THURSDAY, SEPTEMBER 6, 1883

NEOCOMIAN FOSSILS

The Fossils and Palæontological Affinities of the Neocomian Deposits of Upware and Brickhill. Being the Sedgwick Prize Essay for 1879. By Walter Keeping, M.A., F.G.S. Large 8vo, pp. 167, with eight plates of fossils. (Cambridge, 1883.)

PHOSPHATIC deposits may be said to occur, in this country, on all horizons from the Bala limestone to the crag, yet do they most abound in the "strata below the chalk," and particularly in those portions of the Cretaceous system which underlie the chalk of the south-east Midlands. Thus Cambridge is almost as famous for its *coprolites* as Newcastle for its *coals*, and the economic inferiority of the Mesozoic rocks has of late years been partially redeemed, in consequence of the numerous workings in these valuable beds.

The immense collections of fossils from the several "phosphate diggings," now in the Woodwardian Museum, afford those who have watched the growth of these accumulations a splendid opportunity for studying an unusually rich vein of palæontology, and at the same time of forming more correct views as to the physical history of these much debated deposits.

The coprolite beds *beneath the gault* at Upware were vigorously worked some sixteen years ago, but as the phosphates were inferior in quality, the work presently slackened, though not before large quantities of fossils had been secured by gentlemen from Cambridge, who have ever been foremost in studying these and the allied deposits. The workings at Little Brickhill were described by the author himself in the *Geological Magazine* (volume for 1875, p. 372), but since then the knowledge of its fauna has been largely increased, "so that the Brickhill bed is now only second to Upware and Farringdon in its organic richness." Recently (1879) the Upware sections have again been exposed.

There can be no doubt that Mr. Walter Keeping is very well qualified to perform the task he has undertaken, and that the Sedgwick Prize Essay for 1879 must rank as one of the most useful contributions to Neocomian geology and palæontology that has appeared in this country. It is, in fact, the outcome of a long experience judiciously applied. The author summarises his own work in the preface, so that readers may know what they have to expect: his conclusions as to the age of the ironsand and phosphatic series are stated to be in near accordance with the opinions of Messrs. Walker, Teall, Meyer, and Barrois, "all of whom have placed the Upware bed in the Upper Neocomian or Aptian series (Lower Greensand)." The *mammillaris* zone he regards as the basement bed of the gault, and to this horizon refers the Downham phosphate bed.

The first part of the work deals with the deposits generally, the indigenous fauna, the "derived" fossils, the British and foreign relations of the beds. The second part is devoted to special palæontology.

In discussing the question of phosphatisation he observes that the nodules of Upware and Brickhill have been derived, for the most part, from the Upper Jurassic rocks, where as a rule the majority of the Jurassic fossils are *not phosphatised at all:* and he concludes from the similarity in the general character of the phosphate of lime nodules, whether from Oxford Clay, Kimmeridge Clay, or Portlandian Rock, that the phosphatic matter was derivative, and all, or nearly all of one age. At page 30 he speaks of a coprolite heap near Ampthill, as looking like one mass of *Ammonites biplex*, mostly worn and fragmentary, whilst the Ammonites of the Oxford Clay are composed of limonite, and some of the fragments of fossil wood are silicified. Strangest fact of all—the Coral Rag fossils from the neighbouring rock have not been phosphatised in the least. The author (p. 15) suggests that this purer form of carbonate of lime was "uncongenial to the phosphatic matter," which would have more affinity for argillaceous substances, and yet he quotes the case of a stalagmitic deposit having become phosphatised by percolation.

It is not a little suggestive that whilst *Ammonites biplex* from the Upper Kimmeridge (Middle Portlandian of the French) is phosphatised in heaps, the Oxford Clay Ammonites are in the condition of limonite. This seems to show that *original conditions* have had something to do with the case. Both Oxford Clay and Kimmeridge Clay Ammonites and casts are often pyritised in their own beds; on the other hand, the Kimmeridge Clay as a rule, especially in the Valley of Aylesbury, has all the appearance of being rich in phosphatic matter. The process of replacement, therefore, whereby the fossil cast became the phosphatic nodule, may have been inaugurated during the progress of denudation, assisted possibly by accumulations of contemporary animal matter due to abundant aquatic and semi-aquatic life. In this way the phosphatisation was probably completed shortly after emergence, and the future coprolites were collected in banks and shallows, to be distributed subsequently, along with lydites and anything that could stand knocking about by the action of waves and currents, throughout the shore deposits of a slightly later period. Hence we venture to suggest that the phosphatisation of the Upware coprolites was effected at some distance from their present billet, and thus that the fragments of Coral Rag were never exposed to the temptation of having their carbonic acid replaced by phosphoric.

The principal object of the essay is of course to describe the indigenous fauna, and to correlate the deposits generally with others of the period, whether British or foreign; the similarity of the Upware and Brickhill fossils to those of the Neocomian beds of the Brunswick area at Shöppenstedt and Berklingen being especially mentioned (p. 73). This, together with the special palæontology, has been very satisfactorily worked out. We have already alluded to the general conclusion based on these investigations, and it only remains to notice some of the more detailed matters.

For instance (p. vi.), the author notes the close palæontological relationship of the ironsand and phosphatic series as found at Upware, Potton, Brickhill, and Farringdon, the great difference in the fauna at Potton being due, he conceives, to the influence of physical conditions. He further alludes to the *special* character of the native forms of life, and to the marked preponderance of Brachiopods, Polyzoa, and Sponges; to the profusion of Brachiopod shells, both individually and specifically, and the graduation of the various types (species) into one another (p. 22).

In dealing with this latter subject the author has possessed unusual facilities, since himself and his father have availed themselves of the 15,000 Brachiopods collected from Brickhill to arrange sets of connecting forms between recognised species of *Terebratula*, *Waldheimia*, and *Terebratella*. It must not be supposed that between all the species enumerated the connecting morphological varieties are equally evident or of equally frequent occurrence: between some species the passage is simple and clear, both as to the main line and the offshoots, whilst between others much more searching is required to establish the connecting series.

Brickhill indeed seems to have been a centre, as regards the Brachiopoda, of inordinate fecundity accompanied by considerable inosculation of form, just one of those places, in fact, where the oft-demanded missing link was manufactured on a large scale, whilst at Upware and other places on the same horizon the form-groups known as "species" had somewhat contracted their circle of variation. Doubtless almost every zoological group has had its Brickhills in the course of ages, though the chances of preservation and subsequent discovery must limit the number accessible to research. Mr. Keeping, having given us valuable and cogent proofs of the mutability of the forms of Brachiopoda, and apparently somewhat uneasy as to the results of his own conclusions, proceeds to assure us that the value of "species" has been considerably enhanced by these investigations both to the naturalist and the stratigraphist.

Glancing briefly at the part devoted to special palæontology, we learn that the vertebrate remains of Upware are in a great part truly Neocomian species native to the deposit. The probable identity of form of some of the palatal teeth of Jurassic and Neocomian species is insisted on especially in the case of *Sphærodus*.

Coming to the Invertebrata, we find that Cephalopoda are by no means individually numerous; they are for the most part well-known Aptian species. Neither are the Gasteropoda at all plentiful, though some new species are described, including two of *Nerinæa*, both very rare. This is the more remarkable as the uppermost Jurassic rocks of England are, as far as we know, devoid of this genus. The oysters form an important feature, and, excluding the plaited species, greatly resemble those of the Jurassic rocks. Mr. Keeping is convinced that the shell he refers to *Gryphæa dilatata*, Sow., is a genuine native. It is somewhat singular that the Oxfordian *G. dilatata* should have been resuscitated rather than the Portland-Kimmeridge *Ostrea expansa*, Sow., which swarms in the Upper Kimmeridge (Middle Portlandian) of Bucks and in the Portland stone of several localities. On the whole there is a fair list of Monomyaria, including some new species.

Of the Dimyaria one species of *Trigonia* occurs, and is restricted, it would seem, to Upware. The Arcadæ are well represented, three species of *Pectunculus* being given. Of the remaining genera *Cypricardia* and *Cyprina* have the most species, but none are quoted as abundant, though some new species are described. The Brachiopoda, Polyzoa, and Sponges, as every one knows, make up the bulk of the fossils, many of the latter being identical with those of Farringdon.

The table tells us that 151 species are listed from Upware and 88 from Brickhill. Of these 45 occur at Farringdon, 39 at Godalming, 24 at Speeton, 21 at Potton, 19 at Tealby, Shanklin, and Atherfield respectively, 16 in the Hythe beds, 6 in the Folkestone beds, 1 in the Hunstanton Red Chalk, and a doubtful case in the Folkestone Gault.

The book is conveniently got up, not being too large, is well illustrated by Foord, and altogether forms a most desirable volume for the Mesozoic geologist.

W. H. H.

OUR BOOK SHELF

Sound and Music. By Sedley Taylor. Second Edition. (London: Macmillan and Co., 1883.)

THAT this excellent elementary work has at last reached a second edition is certainly in one respect satisfactory. But that nine years should have been occupied in the process, while the "popular" rubbish of the paper scientists has in many cases (or at least is proclaimed as having) annual or biennial reproduction, is matter for profound regret and meditation.

We noticed so fully (NATURE, vol. x. p. 496) the first edition of Mr. Taylor's work, that it is not necessary to say much now. Some of the parts to which we formerly took exception have been considerably modified; in all cases but one, we think, for the better. The one exception is that about the use of the word *force* (or opposite systems of forces) in the explanation of the mutual destruction of sounds by interference.

The word "*timbre*" has been excised, and its place supplied by "quality"; but the hideous misuse of the English word "clang" in the sense of a harmonious combination of sounds still disfigures the later pages. It is time that a definite and suitable nomenclature should be once for all introduced into this part of the subject. There are many words, such as "sound," "note," "tone," &c., which every one seems to think himself entitled to employ as it pleases him, even to the extent of using one of them occasionally in two perfectly incompatible senses. But almost anything would be preferable to a literal transcription of Helmholtz's words into an English book, without regard to the inevitable incongruities.

Southern and Swiss Health Resorts. By William Marcet, M.D., F.R.S. 12mo, pp. 408. (London: Churchill, 1883.)

Nice and its Climate. By Dr. A. Barέty, translated, with additions, by Charles West, M.D. 12mo, pp. 162. (London: Edward Stanford, 1882.)

THIS work of Dr. Marcet is written in an easy, popular style, and gives people very much the sort of information they want. It begins with advice to invalids about to visit the Riviera regarding dress and food, next has something to say regarding hotels, boarding-houses, apartments, and villas; gives some general ideas of social life in the health resorts of the Mediterranean coast, and then proceeds to a more purely climatological description of the Riviera in general and of the particular characteristics of the different towns upon it. Dr. Marcet's residence for some years on the Riviera gives his description of the health resorts there all the accuracy and fulness, without unnecessary detail, which personal acquaintance alone can secure. The same may be said of his description of the health resorts of Switzerland, and his account of the Swiss resorts at low or moderate elevations are particularly interesting and useful. As a guide to invalids the book is rendered more complete by a short account of Italian health resorts, Algiers, Egypt, Madeira, and Teneriffe. The book will be useful both to invalids who are meditating a winter abroad and to medical men by aiding them in the selection of the proper places to which to send patients.

Dr. Baréty's description of Nice and its climate is naturally very much fuller than Dr. Marcet's, and while the latter is a useful guide to the selection of a health resort, the former will be of great service to those who have already fixed upon Nice. The climate of this town varies very much in its different parts, and the proper selection of an hotel or residence is of considerable importance. As a guide in this, and also as a general handbook for reference when residing in the town, Dr. Baréty's book is to be recommended.

Vichy and its Therapeutical Resources. By Prosser James, M.D., M.R.C.P., &c. Fifth Edition. Pp. 84. (London: Baillière, Tyndall, and Cox, 1883.)

THIS is a small guide-book to Vichy, pleasantly written. It contains, as usual in such books, a general account of the place, its springs bathing establishments and environs, analyses of the waters, and an enumeration of the diseases in which they are said to be useful. We doubt whether the latter part will be of very much service either to medical men or to invalids; it might, we think, have been omitted with advantage.

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.]

Arithmetic Notation of Kinship

MANY writers have endeavoured to devise a simple method of describing the various forms of kinship, which, when expressed verbally, are cumbrous and puzzling in the highest degree. I suspect, however, that if we had always been as familiar with the binary system of arithmetic as we are with the decimal, that the facilities afforded by a numerical system of notation of kinship would have been so obvious that it would have been adopted as a matter of course. The notation I am about to propose is numerical, but it is not binary. It however contains implicitly, as we shall see, owing to the laws that govern numbers, the most important advantages of the binary notation, and it seems better to begin to explain it from the latter point of view.

The number of direct ancestors that a person has in successive generations is . . . 2^4, 2^3, 2^2, 2^1, followed by 2^0 for himself, the corresponding binary notations being 10,000, 1000, 100, 10, 1 respectively. We also see on a little reflection that, this being the case, every direct ancestor in the nth degree admits of being specified by a particular number, consisting of $n + 1$ places of figures. Thus the two parents may be represented by 10 and 11, the four grandparents by 100, 101, 110, 111, and so on. Let us draw up two schemes of ancestral roots, identical in arrangement, but using in the one the symbols of *f* for "the father of," and *m* for "the mother of," and employing binary notation in the other:—

child

f *m*

ff *mf* *fm* *mm*

fff *mff* *fmf* *mmf* *ffm* *mfm* *fmm* *mmm*

&c. &c.

1

10 11

100 101 110 111

1000 1001 1010 1011 1100 1101 1110 1111

&c. &c.

We see (*a*) that if we disregard the child, and speak only of his or her ancestry, all the even numbers apply to one sex and all the odd ones to the other; (*b*) that each term is derived from its ancestral terms in so simple a way that it carries on its face every step in the line of descent, however long it may be, through which each ancestor is related to the child. Therefore, as I began by saying, if we were familiar with decimal notation, we should long since have described each form of ancestry by it. Instead of saying that "B was a grandmother, namely, a father's mother of A," we should have said "B was 101 of A." Or again, instead of saying that "C was first cousin once removed to D, the father's father's parents of C being the mother's parents of D," we should have said "the 1000-1 of C are the 110-1 of D." The case might have been one of half-blood, say by the father's side, then "the 1000 of C would be the 110 of D," a notation which grows in simplicity as the verbal equivalent grows in complexity.

Being, however, unfamiliar with binary notation, we fall back on the decimal, and translate the above numbers into their equivalents, which are those I propose for the arithmetic notation of kinship, as entered in the table below.

Table of Ancestral Roots

Grade of kinship.	Father's side.	Mother's side.
Child	1	
Parents	2	3
Grandparents ...	4 5	6 7
Great-grandparents &c.	8 9 10 11 &c.	12 13 14 15 &c.

The sex of 1 is unspecified, it is equivalent to the word "child," but all other odd numbers refer to females, and all even numbers refer to males. If n is the register number of any ancestor, the register numbers of his parents are $2n$ and $2n + 1$. We can thus construct or analyse any register number with great facility. It is not worth while giving an example of construction, but I may give one of analysis. I write down the number and append to it a series of successive halvings, so far as the numbers are, or come out, even; otherwise I substract 1 before taking their halves. Then I write *f* (= father of), or *m* (= mother of), as the case may be, below each entry. Let 253 be the number, then I get—

253	126	63	31	15	7	3	child
m	*f*	*m*	*m*	*m*	*m*	*m*	child

For purposes of exhaustive inquiry into the antecedents of a family, this notation has the advantage of an index, and of showing very compendiously how much has been done, and how much remains to do. FRANCIS GALTON

"Stachys palustris" as Food

THERE is no reason to think that *Stachys palustris*, L., is anywhere used now for food in the British Isles. The cultivation of the potato must have long since put it out of court for any such purpose. But that it was once so employed there seems abundant evidence. The part eaten, however, was not the "rhizomes," but the subterranean tubers. That the use of these is now quite forgotten may be inferred from the fact that the tubers themselves are not even mentioned in standard systematic books. Yet Irmisch (see *Botanical Gazette*, vol. ii. p. 293) gives the potato and *Stachys palustris* as well known instances of dicotyledonous plants producing stem tubers which become detached by the dying away of the older parts of the parent plant which produced them.

Johnson, in the second edition of Gerarde's "Herbal" (1636), has nothing to say about the edibleness of the tubers. But he evidently thought that they required notice. He has had them added to a figure of the plant (which he reproduces from "Tabernæmontanus," as he explains, "with addition of the joynted tuberous roots as they are in Winter; yet by the Caruer's fault they are not altogether so exquisitely exprest as I intended." Withering ("Botanical Arrangement of British Plants," ed. ii., 1787, p. 613) has, "The roots, when dried and powdered, will make bread." And in Bromfield's "Flora Vectensis" (1856), the note occurs:—"The roots of *S. palustris* are said to become edible by cultivation. See Curtis, 'Brit. Entom.,' vi." This last is a book to which I have not convenient access; but the reference may give your correspondent a useful clue.

I may mention that I do not find the name "Base Horehound" given at all in Britten and Holland's very comprehensive dictionary of English plant-names. The old name for *Stachys palustris* was "Clown's All-heal."

W. T. THISELTON DYER

Garfish

HAVING been absent from England for some time, I have only just noticed the two letters published in NATURE for July 5 and 12 (pp. 226 and 245), on "Garfish." I have little doubt that the fish described by Mr. S. Archer as having cut a slit in a felt hat was, as he believes, a garfish, a large Belone, not a Hemirhamphus, and not a swordfish or sawfish of any kind, as suggested by Mr. Goodsir. It is the constant habit of large Belones, some species of which attain, according to Dr. Günther, a length of five feet, when startled to move along the surface of the water by a series of rapid bounds for thirty or forty yards at a time, with astonishing rapidity. I have often seen them thus spring out of the water when scared by a boat. I was told that in some of the Pacific Islands these fish not uncommonly cause the death of the natives, who, when wading in the water, have their naked abdomens speared by the sharp snouts of the fish, with the result of causing peritonitis. The fish appear to bound blindly away from danger, and strike any obstacle in their way haphazard. As a good many natives wade in together in many of their fishing operations, as at Fiji, for example, where one party drives the fish into the nets held by another, such accidents may easily occur. I do not think a sawfish could possibly jump over a boat. I have described the jumping habits of the large garfish, and alluded to their fatal effects in "Notes by a Naturalist on the *Challenger*," p. 480.

H. N. MOSELEY

Continuous Registration of Temperature

IN your issue of July 26 (p. 306) there is a description of an apparatus lately devised by Messrs. Negretti and Zambra, by which a record of twelve temperatures in succession can be obtained by the somewhat elaborate arrangement of twelve thermometers, a clock, and a series of electromagnets and battery. I wish to bring under your notice a simple machine invented by Mr. Bowkett, late resident medical officer of the Leeds Fever Hospital, by which a continuous record of atmospheric temperature can be obtained by means of an apparatus consisting solely of a "bourdon" steam gauge tube, a clock, and a writing lever, costing little more than a few shillings.

Mr. Bowkett devoted great mechanical skill during several years of experimentation to the perfecting of a form of this instrument sufficiently small and accurate to be used for medical purposes, *i.e.* to register the temperature of the human body. For this purpose the instrument has to be somewhat more complicated, and accordingly more costly. Many of these are in use in our hospitals and elsewhere, and are of the greatest possible value.

These instruments can be made of any size, and when large are of very great strength, and might easily be applied for thermal regulation by attachment to valves or other ventilating arrangements. The instrument constructed by Mr. Bowkett for registering the temperature of rooms was of the size of a small clock, of the simplest possible character, requiring very little care in its use. Messrs. Salt of Birmingham are the licensees of the patent.

ERNEST M. JACOB

12, Park Street, Leeds

Aurora and Thunderstorm

A DISPLAY of aurora was seen here on the 30th ult., which may perhaps be of sufficient interest for insertion in NATURE.

