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THURSDAY, OCTOBER 27, 1870

THE REPRESENTATION OF SCIENCE AT THE SCHOOL BOARD

AN impression seems to prevail that only those persons should be placed on the Metropolitan School Board who are already acquainted with the details of education. Against this principle we protest. We hope the new schools will be great improvements upon those which already exist. When we are told by the Bishop of Manchester that a third of these schools only are efficient, that a third are inefficient, and that a third are wholly useless, if not pernicious, it is high time that the whole system should be looked into by those who will come fresh to the inquiry, unencumbered with the ideas that have led to such disastrous results. We think, then, the public should look to that instructed body of men who are known as cultivators of science to represent them on the London School Board. Already we are glad to see signs that the class of persons we have named have found favour in the eyes of London electors. The selection of Professor Huxley and Dr. Elizabeth Garrett, as candidates for Marylebone, is highly creditable to that district of the metropolis; but their hands must be upheld by a very much larger number of candidates, if common sense and intelligence are to prevail at the councils of the School Board.

The points to which we think the earliest attention of the School Board ought to be directed, and in which men of science are likely to give the greatest assistance, are the following :—

1. *The sanitary condition of the schools.*—It is well known that in many cases our schools are foci of contagion, and the means of spreading contagious diseases. Children are frequently sent to school, not only from families where contagious diseases are present, but actually with disease upon them. No trouble should be spared to prevent this, and if necessary a short clause should be added by Act of Parliament to the Education Act, in order to punish those who in this way are the means of spreading around the destructive diseases. Nor is this all that is required. The school-rooms should be well ventilated, clean, and not overcrowded. Every Government school should be placed under the superintendence of the medical officer of health of the district in which it is placed, and he should report periodically to the School Board on the state of the school and on any departure from sanitary rules. Cleanliness should especially be encouraged and insisted on amongst the children attending the school, and if no means exist at home, baths and lavatories should be provided at the schools.

2. *The times of study.*—It is a fact well known to the physiologist, that the attention of the human mind can only be given with success to a particular subject for a limited time. The younger the brain is, the less the time during which knowledge can be taken in or retained. In opposition to these obvious facts, children are kept at their studies or in school for much longer periods than they can successfully learn. The consequence is that they remain in the close school-room whilst they ought to have been in the yard at play. This system is doubly wasteful, for both health and learning are sacrificed. The whole system of hours of study, and of play or of work, requires to be revised in our primary schools. The importance of play-grounds in the open air can hardly be overrated. It is only the practical physiologist who can appreciate the real value of muscular exercise, and the influence of fresh air from time to time during the day, to enable children to pursue their studies with success.

3. *The course of studies to be pursued.*—Here is where the Augean stable of a past education needs to be purified. The notion that when a child has learned to read, write, and cipher, he is educated, must be eradicated. These are at best but means, and are only the instruments by which education is conducted. It will be for the man of science to show his colleagues on the School Board that perhaps the better half of a liberal education may be obtained without books at all. This is the error that lies at the foundation of all our systems of education, whether conducted in our highest, middle-class, or national schools. The education of the senses by which the man is to get his living and to perform his duties in life is entirely neglected. Where attempts have been made to introduce the study of the natural sciences, it has been done solely by the aid of books, and not with that demonstration of the facts to the senses which is the only way in which such knowledge can be made useful. In a word, henceforward there must be a portion of every day taken up with teaching children by objects, specimens, or experiments, the nature of the great laws by which the universe is governed. We cannot argue here on the necessity for this knowledge. Look at that great German army, recently spoken of as the most wonderful military engine ever seen on the face of the earth. What makes it so? The intelligence of each individual of which it is composed. It is the same with wheels and pistons, spindles, hammers, chisels, and ploughs, as with guns and bayonets: the more intelligent the man is who wields or superintends them, the more successfully and prosperously will they do their work. Ten years ago Mr. Whitworth astonished the Manchester manufacturers with the account of the machines he had seen in America. "Why should we not have such machines here?" said the Manchester men. "Because," said Mr. Whitworth, "you have not intelligent hands to work them." And for these long ten years we have gone on talking about educating our working classes, and allowing priceless treasures to pass out of our hands. Every portion of Europe, as well as the United States of America, is stealing something of our rightful wealth and increasing our pauperism, because of our stolid indifference to the introduction of those branches of human knowledge which alone can properly develop the powers of industry and application, of which the English people are so wonderfully capable.

This great question of the introduction of Natural Science into all schools must be taken up by our School Boards throughout the kingdom. To delay it is to shelve it, and to commit an irretrievable error. It is now or never. If the present opportunity is neglected, all is lost. Let no heed be given to the cry that it is impossible to find teachers. If teachers cannot be found they must be made, and all old teachers must be told that unless they qualify in this respect they will be of no use. The cry of the example of our Universities must not be listened to.

We have nothing to do here with their failure to teach Natural Science, and thus to mislead where they ought to have led. What we now ask the people of England, and especially the people of London, is to put Men of Science on their School Boards.

E. LANKESTER

THE GLACIATION OF BRAZIL

Thayer Expedition: Scientific Results of a Journey in Brazil, by Louis Agassiz and his travelling Companions. Geology and Physical Geography of Brazil. By Ch. Fred. Hartt, Professor of Geology in Cornell University. With Illustrations and Maps. (Trübner and Co.)

THIS thick volume of 620 pages is the result of two visits to Brazil, the first with the Thayer Expedition, the second during a vacation holiday of "some months." The author has proposed to combine with his own personal observations all the information on this subject obtainable from other sources, and thus give a complete view of the present state of our knowledge of the geology and physical geography of this vast and interesting region. The design is an admirable one, but the execution of it is, in some respects, disappointing.

The first great fault of the book is, that it has been swelled by the introduction of much irrelevant matter. Mr. Hartt's own journeys were mainly along the coast, from Rio Janeiro to the Amazon, with occasional trips of a hundred miles or so into the interior, and he inflicts upon us pages of unimportant detail on the topography of small rivers, creeks, and harbours, which have no bearing on the geology or physical geography of the country. Detailed descriptions of the marine animals and fossils collected would also have been better in an appendix than in the body of the work where they are given. The arrangement of the book, too, is faulty, since it treats of the provinces of Brazil in succession, and makes no attempt to indicate the great physical divisions of the country, and there is not a single geological or physical map of Brazil, or of any part of it; the maps alluded to in the title-page being mere outline or sketch maps of small districts, or plans of harbours and mouths of rivers. Another strange defect is the absence of all measurement of heights. The author travelled without barometer or aneroid; he, consequently, everywhere roughly estimates his heights, and gives no sections, but a few "ideal" ones. Notwithstanding the bulk of this volume, it does not complete the geology of the voyage, for we are informed that Mr. St. John, another geologist attached to the expedition, who travelled more in the interior of the country, will give the results of his observations in a separate work.

But although we have thus plainly indicated the defects of the book, there is much valuable matter to be found in it. The author has been very diligent in examining all the chief authorities on Brazil, and has extracted from them most of their geological matter; and among the extracts from Spix and Martius, Prince NeuWied, Darwin, Gardner, Halfield, and others, are to be found many interesting passages descriptive of the peculiarities of the scenery and geology of the country. The chapters on the coral reefs of the Abrolhos and on the gold mines of Brazil, the account of the exploration of the bone-caves by Lund, and the appendix on the Botocudos Indians, will furnish some interesting matter for the general reader, while the student of science will obtain (though with some difficulty) a notion of the general physical and geological characteristics of an almost unexplored region.

The most striking geological feature of tropical South America east of the Andes is the enormous extension of gneissic rocks, which appear to form the whole foundation and much of the surface of the country, from the cataracts of the Orinooko to Paraguay and the southern frontier of Brazil. All the great mountain tracts of Brazil and Guiana, as well as the low plain which separates the watersheds of the Orinooko and Amazon, are of this rock, which is considered to be of Laurentian age. Its characteristic features are the great dome-like masses and the conical peaks or pillars, generally of more or less smooth and rounded outlines, a peculiarity dependent on the decomposition of all exposed surfaces, which fall away in concentric flakes. Great hemispherical domes up to a thousand feet in diameter are one of the results of this decomposition wherever a more resisting mass has occurred. Still more extraordinary are the vertical pillars of rock, that rise up at intervals out of the forest to some hundreds, or, in the case of the Pedra lisa, in the province of Rio de Janeiro, to more than three thousand feet high. Similarly formed peaks or pillars in Fernando, Noronha, and St. Helena have been formed by injections of fluid felspathic lava. What an enormous amount of denudation do these isolated pillars indicate!

In South Brazil a few tracts of Silurian and Carboniferous rocks occur, but the next formation of any extent is the Cretaceous, which consists of sandstones, generally upheaved and fractured. Other sandstones, which cover an immense extent of country, and form the ranges of flat-topped hills from one to nearly three thousand feet high, called *taboleiros*, are in perfectly horizontal strata, and as these lie unconformably on the cretaceous rocks they are presumed to be tertiary, although no fossils have yet been found in them.

We now come to a very wide-spread, yet recent and superficial deposit, which is at once the most puzzling and the most interesting feature in Brazilian geology. This is a layer of clay or loam, varying in thickness from a few feet to one hundred, and wrapping in its folds hill and valley, over vast tracts of country, including the steep slopes and summits of some of the highest mountains. All Rio de Janeiro, and all the coast provinces visited by our author, were thus covered. It has been described in Minaes Geraes and San Paulo, and Prof. Agassiz has observed it in all the northern provinces as far as the Amazon valley. It covers alike the gneiss and the tertiary formations. This clay is of a red colour, and is evidently formed of the materials of the adjacent and under-lying rocks, but ground up and thoroughly mixed. There is never the least sign of stratification throughout its mass, although it very frequently rests on a thin layer of quartz pebbles. It contains, scattered through it, rounded and angular boulders of quartz, gneiss, and other rocks, and the surfaces upon which it rests are always more or less smooth and rounded. Our author always speaks of this formation as "drift," and he agrees with

Prof. Agassiz that its peculiarities are such as unmistakeably to indicate its glacial origin.

This is truly a startling conclusion, and one which has hitherto been received in this country with some incredulity. Prof. Agassiz was thought to be glacier-mad; but if we separate his theories from his facts, and if we carefully consider the additional facts and arguments adduced by Mr. Hartt in this volume, we shall be bound to conclude that, however startling, the theory of the glaciation of Brazil is supported by a mass of evidence which no unprejudiced man of science will ignore merely because it runs counter to all his preconceived opinions.

