous snakes.

The Philadelphia observers came to the conclusion that the venom of the crotaline snakes with which they have chiefly operated can be subjected to the action of the boiling temperature of water without completely losing its poisonous power. The toxicity of the venom, however, of the *Crotalus adamanteus* seems to be destroyed by a temperature below 176° F. Mitchell some years ago showed that the venom of *Crotalus durissus* is not destroyed by boiling, and they remark on the curious fact that the venom of *C. adamanteus* should thus differ from the venom of other snakes.

The symptoms caused by the venom of the different snakes with which they have operated do not, they say, differ radically save in degree, but there are certain symptoms which they think make it probable that further investigation will enable them to point out certain differences by which it will be possible to discriminate one form of poisoning from the other. This is in accordance with what has already been done by observers in India, and notably by Dr. Wall.

The investigations of Drs. Weir Mitchell and Reichart so far, lead them to conclude that the poison of the cobra is the most active, next the copperhead, then the moccasin, and lastly the rattlesnake; but their researches on this head are not yet complete.

They are unable to confirm the statement of Gautier of Paris that an alkaloid resembling a ptomaine exists in cobra poison; or that of Prof. Wolcott Gibbs, that the poison of crotalus yields an alkaloid; but they have satisfied themselves that the venom contains three distinct proteid bodies, two of which are soluble in distilled water, one which is not soluble. These bodies have certain properties and reactions, which are detailed in their monograph on the subject.

Hitherto observers have regarded the venom of different snakes as each representing a single poison, but it appears from these researches that, of the three proteids before mentioned, one is analogous to peptone and is a putrefacient poison, another is allied to globulin, and is a most fatal poison, probably attacking the respiratory centres and destroying the power of the blood to clot, while the third resembles albumen, and is probably innocuous. The separation of the two poisons necessitates a long and elaborate series of researches, the results of which will be subsequently reported.

They have also ascertained that the poison of the Rattlesnake (*Crotalus adamanteus*), Copperhead (*Trigonocephalus contortrix*), and Moccasin (*Toxicophis piscivorus*), are destroyed by bromine, iodine, hydrobromic acid (33 per cent.), sodium hydrate, and potassium permanganate. It is to be hoped that these important and valuable researches will be continued until the true chemical nature of these poisons be completely made known.

J. FAYRER

NOTES

AT a meeting of the subscribers to the Balfour Memorial Fund, held at Cambridge on the 26th inst., it was stated that 8309*l.* had been promised, all except 100*l.* of which had been paid. Of this 8078*l.* had been invested, yielding an annual income of 284*l.* 10*s.*, which it was hoped further subscriptions would raise to 300*l.* Among the regulations agreed to were the following:—The income of the fund shall be applied (1) to endow a Studentship the holder of which shall devote himself to original research in biology, especially animal morphology; (2) to further by occasional grants of money, original research in the same subject. The Student shall not necessarily be a member of the University, and during his tenure of the Studentship shall devote himself to original biological inquiry, and shall not systematically follow any business or profession or engage in any educational or other work which in the opinion of those charged with the administration of the fund would interfere with his original inquiries. The place and nature of the studies of the Student shall be subject to the approval of the managers provided that the Student shall be bound to pursue his studies within the University during at least three terms during his tenure of the Studentship, unless the managers shall, with the approval of the Board, dispense with this requirement for special reasons. The managers shall take such steps as they may think necessary to satisfy themselves as to the diligence and progress of the Student, and may require from him any reports or other information on the subject of his studies which they may think desirable. The Studentship

shall be tenable for three years, but it may be continued over a second term of three years (but no longer) to the same person if the managers and Board decide that it would be clearly in the interests of biological research. The balance of the income of the fund, after providing for the Studentship and for any necessary expenses connected with the election, shall be devoted to the furtherance of original research in biology, especially animal morphology. Grants may be made for this purpose either to the holder of the Balfour Studentship or to any other person engaged in research.

THE subscription list for the memorial bust of Prof. Henry Smith, to be placed in the University Museum, will be closed at the end of the present term. It would be convenient if subscribers would, as soon as possible, pay their subscriptions into the Old Bank, or send cheques to any of the following gentlemen:—Mr. W. Little, Queen Anne's Mansions, S.W.; Mr. R. L. Nettleship, Balliol College, Oxford; or Mr. E. Chapman, Frewen Hall, Oxford.

DR. JULIUS VON HAAST has been created a Companion of the Order of St. Michael and St. George.

TWO statues which have been erected in front of the Berlin University to the Brothers Alexander and Wilhelm von Humboldt were unveiled on Monday with great ceremony. The Emperor and some of the members of the Imperial family witnessed the proceedings from the Royal Palace, which immediately faces the University, and the Emperor afterwards went on foot to inspect the statues.

DR. GABRIEL GUSTAV VALENTIN, one of the most eminent professors of the University of Berne, and a distinguished physician, died at that city on May 24. Dr. Valentin was born at Breslau in 1810, graduated in 1832, and began practice in his native town in the following year. In 1835 he published a handbook of the history of evolution ("Entwicklungsgeschichte"), and in 1836 was appointed Professor of Physiology in the University of Berne, a position which he held until 1881, when ill health compelled him to resign. He stood very high in his profession, and was the author of many scientific works, two of which were written in Latin, "De phænomeno generali et fundamentali motus vibratorii continui" and "De functionibus nervorum cerebralium et nervi sympathici libri quatuor." He wrote also a "Text-Book of Physiology," a book entitled "Groundwork of Human Physiology," a "Repertory of Anatomy and Physiology," an "Examination of the Effects of Polarised Light on the Life of Plants," an elaborate work on the "Adaptation of the Spectroscope to Physiological and Medicinal Purposes," and several others which attest his vast knowledge and untiring industry.