A thunderstorm passed from west to south during the afternoon. Thunder and lightning commenced between 3 and 4 p.m. and continued till about 9 o'clock. The storm centre was about two miles from the city; no rain fell here, though a heavy hail shower fell to the west in the afternoon. Lightning was vivid till past midnight in the south. From 11.30 to midnight an auroral light passed over the zenith from west to east, of well defined nebulous light. It was 10° in width as measured by a sextant. This was joined on the north by a horizontal band of aurora 18° altitude. There were no great flashing lights from this.

On the northern horizon was a small arc throwing up short flashes. The horizontal band was the brightest.

The temperature and force of wind and barometer readings were as under:—

	Barometer.	Wind.	Temperature. Dry Bulb.	Wet Bulb.
			°	°
3 p.m.,	29·857 ...	...	79·8 ...	71·9
6 ,,	29·855 ...	...	71·4 ...	64·0
9 ,,	29·862 ...	S.S.E. 5 miles ...	66·4 ...	64·0
12 ,,	29·851 ...	S.S.E. 4 miles ...	60·9 ...	60·3

Maximum shade reading of the day, 83°·5.
,, ,, of the 31st, 82°·0.

The observatory is 764 feet above the sea level, and I am indebted to the observer for the above figures.

ALAN MACDOUGALL

Winnipeg, Manitoba, August 10

A Complete Solar Rainbow

ON Thursday, August 16, while R.M.S. *Norham Castle* was in lat. 2° 20′ N., long. 13° 58′ W., a phenomenon entirely new, at least to the officers and passengers on board, appeared at 11 a.m., and lasted until 12.30 p.m. This consisted of a complete rainbow round the sun, when nearly and at the zenith, having an inner diameter—taken by Capt. Winchester, R N.R.—of 43° 08′. The day was bright and warm, with a slight haze above. The rainbow appeared to crown the whole of the upper dome of the sky, and to possess all the normal colours, only very slightly dimmed. Whether connected with this appearance or not I cannot say, but the next two days were squally, with heavy rains.

D. MORRIS

Kew, September 5

Animal Intelligence

I AM a constant reader of NATURE, and have read with much pleasure the several instances recently communicated by correspondents of animal intelligence, a subject in which I take great interest.

It has struck me that some of your readers might in turn be interested in hearing of the intelligence and powers of observation of a collie bitch called "Winifred," my constant companion.

In one of the fields attached to my house there is a large pond well stocked with fish, and especially with eels. I very often spend an evening fishing for these latter, using several lines at different points round the pond, the rods lying on the grass, each one receiving my attention whenever its respective float indicates that there is a bite.

The collie "Winifred" is constantly with me on these occasions, and has always taken the greatest interest in her master's proceedings, watching every movement most intently. It was for a long time a source of considerable amusement to me to notice that by constant observation the dog had come to understand the connection between the bobbing and final disappearance of a float and the subsequent exciting proceedings of pulling up an eel, disengaging it from the hook, and putting it into the creel. The cocked ears, head on one side, and eager eyes of "Winifred" when she saw a float bobbing gave plain proof that she was as much interested in the fishing as her master.

One evening some six weeks ago it happened that I was at one end of the pond baiting a hook, while the dog had remained at the other end, lying on the grass close to one of the rods. Suddenly I observed her showing signs of excitement, and, on looking across, saw one of my floats finally disappearing under the water. As I did not come, "Winifred" got very excited indeed, uttered one or two sharp yelps, and ended by seizing the rod in her mouth and "backing" with it, attempting to pull out the line from the water. I hurried to take the rod from her, fearing the effects on my tackle of the lack of skill of this canine disciple of Izaak Walton! There was a goodly eel on the hook, sure enough.

Since then "Winifred" has once again attempted to pull out the line under exactly similar circumstances. Surely this conduct shows powers of observation and of inference of no mean order?

I may add that the collie is now three years old. She saw me fishing many times last summer, and, as I said before, always showed great interest in what was going on. But it was not till six weeks ago that I had any idea how much she was profiting by what she saw.

Perhaps some of your other readers who fish, and are accompanied by intelligent dogs, may have observed similar instances of reasoning power. Seeing to what perfection dogs can be trained to take part in other branches of sport, perhaps it is not very surprising that here and there one should show a little appreciation of the leading points of the "gentle art" of angling.

MORGAN J. ROBERTS

The Hollies, Cwm Newydd, Holywell, North Wales, August 31

Copper and Cholera

NEED we go to Sweden to test the theory that copper is a preservative against cholera? The year before the 1865 epidemic I travelled by train past Swansea, and my attention was called to the utter want of verdure in the surrounding country, due, I was told, to the copper fumes.

Now, according to the official report, the deaths from cholera in Swansea were 88 in 10,000 in 1866, in Neath 79, in Llanelly, 76—all places in the same neighbourhood; thus showing a far greater mortality for the copper-smelting district than any other in England or Wales. The mortality for all England was only 13 in 10,000, and for London 18. The only two places which in any degree approach Swansea are West Ham with 50, and Liverpool with 54; in both of which it is well known cholera was especially severe. The epidemics of 1849 and 1854 present Swansea in a more favourable light.

Perhaps some of your Swansea readers, by giving the number of deaths—if any—among the actual workers in the metal, can help those who, like myself, are inclined to believe in copper as a prophylactic; in what way I scarcely know, unless it be according to the principles of homœopathy, as my experience on three occasions—and a lively time I had of it—lead me to believe that copper added to plums to preserve their colour should be eschewed, at any rate in cholera times.

Dulwich, September 1 B. G. JENKINS

The Meteor of August 19

THE same meteor was undoubtedly seen by Mr. Crispin at Wimbledon, Mr. Pooley at Cheltenham, and myself at Llandudno, and I think I can remove Mr. Crispin's difficulty.

The apparent fall of meteors towards the earth is generally an effect of perspective. An object at a great height moving directly away from the observer appears to move perpendicularly downwards. If moving away obliquely to right or left, it appears to have a more or less horizontal path with a downward inclination.

This meteor was evidently not moving towards the earth, but was one of those that skim the upper atmosphere, white-hot at their surfaces while the resistance is sufficient, and dark again as soon as they pass into a thinner medium. I suppose it to have first become luminous when directly over Essex, not far from Chelmsford, at a height of about seventy miles, passing north-east over the sea, and vanishing near the Texel. Its appearance along such a path would agree very fairly with the three observations, except that, if Mr. Pooley saw it first quite south-east by compass, it must have been luminous for a second or two before Mr. Crispin or myself observed it, and the starting-point would be nearly over London.

I was wrong at first in referring to the Yorkshire coast. The visible path was clearly south of the Humber.

ALBERT J. MOTT

Crickley Hill, Gloucester, September 2

THE ISCHIAN EARTHQUAKE OF JULY 28, 1883

SINCE my last letter to NATURE most of my time has been occupied in visiting different parts of the island, and although there are still a number of objects to be carefully examined the general features of the catastrophe I hope to have cleared up.

The actual moment of the earthquake is unknown, but seems to have been about 9.25 p.m.; so, supposing the shock registered at Naples and Vesuvius to be identical with that of Casamicciola, had the observation of time at the latter locality been correct, we could calculate the velocity of transmission, but which it is to be feared is impossible.

As mentioned in NATURE, the shock was preceded by general seismic disturbances throughout Southern Europe. In the island itself we have the most contradictory statements as to premonitory signs and symptoms. One gentleman noticed on two occasions previously his watch, which was suspended by a nail to the wall, swing backwards and forwards. The assertion about the water at Gurgitella being much hotter some days before is of little value without proper thermometric observations, since it is known commonly to vary 20° C., and may reach more than 40° from time to time, and I am acquainted with a thermal spring at Bagnoli, near Naples, that varies 23° C., ranging from 13° to 36° C. Perhaps the most remarkable of these kinds of statements was couched in these words:—"The syndic of Serrara Fontana (a town on the south of the island) telegraphed to the Minister of Public Affairs to the effect that in that country a fissure one kilometre long, thirty metres broad, and of unknown depth, from which were issuing dense columns of vapour." On reading this I started immediately for Serrara, and there the syndic placed at my disposal his two informants as guides. After a climb of three hours and a half along the almost impassable sides of Epomeo, we came to its northern slope over Lacco Ameno, with the two landslips I had visited and photographed thirty-six hours after the shock. The fissures were such as take place along the edges of all landslips. No vapour was issuing, and its presence for a short time after the earthquake could be easily explained: the locality is part of the old fumarole area of Monte Cito, where alum was manufactured centuries ago; the rock is much decomposed by the continual escape of acid vapour, and only required the earthquake to shake it down; when the displacement took place a large surface of hot and moist tufa was exposed, and no doubt for some time gave off a quantity of vapour.

It will be seen that not a single point of the size, locality, and characters of the fissure described by the newspaper was correct or free from gross exaggeration. I have visited with care all similar sites of supposed fissures, but after some days of want of shelter, sleep, an abominable starvation diet of bad bread and rotten cheese, combined with continual climbing from daybreak to sunset in an extraordinarily hot Neapolitan summer in the hope of finding some evidence of volcanic action, I did not meet with the slightest success. I was accompanied in these excursions by my friend, Prof. P. Franco of Naples, who shared my disappointment and disgust. Holding as I do the volcanic nature of the earthquake, the appearance of any such phenomena would have been greedily accepted.

If we draw isoseismal lines over the injured districts, we find that they assume the form of elongated ellipsoids whose major axes run nearly east and west.

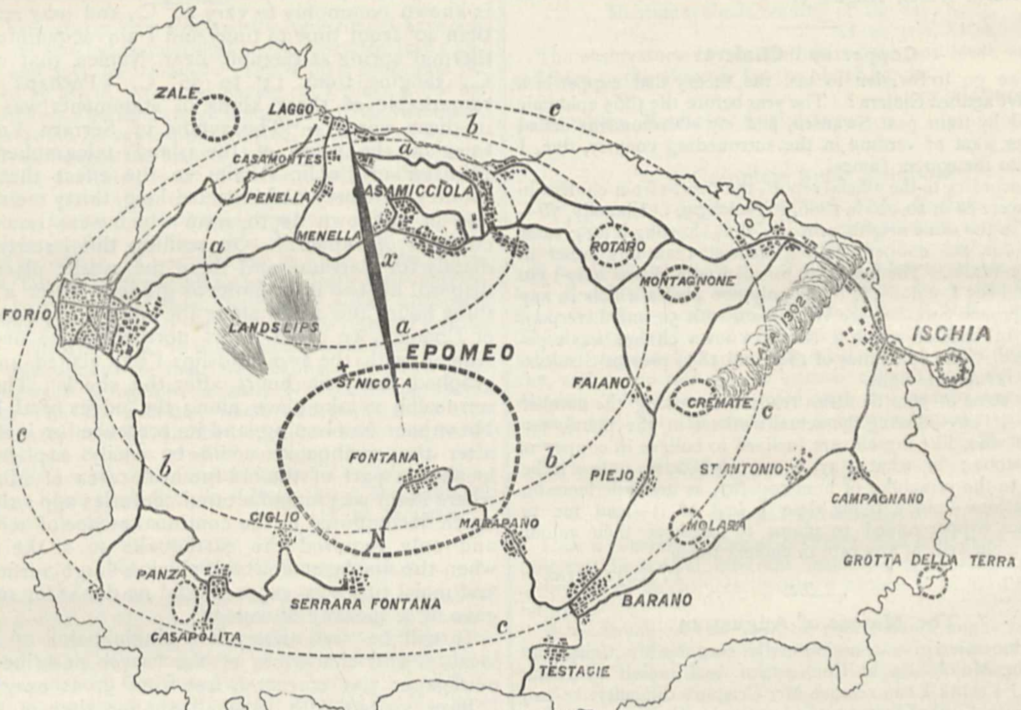
The fourth isoseismal area, in which houses are only very slightly fissured, not only includes the whole island but must extend into the sea some distance.

One remarkable fact is the manner in which the houses of the marinas have suffered much less than others in their immediate neighbourhood, or even farther away from the seismic vertical. This is no doubt due to their foundation reposing on sea sand, which, from the looseness of its particles and therefore inelastic nature, acted as a mattress and absorbed the earth waves. The same fact is observable in all buildings that have their foundations of the loose alluvial tufas and ash of the plains and valley bottoms, whereas the destruction of houses built on the rock was terrible, thus reversing the well known parable.

Putting aside, however, the speculative side of the question, let us look to facts. From a careful examination of observed azimuths and angles of emergence all point to a plate-shaped focus, whose strike extends in a line from Fontana, just west of Menella, to near the beach at Lacco. The plane of this fissure is probably roughly perpendicular to the surface, but may slightly dip towards the east as the isoseismals are slightly nearer on the eastern side of the seismic vertical, which as a necessity is not represented by a point, but a line on the surface. The rupturing of this plate-like fissure was apparently greatest at a point nearly midway between its extremities.

The remarkable fact that I observed in the earthquake of 1881 that at Fontana the shock was almost vertical like that at Menella is again repeated; for which I then proposed as an explanation the conduction along a tube or column of highly elastic trachyte, filling the old chimney of Epomeo, whilst the surrounding districts were protected by the less elastic mattress of subjacent tufa. On the present occasion Fontana, as before, has all its houses fissured in such a manner as to demonstrate a vertical shock; but besides there is another set of cracks which show a north to south path for a wave at a low angle of emergence. At St. Nicola part of the altar has rocked in the same direction, and between these two localities a rock has been ruptured and projected to the south. Whatever may be the explanation accepted, the fact remains as unique in seismological history.

That Vesuvius did not, or only to a slight extent, sympathise with the seismic movement in the island of



Sketch Map of the Island of Ischia. Scale 1 to 80,000. Isoseismals marked thus ....; ancient eruptive centres and craters in dotted circles. *a*, mesoseismal area = total destruction; *b*, first isoseismal = many houses fallen, rest require rebuilding; *c*, second isoseismal = severely fissured. The third isoseismal does not seem to cut the land; its limits, therefore are unknown. *x*, fissure, or focal centre.

Ischia is no proof against the volcanic origin of the earthquake. Admitting the hypothesis of a seismic wave traversing a large tract of volcanic matter underlying Southern Europe, such a wave may produce very slight variation at volcanic vents, but may yet be sufficient to determine the extension by rupture of a fissure, where the resistance of the rock, and the tension of the volcanic matter near the point of extension, are nearly equal. In a paper lately read before the Geological Society of London I endeavoured to prove that the explosive violence of lava is due to the assimilation or solution of water taken up from the water-bearing strata it traverses in its passage from the main source towards the surface. Under such conditions we may have very violent phenomena produced, locally, without any sympathy in neighbouring volcanic vents; which at the same time explains I believe one volcano bursting out in a violent paroxysmal eruption, whilst a near companion is in no way affected. Why therefore should an abortive paroxysm disturb a neighbouring active vent, as in the present example?

In various points that I have examined the coast of the island, there was no apparent change of level, nor did any success attend endeavours to discover any signs of depression of the surface.

It has been stated that the sea receded, but I could not obtain any confirmation of the fact. As the steamer that lay at anchor in the roads felt the shock, it may probably have been due to the disturbance of the water by the earth waves.

As far as my inquiries have gone, the first symptom was a distant sound like that of a carriage, almost immediately accompanied by a tremor, then a terrific explosion shading off into a number of reports. Most people not in the mesoseismal area felt first the "susultatorio" or vertical movement, followed by the undulatory, or, more properly, lateral motion. This, as is well known, is due to the arrival first of the emergent wave, followed by the progress outwards and along the surface of others from the seismic vertical. On the contrary, those in the meso-seismal area felt the blow and report apparently simultaneously; the walls fell before any attempt at an escape could be made. It appears therefore that the sound waves travel faster than those of the earth, though the difference in arrival is inappreciable at short distances from the seismic vertical.

Since the principal shock the following minor ones have been felt:—

July 29, 1883	Slight shock in the morning?
Aug. 1, ,, ...	,, 3.10 p.m.
,, 1, ,, ...	,, 4.50 ,,
,, 1, ,, ...	,, 11.15 ,,
,, 2, ,, ...	,, 2.30 ,,
,, 3, ,, ...	,, 1.15 or 2.15?[1]
,, 8, ,, ...	,, 10.40 a.m.
,, 12, ,, ...	,, morning?

The accessibility of the island, the advanced state of our geological knowledge of it, and the small extension of the earthquake area, make it most suitable and convenient for the study of its terrestrial movements. What is required is a number of seismographs scattered over the island, which should be capable of registering azimuth, angle of emergence or molecular velocity, with the exact time of each movement so as to obtain velocity of transmission. These should be distributed in two circles around the seismic vertical, and should be at least sixteen in number, eight being in each circle, one or more for registering vertical waves to be placed along the seismic vertical. Accurate thermometric measurements of the principal fumaroles and mineral springs to be registered hourly, and if possible some device for measuring quantity of outflow of mineral waters, and pressure of vapour in fumaroles. To these it might be useful to add microseismic observations. The changes in sea level would be of interest if compared with those of Naples.

The principal expense would be providing the instruments, which could be placed in caves cut in the solid tufa, of which there are hundreds in the island that could be obtained for almost nothing, if not entirely free of expense.

By such means we might study the true nature of these shocks, the progress of the focus towards the surface, and verify whether any premonitory signs are to be depended upon preceding an earthquake.

I would impress on all persons charitably inclined that money spent on such an enterprise would be productive of far more good than when distributed to be spent in rebuilding the perilous houses of masonry in preparation for another catastrophe. Not six days after the terrible event, masons were at work repairing the most dangerous walls, and many inhabitants have already returned to reside in their fissured and crumbling abodes. Besides, if another shock occurs more violent than the last, a large number of additional localities would suffer, such as Forio and Ischia, besides the villages on the south coast of the island. H. J. JOHNSTON-LAVIS

P.S.—In collecting evidence of the Ischian earthquake a very remarkable fact was communicated by Mr. Petersen, the engineer of the Zoological Station at Naples. Whilst dredging on the north side of the unfortunate island, about opposite the cemetery of Casamicciola, a number of pieces of pumice were found floating on the water, some of them as large as a man's head; they had quite a fresh appearance. The conclusion is that there has been a submarine eruption somewhere near the island. Such would explain the sensations felt on board the steamers and the apparent disturbance of the coast line. On the other hand it is strange that the eruption left no other signs, and that nothing was observable the next morning. No dead fish were noticed. The pumice might be derived from loose deposits containing that material, which form some of the sea cliffs which were shaken down by the earthquake. Whatever be the real cause, we propose to investigate it thoroughly by dredging and diving, as the water rarely exceeds twenty to thirty fathoms at the most. H. J. J.-L.

Naples, August 31

[1] Which was much stronger and produced slight damage.

THE BERNISSART IGUANODON[1]

THE wonderful discovery of remains of Iguanodons made at Bernissart in 1878 caused quite a sensation amongst naturalists at the time, and the publication of the scientific results of that grand find have been awaited ever since with eager expectation. Nevertheless, as five years have elapsed since the discovery was announced, it is well that the memory should be refreshed by a few brief details as to the circumstances of the find itself before the results as to the nature of the Iguanodons themselves, lately made public, are referred to. Bernissart is in Belgium, situate between Mons and Tournai, close to the French frontier. In the spring of 1878, in one of the galleries of a coal mine there, were discovered in Wealden clays a large number of bones. Specimens of these bones were forwarded to Professor P. J. van Beneden, who at once recognised them as belonging to Iguanodon.