Mr. Hartt's facts and deductions have the more weight, because he is evidently not very enthusiastic on the subject, and because he fairly states the sources of error in observation, and fully discusses such other modes of explaining the facts as have been proposed. He acknowledges that in some cases the decomposed gneiss cannot be distinguished from the "drift;" but he shows that in the former the materials remain *in situ*, especially the quartz veins, while in the latter all are mixed and ground up together; and wherever the two are seen in contact for any distance, the sudden cutting off of the quartz veins at the drift, and other well-marked characters, render it impossible to confound them. He also adduces several other phenomena which are strongly indicative of a glacial origin. Both in the Orang Mountains and in Bahia there are valleys with no outlet, and in Alagoas there are many deep lakes in rock-basins. In the province of Bahia there are extensive bare, elevated, rocky plains thickly strewn with angular blocks of stone, some of which are erratics, and *exactly resembling the drift-covered plains of the north*. On similar elevated plains, far removed from any higher land, Mr. J. A. Allen (another member of the Thayer Expedition) found numerous deep and smooth pot-holes worn in solid gneiss. They were of various sizes, the largest seen being elliptical, eighteen feet long by ten wide, and twenty-seven feet deep. Similar pot-holes are known to be formed by glacial waterfalls, and they are found over the glaciated regions of New Brunswick and Nova Scotia. Heaps of débris, exactly resembling glacial moraines, have also been found both in the south and north of Brazil. Mr. Hartt is satisfied of their resemblance to true moraines in the valley of Tijuca near Rio, and Prof. Agassiz has described others still more perfect in Ceara, only four or five degrees south of the equator. After describing these in detail, he concludes: "I may say that in the whole valley of Hasli there are no accumulations of morainic materials more characteristic than those I have found here, not even about the Kirchet; neither are there any remains of the kind more striking about the valleys of Mount Desert in Maine, where the glacial phenomena are so remarkable; nor in the valleys of Loch Fine, Loch Awe, and Loch Long, in Scotland, where the traces of ancient glaciers are so distinct." It can hardly be maintained that the discoverer of glacial phenomena in our own country, and who has since lived in such a pre-eminently glaciated district as the Northern United States, is not a competent observer; and if the whole series of phenomena here alluded to have been produced without the aid of ice, we must lose all confidence in the method of reasoning from similar effects to similar causes which is the very foundation of modern geology. The only objection of any weight that has been made to this interpretation of the phenomena, is the fact of the absence of glacial striæ; but Mr. Hartt states that no striæ or polished surfaces have yet been found even in the extreme south of the continent where the glacial phenomena are undisputed. It is at best a negative argument, and as such cannot neutralise those of a positive nature. We must also remember that we have no indication of the age of the Brazilian or southern glacial epoch. It may have occurred much earlier than that of the northern hemisphere, and the greater lapse of time, combined with the powerful decomposing and denuding agency of tropical rains, may have obliterated most of such marks.

The physical difficulty of the conception of tropical glaciers may be resolved into the question of whether a subsidence to the extent of ten or twelve thousand feet may not have subsequently occurred; since a greatly increased elevation at a time when a severe glacial epoch reigned in the south temperature zone, would probably lead to the glaciation of the southern tropics down to what is now the sea-level.

A much more serious objection seems to be the biological one. If the whole surface of what is now Brazil was recently covered with a sheet of ice, whence has arisen the wonderfully rich and varied, and, in many respects, peculiar flora and fauna that now inhabit it? Judging from the map of the Atlantic given in Maury's "Physical Geography of the Sea," a rise of twelve thousand feet would only add a belt of about four hundred miles in width to tropical America; but a great part of this might have enjoyed a truly tropical climate, just as the valleys of Switzerland have a warm summer though in the immediate vicinity of glaciers. It seems probable, also, that the glaciation was a southern one, and did not extend far north of the equator, if it even reached so far, so that the whole of Venezuela and Guiana, with the additional belt of land due to elevation, might have been even more luxuriant and more densely populated than at present. There would thus have been an ample surface to support the ancestors of the existing fauna and flora of Brazil during the glacial epoch, just as there was sufficient land in Europe to support the ancestors of the existing European fauna and flora even when so much of the present surface was covered by a thick mantle of ice.

It must be stated that Mr. Hartt does not accept Prof. Agassiz's extraordinary hypothesis (which rests on a very slender basis of fact) of a great Amazonian glacier. He believes that the wide-spread beds of clays and sandstones, which Prof. Agassiz classes as glacial, are marine, and states that they agree perfectly with the tertiary beds in other parts of Brazil. The patches of drift, with erratics in the Amazon valley, may well have been produced by detached masses from the glaciers of the Andean and Brazilian highlands, which melted and deposited their load of drift in the warm waters of the ancient Amazon.

We have devoted so much of our space to this question of the Glaciation of Brazil, in the hope of attracting the attention of geologists to a country which offers such an interesting subject of inquiry, and which it is so easy and agreeable to explore. The facts, as stated by two careful observers, both thoroughly experienced in glacial phenomena, are undoubtedly such as to warrant the main conclusion drawn by them; and it is to be hoped that geologists will not ignore the facts because the conclusions seem improbable, as they so long ignored facts proving the antiquity of man for no other reason.

A. R. WALLACE

MODERN ANGLING

The Modern Practical Angler. A complete Guide to Fly-Fishing, Bottom-Fishing, and Trolling. By H. Cholmondeley-Pennell, Inspector of Fisheries. Illustrated by Fifty Engravings of Fish and Tackle. 16mo, pp. 286. (London: Fred. Warne and Co., 1870)

TO those readers of NATURE who are not acquainted with Mr. Pennell, the following quotation may serve as an introduction: "Fishing has been in a special sense my mistress—the fairest and most loving wife—in many a wild and lonely spot where, but for her gentle companionship and solace, I should have felt myself in every sense of the word alone;" whilst those of us who have for some time had an acquaintance with his writings, know that in making this confession he is perfectly sincere, and that he is one of the most devoted disciples of Izaak Walton; so that we cannot help wishing he were an "Inspector of Fisheries" (as he describes himself on the title-page), instead of being appointed by the Government to investigate the causes of failure and possibilities of improvement of our *oyster* fisheries. His book has only a partial resemblance to Walton's "Complete Angler." Those passages of pleasing simple eloquence, those fine sentiments, those virtuous precepts, in short, all those characteristics which have rendered Walton's book immortal, must not be looked for in Mr. Pennell's "Modern Angler." To imitate Walton successfully, would, indeed, require a genius of no common order; and Mr. Pennell has contented himself with giving a mere manual of the piscatorial "art" and "science" (we must not be too severe with enthusiasts about terms); and judging of it as such, we can sincerely say that it is the best and most useful handbook we have yet seen.

The book is divided into four parts, treating minutely of tackle, fly-fishing, trolling or pike-fishing, and bottom-fishing. The author takes credit for several inventions or improvements. Thus, for instance, he describes or figures the "Pennell-hook," in which "the medium between theoretical and practical requirements" is believed to be hit. We are glad to see him advocating a reduction in the number of artificial flies used at present; he proposes to substitute six typical flies, three for salmon and grilse, and three for trout, grayling, &c. We feel sure that these flies, together with those which are especially used at certain localities, will be quite sufficient for all purposes. Mr. Pennell has thought a great deal at the river-side; he is never satisfied with simply describing what, according to his experience, has proved to be the most successful method or the deadliest instrument; he always gives the reasons. Thus, in one of the chapters, we find expounded the "true theory of trout-flies," in a seconn the "theory of salmon-flies," and in a third, of white trout-flies; however, we are afraid that in expounding theories he will not be more successful in convincing his readers than the majority of theorists. For instance, to the question, For what does the salmon take the artificial fly? he gives the answer, "For its beauty and tempting appearance; probably it has an appetising effect." Let Mr. Pennell once watch a prawn (one of the principal articles of food of salmon in the sea) swimming in jerks through the water, and he will at once perceive that by means of our rod we impart to the fly the peculiar motion of the prawn, whilst the iridescence of the real creature is reproduced by the colours of the fly, which must vary according to the physical changes of the sky and water. No two things can be more unlike than a prawn and a dry artificial fly; no two things are more alike than a swimming prawn and that same fly in the water worked by a skilful hand.

But we must conclude our notice of this book, welcome and useful to every class of anglers. It is illustrated by numerous well-executed woodcuts, which are more instructive than the best descriptions. Lithographic plates of some of the more common freshwater fishes are evidently reproductions from the *Fisherman's Magazine.*

A. GÜNTHER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Government and the Eclipse Expedition

WE are now within two months of the date fixed by Nature, whose name you so worthily wear, for a total Eclipse of the Sun, and it is not probable that she will postpone her appointment for a period sufficient to enable the joint committee of the Royal and Astronomical Societies to renew and succeed in their endeavour to teach the Government what such a phenomenon may, if duly observed, contribute to human knowledge. It may be taken for granted that no encouragement will be afforded by our thrifty rulers to an expedition of sixty-eight astronomers, projected for the quixotic purpose of collecting intelligence not calculated to increase the revenue.

This well-timed and praiseworthy frugality reminds me of some imputations which were, not long ago, cast at the Government by two eminent men of science, one of whom, the Astronomer Royal, lives to repent his injustice; the other, the late Dr. W. A. Miller, alas! can speak no more.

On the 31st March last, I read at a Society of Arts' conference, by request of the Council, a paper on the subject of the Relations of the State to Science. A discusssion followed, in the course of which the Astronomer Royal remarked that "having had a somewhat long connection with the Government, he was quite competent to say that there had never been any unwillingness, as far as instances occurred to him, to promote liberally the purpose of speculative science when brought before the Government, with a good cause shown, and upon the responsibility of some person in whom they placed confidence."

Here Mr. Airy lays down, with his usual clearness, the conditions necessary to induce our Government to promote liberally speculative science, namely, "a good cause shown, and on the responsibility of some person in whom they placed confidence." Our failure, then, to obtain aid on the occasion in question must have been due to one of three things—either a good cause was not shown, or the Government had no confidence in the persons showing it, or the Government differs considerably from Mr. Airy's portrait of it. Who were the persons whose worthiness of confidence is thus doubtful? The Astronomer Royal himself, the President of the Royal Society, and the President of the Astronomical Society; and these untrustworthy beguilers of our too liberal guardians of the public purse were the accredited representatives of a select joint committee of the two first scientific societies in the kingdom. As to the goodness of the cause, that is to be inferred from the character, requirements, and position of the three personages above indicated, who had the presumption to advocate it. To these two causes it is evident that the failure is due, and not to any want of liberality in statesmen, for whose readiness to promote speculative science Mr. Airy himself, one of the unsuccessful petitioners, has vouched.

The late Dr. Miller spoke as follows on the occasion above referred to :—"Gentlemen," he observed, "representing great branches of science, like the Astronomer Royal, were met by the Government with liberality; and scientific bodies, such as the Royal Society, when they made an application, after careful consideration of the objects they had in view, were also listened to with respect, and, in the majority of instances, their applications were granted. It appeared, therefore, that the great difficulty on the part of the Government was to be sure that what they expended was wisely laid out."

Dr. Miller, as Treasurer of the Royal Society, spoke with authority; he shared with the Astronomer Royal the error of imputing to Government a liberality towards science utterly inconsistent with that true economy for which they have, in so many other matters, earned high renown.