THE following remarks by our American contemporary, *Science*, on the subject of the Canadian meeting of the British Association in 1884, are deserving of attention:—"It is to be observed that in the present year the meeting of the American Association, at Minneapolis, is early (August 17); while that of the British Association, at Southport, which is, besides, in the immediate vicinity of Liverpool, is unusually late (September 19). This will allow members of the American Association to attend both meetings, and it is stated that the retiring President of the American Association, and possibly others of its members, may avail themselves of this privilege. This may possibly permit arrangements to be made which might substantially unite the meetings of the two Associations in 1884, and so prepare for an international meeting in the future. If the meeting of the American Association for 1884 can be fixed for some north-eastern city, sufficiently near to Montreal, and can be timed so as to occur a week before or after that of the British Association, there can be no doubt that a great number of the members of the latter body would take advantage of the opportunity to enjoy the companionship of their American *confrères*, while, on the other hand, many of these would gladly spend a few days at the meeting of the British Association. In this way it would seem that a greater benefit to science might result than even from an international meeting. There would be time for the complete transaction of the business of both Associations. Neither would suffer, either pecuniarily or in the value of its proceedings; and there would be the best possible opportunity for interchange of ideas between the scientific men of the United States, Great Britain, and Canada. Nor is it unlikely that some scientific workers from the continent of Europe and elsewhere may be attracted by a combination so unusual. It may thus be hoped that the proposed meeting of the British Association in Canada may not only be one of the most successful that this mother of Associations has held, but may inaugurate an epoch of renewed activity and progress in the widely-spread scientific work of the two great Associations of the English-speaking race."

THE New Parkes Museum of Hygiene at 74*a*, Margaret Street, Regent Street, was opened on Saturday under favourable and distinguished auspices. The Duke of Albany presided and formally opened the Museum, and gave besides a sensible and thoughtful speech. "Hygiene," His Royal Highness said, "as we now understand it, is a branch of knowledge of modern growth. It is one of the natural results of the great advance of science which this century has witnessed, and might, I fancy, not inaptly be defined as the application of scientific principles to the varying conditions under which we are called upon to live. Thanks to the labours of many eminent men, we have now advanced some way towards an accurate knowledge of the conditions which are necessary for health; and most of these conditions have long been familiar to the few. One object of the Parkes Museum will be to make them familiar to the many. We have learned, and are daily learning, that many of the luxuries and conveniences of modern life may become sources of danger to us if they be ignorantly used. London would be almost uninhabitable were it not for its wonderful system of sewers; but while enjoying the blessing of effective sewerage, we have had to encounter the difficulty of keeping the air of the sewers out of our dwellings. We all appreciate the brilliant light which is given by a gas lamp; but its wholesome use, we are now beginning to find, involves questions of ventilation which scarcely troubled those who were content with the comparative dimness of a candle. Again, the open coal fire has long been regarded as one of the chief luxuries of the Briton, but the collected smoke of the fires of 4,000,000 of people has become a nuisance too grievous to be borne, and one for which a remedy must be sought. It is notorious that many of our public and private buildings in this country have been constructed without due attention, or, indeed, any attention, to those details which alone make a dwelling wholesome. The experiences of my own family in this matter have indeed been singularly hard. We hope that this museum will tend to hasten the end of this state of things, and that henceforward 'healthiness' will be considered as an essential condition of true architectural beauty. For the healthiness of our dwellings we have to depend, not only upon the master mind which furnishes the plan, but even to a greater extent upon the intelligent hands of those who are called upon to carry out the details. Unless the work of these latter be done with intelligence and faithful honesty, the schemes of the wisest architect avail us little. The instruction which has been and will be given here to the artisans who carry out the sanitary details of our houses must be productive of good results. At least, let us hope that some of the specimens of defective workmanship to be found upon our shelves will impress upon them that death, disease, and sorrow may be the results of ignorance or carelessness on their part." Among the other speakers

were Sir Charles Dilke, Prof. Tyndall, and the Archbishop of York. It is to be hoped that the public, and especially those on whose skill and honesty our sanitary arrangements are dependent, will take ample advantage of the opportunities offered by the new museum.

THE seventh Congress of Russian Naturalists and Physicians will be held this year at Odessa, from August 30 to Sept. 9.

THE district of Pergamos in Asia Minor is now so infested with sparrows that application has been made to the Turkish Government for aid against them. It will be remembered that this district is subject to occasional invasions of rodents.

THE Marine Excursion Committee of the Birmingham Natural History and Microscopical Society announce that, in response to a wish expressed by many members, they have arranged a second excursion to Oban and the West Highlands of Scotland, similar to that which proved so successful in the year 1881. The party will leave on Friday, June 29 next, to reach Oban about 5 p.m. on Saturday. The screw steam yacht *Aerolite*, of about sixty tons, has been hired of Messrs. Ross and Marshall of Greenock for a week, commencing Monday, July 2; facilities will thus be afforded for dredging excursions not only in the district previously worked, but also in distant localities. Arrangements are being made for excursions to several places of interest in the neighbourhood of Oban.