It is to M. Fagès the director general of the Bernissart Mining Company that the discovery is due. He interested himself greatly in the matter, and from first to last the mining company has most generously and meritoriously devoted its best resources to the recovery from the depths of the earth in the most perfect condition possible of these most remarkable scientific treasures. It has presented them all to the Royal Museum of Brussels. The actual removal of the specimens from their beds and their transmission to the surface, was performed under the immediate superintendence of Mr. Gustave Arnould, chief engineer, and of M. De Pauw, the latter being the superintendent of workshops at the Brussels Museum, who has since successfully mounted the enormous skeleton shown in the accompanying engraving. So immensely abundant were the remains found to be that Mr. De Pauw assumed for three years the habits of a miner, watching and controlling the removal of every specimen. He invented an ingenious method of hardening the bones *in situ* which prevented their crumbling when exposed to the air, which at first occurred. The bones exposed on the surfaces of the blocks excavated were covered with a coating of plaster for protection, and the masses thus formed were then raised to the surface, a distance of more than 1,000 feet and removed to cellars under the natural history galleries of the Brussels Museum, to be worked out at leisure. M. Dupont, Director of the Museum, confirmed Professor van Beneden's determination of the bones, and at the same time fixed the exact age of the deposits in which they occurred.

Some surprise has certainly been felt by naturalists that so very little information about the Bernissart skeletons has been published during the time which has elapsed since their discovery, but it must be borne in mind that it took three years even to get the rough material out of the pit, and that every mass of matrix containing bones requires a great deal of most careful labour to be expended on it before the bones in it are fully exposed for study. M. L. Dollo, a distinguished former pupil

[1] M. L. Dollo, "Première Note sur les Dinosauriens de Bernissart." *Bulletin du Musée Royal d'Hist. Nat. de Belgique*, T. i. 1882. Deuxième note, *Ibid.*, *l.c.* Troisième note, *Ibid.*, T. ii. 1883. "Note sur la présence chez les oiseaux du Troisième Trochanter des Dinosauriens et sur la fonction de celui-ci," *Ibid.*, *l.c.* "Les Iguanodons de Bernissart." *Bulletin Scientifique de pédagogique de Bruxelles*, April 1, 1883, No. 2, p. 25.

of Prof. Giard of Lisle, was appointed about two years ago as assistant naturalist to the Museum for the purpose of investigating the Iguanodons. He is full of enthusiasm, as an ardent naturalist such as he is well may be with the whole Bernissart material before him. He works incessantly at the subject, but he does not see prospect of publishing the complete monograph on the Iguanodons which he intends to issue sooner than five or six years hence. He will not of course venture to prepare the final monograph until he has the whole of the material concerned before him. He estimates the number of individuals represented by skeletons in the find as twenty-three, two of which belong to the species *I. Mantelli*, and twenty-one to *I. Bernissartensis*. Of these twenty-three, fifteen have as yet been chiselled out of the blocks ready for study, eight remain as yet to be worked at, and although four or five skilled artificers are constantly at work on the specimens progress is necessarily slow. The cellars full of the material present an astonishing appearance. One first enters an extensive, dimly lighted vault, the whole floor of which is covered with large blocks, many still in the condition in which they came from the mine, of all shapes, and lying in all sorts of positions, so closely placed that it is very difficult to get about amongst them to inspect them more closely. All contain huge bones, forming parts of the skeletons of the Iguanodons, often covered up by the protective plaster, but with here a hand, there a foot, elsewhere a range of vertebræ showing out. In an adjoining cellar is the workshop where various blocks are seen in the process of the removal of the matrix, whilst at one end, hung up to stout beams, are the results of the operation, a vast collection of all imaginable segments of the skeletons of Iguanodons suspended in the air, and suggesting the idea of joints of meat in the shop of some Brobdingnagian butcher.

As before mentioned, one of the skeletons of *Iguanodon Bernissartensis* has been restored and mounted by Mr. J. F. de Pauw. The specimen is almost entirely complete, only a few phalanges and one or two minor details having required to be reconstructed. It was not found possible to detach the bones from one another before mounting them. They are mostly mounted still joined to one another in sections by the matrix as removed from the mine. It was therefore impossible to give to the skeleton as natural a pose as might have been wished, and as M. de Pauw hopes to accomplish with some of the other specimens more favourably preserved; but taking all circumstances into consideration the present result of his work is a marvellous success, in which it needs a very trained eye indeed to detect anything amiss. The grand skeleton is set up in a huge glass chamber in the court of the Museum. As it stands in the natural attitude of progression of the animal on land, erect on its hind limbs, the top of its snout is at an elevation of a few inches over 14 feet from the ground, whilst from the tip of the tail outstretched behind to a point immediately beneath the tip of the snout the skeleton covers a horizontal space of floor about 23 feet in length.

As soon as M. Dollo set to work on the details of the structure of the Iguanodons, he very wisely determined to publish at once a series of preliminary notes giving the main results of his investigations. Four of these have now been issued as enumerated at the commencement of the present article, and from the third memoir is copied the figure of the entire skeleton, here reproduced somewhat reduced in size. From these notices is taken the information which follows.

M. Dollo's first care was to determine the species of the Iguanodons with which he has to deal. It will be remembered that his predecessor, M. G. A. Boulenger, who left Brussels to join the zoological staff of the British Museum, recognised among the remains a new species of Iguanodon, characterised by having six sacral vertebræ instead of five as in *I. Mantelli* and four in *I. Prestwichii*. Professor P. J. van Beneden, however, in the absence of further detailed information, held the opinion that the number of the sacral vertebræ could not be regarded as a specific character amongst Iguanodons, and that our knowledge then on the matter could only be expressed by stating that in the Dinosauria the sacral vertebræ vary in number from four to six. He did not therefore accept M. Boulenger's determination as valid, but regarded the whole of the specimens as belonging to *I. Mantelli*. M. Dollo, however, confirms M. Boulenger's conclusions; he finds that there are two forms of Iguanodons present, a large one and a small one, and the small one is certainly not the young of the large one. It is a remarkable fact that there are no young examples amongst the whole of the Bernissart Dinosaurians, as is shown by the facts that in all of them the cranial sutures are obliterated, and the sternal bones fully ossified, that the neurocentral sutures have disappeared in all the vertebræ and that the osseous tissue is equally dense in all the specimens. Traces of young have been most carefully sought for, but most unfortunately not a bone of a young animal has been found.

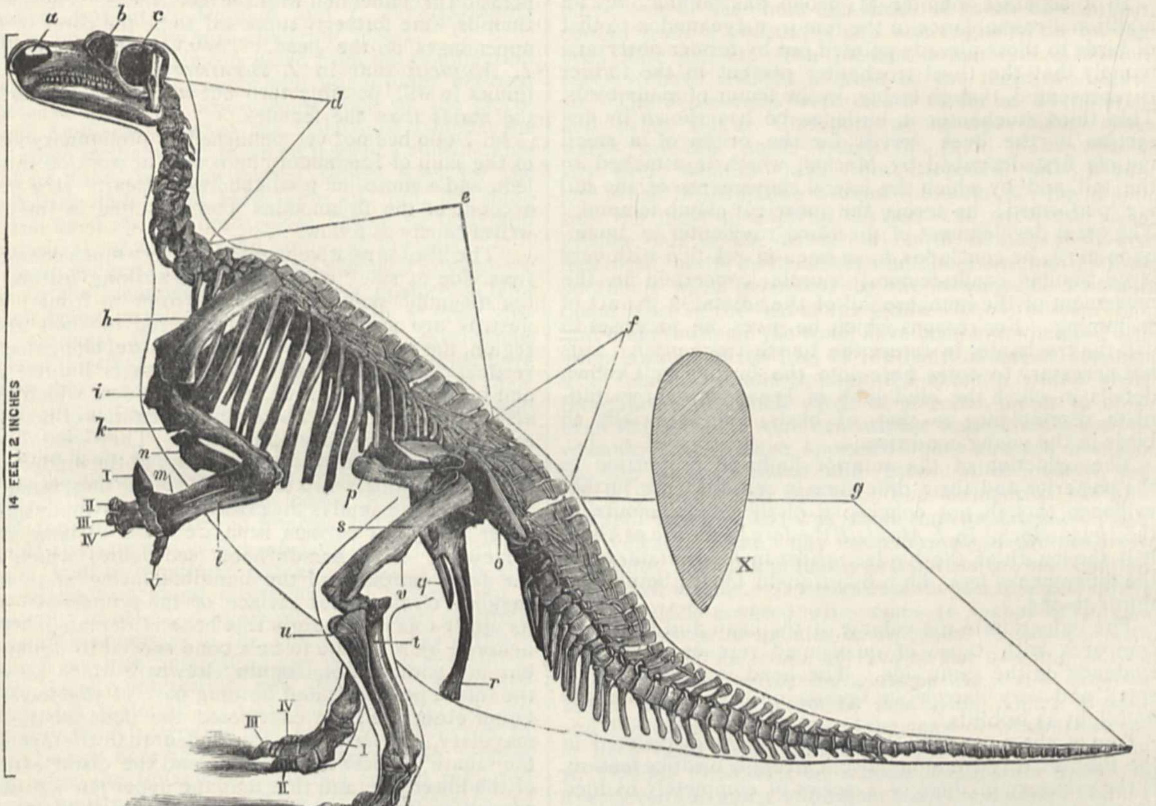
The differences between the two forms of Iguanodon are also not merely sexual. They are well marked and certainly of specific value. The number of sacral vertebræ seems to be quite constant in the several species of Iguanodon, and Prof. Marsh, who has had several hundred individuals of Dinosaurians through his hands, representing numerous genera and species, has made use, amongst other characters, of the number of sacral vertebræ present as generic distinctions. After carefully comparing full size drawings of the bones with those of the type specimen of *I. Mantelli* (Owen) in the British Museum, M. Dollo is quite convinced that his smaller form with five sacral vertebræ is identical with this. There are two other well identified species of Iguanodon known, namely, *I. Prestwichii* and *I. Seeleyi* of Hulke. The larger form from Bernissart cannot be *I. Prestwichii*, which has only four sacral vertebræ, but it is just possible that it may be identical with *I. Seeleyi*, since its large bones resemble closely those described by Mr. Hulke as characteristic of that species. There is, however, this remarkable discrepancy. Mr. Hulke discovered bony plates, forming as he believes a dermal armour over the tibia of *I. Seeleyi*. Now amongst the remains obtained from Bernissart are specimens of the integument of both *I. Mantelli* and the larger form. And these indicate that the skins of both these animals were either quite naked or at the most covered with epidermic scabs. M. Boulenger's name, *I. Bernissartensis*, is retained for the larger Bernissart form, for even if *I. Seeleyi* should prove in the end to be identical with it that name must fall through lack of priority. M. Dollo, taking into consideration the results as yet attained by him, characterises the order Ornithopoda of the Dinosauria to which the family Iguanodontidæ belongs as follows:—

Ornithopoda.—Foot digitigrade, ungulate, five functional digits on the hand and from three to four on the foot. Pubis projecting freely in front; post-pubis present. Vertebræ solid. Anterior limbs reduced, limb bones hollow. Premaxillaries toothless, at least in their distal region.

And the family Iguanodontida thus:—

A single row of teeth. Three functional digits on the foot. Two symmetrical sternal plates.

The pair of sternal plates were mistaken by Professor Marsh, who studied them in specimens in the British Museum, for clavicles; and the presence of clavicles was included by him in his definition of the family Iguanodontidæ; but in the Bernissart specimens the pair of bones are found in many specimens preserved in their natural relations, and are seen at once to be sternal, clavicles being altogether absent. A specimen is figured by M. Dollo, in which the two sternal bones with the coracoid and scapula of one side are seen *in situ*, all in their proper relative positions, and the humerus with its head still within the glenoid cavity. The circumstance that in the case of these Bernissart skeletons the bones are so largely preserved in their immediate natural relations adds immensely to their importance, for the position of every bone can be determined with certainty. The nearest approach to the peculiar structure of the sternum in Iguanodon appears to M. Dollo to be that existing in some young birds, especially in *Vanellus cristatus* as figured by Parker. Professor Marsh regarded the supposed presence of clavicles in Iguanodon as an important point in them of resemblance to birds; the point must now drop, but there are abundance of others in the Iguanodon skeleton in which the remarkable resemblances between the Ornithopoda and birds, pointed out by Professor Huxley with such surpassing sagacity more than twelve years ago, are borne out in a most remarkable manner. Professor Huxley had very imperfect material to guide him in his ideal restoration of the Iguanodon skeleton, and it is wonderful in how few matters of detail his results need correction now that one can stand at Brussels with a perfectly complete skeleton of Iguanodon towering over one's head, and test his results with as it were a complete solution of the puzzle at command. First of all there seems to be little doubt possible that the Iguanodons walked, as be pointed out, on their hind limbs erect like birds, in somewhat the attitude shown in the accompanying figure. Several



Iguanodon Bernissartensis, B'gr. At the Brussels Royal Museum of Natural History. Restored and mounted by M. L. F. De Pauw. Head, *a*, left nostril; *b*, left orbit; *c*. left temporal fossa. Vertebral column, *d*. cervical region; *e*, dorso-lumbar region; *f*, sacral region; *g*. caudal region; *h*, left scapula; *i*. left coracoid; *k*, left humerus; *l*, left ulna; *m*, left radius; *n*, sternum; *o*, left ilium; *p*, left pubis; *q*, left post-pubis; *r*, left ischium; *s*, left femur; *t*, left tibia; *u*, left fibula; *v*, third (fourth) trochanter. I, II, III, IV, V, digits. *X*, diagrammatic transverse section of the body between the fore and hind limbs.

different lines of evidence, as M. Dollo points out, tend to prove this. Firstly the remarkable resemblances between the structure of the pelvis and the posterior limbs of birds and the corresponding parts in the Iguanodons. The points of resemblance of the ilium and ischium, pointed out by Professor Huxley, are fully confirmed by the Bernissart specimens; with regard to the pubis Huxley only recognised a part in Iguanodon, the post-pubis; and Hulke was the first to give a nearly correct figure of the whole. The actual pubis is very large in Iguanodon, as will be seen in the figure, and projects forwards and outwards, forming an obtuse angle with the post-pubis. Mr. Hulke was therefore not quite correct in his conclusions as to its attitude, and there is no symphysis pubis present; the post-pubis is long and slender, and directed backwards alongside the ischium, as in birds, for a considerable distance beyond the ischial tuberosity. It is not incomplete, as supposed by Marsh (from the examination of drawings of Bernissart specimens in which it was imperfect). M. Dollo is inclined to follow Professor Marsh in identifying the Dinosaurian pubis with the pectineal process of the pelvis of birds, a conclusion which receives most interesting support in the valuable memoir lately published by Miss Alice Johnson of Cambridge on "The Development of the Pelvic Girdle in the Chick,"[1] in which it is shown that in the embryo fowl the cartilaginous representative of the pectineal process is at first much larger and more prominent in proportion to the dimensions of the pelvis than subsequently, and becomes gradually reduced as development proceeds. The peculiar form of the

[1] *Quarterly Journal o Microscopical Science*, July, 1883.

pelvis is no doubt directly connected with the muscular requirements concerned in the erect posture, originated probably in the Dinosauria, and transmitted to birds, in which it has been improved upon by the elimination, almost complete, of the original pubis through disuse.

M. Dollo takes the view that the post-pubis is a bone peculiar to Dinosaurians and birds. As he pointed out to me in the mounted specimen, probably a male, the aperture inclosed between the two ischiatic bones posteriorly is a very narrow slit through which, if the Iguanodon was by any chance oviparous, no egg of size proportionate to the animal could have passed, and it is, he thinks, just possible that in females he may find the ischia bowed so as to inclose a widely open passage above the symphysis.

In a separate memoir M. Dollo has pointed out an additional resemblance in the femur of Iguanodon to that of birds to those already pointed out by former observers, namely that the third trochanter present in the former is represented, though feebly, in the femur of many birds. This third trochanter in birds, as he has shown by dissection in the duck serves for the origin of a small muscle first described by Meckel, which is attached to the tail, and by which the lateral movements of the tail are performed; he terms the muscle "caudo-femoral." The great development of the third trochanter in Iguanodon must, he concludes, have been in relation with very large similar caudo-femoral muscle concerned in the movement of the immense tail of the animal in the act of swimming. For reasons which he gives, he proposes to call the trochanter in future the fourth trochanter. It is not necessary to enter here into the further well known details in which the hind limb of Iguanodon shows intimate resemblance to that of birds, and especially in birds in the young condition.

The reduction of the anterior limbs in proportion to the posterior and their difference in structure are further evidence, though not conclusive, of the erect posture of the Iguanodons. In *I. Mantelli* the fore limbs are of about half the length of the hinder, whilst in *I. Bernissartensis* the difference is less, the proportion in length being two-thirds to one.

The reduction in the volume of the head and thorax as compared with those of quadruped reptiles is further evidence on the same side. The head is comparatively small and very narrow in Iguanodon, the neck flexible and light as in birds.

One of the most remarkable new points discovered in the Bernissart Iguanodons, also a strongly birdlike feature, is the presence in them of a series of completely ossified ligaments stretching along the sides of the dorsal spines of the vertebræ (see figure), and binding the whole dorso-lumber region into a rigid mass as in birds, whilst the region of the neck and hinder region of the tail are free from any such ligaments. No traces of ossified tendons, such as occur in birds, have been found in connection with the limbs of the Iguanodons.

M. Dollo sums up as follows:—"In short the position of the occipital condyle, the length and the mobility of the neck, the rigid attachment of the dorso-lumbar region to the pelvis, the number of the sacral vertebræ, the massive nature of the tail, in fact, the entire structure of the vertebral column, agree in demonstrating that Iguanodon was biped in its gait. "But the most convincing proof of all, perhaps, lies in the evidence afforded by the footprints of Iguanodon in the Wealden strata. Of the eight Dinosauria known from the Wealden, Iguanodon is the only one which could leave tridactyle footprints. M. Dollo obtained a series of casts of the tridactyle Wealden footprints from Mr. Struchman from the neighbourhood of Hanover; choosing one of the right size, he introduced the three toes of the corresponding foot of one of the Bernissart *I. Mantelli*, and also the three metatarsals still united together, giving them a digitigrade position, the only one in which they would enter the impression, and an exact fit of the whole was the result. There can remain no doubt as to the complete correspondence of the two in the mind of any one who has seen the foot and impression thus fitted together. The hand of Iguanodon (see fig.) is pentadactyle, with the thumb transformed into a huge spur which must have been covered with a horny spine when the animal was living. If the animal had walked on all fours, it is impossible but that pentadactyle impressions should have occurred with the tridactyle, but such is not the case. Long series of the tridactyle prints are found without a trace of pentadactyle marks. The arrangement of the tridactyle tracks shows that Iguanodon walked on its hind feet, and did not spring like a kangaroo with the aid of its tail. This merely dragged lightly behind and has left no impression in connection with the foot tracks. The spur-like thumbs were formerly supposed to be the cores of horny appendages of the head. They are much smaller in *I. Mantelli* than in *I. Bernissartensis*, and M. Dollo thinks it will possibly turn out that they are larger in the males than the females.

M. Dollo has not yet published a preliminary account of the skull of Iguanodon, he is now at work on this subject, and a notice of it will shortly appear. In a popular account of the Iguanodons (the last cited in the list) he writes briefly as follows:—

"The head is relatively small, and very much compressed from side to side" (this is a most striking feature when the mounted skeleton is viewed from in front). "The nostrils are spacious and chambered in their anterior region, the orbits are of moderate size, elongated in a vertical direction. The temporal fossa is limited above and below by a bony arch, an arrangement which occurs else only in Hatteria. The distal extremities of both upper and lower jaws are devoid of teeth. They were no doubt during life covered by a horny beak; in the hinder part of the jaws are ninety-two teeth." One of the most remarkable features of the skull is the presence at the symphysis of the lower jaw of a curious separate mass of bone shaped somewhat like a horse's hoof (see figure) which forms the distal extremity of the mandible, fitting in to an excavation on the upper surface of the symphysis. Along its upper rounded margin this bone is dentated. This is believed by M. Dollo to be a bone special to Iguanodon, but not without homologues elsewhere which he will in the future point out, and forming part of the lower jaw. Other observers have considered the bone as the intermaxillary, and have thus concluded that the opening of the mouth lay between the bone and the distal extremity of the lower jaw, and that thus the upper jaw was shaped something like a parrot's beak, shutting into a depression at the symphysis of the lower. A slight inspection of the complete cranium and lower jaw cleared completely of the matrix, which M. Dollo has before him, seems sufficient to carry conviction that his view as to the position of the bone and mouth aperture is the correct one.