The whole transaction, considered as the latest notion in civilisation, is so ludicrous that it is difficult in speaking of it to do so seriously. It may be argued that Mr. Airy and Dr. Miller were perfectly correct in their picture of former Governments, but that our present Ministry act on different principles, and refuse to promote what their predecessors would have thought worthy objects. This hypothesis is so extremely disparaging to the Ministers of to-day, that I for one cannot adopt it. When the speeches I have cited were made I offered no reply, but I was far from assenting to the estimates of Government liberality to science in England given by Mr. Airy and Dr. Miller. I do not now for the first time say that, not only have English Governments, past and present, been backward in promoting science, but that no English Government has ever shown that it understood the value of science—how it should be promoted, or what were their duties respecting it. This inability to realise the requirements of Science is not confined to Governments—it largely pervades the most intelligent portion of the community—the whole governing class. For one man of high position and culture who comprehends the mode of advancing science, and the certain effects of advancing it, there are, not hundreds, but thousands, who have clear views regarding art, literature, and general education. Every man of wealth covers his walls with pictures and books, but how many equip a laboratory or an observatory? The knowledge of science which most educated men possess has been acquired by reading only, and their notions of what is needed to advance science, of the extent of field remaining still unoccupied, of the means requisite for cultivating it, and of the value of the produce, are, we find from every-day experience, exceedingly hazy.

The true explanation of the difference between the distinguished speakers and those who think with me I believe to be this—that the demands made upon the State by scientific men and bodies have hitherto been so moderate, so desultory, and so infrequent, that few of them could, with decency, be denied by men not utter barbarians. I am of opinion that the moderation of scientific men has been exceedingly detrimental to the nation, and utterly opposed to true economy. For it has led to the waste of the two most valuable gifts of Providence—the human intellect and the forces of nature. This moderation has encouraged the comfortable creed of English statesmen that private enterprise will do all that is needful, and that the utilisation of those two gifts forms no part of their national duties. But suddenly those who have suffered Government to hug itself in its inaction, and live in a fool's paradise of indifference to duties which they do not understand, are startled by their own teachings recoiling on themselves in a rebuff unequalled for its narrow-mindedness since the time of Galileo.

But a new school of scientific men is now astir, and this rebuff will quicken its energies and so do good. A Royal Commission on Science now sits, and at such a tribunal this wrong to Astronomy must be heard and judged. Let us hope that when next the sun is eclipsed, the darkness on the minds of men may not equal the darkness on the earth's face.

ALEX. STRANGE, Lieut.-Col.

The Geological Bearings of Recent Deep-Sea Explorations

YOUR report of the Proceedings of the Geological Section of the British Association (No. 51, p. 503) makes Sir Roderick Murchison say that "he hoped Mr. Jeffreys did not share the opinion of his colleague Dr. Carpenter, that their discoveries tended to upset modern geology." I have the authority of Sir Roderick to state, that he did not accuse me of any such absurdity; and that I should find what he did say on that occasion, in dissent from some of the views put forth by Prof. Wyville Thomson and myself, fully expressed in his introductory address, of which he has given me a copy. As, however, he there attributes to a passage in a lecture which I delivered eighteen months since at the Royal Institution, a sense which I never meant it to convey, I shall be obliged by your allowing me to give a precise explanation of my meaning.

The passage cited by Sir Roderick is as follows :—"The facts I have now brought before you, still more the speculations which I have ventured to connect with them, may seem to unsettle much that has been generally accredited in Geological Science; and thus to diminish rather than to augment our stock of positive knowledge; but this is the necessary result of the introduction of a new idea into any department of scientific inquiry."

Now I gave not the remotest hint of impugning those great doctrines of Stratigraphical and Palæontological succession, to which Sir Roderick refers as accepted by Uniformitarians, Catastrophists, and Evolutionists alike; my chief heresy being the indorsement of the doctrine of which my colleague, Prof. Wyville Thomson (himself a sound and accomplished Geologist), was the originator, "that we may be said to be still living in the Cretaceous Epoch." Our meaning was this :—There can now be no question that a formation, corresponding with the Chalk of the Cretaceous Epoch, alike in its material, and in the general character of its Fauna, is at present going on over a large part of the North Atlantic Sea-bed. This similarity is marked, not by the occurrence of a few types of life (like the Lingulæ and Terebratulidæ of the older formations, referred to by Sir Roderick Murchison), but by the persistence of those which constitute the formation itself, viz., the Globigerinæ, the Coccoliths and the Coccospheres; as also of numerous types of Echinodermata that were formerly considered as essentially Cretaceous, and of a great variety of those Sponges (including Xanthidia), and Foraminifera, whose abundance in the White Chalk is one of its most important features. The explorations carried on by the United States Coast Survey in the Gulf of Mexico have furnished results entirely coinciding with our own in many of these particulars.

Now it is, of course, quite open to any geologist to maintain that this Formation is a mere repetition of the Cretaceous, at a later date, under generally similar conditions. Such was, I presume, the idea of those who, several years before our researches began, had pointed out the conformity of the material of the Atlantic deposit with that of the old Chalk; and such had been my own belief, until Prof. Wyville Thomson suggested to me the probability of a continuity between the past and the present deposits, on the following ground :—The oscillations of the earth's crust, in the Northern part of the Northern Hemisphere,* during the whole Tertiary period, have not been shown anywhere to exceed 1,500, or at the most 1,800 feet, or 300 fathoms; and as the general depth of the North Atlantic Sea-bed ranges from twice to ten times that amount, there is no reason to suppose that the formation and accumulation of Globigerina-mud have been interrupted in any part of its duration. Now the termination of the Cretaceous Epoch is commonly regarded as having been marked by the elevation of the Cretaceous deposits of the European area into dry land; but there is no evidence that this change of level stopped the formation of Chalk in the deep sea elsewhere. On the contrary, according to the received doctrine of Geology, it is highly probable that coincidently with the elevation of the European area, there was a gradual subsidence of what is now the North Atlantic Sea-bed; so that the Globigerinæ and Coccoliths of the former area, with such accompanying types of animal life as could accommodate themselves to the change of conditions, would progressively spread themselves over the latter.

Now there is nothing more heterodox in this view than in M. Barrande's doctrine of "colonies," which is now, I believe, universally accepted as the explanation of a large and very important series of geological facts—the persistence, in certain outlying localities, of a Fauna characteristic of a formation stratigraphically inferior to that in which it presents itself. The only difference here is in the relative extent of the existing Cretaceous deposit in the North Atlantic, which may hold to that of Europe somewhat the relation that the English-speaking race wh'ch has colonised America does to that of the mother country, instead of

This statement has been recently met by our friend Mr. J. Gwyn Jeffreys, who adverts to the well-known fact of the elevation of Tertiary strata in the South of Euaope to 11,000 feet. But there is no evidence, so far as we know, of any such elevation in the latitude of Great Britain, or north of it.

that which the Norse-speaking Icelanders hold to the modern Scandinavians.

If the facts be as I have now stated them, the *onus probandi* seems to me to lie upon those who affirm that a complete stop was put to the formation of Chalk before the commencement of the Tertiary period. If, on the other hand, the continuity of the existing Chalk-deposit with that which formed the Chalk of Dover Cliffs be admitted, the question, whether we can be rightly said to be "still living in the Cretaceous Epoch" seems to me one of terms rather than of essentials.

That we could not expect to find the Cretaceous Fauna *as a whole* in our modern Chalk, is evident from the considerations admirably set forth in a parallel case by the President of the Linnean Society, in his last Annual Address.

The difference is undoubtedly most marked in the *Mollusca;* only one shell, the *Terebratula caput-serpentis*, having at present been shown to be common to the Cretaceous and the Modern period. But the positive evidence of continuity afforded by the persistency of all the types which *make* the chalk, as well as of numerous forms of Echinoderms, Sponges, and Foraminifera which are among its characteristic inhabitants, appears to us to outweigh the negative evidence afforded by the l rger amount of change that has taken place in the Molluscan Fauna, for which it would not be difficult to assign probable reasons.

Further, it is to be borne in mind that the successive beds of the Cretaceous formation differ from each other in a very marked manner; so that we could not expect to find, in any one deposit, more than a small part of that *ensemble* which is commonly spoken of as "the Cretaceous Fauna." What we mean by the expression to which Sir Roderick Murchison has taken exception, is simply that the facts and deductions we have brought together justify the assumption of the continuous prevalence of the same general Physical and Biological conditions, in the deep sea that separates the northern parts of the European and American continents, from the time when the Chalk of those continents was raised into dry land to the present date. This is perfectly compatible with those changes in the conditions of the shallower portions, which have given origin to the long succession of Tertiary deposits.

Passing from this topic, I now proceed to other points on which the researches of Prof. Wyville Thomson and myself appear to us to invalidate Geological doctrines that have gained general currency.

Up to the commencement of the recent exploration of the Deep Sea bottom by means of the Dredge, the doctrine propounded by Prof. Edward Forbes as to the limitation of Animal Life to a depth of 300 or 400 fathoms, and the consequently *azoic* character of all deposits formed at depths exceeding that amount, was generally accepted by Geologists; partly on account of the deservedly high authority of its originator, and partly because it appeared to afford a simple explanation of phenomena which had long perplexed Geologists and Palæontologists, viz., the occurrence at various epochs of vast accumulations of sedimentary strata apparently altogether devoid of organic remains. The indications obtained by the Sounding apparatus, of the existence, not merely of humble Foraminifera, but of Annelids, Echinoderms and Crustacea, at depths far exceeding Edward Forbes's limits, were not generally accepted, either by Zoologists or Palæontologists, as indicating the presence of a varied and abundant Fauna on the ocean bottom; for although Dr. Wallich, with a sagacity to which I have uniformly endeavoured to do full justice, had argued that they should be, it was specially noticed that these researches gave no evidence of the existence, at great depths, either of Mollusks or of Zoophytes—the two groups whose fossil remains are usually of the highest Palæontological significance.

Now the Dredgings which were carried down in the *Lightning* Expedition of 1869 to 650 fathoms, and in the *Porcupine* Expedition of 1869 to 2,435 fathoms, have established beyond all reasonable question that a varied and abundant Fauna may exist on the sea-bottom without any limit as to *depth* and *pressure;* and they have further rendered it probable that, putting aside those Animals which are necessarily restricted by the nature of their food to the depth to which living Vegetation extends, a large proportion may accommodate themselves by gradual modification to any amount of *change* in depth and pressure; so that the assumption that the occurrence of particular types is significant of the depth at which a formation was deposited, can no longer be upheld, except in the case of animals essentially littoral. For example, no doctrine has been more generally accepted than that of the limitation of the Pedunculate Crinoids to comparatively shallow water; the large West Indian representatives of that group being found growing on coral reefs, and a like *habitat* having obviously been peopled by them in the Carboniferous epoch. Yet, in the *Porcupine* dredgings of the present year, a large Pentacrinus, closely allied to the West Indian species, has been obtained near the coast of Portugal from a depth of about 800 fathoms; and the little Rhizocrinus, with another small Apiocrinoid, which I hope soon to describe under the name of Wyvillocrinus, were found last year, the former at 862 fathoms, the latter at 2,435.