THE sixth annual meeting and *conversazione* of the Midland Union of Natural History Societies will be held at Tamworth on June 12 next. Excursions have been arranged for that day and the 13th. The Darwin Gold Medal for 1882 will be presented to Prof. A. M. Marshall and W. P. Marshall, for their paper on the Pennatulida.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr. C. D. Gordon; two Grisons (*Galictis vittata*) from South America, presented by Mr. Percy Kenyon Slaney; two Sloth Bears (*Melursus labiatus*) from India, presented by Mr. F. A. Curteis; a Surucucu or Bushmaster (*Lachesis mutus*) from Pernambuco, presented by Mr. J. Y. Barkley; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. Henry W. Weguelin; a Chimpanzee (*Anthropopithecus troglodytes* ♂) from West Africa, two Welsh Sheep (*Ovis aries*) from Wales, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, five Margined Tortoises (*Testudo marginatus*), thirteen European Pond Tortoises (*Emys europæa*), South European, deposited; a Common Seal (*Phoca vitulina*) from British Seas, a Grey-headed Porphyrio (*Porphyrio poliocephalus*), a Conical Worm Snake (*Gongylophis conicus*) from India, purchased; a Hybrid Tapir, ♀ (bred between *Tapirus roulini* ♂ and *Tapirus americanus* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE MINOR PLANET, ANDROMACHE.—Among the small planets mentioned in the last volume of the *Berliner Astronomisches Jahrbuch* as having been observed at one opposition only, though several oppositions have taken place since their discovery, is No. 175, detected by the late Prof. Watson of Ann Arbor, U.S., on October 1, 1877, and named *Andromache*. The orbit has a considerable eccentricity, and the planet recedes to a greater distance from the sun at aphelion than is the case with any other member of this now numerous group so far calculated; indeed at this point of its orbit it is distant from the sun 4·723 (the earth's mean distance being taken as unity), and only 0·594 from the orbit of Jupiter. There should be no great difficulty in recovering this planet during the month of June or in July. According to the most accurate elements calculated by Prof. Watson it will be in perihelion about July 25, and in opposition a fortnight earlier, its computed intensity of light being equal to that of a star of fully the ninth magnitude. Its considerable south declination will give an advantage to a search at one of the observatories of southern Europe. To facilitate its reobservation we subjoin positions deduced from the orbit last published :—

At Greenwich Midnight

	R.A. h. m.	Decl. ° ′	Log. Distance from Earth.	Sun.
June 4 ...	19 40·3 ...	−27 23 ...	0·1547 ...	0·3635
12 ...	19 39·0 ...	27 44 ...	0·1368 ...	0·3622
20 ...	19 36·0 ...	28 7 ...	0·1222 ...	0·3610
28 ...	19 31·5 ...	28 29 ...	0·1117 ...	0·3600
July 6 ...	19 25·9 ...	28 48 ...	0·1059 ...	0·3594
14 ...	19 19·9 ...	−29 1 ...	0·1055 ...	0·3590

The planet will probably be situated at some distance in R.A. from these positions, which are only intended as an approximate indication of its places. The last reference to a search for it which we find in the circulars of the *Berliner Jahrbuch*, occurs in No. 118 (Correspondenz), 1881, March 3, where we read, "*Andromache* innerhalb −6m. 30s. bis −3m. 55s., und −2m. 20s. bis +4m. 15s. vergeblich gesucht." A special rough chart of stars in the vicinity to the tenth magnitude inclusive would be readily formed with the stars in the Bonn and Washington Zones as reference points.

THE GREAT COMET OF 1882.—M. W. Fabritius of Kieff has calculated the following elliptical elements of this comet from two normal positions for September 9 and October 6, and an observation at Königsberg on March 3 in the present year :—

Perihelion passage, 1882, September 17·2753 M.T. at Berlin.

Longitude of perihelion	276° 28′ 40″·1	M. Eq. 1882·0
,, ascending node	345 58 4·1	
Inclination	38 0 44·7	
Log. (1 − *e*)	5·938209	
Log. semi-axis major	1·943548	
Log. perihelion distance	7·881757	

Motion—retrograde.

The corresponding period of revolution is a little less than 823 years, and as M. Fabritius attaches some weight to his result, he thinks the comet must have appeared about the middle of the eleventh century.

We shall doubtless have in due course a thorough discussion of all reliable observations; those made since September 30, when the disintegration of the nucleus commenced, will need special treatment.

THE OBLIQUITY OF THE ECLIPTIC.—In NATURE, vol. xxvii. p. 618, we quoted 23° 41′·1 as the value of the obliquity of the ecliptic at the assigned epoch of Ptolemy's catalogue. With reference to this statement Mr. W. J. Cockburn Muir, of Melrose, N.B., has made a discovery, on which he writes us as follows :—"In NATURE of April 26, at p. 618, I read that the 'obliquity of the Ecliptic' is 23° 41′·1, and I wondered much what had suddenly happened in the Kosmos. So I took means to ascertain from the Royal Observatory of Greenwich how the record stands, and I am comforted to find that, by the determinations in 1882, the earth's axis still remains at home—23° 27′ 16″·8." Our correspondent may be referred to any elementary treatise on astronomy.

GEOGRAPHICAL NOTES

MR. OSCAR DICKSON'S Greenland Expedition, under the command of Baron Nordenskjöld, sailed from Gothenburg in the *Sofia*, 180 tons, 65 horse-power, drawing 10 feet, and of 11 knots speed, navigated by Capt. Nilsson and a crew of 13 men. With Baron Nordenskjöld are Dr. Nathorst, geologist; Dr. Berlin, doctor and botanist; Dr. Forsstrand, zoologist; Dr. Hamberg, hydrographer; Herr Kolthoff, zoologist; Herr Kjellström, typographer and photographer; two Laplanders, two Norwegian icemasters, and one harpooner. There is on board a complete scientific equipment and 14 months' provisions for subsistence on the inland ice. Eight or nine picked men accompany Baron Nordenskjöld. Count Stromfeldt, botanist; Dr. Arpi, archæologist and philologist; and Herr Flink, mineralogist, will disembark on the coast of Iceland for the purposes of

study and collection. The *Sofia* called at Thurso for coal on Sunday and left on Tuesday.

In connection with Prof. Fries' suggestion of colonising Greenland by mountain Lapps, to which we referred last week, we learn that Baron Nordenskjöld takes with him to Greenland two Lapps from Jockmock, to give their opinion of the country. One of them is thirty, and the other thirty-three years of age.