The roof of the mouth of Iguanodon in its anterior region is moulded into rounded, ridge-like prominences, which as M. Dollo pointed out have some curious resemblances in form to those occurring in the corresponding position in a duck. The animal was an inhabitant of marshes—as far as yet known apparently of freshwater marshes only—and fed probably largely on ferns, abundance of which were found with the Bernissart specimen. No results of importance as to this question have as yet been obtained from the examination of their coprolites.

The outline of the body shown in the present figure was roughly sketched in by M. Dollo on request, in order to give an idea of his present conjecture as to the probable shape of the living Iguanodon. It is most distinctly to be regarded as merely tentative he reserves any expression of final opinion till the whole material has passed through his hands. On examining the outline, it will be seen that the

Iguanodon probably was shaped, excepting for the long huge tail, which, as Professor Owen long ago pointed out, is shaped like that of a crocodile, being a powerful swimming organ, somewhat like a duck. In accordance with the birdlike modification of the pelvis a large mass of the viscera were post-acetabular in position, as in a greater degree in birds, thus tending to aid the long tail to erect the head and fore part of the body by depressing the hinder region of the spinal column on the acetabular axis as a fulcrum. Like the head the body was very much compressed laterally, so that its transverse section was somewhat as represented in the diagram, *X*. The neck of the Iguanodon was comparatively slender, and is found to be capable of very free movements. The necks of the fossilised specimens are found to be twisted without dislocation into most varied attitudes. The skin, as already mentioned, was in *I. Mantelli* and *I. Bernissartensis* smooth or covered only with epidermic scales.

Several observers have concluded from the examination of the footprints that a slight web was present between the toes. Judging from observations made on the crocodile and Amblyrhynchus of the Galapagos Islands, the animal when in the water, in which it spent a considerable part of its time, when swimming slowly, used for the purpose both its fore and hind limbs and tail, but when going fast fixed its fore limbs close beside its body and drove itself along with its hind limbs and tail only.

M. Dollo suggests that one of the principal advantages gained by the Iguanodons by their erect posture on land was their being enabled thereby to discern at great distances amongst the vegetation the large carnivorous animals of their age to which as herbivora they must have formed a prey. Possibly when attacked they seized their aggressor in their short arms and made use of their thumb spurs as daggers.

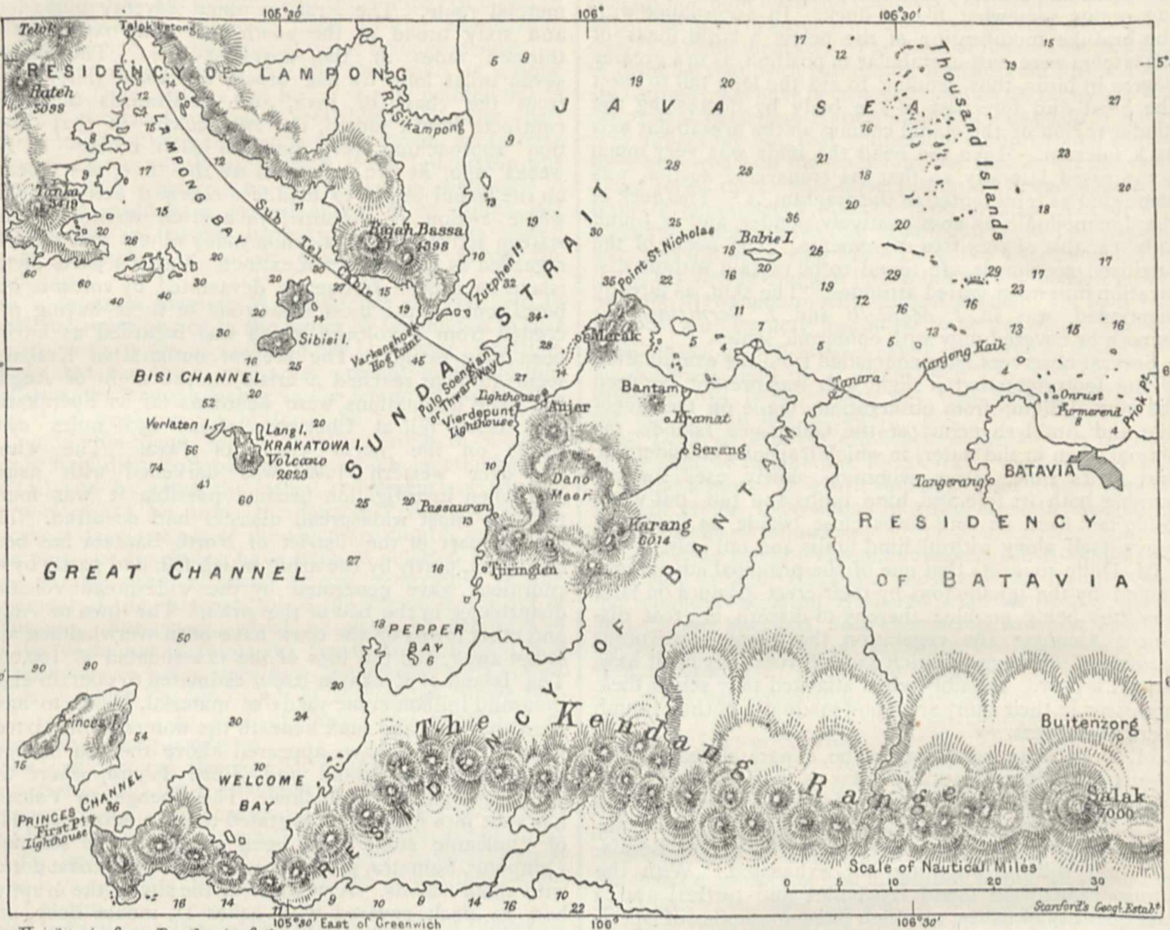
M. Dollo is in every way to be congratulated on the results of his investigations, so far as they have yet gone, and his final monograph may be looked forward to as a work of the utmost value and interest, but with the completion of the Iguanodons the working up of the Bernissart find will be anything but exhausted. With the Dinosaurians were found crocodiles and turtles, and a vast quantity of fishes, of which piles upon piles of specimens await his energies in the future. He has already discovered two most interesting new genera of crocodiles, and an equally interesting new genus of Chelonians amongst this material. Every naturalist who has an opportunity should certainly find his way to Brussels to see the skeleton here figured. It is proposed in process of time, when the Iguanodon skeletons are all prepared from the matrix and mounted as far as necessary, to build a new museum of natural history at Brussels in the Parc Leopold, formerly the zoological garden, and in this museum to construct a special gallery to contain all the Bernissart fossils, a rotunda of twenty-five metres in diameter.

H. N. MOSELEY

THE JAVA UPHEAVAL

THE details which have reached us during the past week of the terrible seismical manifestation at Java prove it to be one of the most disastrous on record; probably, moreover, it is the greatest phenomenon in physical geography which has occurred during at least the historical period, in the same space of time. The accompanying sketch-map will afford some idea of the extent and nature of the change which has taken place, and the character of the sea bed and the land in the region affected. Next week we shall attempt to show what light science can shed on the occurrence; meantime we shall content ourselves with gathering together the facts that have come to hand.

The volcanic Island of Krakatoa lies about the middle of the north part of the passage between Java and Sumatra, a passage which has formed an important commercial route. The strait is about seventy miles long and sixty broad at the south-west end, narrowing to thirteen miles at the nort-east end. The island, seven miles long by five broad, lay about thirty miles from the coast of Java, and northwards the strait contracts like a funnel, the two coasts in that direction approaching very near to each other. A few weeks ago, as we intimated at the time, the volcano on the island began to manifest renewed activity. The whole region is volcanic, Java itself having at least sixteen active volcanoes, while many others can only be regarded as quiescent, not extinct. Various parts of the island have been frequently devastated by volcanic outburts, one of the most disastrous of these having proceeded from a volcano which was regarded as having been long extinct. The present outburst in Krakatoa seems to have reached a crisis on the night of August 26. The detonations were heard as far as Soerakarta, and ashes fell at Cheribon, about 250 miles eastwards on the north coast of Java. The whole sky over western Java was darkened with ashes, and when investigation became possible it was found that the most widespread disaster had occurred. The greater part of the district of North Bantam has been destroyed, partly by the ashes which fell, and partly by an enormous wave generated by the widespread volcanic disturbance in the bed of the strait. The town of Anjer and other towns on the coast have been overwhelmed and swept away, and the loss of life is estimated at 100,000 The Island of Krakatoa itself, estimated to contain eight thousand million cubic yards of material, seems to have been shattered and sunk beneath the waters, while sixteen volcanic craters have appeared above the sea between the site of that island and Sibisi Island, where the sea is comparatively shallow. The Soengepan Volcano has split into five, and it is stated that an extensive plain of "volcanic stone" has been formed in the sea near Lampong, Sumatra, probably at a part of the coast dotted with small islands. A vessel near the site of the eruption had its deck covered with ashes 18 inches deep, and passed masses of pumice-stone 7 feet in depth. The wave reached the coast of Java on the morning of the 27th, and, 30 metres high, swept the coast between Merak and Tjiringin, totally destroying Anjer, Merak, and Tjiringin. Five miles of the coast of Sumatra seem to have been swept by the wave, and many lives lost. At Taujong Priok, fifty-eight miles distant from Krakatoa, a sea seven feet and a half higher than the ordinary highest level suddenly rushed in and overwhelmed the place. Immediately afterwards it as suddenly sank ten feet and a half below the high-water mark, the effect being most destructive. We shall probably hear more of this wave, as doubtless it was a branch of it which made its way across the Pacific, and that with such rapidity that on the 27th it reached San Francisco Harbour, and continued to come in at intervals of twenty minutes, rising to a height of one foot for several days. The great wave generated on May 10, 1877, by the earthquake at Iquique, on the coast of Peru, spread over the Pacific as far north as the Sandwich Islands, and south to New Zealand and Australia; while that at Arica, on August 13-14, 1869, extended right across the Pacific to Yokohama (NATURE, vol. i. p. 54). It is misleading to speak of such waves as tidal; they are evidently due to powerful, extensive, and sudden disturbances of the ocean bed, and are frequently felt in the Pacific when no earthquake has been experienced anywhere, though doubtless due to commotions somewhere in the depths of ocean. So far these are all the facts that are known in connection with this last stupendous outburst of volcanic energy. It has altered the entire physical geography of the region and the condition of the ocean bed. The existing charts of the strait with their careful soundings are useless for purposes of navigation, and when quiescence is restored a new series of soundings will be necessary. Doubtless the results of



Heights in feet, Depths in fathoms.

the outbreak will receive minute attention at the hands of the Dutch Government, and when all the data are collected they will form valuable material for the study of the physical geographer.

NOTES

THE next meeting of the American Association for the Advancement of Science will be held in Philadelphia, probably during the first week in September, 1884. At the session in Minneapolis the following persons were chosen as officers for the Philadelphia meeting:—President, Dr. J. P. Leslie, of Philadelphia; Vice-Presidents: Section A (Mathematics and Astronomy), Prof. H. T. Eddy, of Cincinnati; B (Physics), Prof. John Trowbridge, of Cambridge; C (Chemistry), Prof. J. W. Langley, of Ann Arbour; D (Mechanical Science), Prof. R. H. Thurston, of Hoboken; E (Geology and Geography), Prof. N. H. Winchell, of Minneapolis; F (Biology), Prof. E. D. Cope, of Philadelphia; G (Histology and Microscopy), Prof. T. G. Wormley, of Philadelphia; H (Anthropology), Prof. E. S. Morse, of Salem; I (Economic Science and Statistics), Hon. John Eaton, of Washington; permanent secretary, Mr. F. W. Putnam, of Cambridge; general secretary, Dr. Alfred Springer, of Cincinnati; assistant general secretary, Prof. E. S. Holden, of Madison.

M. JANSSEN, who has returned from Caroline Island, was present at the meeting of the Academy of Sciences of September 3. He read the first part of the documents he brings with him, viz. the reports drawn up by Palisa, Tacchini, and himself, while Trouvelot read his own account. The reading was long and interesting, and will be continued next week. M. Janssen stated that he believed the region around the sun was full of material almost corpuscular, and reflecting the light from the sun. He was received enthusiastically, and M. Blanchard, the president, spoke in praise of his merits and efforts for the promotion of science. M. Janssen returned thanks, acknowledging that great efforts must be made by him to be worthy of such a reception.

WE regret to announce the death of Mr. Cromwell Fleetwood Varley, F.R.S., M.I.C.E., &c., on Sunday night last, at his residence at Bexley Heath, Kent. He was born in Kentish Town, April 6, 1828. He devoted himself to the engineering branch of telegraphy, and devised a method of locating distant faults in land wires which attracted the special attention of engineers and electricians. Distinguishing himself by one discovery after another, Mr. Varley finally became chief engineer and electrician to the Electric and International Telegraph Company, and held this office until the taking over of the telegraphs by the Government. His inventions were very numerous. Prominent among his early inventions was an apparatus for transmitting electrical signals, which so much increased the sensitiveness and trustworthiness of the relay that it became practicable for the first time to work from London to Edinburgh direct—a feat impossible in the conditions of insulation previously existing. Mr. Varley was associated with Robert Stephenson, Sir William Fairbairn, and others in devising the first Atlantic cable which may be said to have achieved success. By means of a working model apparatus he demonstrated approximately the speed of electricity when on its travels.

MR. V. T. CHAMBERS, an entomologist well known for his studies on Tineina, died at his residence in Covington, Ky., U.S., on August 7.

DURING the past year, we learn from *Science*, original investigations in the following subjects, among others, have been carried on in the physical laboratory of Johns Hopkins University under the direction of Prof. Rowland and Dr. Hastings: on the photography of the spectrum by means of the concave grating; on the determination of the B. A. unit of electrical resistance in absolute measure; the determination of the specific resistance of mercury; the variation of the specific heat of water with the temperature; the relative wave-lengths of the lines of the spectrum by means of the concave grating; the effect of difference of phase in the harmonics on the timbre of sound; and on the variation of the magnetic permeability of nickel by change of temperature.

MR. THOMAS PLANT, the well-known meteorologist of Birmingham, died suddenly last week. Mr. Plant was sixty-four years of age, and was a native of Lowmoor, Yorkshire. From early manhood he had a passion for the study of the wind and the weather. This passion took a very systematic shape in the compilation of regular records of rainfall, windage, and temperature; and, to the student of meteorology, these records, the result of Mr. Plant's life-long study, will doubtless prove valuable. They are said to be complete for upwards of forty-six years. In 1862 he read a paper before the British Association at Cambridge on "Osler's Anemometer at the Birmingham and Midland Institute," and described the working of the instrument by means of lithographed drawings which he had himself prepared. Three years later he read another paper before the same Association at Birmingham on the "Anomalies of our Climate." A paper on the "Health of the Borough of Birmingham" was read in 1868 by Mr. Plant before the Social Science Congress at Birmingham. He frequently lectured on meteorology, and was a constant contributor to the local press on the same subject.

THE Earl of Crawford and Balcarres has been elected an honorary member of the Berlin Academy of Sciences.

DR. HICKS is reported to have made an interesting discovery in a cave at the back of the Ffynnon Beuno, Flintshire. The cave is a waterworn cave in the limestone rock, similar, though on a smaller scale, to the celebrated Cefn bone caves on the other side of the Vale. Dr. Hicks, after a general inspection of the interior, determined to examine beneath the floor of the cave at the entrance. The removal of a few inches of surface debris disclosed a virgin floor of stalagmite, so well known to cave explorers. Below this were found pieces of bone belonging most evidently to the mammoth or rhinoceros. One piece was embedded in the stalagmite floor. The largest piece—nearly six inches by four—must have formed part of a bone some eighteen inches in circumference. Below was another floor of stalagmite covering a quantity of drift gravel which rested on the bottom of the cave.

MR. FLOYD DELAFIELD of Noroton, Conn., has brought out new dynamo, the novel feature being that the armature is a tube of copper. One of the field magnets is terminated at either end by a tubular pole piece; within this pole piece rotates a tubular armature. On either side of the central magnet runs an auxiliary magnet, which is attached to the axle of the armature. Thus the tubular armature has one pole as its axle, whilst the other pole completely surrounds it. The current is drawn off at either end of the cylinder by brushes. The machine is so arranged that one armature can be used to excite the magnets, whilst the other is used for the main circuit, which gives a good current for plating purposes, or, when required for incandescent lighting, the magnets may be excited by a small high tension dynamo, and then the two armatures may be used for main circuit purposes.

SCIENTIFIC authorities are not at rest with giving Philipp Reiss the merit of inventing the telephone. The latest claimant put forth is Charles Bourseul, a Frenchman, who is said to have invented the telephone in 1854. This invention is said to have been communicated in 1854 to the French Academy, and to have appeared in the *Didaskalia*, a supplementary paper to the *Frankfurter Journal*, for September 28th, 1854. M. le Comte du Moncel is advocating the claims of Bourseul.

M. BERTHELOT has been investigating the speed of gaseous explosions. For this purpose he used an iron tube 16 inches long and ⅕ inch bore. The gases were exploded by a spark, and the explosion registered at the centre and end of the tube. The gases he used were carbonic oxide and oxygen, their rate of explosion he observed to be 2500 metres per second. This is a far greater speed than was expected.

IN the experiments which have been made at Grenoble for the transmission of electric force from a distance of 14 kilometres, the wire was of silicated bronze 2 mm. diameter, instead of iron as on former occasions. According to *L'Électricité* the results have been very poor, a motive power of 45 horses having been required to convey 7½ horse-power.

THE observatory at Montmartre, Paris, which belongs to Dr. Gruby, has been reorganised, and M. Cassé has been appointed director. It is a private establishment devoted to meteorology, the results being published in a number of the Paris daily papers. It is built in the vicinity of the Moulin de la Golette, and is now, except the latter establishment, the most elevated point in Paris.

MM. TISSANDIER have completed the construction of their apparatus for preparing hydrogen by a continuous process for filling large balloons. It was tried with a balloon of 300 cubic metres, which conveyed the two brothers to some distance from Paris. This system is a simplification of the apparatus which was used by M. Giffard in his large captive balloon. It will be used for filling the electric balloon now being built by MM. Tissandier.