Further, the *Lightning* and the *Porcupine* dredgings have fully established the position that the distribution of marine life is much more closely related to the *temperature* of the ocean-bottom, than to its depth. This is most clearly evidenced by the results of the careful exploration of the Channel of from 500 to 650 fathoms depth, which separates the plateau that supports the northern extremity of Scotland from the Faroe Banks. For we have shown that, whilst the *surface*-temperature of this Channel is everywhere nearly the same, and indicates a derivation of its upper stratum from a warmer source, a considerable part of the deeper portion of this Channel is covered by *a frigid stream*, bringing a temperature as low as 29·5° from the Arctic Ocean; this stream having in some places a depth of 2,000 feet. Thus the bottom of this Channel is divided into a *warm area*, on which the bottom-temperature at depths of from 500 to 600 fathoms is about 45°, and a *cold area* on which the bottom-temperature at like depths is 30°, or even lower. We have traced these two areas at corresponding depths within about twenty miles of each other; and where the bottom was unequal,—the slope of the plateau at the edge of the cold area, or of a bank in its midst, raising its bottom out of the *cold* stream into the *warm* which overlies it—a difference of 18·5° was found within *eight miles*. No contrast could well be more striking than that which presented itself between the Faunæ of these two areas. The Globigerina-mud was rigorously limited to the *warm;* and of the animals living on its surface a large proportion were characteristic of the warmer-temperate seas. The bottom of the *cold* area consisted of sand and stones; and of the animals which were abundantly distributed over it, a large proportion were essentially Boreal. In the shallower portions of the cold area, where an intermediate bottom temperature prevailed, an intermixture of the two Faunæ, corresponding with the border position of this area between the Temperate and the Boreal provinces, was readily traceable.

Here, then, we have the remarkable fact that two deposits may be taking place within a few miles of each other, at the same depth and on the same Geological horizon (the area of one penetrating, so to speak, the area of the other), of which not only the Mineral character but the Fauna are alike different;—that difference being due on the one hand to the direction of the current which has furnished their materials, and on the other to the temperature of the water brought by that current. If the *cold* area were to be raised above the surface, so that the deposit at present in progress upon its bottom should become the subject of examination, by some Geologist of the future, he would find this to consist of a Sandstone formed by the disintegration of older rocks, the Fauna of which would in great degree bear a Boreal character: whilst if a portion of the *warm* area were elevated at the same time, the Geologist would be perplexed by the stratigraphical continuity with the preceding of a Cretaceous formation, the production of which entirely depends upon the extensive development of the humblest forms of animal life under the influence of a higher temperature, and which includes not only an extraordinary abundance of Sponges, but a great variety of other animal remains, several of them belonging to the warmer-temperate regions. He would naturally suppose these widely different climatic conditions to have prevailed at different periods, and would probably have had recourse to the hypothesis of a "fault" to account for the phenomenon. And yet these Formations have been shown to be going on together, at corresponding depths, over wide contiguous areas of the sea-bottom; in virtue solely of the fact that one area is traversed by an Equatorial, and the other by a Polar current. Further, in the midst of the land formed by the elevation of the Cold area, our Geologist would find hills some 1,800 feet high, covered with a Sandstone continuous with that of the land from which they rise, but rich in remains of animals belonging to a more temperate province; and might easily fall into the mistake of supposing that two such different Faunæ occurring at different levels must indicate two distinct climates separated in time; instead of indicating, as they have been shown to do, two contemporaneous but dissimilar climates, separated only by a few miles horizontally and by 300 fathoms vertically.

But further: the Temperature-soundings taken in the *Porcupine* Expeditions of 1869 and 1870 have conclusively shown that a temperature as low as 36·5° prevails over the deeper parts of the North Atlantic sea-bed; this reduction being due to the pervasion of Arctic and Antarctic waters, which come to replace the superficial flow of Equatorial water (as in the Gulf Stream and other currents) towards the Polar areas. In conformity with this depression of temperature, many species of Mollusca, Crustacea, and Echinodermata, formerly supposed to be purely Arctic, have been found to range southwards in deep water as far as the Straits of Gibraltar; and we have shown it to be highly probable that an extension of the same mode of exploration would bring them up from the abysses of even intertropical seas, over which a similar climate prevails, and that an actual continuity may thus be found to exist between the Arctic and the Antarctic Faunæ. This idea was well put forth some years since by our excellent friend Prof. Lovén, of Stockholm, in his discussion of the results of the deep-sea dredgings executed by the Swedish Spitzbergen Expedition of 1861, under Torell. "Considering," he says, "the power of endurance in these lower marine animals, and recollecting the facts that properly Arctic species which live also on the coast of Europe, are generally found there at greater depths than in their proper home, and that certain Antarctic species very closely agree with Arctic species, the idea occurs that, while in our own seas and those of warm climates, the surface, the coast line, and the lesser depths are peopled with a rich and varied Fauna, there exists in the great Atlantic depression, perhaps in all the abysses of our globe, and continued from Pole to Pole, a Fauna of the same general character, thriving under severe conditions, and approaching the surface where none but such exist in the coldest seas."

But whilst the question of Deep-sea Temperature is one of the greatest Biological interest, its determination is of even greater importance to the Geologist, as affecting his interpretation of the phenomena on which his belief in a former general prevalence of a Glacial climate is founded. For if a Glacial temperature should be found now to prevail, and types of Animal life conformable thereto should prove to be diffused, over the deeper portion of the *existing* Sea-bed in all parts of the globe, it is obvious that the same may have been the case at *any* Geological epoch; for there must have been deep seas in all periods, and the Physical forces which maintain the oceanic circulation at the present time must have been always in operation, though modified in their local action by the distribution of land and water existing at any particular date. And as the elevation of the present deep-sea bed of even the Intertropical oceanic area would (if we have correctly interpreted the results of our own and others' observations) offer to the study of the Geologist of the future a deposit characterised by the presence of Polar types, so must the Geologist of the present hesitate in regarding the occurrence of Boreal types in any marine deposit as adequate evidence *per se* of the general extension of Glacial action into temperate or tropical regions. At any rate, it may be considered as having been now placed beyond reasonable doubt, that a Glacial Submarine climate may prevail over any area, without having any relation whatever to the Terrestrial climate of that area.

These views are offered by us with the more confidence, since they are in harmony with the deductions already drawn by Geologists of eminence from facts observed by them. Thus I find on my return from the Mediterranean a letter from Principal Dawson, of Montreal, from which I am sure he would permit me to make public the following extract:—

". . . In reading your recent interesting publications on the Life of the Deep Sea, it occurred to me to mention to you that the fact which you have proved on the European coast, as to the existence and action of cold Arctic currents on the bottom of the ocean, was affirmed by me years ago for the American coast, on geological and geographical evidence, and was applied to the explanation of the Post-pliocene climate. On the American coast we have the cold currents in shallower water than you have now; though in the Post-pliocene you had them in shallow water also. It is true that the Glacial theories of Agassiz and others have prevented the proper amount of attention to these facts; but I have insisted on them again and again, and fully believe that the varying distribution of the cold and warm currents, depending on the elevation and depression of the sea bottom, will account for most of the differences of climate indicated by fossils and boulders from the Laurentian to the Modern period. I have some new and unpublished facts on this subject, which I intend to bring out in connection with the work I am now doing with the help of your brother, in the Post-pliocene geology of Canada."

In conclusion, I venture to anticipate that the words with which I concluded my lecture at the Royal Institution, "On the Results obtained in the *Lightning* Expedition of 1868," will be found to have been fully justified by those of the "*Porcupine* Expeditions" of 1869 and 1870; and that whatever may be thought of the notion that "we are still living in the Cretaceous epoch," we have furnished adequate proof that the formation of Glacial beds was not limited to any special Geological period, but that they are now, and have been through all time, in course of deposition:—"The facts I have now brought before you, still more the speculations which I have ventured to connect with them, may seem to unsettle much that has been generally accredited in Geological Science, and thus to diminish rather than to augment our stock of positive knowledge; but this is the necessary result of the introduction of a new idea into any department of scientific inquiry. Like the flood which tests the security of every foundation that stands in the way of its onward rush, overthrowing the house built only on the sand, but leaving unharmed the edifice which rests secure on the solid rock, so does a new method of research, a new series of facts, or a new application of facts previously known, come to bear with impetuous force on a whole fabric of doctrine, and subject it to an undermining power which nothing can resist, save that which rests on the solid rock of Truth. And it is here that the Moral value of Scientific study, pursued in a spirit worthy of its elevated aims, pre-eminently shows itself. For, as was grandly said by Schiller in his admirable contrast between the Trader in Science and the true Philosopher, 'New discoveries in the field of his activity which depress the one, enrapture the other. Perhaps they fill a chasm which the growth of his ideas had rendered more wide and unseemly; or they place the last stone, the only one wanting, to the completion of the structure of his ideas. But even should they shiver it into ruins, should a new series of ideas, a new aspect of nature, a newly-discovered law in the physical world, overthrow the whole fabric of his knowledge, *he has always loved truth better than his system*, and gladly will he exchange her old and defective form for a new and fairer one.'"

WILLIAM B. CARPENTER

On a Method of Ascertaining the Rate of Ascent of Fluid in Plants

WHEN conducting a series of physiological experiments on the transpiration of fluid by leaves, it became a matter of importance to determine the rapidity of ascent of fluid. My colleague, Prof. Church, had suggested for another series of experiments the use of lithium citrate, a salt easily taken up by plants, and one which can be detected with the greatest readiness by means of the spectroscope. Preference was given to the citrate, because of its containing an organic acid, and on this account not likely to meet with any obstruction to its passage from the tissues. This method I have used with great success. In one experiment the fluid had risen nine inches in thirty minutes, in another five and a half inches in ten minutes. This method is greatly superior to the use of colouring matters, which seem to experience considerable resistance in their passage through the vessels. Full particulars of these and numerous other experiments in the same direction will shortly be published. W. R. M'NAB, M.D.

Royal Agricultural College, Cirencester, Oct. 20

The Aurora Borealis

HAVING read the two accounts of Aurora Borealis in this week's number of NATURE, I hope the following brief account of the very beautiful one that occurred here may not prove uninteresting. On Friday, the 14th Oct. at 8.15 p.m., I noticed a bright appearance towards the north-west, somewhat resembling the moon rising, and on going to the front of the house which faced the north, saw that the whole of the horizon from west to south-east was lit up with a bluish white light. Gradually long streaks of the same colour stretched themselves up almost to the zenith, and then a blood-coloured light formed the higher portions, while the lower kept the bluish white colour already noticed.

Then appeared in the west a long broad band reaching to the zenith, consisting of a number of narrower bands, alternately red and bluish white, and through this the stars could be seen in their natural colour. At 9.30 the blood-red colour of the higher parts had almost disappeared, and long narrow streaks of yellowish white light extended up from the horizon.