We learn from the last annual report of the East Siberian branch of the Russian Geographical Society that this Society, which has contributed so largely to the increase of our knowledge of Siberia, is beginning to recover from the losses it sustained during the great fire at Irkutsk. Private subscriptions have been raised for the reconstitution of the library and museum to the amount of 2170*l.*, and both are in a fair way of development. The library already has about 4000 volumes, but is in great want of foreign geographical publications, and makes an appeal to the geographical societies throughout the world to send their publications and, if possible, series of former publications, which ought to be addressed to the Secretary of the East Siberian branch at Irkutsk. The chief occupations of the Society were: the geological exploration east of Lake Baikal, by M. Chersky, who has already published a map of the western coast of the lake; archæological researches as to the prehistoric inhabitants of Siberia, by MM. Agapitoff, Khangaloff, Witkovsky, and Bogolubskiy; and the part it took in the organisation of the Arctic Meteorological Station at the mouth of the Lena, and of a series of four intermediate stations between Irkutsk and this station. This last scheme could not be realised in full, but two stations have already been opened at Verkholensk and at Preobrajenskoye. The last number of the *Journal* of the Society contains, besides the annual report and the proceedings, a list of new determinations of latitudes and longitudes in Transbaikalia; a notice on Shamanisur with Yakuts; a paper on the populations of the basin of the Amur, according to Prof. Schrenck; a paper on the inscriptions on stones and rocks in the district of Minusinsk; and several notes, on the Lena Meteorological Station, on the Usuri region, &c.

Petermann's Mittheilungen for May contains a paper by Mr. Carl Bock describing a journey recently made by him from Bankok to the frontiers of the independent Shan States. He travelled along the Menam River in a boat given him by the Siamese Government, as far as Raheng, where he diverged into the Me Ping. He then proceeded partly by the river, partly by land through Lakon and Lampun, to a town which he calls Tschengmai, but which is more generally known as Kiangmai, or Zimmé. This place, which is the capital of the Shan States tributary to Siam, is an important point in Mr. Colquhoun's proposed railway from Rangoon and Moulmein, into south-western China. It formed the proposed terminus, too, of that gentleman's recent journey through Yunnan and the Shan States. Mr. Bock described it as a fortified town of about 700,000 people, lying in a fertile plain of uninterrupted rice fields, about 500 yards from the Me Ping, which is here 400 feet wide. Even now it is of great political and commercial importance, as it controls the trade of these regions both with Siam and with British Burmah. The teak forests of the States he describes as almost inexhaustible, especially higher up near the Meikong, where, however, it is not yet known whether the lumber can be easily floated down to the sea. For this purpose Mr. Bock recommends a careful survey of the various rivers and their tributaries. From Zimme he continued his way higher up to Kiangtsen, in the valley of the Meikong, and on the borders of the independent Shan States. It was his original intention to travel through these States into Yunnan, as it was Mr. Colquhoun's to travel through them from Yunnan, southwards. Failing this, he returned to the Me Ping, with the object of tracing this river to its source. He was prevented from carrying out either project by the native hostility, which, we regret to say, Mr. Bock himself did much to intensify, if not arouse, by his indiscreet behaviour. It would be inconceivable, if we did not have it on his own testimony, that any traveller among a people who, as he was specially warned, disliked even the Siamese, and absolutely hated any white man, should so far forget all discretion as to enter a populous town and "out of his own hand," as he describes it, take possession of the court of justice, and assault with a stick the official who endeavoured to prevent this unjustifiable trespass. He was punished by several days' imprisonment, but it is unfortunate for the cause of science that the hostility thus carelessly and wilfully aroused should have put a speedy termination to a journey full of promise. Mr. Bock, however, has shown beyond doubt that a railway from Bankok to the Shan frontiers is a possibility. It would pass through populous and rich districts in the valleys of the Menam and Me Ping. He says that no one who has not visited Zimmé can understand how extensive the trade of the place is, and his proposed railway would place the Laos States in direct communication with the sea, and attract the commerce not only of the Shan States, but also of Yunnan. These are exactly the arguments by which Mr. Colquhoun supports his scheme for a railway to Rangoon. Let us hope that in days to come, when this colossal project is an accomplished fact, there may be no dispute as to the originator of the idea of attracting the trade of south-western China to the sea by means of a railway through the Shan States.

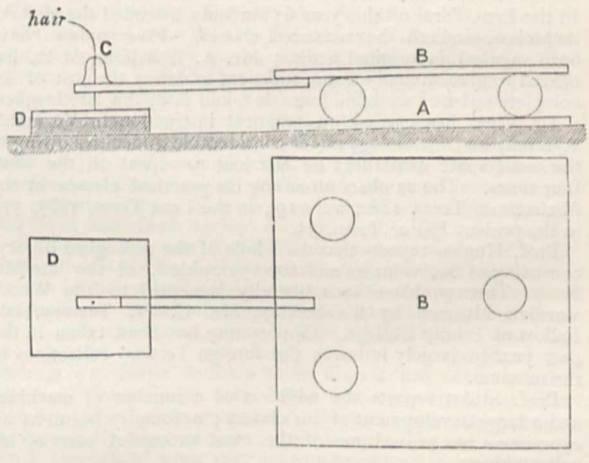
A NEW FORM OF SEISMOGRAPH[1]

NUMEROUS forms of seismometers have from time to time been invented, and having these various instruments, it may be asked why there is any necessity for a new form, and I can best answer this by quoting from a report of a committee of the British Association of 1872, as follows:—"Some simple and cheap method of indicating earthquake movement is thus much to be desired—any apparatus for the purpose should occupy small space, be little liable to derangement, capable of being put up in any apartment not of special construction, and its indications such as any intelligent person could easily interpret and readily note."

Now none of the instruments yet invented fulfil these conditions, and hence I bring before you one which is of the very simplest nature.

The idea of the instrument I propose was suggested to me by the aseismatic arrangement designed by my father, Mr. David Stevenson, for averting damage to buildings and lighthouse apparatus in countries subject to earthquakes (*Trans. Roy. Scot. Soc. Arts.* vol. vii.).