DR. LIEBSCHER of Jena University sends us some remarks in reply to Mr. B. Kotô's article on "Agriculture in Japan" in NATURE, vol. xxviii. p. 231. With regard to Mr. Kotô's statement that in describing the climate of Japan Dr. Liebscher entirely disregarded the fact that the empire "is surrounded on all sides by a large body of water," he refers to his map of Japan and to p. 8 of his work, where he says: "The summer or south-west monsoon, which on its way from the tropics sweeps over the warm Pacific and is saturated with steam . . ." With regard to what Mr. Kotô says concerning Lake Hakone, Dr. Liebscher maintains that the Hakone Pass is situated not at the foot of the Fuji San, but at a distance of thirty-three miles from its foot, or about fifty miles from the summit, on quite a differen range of mountains. Moreover, Dr. Liebscher points out that Fuji San is not an active volcano "which sends out an enormous quantity of scoriæ" like Vesuvius; nobody, Dr. Liebscher states, has ever seen any trace of scoria or smoke about it since the year 1707. As to Mr. Kotô's statement that "the climate of Japan is not so ineffective as Dr. Liebscher has depicted in his work; in reality it is far more conducive to fertility than that of Germany," Dr. Liebscher maintains that in his book the very contrary of what Mr. Kotô implies will be found, indicating especially the conclusion of what he says on the natural foundation of agriculture in Japan (p. 58). There it is stated that, "owing to the climate rather than to the rich soil, an amazingly large number of people can live in Japan on the produce of one field." Similar misunderstanding, Dr. Liebscher writes, has been shown by Mr. Kotô in his remarks on the geology and the soil of Japan, in his opinion concerning the Japanese land-tax system, in what he says on the religion of his countrymen, and in denying the existence of polygamy among them.

A SHARP shock of earthquake was felt at 8 o'clock on September 2 at Frascati, on the Alban Hills, twelve miles from Rome. The movement was undulatory and lasted several seconds, but without causing any damage. The instruments in the observatory of the Roman College noted at the same hour a sensible undulatory movement, in the direction of from north-east to south-west. The earthquake was felt simultaneously at Albano, Ariccia, Genzano, Rocca di Papa, Monte Porzio, and other towns on the Alban Hills. At Rocca di Papa a slight shock also occurred a few days ago. New York papers report an earthquake at Pachuca, in Mexico, by which twenty persons lost their lives. A shock was felt at Fjösanger in Bergens Stift, Norway, on August 17, at 10 p.m.

A CORRESPONDENT points out that an account of Prof. Edlund's theory of the connection between thunderstorms and auroræ will be found in *Petermann's Mittheilungen* for 1879, p. 76.

IT is stated that an important oyster bed has been discovered in the Medway. It is estimated to contain over a quarter of a million of young oysters. The Medway was formerly a famous oyster fishery, and it is hoped from this discovery that it is about to become so again.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus* ♀) from India, presented by Miss Garwood; a Golden Eagle (*Aquila chrysaetos*) from Scotland, presented by Mr. Bertram B. Hagen; two Long-eared Owls (*Asio otus*), British, presented by Mr. Percy F. Fordham; a Mocking Bird (*Mimus polyglottus*) from North America, presented by Mr. A. Townsend; two Marsh Harriers (*Circus æruginosus*), European, presented by Lieut.-Col. E. Delme Radcliffe; two Barbary Apes (*Macacus inuus*) from North Africa, deposited; a Silvery Gibbon (*Hylobates leuciscus* ♂) from Java, an Indian Muntjac (*Cervulus muntjac*) from India, four Passerine Doves (*Chamæpelia passerina*) from America, a Malabar Parrakeet (*Palæornis columboides*) from Southern India, a Boatbill (*Cancroma cochlearia*), an Anaconda (*Eunectes murinus*) from South America, a Sharp-nosed Crocodile (*Crocodilus cataphractus*) from Central America, purchased; two Ostriches (*Struthio camelus* ♂ ♀) from Africa, received on approval.

OUR ASTRONOMICAL COLUMN

TEMPEL'S COMET, 1873 II.—M. Schulhof of Paris has published elements and an ephemeris of this comet for the approaching return to perihelion. The following is the predicted orbit:—

Epoch, 1883 *October* 20·0 *M.T. at Berlin*

	°	′	″	
Mean anomaly...	354	5	43·5	
Longitude of perihelion	306	7	4·4	M.Eq. 1880·0.
,, ascending node...	121	2	8·5	
Inclination	12	45	17·1	
Angle of eccentricity	33	32	29·5	
Mean daily sidereal motion ...	681″·1068			
Log. semi-axis major	0·477861			

From these elements we find the time of perihelion passage November 20·17155 G.M.T., and the period of revolution 1902·77 days. M. Schulhof's ephemeris so far published extends from August 28 to November 8; during which period the comet is slowly receding from the earth. We extract a few positions:—

At Berlin Midnight

	R.A. h. m. s.	N.P.D. ° ′	Log. distance from Earth.	Sun.
Sept. 21	15 19 50	101 51·0	0·2732	0·1739
23	15 25 3	102 24·8		
25	15 30 22	102 58·3	0·2739	0·1687
27	15 35 47	103 31·8		
29	15 41 17	104 4·6	0·2744	0·1637
Oct. 1	15 46 53	104 37·2		
3	15 52 35	105 9·4	0·2750	0·1589
5	15 58 22	105 41·2		

Unless the comet is observed at the present return, observations will hardly be possible before the spring of 1894.

THE GREAT COMET OF 1882.—Dr. B. A. Gould, director of the Observatory at Cordova, informs us that this comet was last seen there with the naked eye on March 7, when Mr. Thome found it already very faint in the telescope, and no nuclear condensation perceptible. His last observation was on June 1, but it was not possible to use the filar micrometer, and he had to depend upon the circles of the equatorial. Had it not been less than an hour high at nightfall, he thinks he could have observed it for a month longer. The Cordova refractor is of ten inches aperture. On March 7 the distance of the comet from the earth was 3·07.

THE MINOR PLANET, NO. 234.—Prof. Krueger communicates in a circular two observations of the small planet last discovered, telegraphed by Mr. O. C. Wendell, from which it appears that the daily motion in N.P.D. is as much as 21′, or, reducing the places for August 12 and 24 to longitude and latitude, we find a change of latitude of 3° 13′ in the interval, the descending node being passed on August 23. This seems to point to a considerable inclination of the orbit. The Harvard positions are as follow:—

G.M.T.	R.A. ° ′ ″	N.P.D. ° ′ ″
1883, August 21·7470	318 57 43	105 24 9
24·7274	318 36 0	106 20 34

Of the large number of these bodies now known, *Pallas*, the second in order of discovery, still retains the greatest orbital inclination, 34° 44′ at present.

GEOGRAPHICAL NOTES

IN the interests of anthropology, Dr. A. B. Meyer, curator of the Dresden Ethnological Museum, has just issued some practical suggestions addressed to the officers of the German Imperial Navy visiting the Indo-Pacific waters. The chief object of this "Denkschrift" is the completion of the Dresden ethnographic collection, whose desiderata are mentioned in detail, and special instructions are given as regards the Chinese seaboard, the South Sea Islands, the north-west coast of America, Madagascar, the Eastern Archipelago, and in general such places as lie on the ordinary route of the German Navy. Here is still to be gathered a rich harvest of materials illustrating the usages, traditions, religions, and social culture, especially of the Polynesian, Papuan, Indo-Chinese, Malay, and North American races. Many objects may thus be brought together calculated to throw light on such important historic and religious movements as the spread of Buddhism from India throughout East Asia, and the influence of Hinduism in past times on the local cultures in Further India and Malaysia. Amongst the miscellaneous wants particular mention is made of fishing gear, boat models, and musical instruments from Formosa; blowpipes, krisses, shields, and brass armour from the Sulu Archipelago and Palawan; nets, harpoons, magic wands from Corea and Yesso; wood carvings and idols from New Guinea and New Britain; clubs, spears, stone hatchets, tattoo designs, figures of men and animals in wood or stone from Melanesia; objects of fetish worship from Micronesia; jade ornaments from Polynesia; carved wooden masks of men and animals, clay or stone vessels, tobacco pipes and nephrite objects from the north-west coast of America; talismans, idols, house utensils, and weapons from Madagascar; wicker-work, burnt clay figures of evil spirits, woven materials from Ceylon; specimens of figure or picture writings on palm leaves from the Nicobar Islands. Some of these hints may be found useful by English travellers and others willing to promote anthropological work in the Indo-Pacific regions.

MR. J. T. LAST contributes a paper of unusual interest to the September number of the *Proceedings of the Royal Geographical Society;* he describes a visit to the little known Masai country, the region through which Mr. Joseph Thomson had to pass. Mr. Thomson himself sends a long letter giving an account of the first part of his journey and his forced return to the coast. He was to set out again on July 8, *viâ* the north side of Kilimanjaro for Mosera, far on the way to the south shore of Victoria Nyanza. Meantime it is announced that Dr. Fischer, the German explorer who preceded Mr. Thomson on the same route and excited the hostility of the people, has returned to the coast. It seems impossible that he can have reached his proposed goal, and probably, like Mr. Thomson, has been compelled to turn back.

ON August 28 the gunboat *Urd* arrived at Tromsö with the members of the Swedish Circumpolar Expedition on board, who have wintered at Spitzbergen. During the *Urd's* voyage to the island she encountered a fog off Beeren Island, which continued to Spitzbergen, but only a small quantity of ice was seen, viz. at South Cape. The vessel arrived at Cape Thordsten on August 10. The observations were continued until 12 midnight on August 23, in order to have a full year's magnetical observations. On the 24th the houses were cleared, the windows nailed up, and the doors locked, and on the 25th the *Urd* steamed out of the Icefjord. In Green Harbour the post was taken on board from the Norwegian hunters, and steering west of the Beeren Island the coast of Norway was sighted on the 28th. No ice was encountered. The ship is expected in Gothenburg on the 6th inst.

WE are glad to learn that both the Dutch International Polar Expedition and the Danish Expedition under Lieut. Hovgaard are safe. A Reuter's telegram from Vardoe says:—The steamer *Obi*, belonging to M. Sibiriakoff, has arrived here. The captain picked up on the 25th ult., near Waigatz, the members of the Dutch Polar Expedition steamer *Varna*, which foundered on July 24 in lat. 71, long. 63. The captain further states that the Danish exploring vessel *Dijmphna* had been ice-bound in that region throughout the winter. All was, however, well on board, and the captain of the *Dijmphna* felt confident of getting into open water. The crew of the *Varna*, which left the *Dijmphna* on the 1st ult., will be brought to Hammerfest by the steamer *Noraenskjöld*. The *Varna* had on board the Dutch section of the International Polar Expedition. She left Amsterdam on July 5, 1882, bound for Dickson's Harbour, at the mouth of the Yenisei. The Danish Polar steamer *Dijmphna*, under command of Lieut. Hovgaard, left Copenhagen on July 18, 1882, also bound for the Arctic Seas, and the *Nordenskjöld*, Swedish exploring steamer, left Tromsö about July 3, 1882, bound for Novaya Zemlya. The *Louise* is a trading steamer which left Bremen of June 27 last, and Hammerfest on July 17, bound for the Yenesei.

ELECTRICAL UNITS

THE following is the Report (omitting the appendix) to the Lords of the Committee of Council on Education by the Committee of Advice[1] with respect to the International Congress for the Determination of Electrical Units to be held at Paris in October, 1883.

The first International Electrical Exhibition was held in Paris during the months of August, September, and October, 1881, under the auspices of the French Government, who supplemented it by calling together a Congress of the leading scientific and practical electricians of all countries. England was represented by the following official delegates:—

The Ambassador to France, Sir F. Abel, C.B., F.R.S., Prof. W. G. Adams, F.R.S., Lieut. R. W. Anstruther, R.E., Prof. W. E. Ayrton, F.R.S., Prof. W. F. Barrett, Sir Charles Bright, M.I.C.E., Commissioner at the International Electrical Exhibition, Paris, Prof. Chrystal, F.R.S., Mr. Latimer Clark, M.I.C.E., Prof. R. B. Clifton, F.R.S., the Earl of Crawford and Balcarres, F.R.S., Commissioner-General at the International Electrical Exhibition, Paris, Mr. W. Crookes, F.R.S., Mr. Warren de la Rue, D.C.L., F.R.S., Prof. J. Dewar, F.R.S., Prof. J. D. Everett, F.R.S., Prof. G. Fitzgerald, F.R.S., Prof. G. Carey Foster, F.R.S., Dr. J. H. Gladstone, F.R.S., Mr. J. E. H. Gordon, Mr. E. Graves, Engineer-in-Chief, Postal Telegraphs, Dr. J. Hopkinson, F.R.S., Prof. Hughes, F.R.S., Commissioner at the International Electrical Exhibition, Paris, Prof. Fleeming Jenkin, F.R.S., Mr. J. F. Moulton, F.R.S., Mr. W. H. Preece, F.R.S., Lord Rayleigh, F.R.S., Sir W. Siemens, D.C.L., LL.D., F.R.S., Prof. H. Smith, F.R.S., Mr. Willoughby Smith, Mr. C. E. Spagnoletti, Mr. W. Spottiswoode, D.C.L., LL.D., P.R.S., Mr. A. Stroh, Prof. P. G. Tait, F.R.S.E., Sir William Thomson, LL.D., F.R.S., Prof. J. Tyndall, D.C.L., LL.D., F.R.S., Mr. Cromwell Varley, F.R.S., Mr. C. V. Walker, Lieut.-Col. Webber, R.E., Commissioner at the International Electrical Exhibition, Paris.

Many very important electrical questions were fully discussed, and a universal and international system of units for expressing the results of electrical measurements and observations was determined upon. All parts of the globe being now connected together by a great network of telegraphy, constructed and maintained by every civilised nation, it has become a matter of great commercial as well as scientific importance that uniformity should be introduced in modes of working, measuring, and observing throughout the world. The Paris Congress of 1881 has laid the foundation of such a desirable result.

The Congress of 1881 referred certain questions to a second Conference, held in the month of October, 1882.

This second Conference was divided into three Sections (*i.e.* Commissions); the first dealing with "Electrical Units"; the second with "Earth Currents and Lightning Protectors"; and the third with the question of "A Standard of Light."

Lord Rayleigh, Sir William Thomson, Prof. Carey Foster, Prof. Fleeming Jenkin, and Dr. Hopkinson were nominated as delegates from England, but Sir William Thomson was the only one present, and he devoted his time principally to the first question.

FIRST COMMISSION.—The Electrical Congress of 1881 adopted, as the fundamental system of units for scientific purposes, a system founded upon the employment of the Centimetre, the Gramme, and the Second as units of length, mass, and time respectively, and hence known as the C.G.S. system of units. The Congress also defined, and adopted a nomenclature for, a system of electrical standards of such magnitudes as to be as far as possible generally convenient for practical use, each practical standard being a decimal multiple or submultiple of the corresponding C.G.S. unit.[1] Of these standards, those to which reference is most frequently required are the following, namely:—

The Ohm, defined as one thousand million C.G.S. units of electric resistance.

The Volt, defined as one hundred million C.G.S. units of electromotive force.

The Ampere, defined as one-tenth of a C.G.S. unit of electric current, being the current maintained by an electromotive force of one volt in a conductor of resistance one ohm.

It was further agreed by the Congress that, with a view especially to facility of reproduction, the resistance denoted by the ohm should be stated as being the resistance of a column of mercury at the temperature of melting ice, of one square millimetre in cross-section, and of a length to be ascertained by experiment.

Accordingly the principal question referred to the first section of the Conference of 1882 was the determination of the length of a column of mercury, of the above-mentioned cross-section and temperature, which had an electrical resistance of one thousand million C.G.S. units. In reference to this question the Conference adopted the following resolutions, namely:—

First Resolution.—"The Conference considers that the determinations hitherto made do not present the necessary degree of concordance for fixing the numerical value of the ohm in terms of a column of mercury.

[1] The President of the Royal Society, the late Mr. W. Spottiswoode, was a member of the Committee, but his illness and death prevented his taking part in its proceedings.

[1] It is satisfactory to your Committee to be able to say that the C.G.S. system of units was widely used among English physicists before its adoption by the Electrical Congress in 1881, it having been recommended by a Committee of the British Association in 1875; and also that the system of practical standards adopted by the Congress is nearly identical with that previously in use in England and first suggested in a paper by Mr Latimer Clark and Sir Charles Bright read before the British Association in Manchester in 1861.

"It is therefore of opinion that it is necessary to continue investigations in relation to this question."

Second Resolution.—"The Conference expresses the wish that the French Government should take the necessary measures for placing one or several standard resistances at the disposal of those men of science who are devoting themselves to the investigation of absolute units, in order to facilitate the comparison of results."

Third Resolution.—"The Conference is of opinion that so soon as the results of the various investigations shall be so far accordant that it is possible to guarantee an accuracy of one part in a thousand, it will be proper to accept this degree of approximation for the purpose of fixing the value of the practical standard of resistance."

Fourth Resolution.—"The Conference expresses the wish that the French Government may be pleased to communicate to the Governments represented at the Conference a desire to the effect that each of them, in view of the importance and urgency of arriving at a practical solution, should take the necessary steps to encourage investigations, on the part of its own nation, in relation to the determination of the electrical units."

Upon these resolutions your Committee have to observe that experiments made in the Cavendish laboratory of the University of Cambridge by Lord Rayleigh and other experimenters working in conjunction with him, confirmed by independent experiments by different methods also made in the Cavendish laboratory, appear to have attained a greater degree of accuracy than that agreed upon by the Conference as sufficient for present requirements.

Your Committee are therefore of opinion that, so far as the determination of the standard of electrical resistance is concerned, it is unnecessary to advise the Government to take any steps in the matter until further researches raise fresh questions relating thereto, as the results obtained at Cambridge seem to possess all the accuracy obtainable at present.

In the Second Commission, which dealt with earth currents and lightning protectors, various resolutions were carried, which it will be convenient to deal with separately.

The first resolution proposed that the different Governments should organise regular observations upon the behaviour of atmospheric electricity. In reference to this your Committee understand that regular and continuous observations have been made for some years at Greenwich and at Kew, and without further and more detailed recommendations on the part of the Congress as to the special observations they propose, your Committee are not in a position to recommend any further steps to be taken by the Government.

The second resolution expressed the wish that a detailed study should be made of the effects of thunderstorms upon telegraphic lines and telephonic lines, and upon buildings connected with wires.

In regard to this your Committee have nothing to advise until the Commission have formulated their requirements in more detail; when this has been done, it is understood from the delegates of the Post Office that the fullest consideration will be given to the matter, with a view to afford every assistance in the power of that department.

The third resolution dealt with the question of the observation of earth currents. Your Committee would observe that continuous records are made by photography at Greenwich of all earth currents occurring upon two telegraphic lines proceeding from the Observatory nearly at right angles to each other; and careful returns are collected from all the principal Post Offices in the United Kingdom of every unusual and disturbing magnetic storm; and they recommend that a description of the methods employed in this country, which, with notes bearing on the subject, is appended, be submitted to the Congress, with the view to their universal adoption, if approved, in order that similar observations may be carried out throughout the world. Your Committee at the same time are of opinion that, with a view to meet the wishes of the Congress, some effort might be made to secure observations on Sundays on those telegraphic lines where the staff is necessarily present, but where the number of messages sent is very small.

The fourth resolution suggests the establishment of an international network of telegraph wires for the purpose of automatically registering at a central station meteorological changes.

In view of the great expense that would be incurred in establishing a system of wires for automatically recording telemeteorographical observations, your Committee concur with the Congress in considering the time has not arrived for adopting that proposal.

The fifth resolution expresses the view that lightning protectors and conductors should everywhere be submitted to a periodical inspection.

This recommendation is at present carried out by the War Office in connection with the buildings under its charge, and the subject has been considered by a Committee of Delegates from the Society of Telegraph Engineers, the Physical and Meteorological Societies, and the Institute of British Architects. There is not in England any authority legally competent to discharge the duty as far as the general public is concerned, and it is therefore impossible in England to carry out this proposal in its entirety; but the Committee concur in the advisability of adopting that course where it is found possible to do so.

The sixth resolution implies that the returns of storms and their effects upon buildings and telegraphic lines should be subject to statistical examination.

Your Committee consider that the observations necessary for ascertaining the effects of lightning other than on telegraph wires cannot be carried out by the Government, owing to the non-existence of a competent staff throughout the Kingdom and that such observations must of necessity be left to private observers. Your Committee recommend, however, that the Meteorological Office be supplied with forms of questions such as may be finally adopted by the Congress for distribution to meteorological observers throughout the United Kingdom. The information desired by the Congress would, it is hoped, thus be obtained.