At the lower part of the whiter light, in a northerly direction, appeared shadows having a somewhat rectangular form. (The town lies just in front of where I was standing, so that these shadows may have been caused by the ascending smoke.)

At 10.30 the red colour remained only in the west, and a narrow arc of bluish white light extended from north to about north-west, at about 25° above the horizon. The air was very calm, there being but a slight movement from the north-west. The moon was shining brightly all the time. Not having access to magnetic instruments I am unable to state how they were affected. On the 12th the barometer had fallen suddenly to 28·5°, and a violent westerly wind prevailed all day.

Dublin, Oct 18 T. W. PHILIPS

THIS evening, October 24, occurred one of the grandest displays of auroral lights which has probably ever been witnessed in these latitudes.

As I was, at a few minutes after seven o'clock, passing through the Observatory with the intention of observing with the heliometer, my attention was attracted by the brightness of the northern portion of the sky. On going out into the North Garden, I perceived that this was due to a general illumination of the sky of about that intensity which is produced by the rising of the full moon on the sky immediately above it, the moon itself not being visible. The contrast between this white illuminated sky, and the deep ordinary blue on the south side of the zenith, was very striking, the two portions being with moderate accuracy separated by the prime vertical.

On the south side of the zenith was observed what appeared to be an illuminated cloud, extending nearly from the zenith in a south-easterly direction for about forty degrees. Finally, there was at this time a well-defined arch of light, corresponding pretty accurately in position with the equator, and visible from east to west nearly to the horizon; and, beneath this arch, the sky was unusually dark, the darkness not being due to cloud or mist, as the stars were seen with their usual distinctness.

There was a small tendency at intervals to a display of streamers, but they were not conspicuous. From these phenomena I was led to expect, in the course of the evening, a grand display of aurora, and I was quite prepared for the summons, which I received from Mr. Keating, the assistant on duty, at eight o'clock, to come down and witness it.

The spectacle at this time was most magnificent. The northern portion of the heavens was nearly covered with crimson light of great intensity, and the sight was so fine that, for a few moments, I was occupied only with the admiration which it excited.

On proceeding to observe it more particularly, I saw that it consisted mainly of two large sheets of crimson light, one chiefly on the east side of the sky and the other on the west.

The eastern sheet extended generally from Polaris to Capella towards the zenith, which it did not, however, at this time quite reach. The most brilliant sets of streamers had their centres passing through these stars, and, after a few minutes, the extreme eastern portion was tolerably well defined by Perseus and Cassiopeia.

The western sheet was equally well defined, as lying between *α* Lyræ and *α* Aquilæ, but its brilliancy and the rapid change produced by the streamers were inferior to those exhibited in the other portion at the time when I observed it.

It is also worthy of remark, that the two portions seemed to be connected by an illuminated fleecy or cirrus cloud a little south of the zenith. This apparent cloud was, I believe, also a portion of auroral light, as I examined it a few minutes afterwards when it exhibited more of the auroral character.

At this time the portion of the eastern sheet, which had passed through Polaris, became separated from its more eastern portion passing through Capella, and formed a distinct sheet, while the western sheet was apparently drifting still more towards the south-west.

In a few minutes the intensity of the light diminished rapidly, and, as it was fading, my attention was attracted to a very beautiful feature in the phenomena exhibited. Just below the red light of the most western position, was a most brilliant bluish white light vaguely defined but very intense. It was most probably pure white, the bluish appearance being the effect of contrast with the red. The arch which I had observed earlier in the evening was now much brighter, and extended in the direction of the equator to the eastern portion of the heavens, where there was soon a similar effulgence of white light, but not quite so intense.

The darkness of the sky (perfectly free from cloud) beneath the bright arch was now much more conspicuous than it had been in the earlier part of the evening.

In a few minutes the whole faded away, and, excepting some small remains of the phenomena in the north, nothing unusual was visible. The grandest part of the phenomena continued for about half an hour, that is, from eight till half-past eight o'clock. Later in the evening a tolerably bright bank of auroral light was visible above the Northern horizon, and another brilliant display occurred, as described by Mr. Lucas.

For a considerable time during the first display, Mr. Lucas was watching from the tower of the Observatory, where he commanded a full view of the northern half of the heavens, and saw, at about 8^h 15^m to 8^h 20^m, an ill-defined dark segment along the north horizon, from which white streamers issued through the whole extent, very much resembling an ordinary aurora, but which might be easily passed over in the grandeur of the display.

Mr. Lucas watched from 10 o'clock, but saw nothing except the white light extending under the Great Bear, till 10^h 30^m, at which time white streamers shot up to Polaris and Beta Cephei, changing to intense red. At 11^h 10^m two sets of streamers appeared, one near Alpha and Beta Ursæ Majoris, and the other a little to the east of Beta Draconis, the former going eastward to *h* Ursæ Majoris, and the latter westward past Gamma Draconis. These were succeeded by some at Alpha Lyræ, combining with the last mentioned, and the mass, of an intense red colour, travelled slowly to Alpha Aquilæ, where it remained for a considerable time, as did that of Ursa Major, while the part of the heavens appeared perfectly clear. At 11^h 30^m streamers again shot up at Beta Ursæ Minoris, and between Gamma Draconis and Alpha Lyræ for a short time, and a few faint indications were visible till a little after 12, when the sky appeared to have regained its usual appearance.

From the relation of others it appears that another brilliant display took place still later in the night.

R. MAIN,
Oxford, Oct. 26 Radcliffe Observer

HAVING occasion to leave my house at 6.40 (the time given is Greenwich mean time throughout, and the bearings and directions were estimated from the pole-star, not compass) this evening, I was immediately struck with an unusual amount of uniformly diffused white light in the west and north-west. In a very few minutes a band of the same colour, but brighter, appeared, extending continuously across the sky, from W.S.W. to E.N.E. It was sharply defined, somewhat variable in breadth, but commonly about 6°. It lasted for nearly a quarter of an hour, and gradually disappeared, from the east westward. The diffused light, previously mentioned, remained unaffected. There were a few clouds in the south, but none elsewhere.

From 7.15 I went out of doors at intervals of a quarter of an hour to observe, but saw nothing but the diffused light already spoken of, which seemed to have acquired a greenish tinge, until 8 o'clock; when there was a band, broader than that first noticed, and of fiery rose colour, which extended from due east or not more than a degree or two north of it, towards the west, becoming gradually fainter in the latter direction. In a very few minutes the band became continuous from east to west, from 10 to 12° broad, and at the meridian, where it culminated, about 54° above the horizon at its upper edge, as estimated from the immersion of the pole-star within it, and which was occasionally much obscured by it. The red colour was equally pronounced in the east and in the west, but less so at the meridian and for about 15° on each side of it. Below the band, from N.W. to N.E. the sky was free from the red colour, but still retained the greenish diffused light. Soon after the formation of the complete band, streamers, varying from white to various degrees of red, shot up from every part of the northern half of the horizon towards the zenith, beyond which some of them extended. Those from the north, chiefly of white light, crossed the band at right angles, whilst the others cut it more and more obliquely according as they were nearer to the east and west. I watched it continuously with a large party of friends until something after half-past eight, and then left it in full vigour. Before nine it had in great measure disappeared, but the diffused white light still lingered, gradually becoming more and more limited in extent, though remaining equally pronounced, until 10.30; when it occupied the small segment of the sky comprised within about 5° on each side of the north point of the horizon, and from 6° to 7° above it.

Lamorna, Torquay, Oct. 24 W. PENGELLY

THE AMERICAN GOVERNMENT ECLIPSE EXPEDITION

At the last session of the Congress of the United States of America, an appropriation of 6,000*l.* was made for the observation of the Total Eclipse of the Sun, under the direction of Professor Benjamin Peirce, the Superintendent of the U.S. Coast Survey. This generous act of legislation was suggested by one of the ablest statesmen of America, the Hon. John A. Bingham, of Ohio, and passed both houses unanimously.

An officer was immediately sent to examine the various places, and obtain all the local information which might be required to select the most favourable positions for observation. The expedition has been divided into two parties, each of which consists of about twelve persons. One party is under the immediate direction of Prof. Peirce, and will observe in Sicily; and the other is under the direction of Prof. Winlock, the director of the Observatory of Harvard University, and will observe in Spain. Almost all the astronomers of the expedition were upon the central path of the great eclipse which occurred in America in August 1869, so that they have already been under fire, and are prepared for the sudden outburst of the total obscuration.

The observations for precision will be entrusted in each party to an experienced officer of the survey, who will be upon the ground at least a fortnight before the eclipse. He will have the instruments all properly mounted and protected, the time well observed, and the arrangements made so that the principal observers of the physical phenomena may find everything in readiness when they arrive. Their presence will not, therefore, be required till within a few days of the eclipse. The officers upon whom this duty has devolved are Mr. Schott and Mr. Dean, assistants of the Coast Survey.

The spectroscopic observations have been chiefly arranged by Professor Winlock, assisted by Professors Young and Morton. New and peculiar methods have been prepared for preserving a record of the lines of the spectrum for subsequent measurement and discussion.

The photographic preparations are varied and original. The party of Prof. Peirce will have photographic apparatus prepared by Mr. Rutherford of New York, with lenses especially ground for the purpose under his direction by Fitz of New York, and young Fitz will himself superintend this portion of the observations. The party of Prof. Winlock will have its photographic apparatus prepared, under the directions of the Professor, by Clarke, of Cambridge, and will use lenses ground by Clarke. Alvan Clarke, Jun., will also assist in these observations. Prof. Winlock's new method of photographing the sun through a long tube will be used in a portion of this class of observations. In both parties arrangements are made for long and short exposures in different instruments during the period of totality.

The polariscopic observations will be made by Prof. Pickering in the party of Prof. Winlock.

General observations of the corona will be made by as many of the party as possible, and it is hoped that Steinheil's hand comet-seekers will be especially available for this class of observation. Hand spectroscopes will also be used by several of the party, and it is hoped that in the preparations for this portion of the service material assistance will be derived from Mr. Lockyer's suggestions.

It is worthy of notice that two of the ablest officers of Engineers of the United States' Army have been detailed by the War Department to accompany the Expedition. They are Major Abbott, whose name is familiar to hydraulic engineers through his connection with General Humphrey's Monograph upon the Mississippi river, and Captain Ernst. B. P.