The instrument is shown below, and consists of a ground and polished glass plate (A), about 5 inches square, placed level (once for all), on which rest three accurately turned ivory balls about 1½ inch diameter, and on the top of these



balls is placed a plate (B) similar to the lower, but having attached to it a projecting arm with a long vertical hole pierced through it. Through this hole passes a steel needle (C) with a fine point, which rests by its own weight on a lampblack surface formed on the plate D. A hair about 2 inches long should be fixed to the eye of the needle to assist in adjusting it. The instrument thus becomes a pendulum of infinite length, so that whenever there is any movement of the ground, and therefore of the lower plates, the top plate with its arm and needle attached remain practically steady, and the point of the needle therefore marks on the lampblack surface the amount of motion and the direction in which the lower plate is moved. This instrument, it will be observed, fulfils all the requirements mentioned in the report of the committee of the British Associa-

[1] Abstract of paper read before the Royal Scottish Society of Arts, February 13, 1882, by Charles A. Stevenson, C.E., Edinburgh.

tion, and can be made more or less sensitive. It is impossible from a mere description to form any conception of the efficiency of the apparatus, nor has it been tried by any earthquake, but the instrument before you having been erected on the gable of a dwelling house during the past year, repeatedly registered the shaking of the gable to the amount of 1–16th of an inch.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Museums and Lecture Rooms Syndicate have just issued their annual report, in which they mention the high value of the present of the late Prof. Balfour's scientific instruments and library to the University by his family, and again emphasise the necessity existing for a new chemical laboratory. Mr. Clark records the mounting of the fine male Indian elephant's skeleton in the Zoological Museum, received in an exceedingly complete state last year in consequence of its careful preparation by Mr. A. Haly of the Colombo Museum. The animal was shot by Mr. Le Mesurier of the Ceylon Civil Service; its height was nine feet. A specially interesting skeleton of the adult Gangetic Dolphin has been presented by Sir J. Fayrer. Mr. A. P. Maudslay, M.A., of Trinity Hall, has deposited in Mr. Clark's care a large portion of his ethnological collection made in Fiji and adjacent islands; these are almost certain to be presented to the University at no distant date. The Curator in Zoology (Mr. A. H. Cooke) has catalogued and arranged the British species in the MacAndrew collection. Its completeness may be judged by the facts that of 6 recorded species of Brachiopoda this contains 5; of 159 marine Conchifera this contains 146; of 248 marine Gasteropoda this contains 208; of 125 land and freshwater shells this contains 114 A recent appeal to add missing species has already resulted in the presentation of fourteen species by Mr. J. T. Marshall.

Dr. Michael Foster reports an average class of about 100 in Elementary Physiology, and of over 20 in advanced Physiology, in the three terms of the past year. Additional lecture room accommodation is much needed for these large classes.

The morphological work begun by the late Prof. Balfour has been continued on the same lines by Mr. Adam Sedgwick, Mr. W. H. Caldwell and Dr. Hans Gadow as lecturers, and Mr. Walter Heape and Mr. W. F. R. Weldon as demonstrators. In the Lent Term of this year 63 students attended the elementary class, and 26 the advanced classes. Five students have been engaged in original work. Mr. A. J. Balfour, M.P., has offered to give annually a sum sufficient to defray the cost of the complete series of scientific journals taken in by his late brother.

Dr. Vines has carried on practical instruction in Vegetable Anatomy and Physiology under considerable difficulties owing to the small space available; he has had to repeat all the work four times. The numbers attending his practical classes in the Michaelmas Term, 1882, were 19; in the Lent Term, 1883, 37; in the present Easter Term, 35.

Prof. Hughes reports that the whole of the geological library, consisting of 800 volumes and 1000 pamphlets, of the late Mr. E. B. Tawney, have been liberally presented to the Woodwardian Museum by his brother, Mr. C. H. Tawney, late Fellow of Trinity College. Opportunity has been taken in the past year to largely improve the foreign Tertiary collections in the museum.

Prof. Stuart reports the addition of a number of machines and a large development of his classes; a foundry begun as an experiment has proved one of the most successful parts of his undertaking.

The Philosophical Library in the new Museums has been largely increased by the valuable presents made by the family of the late Prof. Balfour, by Mr. J. W. Clark, by Prof. Darwin, Prof. Humphry, Prof. Newton, and others.

It has been recommended by the Special Board for History and Archæology that a separate Board be created for Archæology, distinct from that of History. This has been concurred in by the General Board of Studies.

The Botanic Garden Syndicate have reported many improvements in the collections of trees, of rock vegetation, and in the Plant Houses. The largest specimens in the Palm House have been safely lowered to about 2½ feet below the ground level. All the genera of carnivorous plants in cultivation and most of the species are now in the collection. *Vitis gongylodes* has been flowered for the first time in this country. The Curator, Mr. Lynch, was deputed to visit the Botanic Gardens at Dublin, Manchester, and Liverpool, and has also visited Chatsworth with the result that much valuable information has been obtained in all departments of management and cultivation, and many important exchanges have been made.

The Adams Prize, for a general investigation of the action upon each other of two closed vortices in a perfect, incompressible fluid, has been awarded to Mr. J. J. Thomson, M.A., Fellow of Trinity College.

Messrs. W. H. Besant and E. J. Routh are the first to be notified as "approved by the general Board of Studies for the Degree of Doctor in Science."

Candidates for the Professorships of Physiology and Anatomy are requested to send their names to the Vice-Chancellor on or before June 7.