With reference to the effect of lightning on telegraph lines, the delegates of the Post Office who attended the meeting of the Committee stated that their department would be able to adopt any form of questions, on which returns could be made, finally proposed by the Congress.

Your Committee recommend that the Government should procure such adoption by the Post Office in the United Kingdom, and should also use its influence to cause the same form to be adopted by the Indian and Colonial Administrations and by the various submarine and other telegraphic and telephonic companies at home and abroad.

THIRD COMMISSION.—This Commission dealt with the establishment of a standard of light by reference to which various electric and other lights could be measured. At the present moment there are two in existence: the one is the French Carcel Lamp, and the other is the English Standard Candle, the former being nearly ten times the latter. No better standard was proposed at the Conference. This question remains in abeyance for further investigation.

Your Committee fully recognise the importance of the recommendation to adopt a uniform standard of light.

A Committee appointed by the British Association are now considering the question, and pending their Report your Committee have at present no recommendations to make.

It will be seen from this Report that there are matters of high scientific and practical importance which will be brought before the approaching Congress, and your Committee are of opinion that England should be represented, to bring the views above expressed before it, and to assist at its deliberations. The value of the decisions at which the Congress may arrive depends mainly on its international character, and the non-representation of this country would be a serious blow to the authority of its utterances, and perhaps cause the same confusion in electrical science which now exists in others where international accord has not been established.

(Signed) W. G. ADAMS.
R. Y. ARMSTRONG, Maj. R.E.
W. H. M. CHRISTIE.
G. C. FOSTER.
J. F. MOULTON.
RAYLEIGH.
C. W. SIEMENS.
G. G. STOKES.
W. THOMSON.
E. GRAVES, W. H. PREECE } representing the Post Office.
J. F. D. DONNELLY, Col. R.E., W. de W. ABNEY, Capt. R.E. } representing the Science and Art Department.

August 8, 1883

SOME UNSOLVED PROBLEMS IN GEOLOGY[1]

MY predecessor in office remarked, in the opening of his address, that two courses are open to the retiring president of this Association in preparing the annual presidential discourse,—he may either take up some topic relating to his own specialty, or he may deal with various or general matters relating to science and its progress. A geologist, however, is not necessarily tied up to one or the other alternative. His subject covers the whole history of the earth in time. At the beginning it allies itself with astronomy and physics and celestial chemistry. At the end it runs into human history, and is mixed up with archæology and anthropology. Throughout its whole course it has to deal with questions of meteorology, geography, and biology. In short, there is no department of physical or biological science with which geology is not allied, or at least on which the geologist may not presume to trespass. When, therefore, I announce as my subject on the present occasion some of the unsolved problems of this universal science, you need not be surprised if I should be somewhat discursive.

Perhaps I shall begin at the utmost limits of my subject by remarking that in matters of natural and physical science we are met at the outset with the scarcely solved question as to our own place in the nature which we study, and the bearing of this on the difficulties we encounter. The organism of man is decidedly a part of nature. We place ourselves, in this aspect, in the sub-kingdom vertebrata, and class mammalia, and recognise the fact that man is the terminal link in a chain of being extending throughout geological time. But the organism is not all of man; and, when we regard man as a scientific animal, we raise a new question. If the human mind is a part of nature, then it is subject to natural law; and nature includes mind as well as matter. On the other hand, without being absolute idealists, we may hold that mind is more potent than matter, and nearer to the real essence of things. Our science is in any case necessarily dualistic, being the product of the reaction of mind on nature, and must be largely subjective and anthropomorphic. Hence, no doubt, arise much of the controversy of science, and much of the unsolved difficulty. We recognise this when we divide science into that which is experimental, or depends on apparatus, and that which is observational and classificatory,—distinctions, these, which relate not so much to the objects of science as to our methods of pursuing them. This view also opens up to us the thought that the domain of science is practically boundless; for who can set limits to the action of mind on the universe, or of the universe on mind? It follows that science must be limited on all sides by unsolved mysteries; and it will not serve any good purpose to meet these with clever guesses. If we so treat the enigmas of the sphinx Nature, we shall surely be devoured. Nor, on the other hand, must we collapse into absolute despair, and resign ourselves to the confession of inevitable ignorance. It becomes us, rather, boldly to confront the unsolved questions of nature, and to wrestle with their difficulties till we master such as we can, and cheerfully leave those we cannot overcome to be grappled with by our successors.

Fortunately, as a geologist, I do not need to invite your attention to those transcendental questions which relate to the ultimate constitution of matter, the nature of the ethereal medium filling space, the absolute difference or identity of chemical elements, the cause of gravitation, the conservation and dissipation of energy, the nature of life, or the primary origin of bioplasmic matter. I may take the much more humble *rôle* of an inquirer into the unsolved or partially solved problems which meet us in considering that short and imperfect record which geology studies in the rocky layers of the earth's crust, and which leads no farther back than to the time when a solid rind had already formed on the earth and was already covered with an ocean. This record of geology covers but a small part of the history of the earth and of the system to which it belongs, nor does it enter at all into the more recondite problems involved; still it forms, I believe, some necessary preparation, at least, to the comprehension of these.

What do we know of the oldest and most primitive rocks? At this moment the question may be answered in many and discordant ways; yet the leading elements of the answer may be given very simply. The oldest rock formation known to geologists is the lower Laurentian, the fundamental gneiss, the Lewisian formation of Scotland, the Ottawa gneiss of Canada. This formation of enormous thickness corresponds to what the older geologists called the fundamental granite,—a name not to be scouted, for gneiss is only a stratified granite. Perhaps the main fact in relation to this old rock is that it is a gneiss; that is, a rock at once bedded and crystalline, and having for its dominant ingredient the mineral orthoclase,—a compound of silica, alumina, and potash,—in which are embedded, as in a paste, grains and crystals of quartz and hornblende. We know very well, from its texture and composition, that it cannot be a product of mere heat; and, being a bedded rock, we infer that it was laid down layer by layer, in the manner of aqueous deposits. On the other hand, its chemical composition is quite different from that of the muds, sands, and gravels usually deposited from water. Their special characters are caused by the fact that they have resulted from the slow decay of rocks like these gneisses, under the operation of carbonic acid and water, whereby the alkaline matter and the more soluble part of the silica have been washed away, leaving a residue mainly siliceous and aluminous. Such more modern rocks tell of dry land subjected to atmospheric decay and rainwash. If they have any direct relation to the old gneisses, they are their grandchildren, not their parents. On the contrary, the oldest gneisses show no pebbles, or sand, or limestone—nothing to indicate that there was then any land undergoing atmospheric waste, or shores with sand and gravel. For all that we know to the contrary, these old gneisses may have been deposited in a shoreless sea, holding in solution or suspension merely what it could derive from a submerged crust recently cooled from a state of fusion, still thin, and exuding here and there through its fissures heated waters and volcanic products.

It is scarcely necessary to say that I have no confidence in the supposition of unlike composition of the earth's mass on different sides, on which Dana has partly based his theory of the origin of continents. The most probable conception seems to be that of Lyell; namely, a molten mass, uniform except in so far as denser material might exist towards its centre, and a crust, at first approximately even and homogeneous, and subsequently thrown into great bendings upward and downward. This question has recently been ably discussed by Mr. Crosby in the London *Geological Magazine*.[1]

In short, the fundamental gneiss of the lower Laurentian may have been the first rock ever formed; and in any case it is a rock formed under conditions which have not since recurred, except locally. It constitutes the first and best example of these chemico-physical, aqueous, or aqueo-igneous rocks, so characteristic of the earliest period of the earth's history. Viewed in this way, the lower Laurentian gneiss is probably the oldest kind of rock we shall ever know,—the limit to our backward progress, beyond which there remains nothing to the geologist except physical hypotheses respecting a cooling, incandescent globe. For the chemical conditions of these primitive rocks, and what is known as to their probable origin, I must refer you to my friend Dr. Sterry Hunt, to whom we owe so much of what is known of the older crystalline rocks,[2] as well as of their literature and the questions which they raise. My purpose here is to sketch the remarkable difference which we meet as we ascend into the middle and upper Laurentian.

In the next succeeding formation, the true lower Laurentian of Logan, the Grenville series of Canada, we meet with a great and significant change. It is true, we have still a predominance of gneisses which may have been formed in the same manner with those below them; but we find these now associated with great beds of limestone and dolomite, which must have been formed by the separation of calcium and magnesium carbonates from the sea water, either by chemical precipitation or by the agency of living beings. We have also quartzite, quartzose gneisses, and even pebble beds, which inform us of sand-banks and shores. Nay, more, we have beds containing graphite, which must be the residue of plants, and iron ores which tell of the deoxidation of iron oxide by organic matters. In short, here we have evidence of new factors in world-building,—of land and ocean, of atmospheric decay of rocks, of deoxidising processes carried on by vegetable life on the land and in the waters, of limestone-building in the sea. To afford material for such rocks, the old Ottawa gneiss must have been lifted up into continents and mountain masses. Under the slow but sure action of the carbonic dioxide dissolved in rain water, its felspar had crumbled

[1] Address of the retiring president of the American Association for the Advancement of Science, Principal J. W. Dawson, LL.D., F.R.S., at Minneapolis, August 15, 1883. Advance proofs of this and other addresses to follow have been kindly sent us by the Editor of *Science*.

[1] June, 1883. [2] Hunt, "Essays on Chemical Geology."

down in the course of ages. Its potash, soda, lime, magnesia, and part of its silica, had been washed into the sea, there to enter into new combinations, and to form new deposits. The crumbling residue of fine clay and sand had been also washed down into the borders of the ocean, and had been there deposited in beds.[1] Thus the earth had entered into a new phase, which continues onward through the geological ages; and I place in your hands one key for unlocking the mystery of the world when I affirm that this great change took place, this new era was inaugurated, in the midst of the Laurentian period.

Was not this time a fit period for the first appearance of life? Should we not expect it to appear, independently of the evidence we have of the fact? I do not propose to enter here into that evidence, more especially in the case of the one well characterised Laurentian fossil, *Eozoon canadense*. I have already amply illustrated it elsewhere. I would merely say here, that we should bear in mind that, in this later half of the lower Laurentian or, if we so choose to style it, middle Laurentian period, we have the conditions required for life in the sea and on the land; and since in other periods we know that life was always present when its conditions were present, it is not unreasonable to look for the first traces of life in this formation, in which we find for the first time the completion of those physical arrangements which make life, in such forms of it as exist on our planet, possible.

This is also a proper place to say something of the doctrine of what is termed "metamorphism." The Laurentian rocks are undoubtedly greatly changed from their original state, more especially in the matters of crystallisation and the formation of disseminated minerals by the action of heat and heated water. Sandstones have thus passed into quartzites, clays into slates and schists, limestones into marbles. So far, metamorphism is not a doubtful question; but, when theories of metamorphism go so far as to suppose an actual change of one element for another, they go beyond the bounds of chemical credibility; yet such theories of metamorphism are often boldly advanced, and made the basis of important conclusions. Dr. Hunt has happily given the name "metasomatosis" to this imaginary and impossible kind of metamorphism, which may be regarded as an extreme kind of evolution, akin to some of those forms of that theory employed with reference to life, but more easily detected and exposed. I would have it to be understood that, in speaking of the metamorphism of the older crystalline rocks, it is not to this metasomatosis that I refer, and that I hold that rocks which have been produced out of the materials decomposed by atmospheric erosion can never, by any process of metamorphism, be restored to the precise condition of the Laurentian rocks. Thus there is in the older formations a genealogy of rocks, which, in the absence of fossils, may be used with some confidence, but which does not apply to the more modern deposits. Still, nothing in geology absolutely perishes or is altogether discontinued; and it is probable, that, down to the present day, the causes which produced the old Laurentian gneiss may still operate in limited localities. Then, however, they were general, not exceptional. It is further to be observed that the term "gneiss" is sometimes of wide and even loose application. Beside the typical orthoclase and hornblendic gneiss of the Laurentian, there are micaceous, quartzose, garnetiferous, and many other kinds of gneiss; and even gneissose rocks, which hold labradorite or anorthite instead of orthoclase, are sometimes, though not accurately, included in the term.

The Grenville series, or middle Laurentian, is succeeded by what Logan in Canada called the upper Laurentian, and which other geologists have called the Norite or Norian series. Here we still have our old friends the gneisses, but somewhat peculiar in type; and associated with them are great beds rich in lime-felspar,—the so-called labradorite and anorthite rocks. The precise origin of these is uncertain, but this much seems clear; namely, that they originated in circumstances in which the great limestones deposited in the lower or middle Laurentian were beginning to be employed in the manufacture, probably by aqueo-igneous agencies, of lime-felspars. This proves the Norian rocks to be much younger than the Laurentian, and that, as Logan supposed, considerable earth-movements had occurred between the two, implying lapse of time.

Next we have the Huronian of Logan,—a series much less crystalline and more fragmentary, and affording more evidence of land elevation and atmospheric and aqueous erosion than any of the others. It has great conglomerates, some of them made up of rounded pebbles of Laurentian rocks, and others of quartz pebbles, which must have been the remains of rocks subjected to very perfect erosion. The pure quartz rocks tell the same tale, while limestones and slates speak also of chemical separation of the materials of older rocks. The Huronian evidently tells of movements in the previous Laurentian, and changes in its texture so great, that the former may be regarded as a comparatively modern rock, though vastly older than any part of the palæozoic series.

Still later than the Huronian is the great micaceous series called by Hunt the Mont Alban or White Mountain group, and the Taconian or lower Taconic of Emmons, which recalls in some measure the conditions of the Huronian. The precise relations of these to the later formations, and to certain doubtful deposits around Lake Superior, can scarcely be said to be settled, though it would seem that they are all older than the fossiliferous Cambrian rocks which practically constitute the base of the palæozoic. I have, I may say, satisfied myself, in regions which I have studied, of the existence and order of these rocks as successive formations, though I would not dogmatise as to the precise relations of those last mentioned, or as to the precise age of some disputed formations which may either be of the age of the older eozoic formations, or may be peculiar kinds of palæozoic rocks modified by metamorphism. Probably neither of the extreme views now agitated is absolutely correct.

After what has been said, you will perhaps not be astonished that a great geological battle rages over the old crystalline rocks. By some geologists they are almost entirely explained away, or referred to igneous action or to the alteration of ordinary sediments. Under the treatment of another school, they grow to great series of pre-Cambrian rocks, constituting vast systems of formations, distinguishable from each other, not by fossils, but by differences of mineral character. I have already indicated the manner in which I believe the dispute will ultimately be settled, and the president of the geological section will treat it more fully in his opening address.

After the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves in the Cambrian, in the presence of a nearly complete invertebrate fauna of protozoa, polyps, echinoderms, mollusks, and crustacea; and this not confined to one locality merely, but apparently extended simultaneously throughout the ocean. This sudden incoming of animal life, along with the subsequent introduction of successive groups of invertebrates, and finally of vertebrate animals, furnishes one of the greatest of the unsolved problems of geology, which geologists were wont to settle by the supposition of successive creations. In an address delivered at the Detroit meeting of the Association in 1875, I endeavoured to set forth the facts as to this succession, and the general principles involved in it, and to show the insufficiency of the theories of evolution suggested by biologists to give any substantial aid to the geologist in these questions. In looking again at the points there set forth, I find they have not been invalidated by subsequent discoveries, and that we are still nearly in the same position with respect to these great questions that we were in at that time,—a singular proof of the impotency of that deductive method of reasoning which has become fashionable among naturalists of late. Yet the discussions of recent years have thrown some additional light on these matters; and none more so than the mild disclaimers with which my friend Dr. Asa Gray and other moderate and scientific evolutionists have met the extreme views of such men as Romanes, Haeckel, Lubbock, and Grant Allen. It may be useful to note some of these as shedding a little light on this dark corner of our unsolved problems.

It has been urged, on the side of rational evolution, that this hypothesis does not profess to give an explanation of the absolute origin of life on our planet, or even of the original organisation of a single cell or of a simple mass of protoplasm, living or dead. All experimental attempts to produce by synthesis the complex albuminous substances, or to obtain the living from the non-living, have so far been fruitless; and, indeed, we cannot imagine any process by which such changes could be effected. That they have been effected we know; but the process employed by their maker is still as mysterious to us as it probably was to him who wrote the words, "And God said, Let the waters swarm with swarmers." How vast is the gap in our knowledge and our practical power implied in this admission, which must, however, be made by every mind not absolutely

[1] Dr. Hunt has now in preparation for the press an important paper on this subject, read before the National Academy of Sciences.

blinded by a superstitious belief in those forms of words which too often pass current as philosophy.

But if we are content to start with a number of organisms ready made,—a somewhat humiliating start, however,—we still have to ask, How do these vary so as to give new species? It is a singular illusion in this matter, of men who profess to be believers in natural law, that variation may be boundless, aimless, and fortuitous, and that it is by spontaneous selection from varieties thus produced that development arises. But surely the supposition of mere chance and magic is unworthy of science. Varieties must have causes, and their causes and their effects must be regulated by some law or laws. Now, it is easy to see that they cannot be caused by a mere innate tendency in the organism itself. Every organism is so nicely equilibrated, that it has no such spontaneous tendency, except within the limits set by its growth and the law of its periodical changes. There may however, be equilibrium more or less stable. I believe all attempts hitherto made have failed to account for the fixity of certain, nay, of very many, types throughout geological time; but the mere consideration that one may be in a more stable state of equilibrium than another so far explains it. A rocking stone has no more spontaneous tendency to move than an ordinary boulder, but it may be made to move with a touch. So it probably is with organisms. But, if so, then the causes of variation are external, as in many cases we actually know them to be; and they must depend on instability or change in surroundings, and this so arranged as not to be too extreme in amount, and to operate in some determinate direction. Observe how remarkable the unity of the adjustments involved in such a supposition. How superior they must be to our rude and always more or less unsuccessful attempts to produce and carry forward varieties and races in definite directions! This cannot be chance. If it exists, it must depend on plans deeply laid in the nature of things, else it would be most monstrous magic and causeless miracle. Still more certain is this conclusion when we consider the vast and orderly succession made known to us by geology, and which must have been regulated by fixed laws, only a few of which are as yet known to us.

Beyond these general considerations, we have others of a more special character, based on palæontological facts, which show how imperfect are our attempts, as yet, to reach the true causes of the introduction of genera and species.

One is the remarkable fixity of the leading types of living beings in geological time. If instead of framing, like Haeckel, fanciful phylogenies, we take the trouble, with Barrande and Gaudry, to trace the forms of life through the period of their existence, each along its own line, we shall be greatly struck with this, and especially with the continuous existence of many low types of life through vicissitudes of physical conditions of the most stupendous character, and over a lapse of time scarcely conceivable. What is still more remarkable is, that this holds in groups which, within certain limits, are perhaps the most variable of all. In the present world no creatures are individually more variable than the protozoa; as, for example, the foraminifera and the sponges. Yet these groups are fundamentally the same from the beginning of the palæozoic until now; and modern species seem scarcely at all to differ from specimens procured from rocks at least half way back to the beginning of our geological record. If we suppose that the present sponges and foraminifera are the descendants of those of the Silurian period, we can affirm, that, in all that vast lapse of time, they have, on the whole, made little greater change than that which may be observed in variable forms at present. The same remark applies to other low animal forms. In forms somewhat higher and less variable, this is equally noteworthy. The pattern of the venation of the wings of cockroaches, and the structure and form of land-snails, gally-worms, and decapod crustaceans, were all settled in the Carboniferous age in a way that still remains. So were the foliage and the fructification of club-mosses and ferns. If at any time members of these groups branched off, so as to lay the foundation of new species, this must have been a very rare and exceptional occurrence, and one demanding even some suspension of the ordinary laws of nature.