DR. W ALLEN MILLER

We have already referred to the lamented death of Dr. W. A. Miller, and now give a short sketch of his life. Dr. Miller was born at Ipswich in 1817, and received part of his education in Merchant Taylors' School. He obtained, however, his first insight into chemistry in a school belonging to the Society of Friends, at Ackworth, in Yorkshire. At the age of fifteen he was apprenticed to his uncle, who was surgeon to the General Hospital at Birmingham, and at the age of twenty he entered King's College, where (we quote from an obituary notice in the *Chemical News*) his knowledge of chemistry attracted the attention of Professor Daniell, who, during the illness of the laboratory assistant, engaged his services. In 1840 he visited Germany, and passed some time in Liebig's laboratory at Giessen. In the same year he was appointed to the post of Demonstrator in the Laboratory of King's College. In 1841 he became Assistant Lecturer to Professor Daniell, and also took his degree of M.D. in the University of London. He also assisted Professor Daniell in various scientific inquiries, and conducted the experiments on the electrolysis of saline compounds, his name being associated with that of Daniell in the paper that appeared in the *Philosophical Transactions* for 1844, In the following year he became a Fellow of the Royal Society, and on the death of Professor Daniell succeeded to the vacant chair. At this period he became greatly interested in the subject of spectrum analysis, in which he worked with great activity as an observer of the various phenomena which were then attracting the attention of the scientific world. He was a member of the Council of the Royal Society from 1848 to 1850 and from 1855 to 1857, being elected treasurer in November 1861, thereby becoming vice-president of the society. About this time his highly-trained mind and great knowledge were utilised to the highest degree in a joint research with Mr. Huggins on the spectra of stars and nebulæ, and in this class of researches his loss will be as severely felt as it will be at King's College, the Council Board of the Royal Society, and other places where his calm and sound judgment was conspicuous.

Professor Miller received the degree of LL.D., Edinburgh; of D.C.L., Oxford; and of LL.D., Cambridge. He also received the gold medal of the Astronomical Society, in conjunction with Mr. Huggins. At the time of his death he was a member of the Royal Commission which is now considering the whole question of scientific instruction and the advancement of science. His contributions to scientific knowledge, beyond those we have mentioned, were not large, his time being much taken up, as is the case with too many of our best scientific men, with teaching. His "Elements of Chemistry" is a valuable work which has long been favourably known, and has gone through several editions.

AUGUSTUS MATTHIESSEN

The sad death of Dr. Augustus Matthiessen, which we briefly referred to in a previous number, has bereft English chemical and physical science of one of the most arduous and successful workers who ever entered her ranks. Born January 1831, in London, he from early youth upwards, manifested a great liking for chemistry, but it was not until he came of age that he entered upon its study in earnest at the University of Giessen, where he subsequently took his doctor's degree, and afterwards at Heidelberg, where, for nearly four years, he worked under the guidance of Bunsen and Kirchhoff. His first paper, "On the Preparation of the Metals of the Alkalies and Alkaline Earths by Electrolysis," appeared in the *Annalen der Chemie und Pharmacie* for March 1855, and was devoted to a description of the preparation and properties of the metals calcium and strontium, then isolated for the first time. Calcium he found to be a metal of the colour and glance of bell metal, exceedingly ductile and malleable; using water as the exciting fluid, he found it to be electropositive to magnesium, and electronegative to sodium and potassium, which at once explained why it could not be obtained from its chloride by the action of sodium or potassium at high temperatures. Next in order is a paper of his in Poggendorf's *Annalen* for 1857, communicated by Kirchhoff, in whose laboratory the results were worked out, entitled, "On the Electric Conductivity of Potassium, Sodium, Lithium, Magnesium, Calcium, and Strontium." Following this, appear in Poggendorf's *Annalen* for 1858 two communications from him "On the Electric Conductivity of Metals," and "On the Thermo-electric Series." On his return to London he worked some time at the Royal College of Chemistry under Hofmann, and published a paper "On the Action of Nitrous Acid on Aniline." Hunt had described phenol, free nitrogen and water as the products of this reaction, but he found that an intermediate reaction took place, by which ammonia was formed; extending his experiments to ethyl and diethylaniline, he obtained ethylamine and diethylamine. It was this reaction which first led him to the study of narcotine, which afterwards in his hands yielded such splendid results. After working diligently several years in a laboratory which he fitted up for himself in Torrington Square, he was appointed Professor of Chemistry to St. Mary's Hospital in 1862. It was about this period that his most important researches were carried out in conjunction with Dr. Vogt, Von Bose, Holzmann, &c., and published in a series of papers in the Philosophical Transactions of the Royal Society, to which he was admitted a Fellow in 1861. Some of the most important of these papers are those "On the Influence of Temperature on the Electric Conducting Power of Metals." It was this research which proved the important fact, that the conducting power of the pure metals decreased to the same extent between 0° and 100° C.; two remarkable exceptions, however, to this law, Iron and Thallium were the subject of a later paper; "On the Specific Gravity of Metals and Alloys;" "On the Chemical Nature of Alloys," in which he showed that nearly all the two-metal alloys may be considered as solidified solutions of the one metal in the other. Also a long series of determinations of the influence of temperature on the conducting power of alloys. He also made a most careful redetermination of the expansion of water and mercury, and found that Kopp's coefficients were slightly too low. He was a very active member of the committee appointed by the British Association "On the Standards of Electrical Resistance," and it was one of the alloys discovered by him which was finally adopted for the reproduction of the now well known B A unit of electrical resistance. His later chemical work is embodied in a series of papers in the *Philosophical Transactions*—"On the Chemical Constitution of Narcotine"—published partly in conjunction with Prof. Foster, and partly with Dr. Wright. In these he shows that one, two, and three atoms of methyl can be successively removed from narcotine, and also describes a large number of interesting derivatives of the same. In 1869 he was appointed Professor of Chemistry in St. Bartholomew's Hospital, and in the same year received the Royal Society's Gold Medal for his published researches on the metals and the opium alkaloids. One of the most important results of his last investigation is the discovery of the relation between morphia and codeia, the latter simply containing one of methyl more than the former; although, however, he succeeded in obtaining apomorphia from codeia, he was never able to reconvert apomorphia into morphia, and thus form morphia direct from codeia. At the time of his death he was occupied with the experiments on the chemical nature of pure cast-iron, of the Committee appointed to inquire into which he was a member, and also with experiments with a view to determine whether the specific heat of platinum was constant at high temperatures, and if so, to employ it in the construction of a standard pyrometer. He was also prosecuting his researches on the opium bases, and had already arrived at interesting results, which we believe will shortly be published. All the beforementioned researches display an enormous amount of manipulative skill, and there is little doubt that his success was mainly due to the wonderful acuteness of his powers of observation, and also to his great perseverance; but it is indeed surprising that, labouring under the physical disadvantages he did, he should have been able to attain such ends.

At a time when England can least afford it, she has lost one who had not only done a vast amount of valuable work, but who, there was every prospect, would do as much more in the future.

BRITISH EDIBLE FUNGI

MUSHROOMS and their congeners seem never have been in good repute since Agrippina employed one of the tribe to poison her husband, and Nero with villanous pleasantry called it the "food of the gods." With proverbial tenacity the bad name thus incurred has clung to the whole family of agarics, and what within certain limits might be called a wholesome dread has become a deep-rooted and irrational prejudice, excluding from popular use a really valuable class of vegetable esculents. We cannot altogether go along with those enthusiastic mycophagists who recognise a substitute for meat in every edible fungus, and dilate on the ozmazome and other nutritious properties of the tribe; but we readily acknowledge that their merits as secondary sources of food-supply have hitherto been unduly neglected. The great difficulty always felt in advocating the claims of the class to more extensive use has arisen from the want of some definite rules, some formula at once simple in expression and universal in application, by which to distinguish the noxious from the innocent members. Pliny, in his Natural History, goes so far as to say that the first place amongst those things which are eaten with peril must be assigned to agarics, and he expresses his surprise at the pleasure which men take "in so doubtful and dangerous a meat." But his observations show that fungi of all sorts, including even such growths as the *Fistulina hepatica*, were known to his countrymen and eaten by them without scruple. Indeed, in one particular the wisdom of the ancient Romans seems to have been superior to that of their descendants, for, while Horace lays down the rule—

> Pratensibus optima fungi
> Natura est; aliis male creditur—

the modern Ædiles of the Roman market condemn to instant destruction every specimen of the meadow mushroom (*A. campestris*) which comes within their reach. Although, however, it is not always easy to distinguish the wholesome from the unwholesome fungus, and the organs of sight and smell require some training before they can be wholly trusted in the matter, yet the dangers have been greatly exaggerated, and, as a matter of fact, hogweed is more often mistaken for parsnip and aconite for horseradish than are *Boletus sátanas* and *Amanita verna* for their innocent brethren. No better opportunity for engaging in the study of this branch of natural history could be found than that which the present season affords; and if the treatises of Mr. Berkeley, Dr. Badham, or Mr. Worthington Smith be not at hand, the following notes on the chief edible fungi which are now to be met with may prove acceptable to some of our readers.*

With the ordinary meadow mushroon *(A. campestris)* and its near relative the horse mushroon (*A. arvensis*), every one is familiar, and both of them have occurred in profusion this autumn. Against the latter an unfounded prejudice prevails in some districts, but its larger size and coarser texture require only a little extra cooking to develop the flavour and correct indigestibility. In spite of all that has been said to the contrary, we maintain that these agarics are entitled to the first place, and for the second much rivalry exists between the orange-milk mushroom (*Lactarius deliciosus*) and the Parasol Agaric (*Agaricus procerus*). Both are readily distinguishable, and may be eaten with equal impunity. The former is chiefly found in plantations of Scotch fir and larch, is of an orange-brown colour, and firm flesh, and yields, when bruised, an exudation of orange-red milk, which turns green after a few minutes' exposure. The latter is common in pastures, and may be recognised by its tall habit, the stalk gradually enlarging at the base, the umbo of a brownish colour with spots or patches, and the gills white and unconnected with the stem. The plum

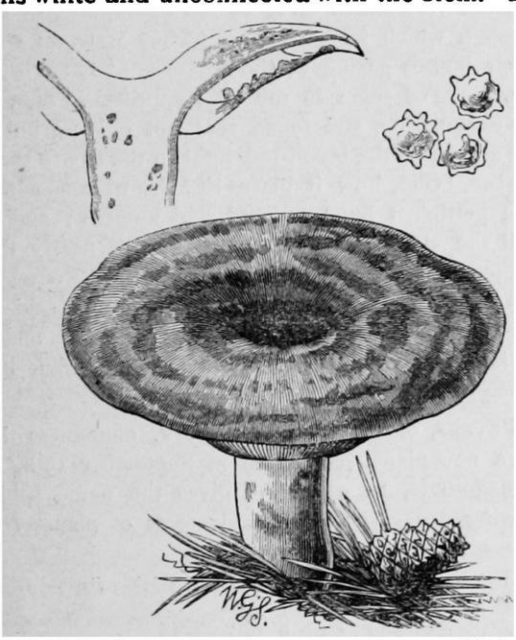

FIG. 1.—*Lactarius deliciosus* (Orange-milk Mushroom). Under fir-trees, in autumn; colour, brown-orange; milk at first orange, then green; diameter, 3 to 10 inches.

mushroom (*A. prunulus*) is for the autumn months what the St. George's mushroom (*A. gambosus*) is for the spring—a large fleshy fungus, delicate in flavour, though not so choice as the *Orcella*, for which it is often mistaken. It is to be found in shady places pretty generally throughout England, and is conspicuous from its whiteness. The gills are close together and of a pale rosy hue, and the smell of the plant has been compared to that of fresh meal.