THE Institute of Agriculture, South Kensington, will give an extended series of lectures next winter, beginning on October 1. The following courses are arranged for:—Mr. Bernard Dyer, Chemistry in Relation to the Soil; Mr. F. Cheshire, Practical Course on the Use of the Microscope (these two courses to be delivered in the Lecture Theatre of the Museum of Geology, Jermyn Street). The next series will be given in the Lecture Room of the Natural History Museum, South Kensington: Mr. Bettany, Vegetable Physiology; Mr. Worthington Smith, Diseases of Farm Crops; Prof. J. W. Axe, Animal Physiology in Relation to Farm Stock; Miss E. A. Ormerod, Farm Insects; Mr. W. Topley, Geology and Physical Geography in Relation to Agriculture. The remaining courses will be given in the Lecture Theatre of the South Kensington Museum: Prof. Tuson, the Chemistry of the Food of Farm Stock; Prof. Buckman, Farm Seeds; Prof. Tanner, Agriculture; Mr. R. Holland, Management of Grass Land; Mr. Gilbert Murray, Breeding and Management of Horses; Mr. W. Housman, Cattle; Mr. H. Woods, and Mr. J. A. Clarke, Sheep; Prof. J. W. Axe, Preventable Diseases of Farm Stock; Farm Implements and Machinery, Mr. W. R. Bousfield and Mr. W. W. Beaumont. A distinct course of lectures will be given on Poultry, Dairy, and Bee Management. The arrangements made enable students to give their undivided attention to one subject at a time, two lectures being given daily till the subject is completed. The fees being at the rate of half a guinea for each week's course of ten lectures, and any student being allowed to attend a single course, the greatest facility exists for persons choosing their work according to their needs or convenience. Thus it is believed, after the success of the tentative courses of the past winter, that many sons of tenant farmers will find this a most valuable and available mode of acquiring an agricultural education.

SCIENTIFIC SERIALS

THE *American Naturalist* for March, 1883, contains:—On the extinct dogs of North America, by E. D. Cope.—On the plains of Michigan, by V. M. Spalding.—Organic physics, by Charles Morris.—Indian music, by E. A. Barber.—On the occurrence of fossiliferous strata in the lower Ponent (Catskill) group of Middle Pennsylvania, by E. W. Claypole.—Pitcher plants, by Joseph F. James.

April, 1833, contains:—The Naturalist Brazilian Expedition, No. 1, from Rio de Janeiro to Porto Alegre, by H. S. Smith.—Unnatural attachments among animals, by J. D. Caton.—Butterfly hunting in the desert, by W. G. Wright.—The extinct Rodentia of North America, by E. D. Cope.—Heterogenetic development in Diaptomus, by C. L. Herrick.—A study of the immature plumage of the North American shrikes to show their descent from a common progenitor, by Thos. H. Streets.

May, 1883, contains:—Wampum and its history, by E. Ingersoll.—The Naturalist Brazilian Expedition, No. 2, by H. S. Smith.—The Polar organisation of animals, by C. Morris.—On the classification of moths, by A. R. Grote.—Heterogenetic development of Diaptomus, by C. L. Herrick.—On the morphology of arteries, especially those of the limbs, by F. Baker.—The hairy woodpecker, by A. G. Van Aken.

Archives Italiennes de Biologie, tome ii. fasc. 2, November 30, 1882, contains among the original articles the following:—On the minute anatomy of the muscles which move the wings of insects, by G. V. Ciaccio.—On the structure of striated muscular fibre in some vertebrates.—On the development and the morphology of the kidney of osseous fish, by C. Emery.—On the substance preventing the coagulation of the blood and lymph whilst these contain peptone, by Jules Fano.—On the germs and lower

organisms found in ordinary and malariac earths, by A. Ceci.—Transfusion of blood and its effects on nutrition, by P. Albertoni.—On the pathological anatomy of the cornea in the glaucomatous eye, by F. Tartuferi.—On the presence of a cordon or slip on the Uncus of the Hippocampus in the brain of man and some other animals, by C. Giacomini.—On the chemical composition of the egg and its envelope in the common frog (*Rana temporaria*), by P. Giacosa.—Anatomical considerations of the doctrine of cerebral localisations, by C. Golgi.

Tome ii. fasc. 3, February 1, 1883, contains anatomical considerations of the doctrine of cerebral localisations, by C. Golgi (continued).—On compensative hypertrophy of the kidney, by C. Golgi.—Experimental studies on hypnotism, by A. Tamburini and G. Seppili.—The origin of the mesoderm and its relations to the vitellus, by G. Romiti.—On the anatomy of a fœtal Otaria (*O. jubata*), by L. Camerano.—On the physiology of smooth muscular tissue, by A. Capparelli.—On the physiological action of certain substances on the vesical muscles, by P. Pellacani.—On the anemia of miners from a parasitological point of view, by E. Perroncito.—On the change in form of uric acid by the action of glycerine, by J. Colasanti.—On Ptomaines, by J. Guareschi and A. Mosso.—On some endoparasitic Protista, by Dr. Grassi.

Tome iii. fasc. i., April 15, 1883, contains:—On the sanitary improvement of the Roman Campagna, by C. Tommasi-Crudeli.—On the anemia of miners (conclusion), by E. Perroncito.—On some endoparasitic Protista (conclusion), by Dr. Grassi.—On the presence of a secretive tissue in vertebrates, by C. Emery.—On vibratile endothelium in mammals, by J. Paladino.—On the attenuation of charbon virus, and on its transmission from mother to fœtus, by E. Perroncito.—On the acoustic epithelium, by A. Tafani.—On the termination of nerves in the striated muscles of torpedo, by J. V. Ciaccio.—The general physiology of smooth muscular tissue, by E. Sertoli.—On a new morphological element of the blood, and its importance in thrombosis and coagulation, by J. Bizzozero.—New studies of the chestnut disease, known as the ink disease, by J. Gibelli.