Certain recent utterances of eminent scientific men in England and France are most instructive with reference to the difficulties which encompass this subject. Huxley, at present the leader of English evolutionists, in his "Rede Lecture"[1] delivered at Cambridge, England, holds that there are only two "possible alternative hypotheses" as to the origin of species,—(1) that of "construction," or the mechanical putting-together of the materials and parts of each new species separately; and (2) that of "evolution," or that one form of life "proceeded from another" by the "establishment of small successive differences." After comparing these modes, much to the disadvantage of the first, he concludes with the statement that "this was his case for evolution, which he rested wholly on arguments of the kind he had adduced;" these arguments being the threadbare false analogy of ordinary reproduction and the transformation of species, and the mere succession of forms more or less similar in geological time, neither of them having any bearing whatever on the origin of any species or on the cause of the observed succession. With reference to the two alternatives, while it is true that no certain evidence has yet been obtained—either by experiment, observation, or sound induction—as to the mode of origin of any species, enough is known to show that there are numerous possible methods, grouped usually under the heads of absolute creation, mediate creation, critical evolution, and gradual evolution. It is also true that almost the only thing we certainly know in the matter is that the differences characteristic of classes, orders, genera, and species, must have arisen, not in one or two, but in many ways. An instructive commentary on the capacity of our age to deal with these great questions is afforded by the fact that this little piece of clever mental gymnastics should have been practised in a university lecture and in presence of an educated audience. It is also deserving of notice, that, though the lecturer takes the development of the Nautili and their allies as his principal illustration, he evidently attaches no weight to the argument in the opposite sense deduced by Barrande—the man of all others most profoundly acquainted with these animals—from the palæozoic cephalopods.

Another example is afforded by a lecture recently delivered at the Royal Institution in London by Professor Flower.[1] The subject is "The Whales, Past and Present, and their Probable Origin." The latter point, as is well known, Gaudry had candidly given up. "We have questioned," he says, "these strange and gigantic sovereigns of the tertiary oceans as to their ancestors,—they leave us without reply." Flower is bold enough to face this problem; and he does so in a fair and vigorous way, though limiting himself to the supposition of slow and gradual change. He gives up at once, as every anatomist must, the idea of an origin from fishes or reptiles. He thinks the ancestors of the whales must have been quadrupedal mammals. He is obliged for good reasons to reject the seals and the otters, and turns to the ungulates, though here, also, the difficulties are formidable. Finally he has recourse to an imaginary ancestor, supposed to have haunted marshes and rivers of the mesozoic age, and to have been intermediate between a hippopotamus and a dolphin, and omnivorous in diet. As this animal is altogether unknown to geology or zoology, and not much less difficult to account for than the whales themselves, he very properly adds, "Please to recollect, however, that this is a mere speculation." He trusts, however, that such speculations are "not without their use"; but this will depend upon whether or not they lead men's minds from the path of legitimate science into the quicksands of baseless conjecture.

Gaudry, in his recent work, "Enchaînements du Monde Animal,"[2] though a strong advocate of evolution, is obliged in his final *résumé* to say, "Il ne laisse point percer le mystère qui entoure le developpement primitif des grandes classes du monde animal. Nul homme ne sait comment ont été formés les premiers individus de foraminifères, de polypes, d'étoiles de mer, de crinoides, &c. Les fossiles primaires ne nous ont pas encore fourni de preuves positives du passage des animaux d'une classe à ceux d'une autre classe."

Professor Williamson of Manchester, in an address delivered in February last before the Royal Institution of Great Britain, after showing that the conifers, ferns, and lycopods of the palæozoic have no known ancestry, uses the significant words, "The time has not yet arrived for the appointment of a botanical king-at-arms and constructor of pedigrees."

Another caution which a palæontologist has occasion to give with regard to theories of life has reference to the tendency of biologists to infer that animals and plants were introduced under embryonic forms, and at first in few and imperfect species. Facts do not substantiate this. The first appearance of leading types of life is rarely embryonic. On the contrary, they often appear in highly perfect and specialised forms; often, however, of composite type, and expressing characters afterwards so

[1] Report in NATURE, June 21 (p. 187), corrected by the author.

[1] Reported in NATURE. [2] Paris, 1883.

separated as to belong to higher groups. The trilobites of the Cambrian are some of them of few segments, and, so far, embryonic; but the greater part are many-segmented, and very complex. The batrachians of the carboniferous present many characters higher than those of their modern successors, and now appropriated to the true reptiles. The reptiles of the Permian and trias usurped some of the prerogatives of the mammals. The ferns, lycopods, and equisetums of the Devonian and carboniferous were, to say the least, not inferior to their modern representatives. The shell-bearing cephalopods of the palæozoic would seem to have possessed structures now special to a higher group, that of the cuttle-fishes. The bald and contemptuous negation of these facts by Haeckel and other biologists does not tend to give geologists much confidence in their dicta.

Again: we are now prepared to say that the struggle for existence, however plausible as a theory, when put before us in connection with the productiveness of animals, and the few survivors of their multitudinous progeny, has not been the determining cause of the introduction of new species. The periods of rapid introduction of new forms of marine life were not periods of struggle but of expansion,—those periods in which the submergence of continents afforded new and large space for their extension and comfortable subsistence. In like manner it was continental emergence that afforded the opportunity for the introduction of land animals and plants. Further, in connection with this, it is now an established conclusion that the great aggressive faunas and floras of the continents have originated in the north, some of them within the Arctic circle; and this in periods of exceptional warmth, when the perpetual summer sunshine of the Arctic regions coexisted with a warm temperature. The testimony of the rocks thus is, that not struggle, but expansion, furnished the requisite conditions for new forms of life, and that the periods of struggle were characterised by depauperation and extinction.

But we are sometimes told that organisms are merely mechanical, and that the discussions respecting their origin have no significance, any more than if they related to rocks or crystals, because they relate merely to the organism considered as a machine, and not to that which may be supposed to be more important, namely, the great determining power of mind and will. That this is a mere evasion, by which we really gain nothing, will appear from a characteristic extract from an article by an eminent biologist, in the new edition of the "Encyclopædia Britannica,"—a publication which, I am sorry to say, instead of its proper *rôle* as a repertory of facts, has become a strong partisan, stating extreme and unproved speculations as if they were conclusions of science. The statement referred to is as follows: "A mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which, or its vital phenomena, depend on the one hand on its construction, and, on the other, on the energy supplied to it; and to speak of vitality as anything but the name for a series of operations is as if one should talk of the horologity of a clock." It would, I think, scarcely be possible to put into the same number of words a greater amount of unscientific assumption and unproved statement than in this sentence. Is "living protoplasm" different in any way from dead protoplasm, and, if so, what causes the difference? What is a "machine"? Can we conceive of a self-produced or uncaused machine, or one not intended to work out some definite results? The results of the machine in question are said to be "vital phenomena;" certainly most wonderful results, and greater than those of any machine man has yet been able to construct. But why "vital"? If there is no such thing as life, surely they are merely physical results. Can mechanical causes produce other than physical effects? To Aristotle, life was "the cause of form in organisms." Is not this quite as likely to be true as the converse proposition? If the vital phenomena depend on the "construction" of the machine, and the "energy supplied to it," whence this construction, and whence this energy? The illustration of the clock does not help us to answer this question. The construction of the clock depends on its maker, and its energy is derived from the hand that winds it up. If we can think of a clock which no one has made and which no one winds, a clock constructed by chance, set in harmony with the universe by chance, wound up periodically by chance,—we shall then have an idea parallel to that of an organism living, yet without any vital energy or creative law; but in such a case we should certainly have to assume some antecedent cause, whether we call it "horologity" or by some other name. Perhaps the term "evolution" would serve as well as any other, were it not that common sense teaches that nothing can be spontaneously evolved out of that in which it did not previously exist.

There is one other unsolved problem, in the study of life by the geologist, to which it is still necessary to advert. This is the inability of palæontology to fill up the gaps in the chain of being. In this respect, we are constantly taunted with the imperfection of the record; but facts show that this is much more complete than is generally supposed. Over long periods of time and many lines of being we have a nearly continuous chain; and, if this does not show the tendency desired, the fault is as likely to be in the theory as in the record. On the other hand, the abrupt and simultaneous appearance of new types in many specific and generic forms, and over wide and separate areas at one and the same time, is too often repeated to be accidental. Hence palæontologists, in endeavouring to establish evolution, have been obliged to assume periods of exceptional activity in the introduction of species, alternating with others of stagnation,—a doctrine differing very little from that of special creation as held by the older geologists.

The attempt has lately been made to account for these breaks by the assumption that the geological record relates only to periods of submergence, and gives no information as to those of elevation. This is manifestly untrue. In so far as marine life is concerned, the periods of submergence are those in which new forms abound for very obvious reasons already hinted. But the periods of new forms of land and fresh-water life are those of elevation, and these have their own records and monuments, often very rich and ample; as, for example, the swamps of the carboniferous, the transition from the cretaceous subsidence to the Laramie elevation, the tertiary lake-basins of the west, the terraces and raised beaches of the pleistocene. Had I time to refer in detail to the breaks in the continuity of life which cannot be explained by the imperfection of the record, I could show at least that nature, in this case, does advance *per saltum*,—by leaps, rather than by a slow continuous process. Many able reasoners, as Le Conte in this country, and Mivart and Collard in England, hold this view.

Here, as elsewhere, a vast amount of steady conscientious work is required to enable us to solve the problems of the history of life. But, if so, the more the hope for the patient student and investigator. I know nothing more chilling to research, or unfavourable to progress, than the promulgation of a dogmatic decision that there is nothing to be learned but a merely fortuitous and uncaused succession, amenable to no law, and only to be covered, in order to hide its shapeless and uncertain proportions, by the mantle of bold and gratuitous hypothesis.

So soon as we find evidence of continents and oceans, we raise the question, "Have these continents existed from the first in their present position and form, or have the land and water changed places in the course of geological time?" In reality both statements are true in a certain limited sense. On the one hand, any geological map whatever suffices to show that the general outline of the existing land began to be formed in the first and oldest crumplings of the crust. On the other hand, the greater part of the surface of the land consists of marine sediments which must have been derived from land that has perished in the process, while all the continental surfaces, except, perhaps, some high peaks and ridges, have been many times submerged. Both of these apparently contradictory statements are true; and, without assuming both, it is impossible to explain the existing contours and reliefs of the surface.

In the case of North America, the form of the old nucleus of Laurentian rock in the north already marks out that of the finished continent, and the successive later formations have been laid upon the edges of this, like the successive loads of earth dumped over an embankment. But in order to give the great thickness of the palæozoic sediments, the land must have been again and again submerged, and for long periods of time. Thus, in one sense, the continents have been fixed; in another, they have been constantly fluctuating. Hall and Dana have well illustrated these points in so far as eastern North America is concerned. Professor Hull of the Geological Survey of Ireland has recently had the boldness to reduce the fluctuations of land and water, as evidenced in the British Islands, to the form of a series of maps intended to show the physical geography of each successive period. The attempt is probably premature, and has been met with much adverse criticism; but there can be no doubt that it has an element of truth. When we attempt to calculate what could have been supplied from the old eozoic nucleus by decay and aqueous erosion, and when we take into account the greater local thickness of sediments towards the present sea-basins, we can scarcely avoid the conclusion that extensive areas once occupied by high land are now under the sea. But to ascertain the precise areas and position of these perished lands may now be impossible.

In point of fact, we are obliged to believe in the contemporaneous existence in all geological periods, except perhaps the very oldest, of three sorts of areas on the surface of the earth: 1. Oceanic areas of deep sea, which must always have occupied the bed of the present ocean, or parts of it; 2. Continental plateaus, sometimes existing as low flats or as higher tablelands, and sometimes submerged; 3. Areas of plication or folding, more especially along the borders of the oceans, forming elevated lands rarely submerged, and constantly affording the material of sedimentary accumulations.

Every geologist knows the contention which has been occasioned by the attempts to correlate the earlier palæozoic deposits of the Atlantic margin of North America with those forming at the same time on the interior plateau, and with those of intervening lines of plication and igneous disturbance. Stratigraphy, lithology, and fossils are all more or less at fault in dealing with these questions; and, while the general nature of the problem is understood by many geologists, its solution in particular cases is still a source of apparently endless debate.

The causes and mode of operation of the great movements of the earth's crust which have produced mountains, plains, and tablelands, are still involved in some mystery. One patent cause is the unequal settling of the crust towards the centre; but it is not so generally understood as it should be that the greater settlement of the ocean bed has necessitated its pressure against the sides of the continents in the same manner that a huge ice-floe crushes a ship or a pier. The geological map of North America shows this at a glance, and impresses us with the fact that large portions of the earth's crust have not only been folded, but bodily pushed back for great distances. On looking at the extreme north, we see that the great Laurentian mass of central Newfoundland has acted as a protecting pier to the space immediately west of it, and has caused the Gulf of St. Lawrence to remain an undisturbed area since palæozoic times. Immediately to the south of this, Nova Scotia and New Brunswick are folded back. Still farther south, as Guyot has shown, the old sediments have been crushed in sharp folds against the Adirondack mass, which has sheltered the tableland of the Catskills and of the Great Lakes. South of this again, the rocks of Pennsylvania and Maryland have been driven back in a great curve to the west. Nothing, I think, can more forcibly show the enormous pressure to which the edges of the continents have been exposed, and at the same time the great sinking of the ocean beds. Complex and difficult to calculate though these movements of plication are, they are more intelligible than the apparently regular pulsations of the flat continental areas, whereby they have alternately been below and above the waters, and which must have depended on somewhat regular recurring causes, connected either with the secular cooling of the earth, or with the gradual retardation of its rotation, or with both. Throughout these changes, each successive elevation exposed the rocks for long ages to the decomposing influence of the atmosphere. Each submergence swept away, and deposited as sediment, the material accumulated by decay. Every change of elevation was accompanied with changes of climate and with modifications of the habitats of animals and plants. Were it possible to restore accurately the physical geography of the earth in all these respects, for each geological period, the data for the solution of many difficult questions would be furnished.

It is an unfortunate circumstance that conclusions in geology arrived at by the most careful observation and induction do not remain undisturbed, but require constant vigilance to prevent them from being overthrown. Sometimes, of course, this arises from new discoveries throwing new light on old facts; but when this occurs it rarely works the complete subversion of previously received views. The more usual case is, that some over-zealous specialist suddenly discovers what seems to him to overturn all previous beliefs, and rushes into print with a new and plausible theory, which at once carries with him a host of half-informed people, but the insufficiency of which is speedily made manifest.

Had I written this address a few years ago, I might have referred to the mode of formation of coal as one of the things most surely settled and understood. The labours of many eminent geologists, microscopists, and chemists in the Old and the New Worlds had shown that coal nearly always rests upon old soil surfaces penetrated with roots, and that coal-beds have in their roofs erect trees, the remains of the last forests that grew upon them. Logan and I have illustrated this in the case of the series of more than sixty successive coal-beds exposed at the South Joggins, and have shown unequivocal evidence of land-surfaces at the time of the deposition of the coal. Microscopical examination has proved that these coals are composed of the materials of the same trees whose roots are found in the under-clays, and their stems and leaves in the roof shales; that much of the material of the coal has been subjected to sub-aërial decay at the time of its accumulation; and that, in this, ordinary coal differs from bituminous shale, earthy bitumen, and some kinds of cannel, which have been formed under water; that the matter remaining as coal consists almost entirely of epidermal tissues, which, being suberose in character, are highly carbonaceous, very durable, and impermeable by water,[1] and are hence the best fitted for the production of pure coal; and finally that the vegetation and the climatal and geographical features of the coal period were eminently fitted to produce in the vast swamps of that period precisely the effects observed. All these points and many others have been thoroughly worked out for both European and American coal-fields, and seemed to leave no doubt on the subject. But several years ago certain microscopists observed on slices of coal layers filled with spore-cases,—a not unusual circumstance, since these were shed in vast abundance by the trees of the coal forests, and because they contain suberose matter of the same character with epidermal tissues generally. Immediately we were informed that all coal consists of spores; and, this being at once accepted by the unthinking, the results of the labours of many years are thrown aside in favour of this crude and partial theory. A little later, a German microscopist has thought proper to describe coal as made up of minute algæ, and tries to reconcile this view with the appearances, devising at the same time a new and formidable nomenclature of generic and specific names, which would seem largely to represent mere fragments of tissues. Still later, some local facts in a French coal-field have induced an eminent botanist of that country to revive the drift theory of coal, in opposition to that of growth *in situ*. A year or two ago, when my friend Professor Williamson of Manchester informed me that he was preparing a large series of slices of coal with the view of revising the whole subject, I was inclined to say that, after what had been done by Lyell, Goeppert, Logan, Hunt, Newberry, and myself, this was scarcely necessary; but, in view of what I have just stated, it may be that all he can do will be required to rescue from total ruin the results of our labours.

An illustration of a different character is afforded by the controversy now raging with respect to the so-called fucoids of the ancient rocks. At one time the group of fucoids, or algæ, constituted a general place of refuge for all sorts of unintelligible forms and markings; graptolites, worm-trails, crustacean tracks, shrinkage-cracks, and, above all, rill-markings, forming a heterogeneous group of fucoidal remains distinguished by generic and specific names. To these were also added some true land-plants badly preserved, or exhibiting structures not well understood by botanists. Such a group was sure to be eventually dismembered. The writer has himself done something toward this,[2] but Professor Nathorst has done still more;[3] and now some intelligible explanation can be given of many of these forms. Quite recently, however, the Count de Saporta in an elaborate illustrated memoir,[4] has come to the defence of the fucoids, more especially against the destructive experiments of Nathorst, and would carry back into the vegetable kingdom many things which would seem to be mere trails of animals. While writing this address, I have received from Professor Crié of Rennes a paper in which he not only supports the algal nature of Rusichnites, Arthrichnites, and many other supposed fucoids, but claims for the vegetable kingdom even Receptaculites and Archæocyathus. It is not to be denied that some of the facts which he cites, respecting the structure of the Siphoniæ and of certain modern incrusting algæ, are very suggestive, though I cannot agree with his conclusions. My own experience has convinced me that, while non-botanical geologists are prone to mistake all kinds of

1 "Acadian Geology," third edition, supplement, p. 68.

2 "Footprints and Impressions on Carboniferous Rocks," *Amer. Journ. Sc.*, 1873.

3 Royal Swedish Academy, Stockholm, 1881.

4 "Apropos des Algues Fossiles," Paris, 1883.

markings for plants, even good botanists, when not familiar with the chemical and mechanical conditions of fossilisation, and with the present phenomena of tidal shores, are quite as easily misled, though they are very prone, on the other hand, to regard land-plants of some complexity, when badly preserved, as mere algæ. In these circumstances it is very difficult to secure any consensus, and the truth is only to be found by careful observation of competent men. One trouble is that these usually obscure markings have been despised by the greater number of palæontologists, and probably would not now be so much in controversy were it not for the use made of them in illustrating supposed phylogenies of plants.

It would be wrong to close this address without some reference to that which is the veritable *pons asinorum* of the science, the great and much debated glacial period. I trust that you will not suppose that, in the end of an hour's address, I am about to discuss this vexed question. Time would fail me even to name the hosts of recent authors who have contended in this arena. I can hope only to point out a few landmarks which may aid the geological adventurer in traversing the slippery and treacherous surface of the hypothetical ice-sheet of pleistocene times, and in avoiding the yawning crevasses by which it is traversed.