We must mention two other fungi, common enough and easily recognised, but of their culinary virtues we do not entertain a very high opinion. These are the puff-ball, and the maned agaric (*Coprinus comatus*). The former needs no description, and perhaps others may be more fortunate than we have been in detecting the latent flavour of omelette which it is said to possess. The latter is called by Dr. Bull the "agaric of civilisation." We have met with it in farmyards, on lawns, on railway-cuttings, and, in fact, in nearly every waste place. It looks like an attenuated cocoon, snow-white at first, but gradually changing in colour and splitting upwards in a dozen places. The gills, white at first, become pink and then black; the last stage, which is very quickly reached, presaging the immediate dissolution of the plant, which gradually deliquesces into an inky-black fluid.

It would be easy to amplify this list, but we desire to avoid all risk of confusing the tyro's mind with too many details, and have purposely confined our remarks to those fungi which belong to the autumn season.

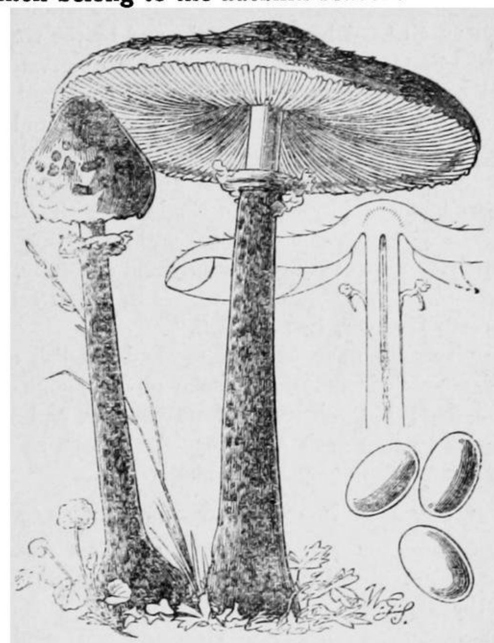

FIG. 2.—*Agaricus procerus* (Scaly Mushroom). Pastures, &c., in autumn colour, pale brownish buff; diameter, 5 to 12 inches.

One caution must be added. All agarics are more wholesome fresh than stale, and with some the neglect of this rule may lead to unpleasant consequences. It is rigidly enforced in the Roman market, where all specimens which are "muffi, guasti," or "verminosi" are seized and thrown into the Tiber, and it should be distinctly understood in every English kitchen into which even the common mushroom is allowed to enter. The fungus which to-day successfully simulates a sweetbread, may to-morrow simulate with equal success a handful of snuff.

FIG. 3.—(1) *Agaricus orcella* and (2) *Agaricus prunulus* (Plum Mushroom Woody places, in autumn; colour, snow white, with pale rose gills: diameter, 2 to 4 inches.

Our illustrations are taken from Mr. W. Robinson's admirable little manual, to which we have already alluded, and are from the facile pencil of Mr. Worthington Smith. Here will be found also instructions for cooking all the most common edible species. C. J. ROBINSON

* At the conclusion of "Mushroom Culture, its Extension and Improvement" (London: Warne, 1870), Mr. W. Robinson gives some useful information, derived chiefly from the above authorities, and from the Proceedings of the Woolhope Field Club.

NOTES

DESIROUS of aiding the English Eclipse Expedition, Prof. Peirce has addressed the following letter to Mr. Lockyer. It is to be hoped that observers will take advantage of the opportunity so magnificently afforded them :—

"Fenton's Hotel

"MY DEAR SIR,—I have been directed by the Government of the United States to have the best possible observations made of the total eclipse of next December. If I could aid the cause of Astronomy by assisting the observers of England in their investigation of this phenomenon I should be greatly pleased. I take the liberty therefore to invite your attendance, and also that of other eminent physicists of England, with either of the parties of my expedition, one of which will go to Spain and the other to Sicily.—Yours very respectfully and faithfully,

"BENJAMIN PEIRCE

"J. Norman Lockyer, Esq., F.R.S."

Of course it would have been better had English observers, who have devoted their attention to solar physics, gone out under the English flag; but science is of no country, and they may well be proud to join such a distinguished corps as that with which they are asked to associate themselves.

WE are also informed that arrangements are being made for a deputation to urge on Mr. Gladstone, as a last resource, the importance of not abandoning the Eclipse Expedition, and to point out the especial inopportuneness of such a course at a time when neither France nor Germany can send out expeditions, and when, if we withdraw, the whole burden of observation will fall upon America. It will be remembered that the reason given by the Admiralty for refusing the loan of a Government ship was "that Parliament did not place money at the disposal of the Naval Department for such purposes." If this should be the only difficulty with the Admiralty, it will not be hard for the Deputation to find precedents, from the time of Captain Cook downwards, for sending out such expeditions. It is to be hoped that Mr. Gladstone may take a more liberal view of the subject than the Lords of the Admiralty have done, and that the leaders of science in this country will show themselves less supine in the matter than they have hitherto done.

WE are informed that the work done by Dr. Carpenter in the *Porcupine* expedition of the present year has satisfied him of the justice of the views advanced in his reply to Prof. Wyville Thomson's lecture, as to the over-estimation of the heating and moving action of the real Gulf-stream. It is Dr. Carpenter's intention to bring his views on this subject before the Royal Geographical Society.

THE Science and Art Department of the Committee of Council on Education have published a second edition of their Directory, with regulations for establishing and conducting Science Schools and Classes, superseding those previously printed.

THE amount of interest shown in Natural Science at Oxford cannot be better illustrated than by noticing the long list of lectures which are to be given this term. These, exclusive of the purely mathematical, are as follows:—1. University Lectures: Professor Phillips, F.R.S., "On the Composition and Structure of the External Parts of the Earth." Professor Lawson, "On the Minute Anatomy of Plants." Professor Rolleston, F.R.S., "On Anatomy and Physiology." Professor Pritchard, F.R.S., "On Astronomy." Professor Clifton, F.R.S., "On Elementary Statics," and a continuation of his lectures "On Heat." Mr. Wyndham, for the Professor of Chemistry, "On Elementary Principles of Chemistry." The Professor of Zoology will give assistance to all who are working at the Articulata, the collection of which he is arranging. All these are free public lectures, and are illustrated by experiments, and are all largely attended. 2. College Lectures, &c.: Mr. A. V. Harcourt, F.R.S., Lees Reader in Chemistry, "On the Chemistry of the Metals," at Christ Church. Mr. A. W. Reinold, Lees Reader in Physics, "On Hydromechanics and Heat," at Christ Church. Mr. J. B. Thompson, Lees Reader in Anatomy, "On Comparative Osteology," at Christ Church. Mr. Heathcote Wyndham, "On Chemical Philosophy," at Merton. Mr. Abbay, "On Elementary Physics," at Merton. Mr. Chapman, "On Anatomy and Physiology," at Magdalen. These lectures are open to all other Colleges on payment of a small fee.

MAGNIFICENT displays of the Aurora Borealis have been witnessed in London on two nights of the present week, Monday the 24th, and Tuesday the 25th inst. It will be interesting to hear from distant subscribers the extent of the area over which the phenomenon was visible. In addition to the letters printed this week, we have received others describing the display from C. Pocklington, Poole; E. C. Walker and T. H. Waller, York; E. Dukinfield Jones, Fermoy, Co. Cork, and others. *Apropos* of one of these displays, a correspondent of the *Pall Mall Gazette* thus states the view taken of it by the inhabitants of a little village through which he passed:—"They were all standing outside their houses gazing at the heavens. 'There is France for you, said one of them to me as I approached him. I requested an explanation, and found that not only he but all his neighbours attributed the blood-red light in the sky to the burning of Paris. 'Gad, how it blazes!' I heard a man remark. 'They're a gettin' it hunder now,' said another; and so on through all the village. At a garden gate of nearly the last house I observed a respectable-looking man with a telescope, with which he was rolling the sky: 'It is rum,' he said to me, 'and very sublime; but the d——d asses, I can't make 'em believe it is only the Southern Cross.' I rather think he was the schoolmaster of the parish."

PROF. PETERS, of Clinton, New York, announces the discovery of a new planet (No. 112) on September 19th, of the eleventh magnitude, to which he gives the name Iphigenia. The following are given as the elements of planet No. 111 (Até):—

Epoch, 1870, Sept. 0, 0 mean Berlin time

M	=	$205° \; 17' \; 21''.0$	
π	=	$122 \; 53 \; 7.3$	mean eq. 1870
Ω	=	$306 \; 26 \; 28.4$	mean eq. 1870
i	=	$5 \; 1 \; 21.4$	mean eq. 1870
ϕ	=	$5 \; 49 \; 10.6$	
μ	=	858.392	
$\log. a$	=	0.4108808	

The planet is now nearly stationary, and is of the twelfth magnitude.

PROFESSOR LUTHER, of the Königsberg Observatory, records in the *Astronomische Nachrichten* the death, at the early age of 26, of Dr. F. Tischler, Observer in the same Observatory. Dr. Tischler was born at Breslau in 1844, and after pursuing his studies at Königsberg and Bonn, and publishing a treatise on the path of Tuttle's comet, was appointed Observer at the former place in 1867. At the outbreak of the Franco-German war, he obeyed a summons to serve in the Prussian infantry, and was seriously wounded in the battle before Metz, on August 14th, and succumbed to his injuries on September 30th.

THE first meeting of the Zoological Society for the session will take place on Tuesday, the 1st of November, when the following papers will be read:—1. The Secretary: "On Additions to the Society's Menagerie." 2. Prof. W. Peters, F.M.Z.S.: "Contributions to the knowledge of *Pectinator*, a genus of Rodent Mammalia from North-Eastern Africa." 3. Mr. C. Darwin, F.R.S.: "Note on the Habits of the *Chrysoptilus campestris*."

THE first meeting for the session of the Chemical Society will be held on Thursday evening, Nov. 3, when Mr. A. H. Elliott will read a paper "On the Analysis of Cast-iron."

THE first meeting of the Linnæan Society will be held on Thursday evening, November 3, when papers will be read by Dr. Mansel Weale "On the Fertilisation of Orchids and Asclepiads," and "On a Solitary Bee from South Africa."

THE forthcoming exhibition of the Photographic Society of London will be inaugurated by a *Conversazione* open to members and their friends, to be held at the Architectural Gallery, 9, Conduit Street, on Thursday evening, Nov. 8, at seven o'clock. The Exhibition will remain open during the remainder of November from 9 A.M. till dusk daily. Intending exhibitors are requested to send in their works not earlier than Nov. 1, nor later than Nov. 3.

WE shall be happy, in accordance with a suggestion from a correspondent, to insert (gratuitously) in our advertisement columns, a list of "Scientific books wanted" by any of our subscribers who are unable to obtain them through the ordinary channels.