THE *Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts* for 1883, part i., contains papers by F. Henrijean, on the part played by alcohol in nutrition; by MM. Valerius and Van der Mensbrugghe, on M. Delaurier's observations on the concentration of solar rays and the transformation of electricity into heat; by W. Spring, on the colour of marine, lacustrine, and fluvial waters; by C. Le Paige, on the homography of the third order in algebra; by Baron Northomb, on the political relations of the Netherlands during the seventeenth century.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 1.—"On the Affinities of Thylacoleo." By Prof. Owen, C.B., F.R.S., &c.

Since the communication of the paper "On Thylacoleo," in the *Philosophical Transactions* for 1871, further explorations of the caves and breccia-fissures in Wellington Valley, New South Wales, have been made, by a grant for that purpose from the Legislature of the Colony, and carried out by E. B. Ramsay, F.L.S., Curator of the Museum of Natural History, Sydney. The present paper treats of the fossils contributing to the further restoration of the great carnivorous Marsupial (*Thylacoleo carnifex*, Ow.) They exemplify the entire dentition *in situ* of the upper and lower jaws of a mature individual; the bones of the forelimb, of which those of the antibrachium and the ungual phalanges are described, are compared with those of other Marsupials, and of placental, especially feline, *Carnivora*. An entire lower jaw with the articular condyles adds to the grounds for determination of the habits and affinities of the extinct Marsupial.

Figures of these fossils of the natural size accompany the paper.

Geological Society, May 9.—J. W. Hulke, F.R.S., president, in the chair.—Rev. William Spiers and H. A. Williams were elected Fellows of the Society.—The following communications were read:—The age of the newer gneissic rocks of the Northern Highlands, by Mr. C. Callaway, D.Sc., F.G.S., with notes on the lithology of the specimens collected, by Prof. T. G. Bonney, F.R.S. The object of the author was to prove that the eastern gneiss of the Northern Highlands, usually regarded as of "Lower Silurian" age, was to be placed in the Archæan. While admitting that this gneiss frequently overlies the quartzo-dolomitic group of Erriboll and Assynt, he held that this relation was due to dislocation accompanied by powerful thrust from the east, which had squeezed both formations into a series of folds, thrown over towards the west, so as to cause a general easterly dip. In Assynt the "Upper Quartzite" was first discussed. The author described several sections which he considered to prove that this band was the ordinary quartzite repeated east of a great fault, which brought up the Hebridean; in one place, Glen Coul, the quartzite being conformably succeeded by the brown flags and dolomite. The "igneous rocks" of Nicol ("Logan Rock" of Dr. Heddle) were regarded as the old gneiss brought up by a fault and thrown over on to the Assynt group to the maximum breadth of more than a mile. The "Upper Limestone" of authors was described as either outliers of the dolomite or a part of the Caledonian series. The "Caledonian" rocks were seen in Glen Coul to be immediately overlying the Hebridean, the Assynt group being caught in the angle between the two gneisses, and bent back in overthrown folds. The mountain groups of Assynt were described as usually consisting of cores of Hebridean gneiss swathed in or capped by sheets of quartzite. In the former case the quartzite on the western slopes was contorted into overthrown folds by the thrust from the east. In the Loch Erriboll district, the "granulite" of Nicol was considered to be a lower division of the Caledonian gneiss, though bearing some resemblances to the Hebridean. In other respects the views of Nicol were regarded as substantially correct. Along the entire length of Loch Erriboll, a distance of about twelve miles, the thrust from the east had bent back the Assynt group into overthrown folds, and pushed the Caledonian gneiss on the top of the inverted quartzite. This had produced the appearance of an "upper" quartzite passing "conformably" below the eastern gneiss. The superior antiquity of the Caledonian was confirmed by the occurrence of outliers of quartzite upon the Arnaboll (Lower Caledonian) series, and by the fact that the granite, which sent numberless veins into the gneiss, never penetrated the quartzite and associated rocks.—On a group of minerals from Lilleshall, Salop, by C. J. Woodward, B.Sc., F.G.S.—Fossil Chilostomatous Bryozoa from Muddy Creek, Victoria, by A. W. Waters, F.G.S.

Chemical Society, May 17.—Dr. W. H. Perkin, president, in the chair.—Capt. W. de W. Abney, F.R.S., delivered a lecture on photographic action studied spectroscopically. The lecturer said he wished that all chemists were photographers; photography occupied the borderland between chemistry and physics; he was firmly convinced that photographic action was interatomic. The action of a developer was then experimentally illustrated; this action is physical. Light causes the liberation of iodine in a film of silver iodide, and the developer precipitates metallic silver. The silver so reduced is infinitesimal, and must be in many cases derived from the film. The positive pole of the electric arc was found to be the best source of light. Gratings could not be used for quantitative work, as they varied so much in their ruling; a glass prism was therefore used to form the spectrum. A film of silver chloride absorbs only the violet end of the spectrum; silver iodide absorbs more, and the bromide most of all; accordingly when a photograph of the spectrum was taken on these three films it was seen that the portion of the chloride acted upon was very much less than when bromide of silver was used. It was shown that a sensitiser essentially takes up the halogen liberated by the action of light. One salt of silver may act as a sensitiser to another salt of silver. Photographic action is completely prevented by the presence of oxidisers, as bichromate, &c. Reverse photographs were discussed, and the action of sodium sulphite in preventing the evil effects of over exposure. The peculiar green condition of silver bromide which is sensitive to ultra-red rays was explained. In conclusion the lecturer said that his principal object was to warn chemists of some of the numerous pitfalls which they might encounter in scientific photography.