No conclusions of geology seem more certain than that great changes of climate have occurred in the course of geological time; and the evidence of this in that comparatively modern period which immediately preceded the human age is so striking that it has come to be known as preeminently the ice age, while, in the preceding tertiary periods, temperate conditions seem to have prevailed even to the Pole. Of the many theories as to these changes which have been proposed, two seem at present to divide the suffrages of geologists, either alone, or combined with each other. These are, (1) the theory of the precession of the equinoxes in connection with the varying eccentricity of the earth's orbit, advocated more especially by Croll; and (2) the different distribution of land and water as affecting the reception and radiation of heat and the ocean currents,—a theory ably propounded by Lyell, and subsequently extensively adopted, either alone or with the previous one. One of these views may be called the astronomical; the other, the geographical. I confess that I am inclined to accept the second or Lyellian theory for such reasons as the following: 1. Great elevations and depressions of land have occurred in and since the pleistocene, while the alleged astronomical changes are not certain, more especially in regard to their probable effect on the earth; 2. When the rival theories are tested by the present phenomena of the Southern Polar region and the North Atlantic, there seem to be geographical causes adequate to account for all except extreme and unproved glacial conditions; 3. The astronomical cause would suppose regularly recurring glacial periods of which there is no evidence, and it would give to the latest glacial age an antiquity which seems at variance with all other facts; 4. In those more northern regions where glacial phenomena are most pronounced, the theory of floating sheets of ice, with local glaciers descending to the sea, seems to meet all the conditions of the case; and these would be obtained, in the North Atlantic at least, by very moderate changes of level, causing, for example, the equatorial current to flow into the Pacific, instead of running northward as a gulf stream; 5. The geographical theory allows the supposition not merely of vicissitudes of climate quickly following each other in unison with the movements of the surface, but allows also of that near local approximation of regions wholly covered with ice and snow, and others comparatively temperate, which we see at present in the north.

If, however, we are to adopt the geographical theory, we must avoid extreme views; and this leads to the inquiry as to the evidence to be found for any such universal and extreme glaciation as is demanded by some geologists.

The only large continental area in the northern hemisphere supposed to be entirely ice and snow clad is Greenland; and this, so far as it goes, is certainly a local case, for the ice and snow of Greenland extend to the south as far as 60° N. latitude, while both in Norway and in the interior of North America the climate in that latitude permits the growth of cereals. Further, Grinnel Land, which is separated from North Greenland only by a narrow sound, has a comparatively mild climate, and, as Nares has shown, is covered with verdure in summer. Still further, Nordenskjöld, one of the most experienced Arctic explorers, holds that it is probable that the interior of Greenland is itself verdant in summer, and is at this moment preparing to attempt to reach this interior oasis. Nor is it difficult, with the aid of the facts cited by Woeikoff and Whitney,[1] to perceive the cause of the exceptional condition of Greenland. To give ice and snow in large quantities, two conditions are required,—first, atmospheric humidity; and, secondly, cold precipitating regions. Both of these conditions meet in Greenland. Its high coast-ranges receive and condense the humidity from the sea on both sides of it and to the south. Hence the vast accumulation of its coast snow-fields, and the intense discharge of the glaciers emptying out of its valleys. When extreme glacialists point to Greenland, and ask us to believe that in the glacial age the whole continent of North America as far south as the latitude of 40° was covered with a continental glacier, in some places several thousands of feet thick, we may well ask, first, what evidence there is that Greenland, or even the Antarctic continent, at present shows such a condition; and, secondly, whether there exists a possibility that the interior of a great continent could ever receive so large an amount of precipitation as that required. So far as present knowledge exists, it is certain that the meteorologist and the physicist must answer both questions in the negative. In short, perpetual snow and glaciers must be local, and cannot be continental, because of the vast amount of evaporation and condensation required. These can only be possible where comparatively warm seas supply moisture to cold and elevated land; and this supply cannot, in the nature of things, penetrate far inland. The actual condition of interior Asia and interior America in the higher northern latitudes affords positive proof of this. In a state of partial submergence of our northern continents, we can readily imagine glaciation by the combined action of local glaciers and great ice-floes; but, in whatever way the phenomena of the boulder clay and of the so-called terminal moraines are to be accounted for, the theory of a continuous continental glacier must be given up.

I cannot better indicate the general bearing of facts, as they present themselves to my mind in connection with this subject, than by referring to a paper by Dr. G. M. Dawson on the distribution of drift over the great Canadian plains east of the Rocky Mountains.[2] I am the more inclined to refer to this, because of its recency, and because I have so often repeated similar conclusions as to eastern Canada and the region of the Great Lakes.

The great interior plain of western Canada, between the Laurentian axis on the east and the Rocky Mountains on the west, is seven hundred miles in breadth, and is covered with glacial drift, presenting one of the greatest examples of this deposit in the world. Proceeding eastward from the base of the Rocky Mountains, the surface, at firse more than four thousand feet above the sea-level, descends by successive steps to twenty-five hundred feet, and is based on cretaceous and Laramie rocks, covered by boulder clay and sand, in some places from one hundred to two hundred feet in depth, and filling up preexisting hollows, though itself sometimes piled into ridges. Near the Rocky Mountains the bottom of the drift consists of gravel not glaciated. This extends to about one hundred miles east of the mountains, and must have been swept by water out of their valleys. The boulder clay resting on this deposit is largely made up of local debris, in so far as its paste is concerned. It contains many glaciated boulders and stones from the Laurentian region to the east, and also smaller pebbles from the Rocky Mountains; so that at the time of its formation there must have been driftage of large stones for seven hundred miles or more from the east, and of smaller stones from a less distance on the west. The former kind of material extends to the base of the mountains, and to a height of more than four thousand feet. One boulder is mentioned as being forty-two by forty by twenty feet in dimensions. The highest Laurentian boulders seen were at an elevation of forty-six hundred and sixty feet, on the base of the Rocky Mountains. The boulder clay, when thick, can be seen to be rudely stratified, and at one place includes beds of laminated clay with compressed peat, similar to the forest beds described by Worthen and Andrews in Illinois, and the so-called inter-glacial beds described by Hinde on Lake Ontario. The leaf-beds on the Ottawa River, and the drift-trunks found in the boulder clay of Manitoba, belong to the same category, and indicate that throughout the glacial period there were many forest oases far to the north. In the valleys of the Rocky Mountains opening on these plains there are evidences of large local glaciers

[1] "Memoir on Glaciers," Geol. Soc. Berlin, 1881. "Climatic Changes," Boston, 1883.
[2] *Science*, July 1, 1883.

now extinct, and similar evidences exist on the Laurentian highlands on the east.

Perhaps the most remarkable feature of the region is that immense series of ridges of drift piled against an escarpment of Laramie and cretaceous rocks, at an elevation of about twenty-five hundred feet, and known as the "Missouri coteau." It is in some places thirty miles broad and a hundred and eighty feet in height above the plain at its foot, and extends north and south for a great distance; being, in fact, the northern extension of those great ridges of drift which have been traced south of the Great Lakes, and through Pennsylvania and New Jersey, and which figure on the geological maps as the edge of the continental glacier,—an explanation obviously inapplicable in those western regions where they attain their greatest development. It is plain that in the north it marks the western limit of the deep water of a glacial sea, which at some periods extended much farther west, perhaps with a greater proportionate depression in going westward, and on which heavy ice from the Laurentian districts on the east was wafted south-westward by the Arctic currents, while lighter ice from the Rocky Mountains was being borne eastward from these mountains by the prevailing westerly winds. We thus have in the west, on a very wide scale, the same phenomena of varying submergence, cold currents, great ice-floes, and local glaciers producing icebergs, to which I have attributed the boulder clay and upper boulder drift of eastern Canada.

A few subsidiary points I may be pardoned for mentioning here. The rival theories of the glacial period are often characterised as those of land glaciation and sea-borne icebergs. But it must be remembered that those who reject the idea of a continental glacier hold to the existence of local glaciers on the high lands more or less extensive during different portions of the great pleistocene submergence. They also believe in the extension of these glaciers seawards and partly water-borne, in the manner so well explained by Mattieu Williams; in the existence of those vast floes and fields of current and tide borne ice whose powers of transport and erosion we now know to be so great; and in a great submergence and re-elevation of the land, bringing all parts of it and all elevations up to five thousand feet suceessively under the influence of these various agencies, along with those of the ocean currents. They also hold that, at the beginning of the glacial submergence, the land was deeply covered by decomposed rock, similar to that which still exists on the hills of the southern states, and which, as Dr. Hunt has shown, would afford not only earthy debris, but large quantities of boulders ready for transportation by ice.

I would also remark that there has been the greatest possible exaggeration as to the erosive action of land ice. In 1865, after a visit to the Alpine glaciers, I maintained that in these mountains glaciers are relatively protective rather than erosive agencies, and that the detritus which the glacier streams deliver is derived mostly from the atmospherically wasted peaks and cliffs that project above them. Since that time many other observers have maintained like views, and very recently Mr. Davis of Cambridge and Mr. A. Irving have ably treated this subject.[1] Smoothing and striation of rocks are undoubtedly important effects both of land glaciers and heavy sea-borne ice; but the levelling and filling agency of these is much greater than the erosive. As a matter of fact, as Newberry, Hunt, Belt, Spencer, and others have shown, the glacial age has dammed up vast numbers of old channels which it has been left for modern streams partially to excavate.

The till, or boulder clay, has been called a "ground moraine," but there are really no Alpine moraines at all corresponding to it. On the other hand, it is more or less stratified, often rests on soft materials which glaciers would have swept away, sometimes contains marine shells, or passes into marine clays in its horizontal extension, and invariably in its embedded boulders and its paste shows an unoxidised condition which could not have existed if it had been a sub-aërial deposit. When the Canadian till is excavated and exposed to the air, it assumes a brown colour, owing to oxidation of its iron; and many of its stones and boulders break up and disintegrate under the action of air and frost. These are unequivocal signs of a sub-aqueous deposit. Here and there we find associated with it, and especially near the bottom and at the top, indications of powerful water-action, as if of land torrents acting at particular elevations of the land, or heavy surf and ice action on coasts; and the attempts to explain these by glacial streams have been far from successful. A singular objection sometimes raised against the sub-aqueous origin of the till is its general want of marine remains, but this is by no means universal; and it is well known that coarse conglomerates of all ages are generally destitute of fossils, except in their pebbles; and it is further to be observed that the conditions of an ice-laden sea are not those most favourable for the extension of marine life, and that the period of time covered by the glacial age must have been short compared with that represented by some of the older formations.

[1] *Proc. Bost. Soc. Nat. Hist.* xxii. *Journ. Geol. Soc. Lond.*, Feb., 1883.

This last consideration suggests a question which might afford scope for another address of an hour's duration,—the question how long time has elapsed since the close of the glacial period. Recently the opinion has been gaining ground that the close of the ice age is very recent. Such reasons as the following lead to this conclusion: the amount of atmospheric decay of rocks and of denudation in general, which have occurred since the close of the glacial period, are scarcely appreciable; little erosion of river-valleys or of coast-terraces has occurred. The calculated recession of waterfalls and of production of lake-ridges leads to the same conclusion. So do the recent state of bones and shells in the pleistocene deposits, and the perfectly modern facies of their fossils. On such evidence the cessation of the glacial cold and settlement of our continents at their present levels are events which may have occurred not more than six thousand or seven thousand years ago, though such time estimates are proverbially uncertain in geology. This subject also carries with it the greatest of all geological problems, next to that of the origin of life; namely, the origin and early history of man. Such questions cannot be discussed in the closing sentences of an hour's address. I shall only draw from them one practical inference. Since the comparatively short post-glacial and recent periods apparently include the whole of human history, we are but new-comers on the earth, and therefore have had little opportunity to solve the great problems which it presents to us. But this is not all. Geology as a science scarcely dates from a century ago. We have reason for surprise in these circumstances that it has learned so much, but for equal surprise that so many persons appear to think it a complete and full-grown science, and that it is entitled to speak with confidence on all the great mysteries of the earth that have been hidden from the generations before us. Such being the newness of man and of his science of the earth, it is not too much to say that humility, hard work in collecting facts, and abstinence from hasty generalisation, should characterise geologists, at least for a few generations to come.

In conclusion, science is light, and light is good; but it must be carried high, else it will fail to enlighten the world. Let us strive to raise it high enough to shine over every obstruction which casts any shadow on the true interests of humanity. Above all, let us hold up the light, and not stand in it ourselves.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

DR. MATTHEW HAY, assistant to the Professor of Materia Medica in the University of Edinburgh, has been appointed to the Chair of Medical Logic and Medical Jurisprudence in the University of Aberdeen, *vice* Prof. Ogston resigned.

THE constitution of the College for North Wales, which is to be established at Bangor, having been approved by the Education Department, arrangements are actively progressing for its opening in January, in order to secure the annual grant of 4000*l.* which has been offered by Government. As in South Wales, temporary premises will be acquired, and possibly the Masonic Hall, a commodious building lately erected by Major Platt, will be so utilised. Nothing definite is yet arranged as to the site of the College; but it is understood that Lord Penrhyn, who has evinced a very active interest in the movement, and to whom will probably be offered the honour of being first president, will afford every facility to the executive committee. About 30,000*l.* has been promised in subscriptions towards the building fund.

SCIENTIFIC SERIALS

Journal of the Russian Chemical and Physical Society, vol. xv. fasc. 6.—On the action of haloidhydric acids upon oxymethylene, by B. Tischenko.—On the constitution of the waters that accompany naphtha and are ejected by mud volcanoes, by A. Potilitzin.—On the formation of bromides of aromatic hydrocarbons by the action of bromine and bromide of aluminium on the volatile parts of naphtha, by G. Gustavson.—On the formation of tertiary alcohols by the method of Butleroff, by W. Markovnikoff.—On propyl-allyl dimethyl carbinol, by M. Putochin.—On the determination of carbon in cast-iron and steel, by G. Zabudsky.—On the decomposition of orthoclase by putrefied matter, by S. Meschersky.—Notes by W. Tikhomiroff and A. Lidoff.—On the application of centres of acceleration of a superior order to the parallelogram of Tchebycheff, by N. Joukovsky.—On the magnetic momentum of bundles of iron-wire, by P. Bakhmetieff.

Bulletin de la Société des Naturalistes de Moscow, 1882, No. 4.—New mints, especially the European ones, by M. Gandoger, being a description (in Latin) of forty-two new species of *Pulegium*, four species of *Preslia*, Opiz., and 135 species of *Mentha*.—On the arrangement of plants for keeping upright, and on the supply of water for exhalation, by V. Meschajeff, being a preliminary account (in German) of researches into the distribution and functions of the so-called mechanical tissue.—On the great comet 1882 II., by Th. Bredichin (in French).—Scientific results of the borings undertaken at Moscow for water supply and canalisation, by H. Trautschold (in German), being the result of twenty-three borings made at Moscow which have pierced the boulder-clay 0·6 to 8 metres thick, or alluvial sands in the valleys; a sheet of eluvium; the four Upper Jurassic layers of green sandstone with *Ammonites fulgens*, Aucella deposits with *Aucella mosquensis* and *Ammonites subditus*; black sand with *Ammonites virgatus*, and the usual black Jurassic clay which affords a compact and widely spread layer; a series of red and mottled clays, which may be Permian, underlie the Jurassic deposits and cover the Upper Carboniferous limestone.—Observations on atmospheric electricity at Murom, by N. Zvorykin.—New additions to the kinetic of liquids, by Th. Sludsky (both in Russian).—The European and Asiatic species of *Erirrhinus*, *Notaris*, *Scaris*, and *Dorytomus*, revised by J. Faust (in German).

Journal de Physique Théorique et Appliqué, August.—On a gravity barometer, by M. Mascart (three diagrams).—Description of a new form of equatorial telescope and its installation at the Paris Observatory, by M. Lœwy (one diagram).—On a synthetic apparatus for producing circular double refraction; on the radiation of silver at the moment of solidification, by M. J. Violle.—The index of refraction of Iceland spar, by M. E. Sarazin.—Selective absorption of solar energy, by Mr. Langley.—On an instrument for correcting gaseous volume, by Mr. A. Vernon-Harcourt.—Change in volume of hydrated salts under the action of heat; the corresponding chemical changes, by M. E. Wiedemann.

Archives des Sciences Physique et Naturelle (*de Genève*).—Memoirs on the new registering barometer in the meteorological observatory of Lausanne, by MM. H. Dufour and H. Amstein.—The structure of glaciers, by M. Ed. Hagenbach-Bischoff.—The rheolyser, by M. E. Wartmann.—On the rotation of polarisation of quartz, by MM. G. L. Soret and E. Sarazin.—Observations on cometary refraction, by M. W. Meyer.—On the amount of hail that fell during the storms of August 21, 1881, and July 13, 1788, and some remarks on the history of hail protectors, by M. Ch. Dufour.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 27.—M. Blanchard, president, in the chair.—A telegraphic despatch received by M. Dumas, through M. Pasteur, from the French Cholera Mission in Egypt, announces several important discoveries of a constant character, which will be communicated in detail later on.—New researches on the mode of action of the antiseptics used in staunching sores, by M. Gosselin. From experiments made on rabbits and frogs, it results that phenic acid, camphorated spirits, and similar solutions, are useful in two ways, partly by destroying germs, and thus preventing putrefaction, partly as astringents, by coagulating the albumen of the blood.—On the law of sequence in the evolution of the first vessels in the leaves of the Cruciferæ (second part), by M. A. Trécul.—Astrophotographic studies, by M. Ch. V. Zenger.—On the production of the fundamental telluric groups A and B of the solar spectrum by an absorbing layer of oxygen, by M. Egoroff.—Remarks on a fœtus which remained fifty-six years in its mother's womb, by M. Sappey.—On some methods for determining the positions of the circumpolar stars, by M. O. Callandreau.—On the measurement of time; a reply to the observations of E. J. Stone, by M. A. Gaillot.—On a formula relative to the velocity of waves; a reply to M. Gouy, by Lord Rayleigh. In the *Comptes Rendus* for May, 1882, M. Gouy, referring to Lord Rayleigh's correspondence in NATURE during the year 1881, recalls a memoir previously published by him in the *Comptes Rendus* for November, 1880, in which occurs the formula

$$U = \frac{dn}{dk} = \frac{d\frac{1}{T}}{d\frac{1}{\lambda}}.$$

To this Lord Rayleigh replies that this formula had already been given by him in the first volume of his work on "The Theory of Sound," published in 1877.—Researches on the groups of finite order contained in the group of the homogeneous quadratic substitutions with three variables, by M. L. Autonne.—On the absorption of the ultra-violet rays by the aqueous humours of the eye and by some other substances, by M. J. L. Soret.—On the measurement of the potential differences and resistances between electrodes, by M. G. Cabanellas.—A new method of preparing the oxychloride of phosphorus, by M. E. Dervin.—Researches on the influence of the recurrent nerves on the respiratory movements, and on the modifications of these movements under the influence of anæsthesia, by M. Laffont.—On a falling star observed at Lille on the evening of August 11, by M. Hequet.

VIENNA

Imperial Academy of Sciences, July 5.—T. V. Tanovsky, on amido-azobenzene-parasulphonic acid.—E. Meissl and F. Strohmann, on the formation of fat by hydrocarbons in the animal body.—A. Gehmacher, researches on the influence of the pressure exerted by the bark on the growth and structure of the tree.—L. von Frankl and C. Freund, on the atrophy of skeletal muscles.—E. Auer von Welsbach, on the earths of gadolinite of ytterbium.—T. Kachler and F. V. Spitzer, on oxy-camphor prepared from camphor-bibromide.—T. Wiesner and R. von Wettstein, researches on the laws of growth of vegetable organs.—S. Fuchs, the histogenesis of the cortex cerebri of man.—A. Lustig, the knowledge of the course of nerve-fibres in the spinal cord of man.—F. K. Ginzel, astronomical researches on eclipses (part 1).—E. von Fleischl, on the laws of nerve irritability (part 7): on the irritability of currentless nerves.

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