Meteorological Society, May 16.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—F. A. Bellamy, T. A. Mercer, Rev. H. J. Poole, and A. Wise, M.D., were elected Fellows of the Society. The following papers were read:—Composite portraiture adapted to the reduction of meteorological and other similar observations, by G. M. Whipple, B.Sc., F.R.A.S. It has often been remarked that one of the main, if

not the chief, of the difficulties the meteorologist has to contend with, is the enormous amount of preliminary labour which has to be expended in the not very pleasing task of forming the observations he may wish to discuss into tables, casting the columns of figures so obtained, and then computing the means. With the view of arriving at results by a shorter cut, the author has been led to consider the possibility of employing a method, suggested by a consideration of the highly ingenious system of composite portraiture, invented by Mr. Francis Galton, F.R.S., and utilised in his anthropological studies.—Note on atmospheric pressure during the fall of rain, by H. Sowerby Wallis, F.M.S. The author discusses the condition of atmospheric pressure while rain was falling, during 1882, and finds that, out of a total of 136 rainy days (which were available for his purpose), on 54 per cent. the rain was accompanied by diminishing pressure, on 27 per cent. by increasing pressure, and on 19 per cent. by steady pressure.—New method of reading a thermometer and hygrometer at a distance by means of electricity, by Arthur W. Waters, F.G.S.—An integrating anemometer, by W. F. Stanley, F.M.S.—Observations on the force of the wind at sea, by D.W. Barker, F.M.S.—Meteorological observations at Zanzibar, east coast of Africa, during 1880 and 1881, by Surgeon-Major C. T. Peters, M.B.—Diurnal rainfall at Bangkok, Siam, by Capt. G. H. Inskip, F.R.G.S.

BERLIN

Physiological Society, April 27.—Dr. Mendel read a paper on the anatomy of the corpus striatum and lenticular nucleus. The older view, which was supported by the valuable anatomical researches of Prof. Meynert, was that the relation of the corona of radiating fibres above the lateral ventricle ("Stabkranz") to the lenticular nucleus and corpus striatum consisted in this, that in it ran bundles of nerve-fibres, which arise from the brain cortex and end in the large ganglia, whereas Dr. Wernicke three years ago propounded the view that a connection did not exist between the brain cortex and the corpus striatum and lenticular nucleus, but that these latter were bodies of the same range as the cortex. Dr. Mendel has for some years past studied the anatomy of these parts of the brain very attentively, and has been brought back to the older view by a series of sections (of the brain) of dogs, monkeys, and men, which series he laid before the Society. He found not only the bundles of outstreaming fibres, which alone were acknowledged to exist by Dr. Wernicke, but also a larger number of in-streaming bundles of fibres which show the connection of these brain-nuclei to the cortex. In the discussion Dr. Wernicke stated that he was not convinced by the paper or preparations of the correctness of the view propounded by Dr. Mendel, whereas Prof. Munk believed that his not-yet-completed physiological experiments afford grounds for Dr. Mendel's view.

Physical Society, May 4.—Prof. Hauck laid before the Society a model of a mechanical apparatus which solves the problem of combining drawings and photograms, which are drawn in two planes into a combination figure in the third plane. Prof. Hauck then explained the principle of the apparatus, and pointed out by means of geometrical figures the conditions which must be fulfilled in order to project any given points of two planes in common points of a third plane. He then proceeded to the complicated problem of bringing points of three planes, which meet in a corner, to a common projection, and applied these figures to the special case of projecting the perspective drawing of a building from its ground-plan and elevation. The model was calculated and arranged for this case, but the apparatus, in which the motions are produced by means of polished lineals, each running upon two pins, can be put to manifold uses in physical space investigations.

PARIS

Academy of Sciences, May 14.—M. Blanchard, president, in the chair.—The following papers were read:—On the pyro-electricity of quartz, by C. Friedel and J. Curie, second part.—On the cultivation of the cacao plant, with an analysis of the constituent elements of the cacao and chocolate berries, which were shown to contain in various proportions albumen, legumine, phosphates, fat, starch, sugar, theobromine, besides the materials entering into the formation of bone.—On the action of birds in flight studied by means of photography, with figures showing the successive positions of a pigeon on the wing at intervals of one-ninth and one-eighth of a second, and a closed curve representing the trajectory of the tip of the wing obtained by means of a special contrivance, by M. Marey.—On a double sulphate of iridium and potassium, by M. Lecoq de Boisbaudran.—On the diminution of virulence in carbon bacterides and their spores under the influence of antiseptic substances, by MM. Chamberland and Roux.—On iodine associated with the sedative alkaloids of opium treated both as a preventative and curative in the case of typhoid fever, by A. Delbovier.—On the immunity against attacks of Phylloxera enjoyed by the vine cultivated in the sandy soil of Algeria, by MM. F. Couvert and L. Degrully.—Observations on the new planet 233 Borelly made at the Paris Observatory, by G. Bigourdan.—On the determination of the meridian in low latitudes, such as that of Rio de Janeiro, by M. Cruls.—On the conservation of energy and periodicity of the solar spots, by A. Duponchel.—On the laws of coincidences between the reductions of periodical fractions of the "two modes," by E. de Jonquières (continued).—On the generalisation of Thermat's theorem of numbers due to M. Serret, by M. Picquet.—On the possibility of extending to any electrolytic field the electro-chemical method in the figuration of potential distribution, by A. Guébhard.—On the influence of atmospheric pressure on the eruptions of gas and water in the Montrond Geyser (Loire), by F. Laur.—On the differences in the temperature of the sea and air, by M. Semmola.—On the quantitative analysis of sulphur and carbon in sulphocarbonates, by A. Müntz.—On the regular surface-fissures in certain rocks, such as the hard eocene limestone used in the construction of the old ramparts of Genoa, by Ch. Contejean.—On new physiological studies of the torpedo, by M. Marey.—On the functions and organs of suction and deglutition in the leech, by G. Carlet.—On a case of purulent ophthalmia produced by the infusion of the seeds of the liquorice plant, by L. de Wecker.—On the fundamental principle of the electric log now in use in the French fleet, by M. G. Le Goarant de Tromelin, who claims priority of invention over the electric log invented by M. Fleuriais.

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