THE following lectures will be delivered during the ensuing season at the London Institution, Finsbury Circus. Educational Courses—Mondays, at Four. Eight lectures on Chemical Action, by Professor Odling; Oct. 31; Nov. 7, 14, 21, 28; Dec. 5, 12, 19, 1870. Six lectures on the First Principles of Biology, by Professor Huxley, Jan. 23, 30; Feb. 6, 13, 20, 27, 1871. Eight lectures on Astronomy, by R. A. Proctor; March 6, 13, 20, 27; April 3, 17, 24; May 1, 1871. Conversazione lectures—Wednesdays, at half-past seven, On Dust and Disease, by Professor Tyndall, Dec. 21, 1870. On Alizarine and other Colouring Matters, by W. H. Perkin, Feb. 15, 1871. Stained Glass æsthetically considered with reference to Modern Art, by Henry Holiday, March 15, 1871. Evening courses—Thursdays, at half-past seven. Two lectures (with instrumental music) on the Acoustics of the Orchestra, by Dr. W. H. Stone, Nov. 10, 17, 1870. Two lectures on Precious Metals and Precious Stones, by Professor Morris, Nov. 24; Dec. 1, 1870. Two lectures on Count Romford and his Philosophical Work, by W. Mattieu Williams, Dec. 8, 15, 1870. Two lectures on Music, Characteristic and Descriptive, by John Ella, Jan. 12, 19, 1871. Four lectures on the Action, Nature, and Detection of Poisons, by F. S. Barff, Jan. 26; Feb. 2, 9, 23, 1871. Six lectures on Economic Botany, or Vegetable Substances used for Food, and in the Arts and Manufactures, by Professor Bentley, March 23, 30; April 6, 20, 27; May 4, 1871.

A COURSE of three lectures (on Nov. 4th, 7th, and 8th) will be delivered at the Gresham College, Basinghall Street, by Dr. E. Symes Thompson, on the Organs of Respiration in Health, on Hay Fever, and on the Respiratory Organs in Disease. The lectures, which are illustrated by diagrams and chemical experiments, are free to the public, and commence each evening at 7 o'clock.

MR. VAN VOORST announces the following works in preparation:—"The Natural History of the British Diatomaceæ," by Arthur Scott Donkin, M.D. "Heads of Lectures on Geology and Mineralogy," delivered at the Royal Military College, Sandhurst, by Prof. Rupert Jones. "The Ornithology of Shakespeare," by J. E. Harting. "The Natural History of the Azores," by F. DuCane Godman, F.L.S. A fourth edition of "Yarrell's British Birds," edited by Prof. Alfred Newton. A fourth edition of Prof. Rymer Jones's "Organisation of the Animal Kingdom." "Dr. Bevan on the Honey Bee," a revised and enlarged edition, by W. Augustus Munn. "Prof. Frankland's Lecture Notes for Chemical Students." Vol. 2 (Organic Chemistry).

THREE hundred bags of a remarkable-looking seed, new to British commerce, have recently been brought into this country as an oil seed; they were shipped from Lisbon to Liverpool, but are believed by the Liverpool merchants into whose hands they have been delivered to have come originally from the east coast of Africa. Mozambique is, in all probability, the port from whence they were first shipped, seeing that they are the seeds of *Telfairia pedata*, a tall climbing cucurbitaceous plant, native of the coast opposite Zanzibar. These seeds have somewhat the colour and appearance of almonds, but they are flat, nearly circular, and about 1½ inches across; they are covered with a very closely reticulated net-work of woody fibre, and the kernel is about the colour and hardness of that of a Brazil nut and contains a large quantity of oil, which is probably intended for use in this country as a culinary oil. The kernels, however, are of a rank, bitter taste, though they are stated in books to be as sweet as almonds. The fruit is very large, and is said frequently to contain as many as 250 seeds. Two species only are known of the genus, the one under consideration and *T. occidentalis*, native of the opposite or West Coast of Africa.

WE are glad to call attention to the fact that the *American Journal of Science and Arts*, which has from its commencement been the leading vehicle for the original papers of the scientific men of America, will be continued after the close of the present year as a monthly journal. This increased frequency of publication will, it is believed, meet a wish often expressed by authors for a more rapid interchange of views, and an earlier knowledge of the progress of research; and the editors hope that the friends and patrons of science will aid in promoting its wider circulation. We believe that there are many public and private libraries, and reading-rooms, throughout the country, which are not yet supplied with this journal, which is certainly one of the most important of existing scientific publications.

THE bastion in front of Fort Bicêtre, known as Bastion No. 87, is manned by the members of the Ecole Polytechnique. The professors of the college have consented to serve under their former pupils, wherever these have been selected as lieutenants. In this bastion may be seen MM. Bertrand, Bonnet, Langier, Frémy, Tissot, Laguerre—all members of the Institute, professors at either the College de France or at the Sorbonne—daily at their posts in the bastion, which has already acquired the reputation of being one of the best mounted among the fortifications of Paris.

THE Council of the Institution of Civil Engineers print a list of 38 subjects, respecting which they invite communications, as well as upon others; such as (1) Authentic Details of the Progress of any Work in Civil Engineering, as far as absolutely executed (Smeaton's Account of the Eddystone Lighthouse may be taken as an example); (2) Descriptions of Engines and Machines of various kinds; or (3) Practical Essays on Subjects connected with Engineering, as, for instance, Metallurgy. For approved Original Communications, the Council will be prepared to award the Premiums arising out of special funds devoted for the purpose.

THE shock of an earthquake was felt last Thursday at 11·30 A.M. both in the United States and in Canada. The motion, accompanied by a loud rumbling noise, was felt at Boston, New York, Montreal, Toronto, and St. Katherine, and throughout the provinces of Ontario and Quebec. The shock lasted twenty seconds, and appeared to travel eastward.

THE towns of Reading and Maidstone have been among the earliest to throw themselves into the new movement in favour of Technical Education. Classes in various departments of Natural Science are being formed at both places.

PROF. ZOELLNER ON THE SUN'S TEMPERATURE AND PHYSICAL CONSTITUTION *

AMONG the characteristic forms of the protuberances † the observation of which the spectroscope with widened slit has rendered possible at all times, is to be found a not inconsiderable number of such, whose appearance at once conveys the conviction to every impartial observer that we have here to deal with vast eruptions of incandescent hydrogen masses.

It is probably impossible, without quitting the range of known analogous occurrences and at the same time the conditions explanatory of cosmical phenomena, to assume any other cause of these eruptions than the *difference of pressure* of the issuing gas in the interior and on the surface of the sun. The possibility of such a difference of pressure presupposes, necessarily, the existence of a separating layer between the inner and outer masses of hydrogen, the latter of which, as is known, form an essential constituent of the sun's atmosphere.

The assumption of the existence of such a separating layer is, at first sight of the above-mentioned protuberance phenomena, so cogent that it even forces itself as undeniable upon observers who, like Respighi, do not hold it to be improbable that electrical forces could be the cause of such eruptions.

Keeping, however, to the more simple and therefore more natural assumption of a difference of pressure, we have to deal with a phenomenon which, on the application of the mechanical theory of heat and gases, is capable of yielding most important information as to the temperature and physical constitution of the sun.

For perfect gases the mechanical theory infers from its premises,—firstly, the law of Mariotte and Gay-Lussac; secondly, the constancy of the relation of the specific heats at constant volume and constant pressure.

This constant, therefore, when determined according to known methods for a definite gas, must, from the point of view of the mechanical theory of gases, be considered, similarly to the atomic weight of a body, as invariable, and must on no account be placed in the category of other empirical constants, such as the conducting power of bodies for heat, or the co-efficient of expansion of solid and liquid bodies, &c. These constants only hold good within those limits within which they are determined by observation, and lose their significance when employed far beyond those limits.

Under this supposition I consider the eruptive protuberances as a phenomenon of the efflux of a gas from one space into another, the pressure in both spaces during the discharge being assumed constant, and neither a communication nor an abstraction of heat as taking place.

Let A be the heat-equivalent of the unit of work,
- v the velocity of efflux of the gas in the plane of the opening,
- g the intensity of gravity on the sun,
- x the relation of the specific heats of the gas at constant pressure and constant volume,
- c the specific heat of the gas at constant volume referred to an equal weight of water,
- t_i the absolute temperature of the gas in the inner space, from which the efflux takes place,
- t_a the absolute temperature of the issuing gas in the plane of the discharge opening,
- p_i the pressure of the gas in the inner space,
- p_a the pressure in the plane of the discharge opening.

Then, according to the dynamical theory of heat, we have, under the assumptions which have been made, the following two relations‡ between these nine magnitudes:—

$$A\frac{v^2}{2g} = xc\left(t_i - t_a\right) \qquad (1)$$

$$\frac{t_i}{t_a} = \left(\frac{p_i}{p_a}\right)^{\frac{x-1}{x}} \qquad (2)$$

Further, let a_1 be the mean height of the barometer in metres of mercury,
- ρ the density of the gas under consideration at the temperature of melting ice, and under the pressure a_1 on the earth's surface,
- σ the density of the gas contained in the inner space under the pressure p_i, and at the absolute temperature t_i,
- a the coefficient of expansion of the gas for 1° C.

Conformably to the law of Mariotte and Gay-Lussac, we then have also the following relation:—

$$\sigma = \frac{\rho}{a_1\, a}\,\frac{p_i}{t_i} \qquad (3)$$

The pressure p_a in the plane of the discharge opening may, under the suppositions made, be considered as identical with the pressure which the atmosphere of the sun exercises at the *niveau* of the above-mentioned separating layer, *i.e.*, at its base.

Let, in this case,
- p_a be the pressure at the base of the atmosphere,
- h a certain height above the base,
- p_h the pressure at this height,
- t the absolute temperature in the atmosphere, which, in consequence of insufficient knowledge of the law of temperature is assumed to be everywhere constant,
- g the gravity of the sun at the base of the atmosphere,
- r the radius of the separating layer,
- ρ_1 the specific gravity of mercury at the temperature of melting ice,
- g_1 the intensity of gravity on the earth's surface,
- a_1 the mean height of the barometer,
- ρ the density of the gas composing the atmosphere at the temperature of melting ice, and under the influence of the quantities g_1 and a_1;

we then obtain, by a known method of deduction, the following relation,

$$\text{log. nat. } \frac{p_a}{p_h} = \frac{\rho g r h}{\rho_1 g_1 a_1 a t\,(r+h)} \qquad (4)$$

In order to combine this with the three previous equations, a double assumption must be made:

First, that the essential constituent of the sun's atmosphere, which produces the pressure p_a, consists of the same gas as escapes from the interior of the sun during eruptive protuberances.

Secondly, that the absolute temperature t of the atmosphere, may be considered as essentially agreeing with the absolute temperature at the *niveau* of the opening during the discharge.

Having regard to the object of the present memoir, I consider the admissibility of the first assumption as sufficiently established by observations, since the discovery of the so-called chromosphere has given the proof that the whole surface of the sun is surrounded by an atmosphere of hydrogen of very considerable extent.

The correctness of the second assumption I infer *from the luminosity of the base of all eruptive protuberances not differing to any extent from that of the chromosphere.* When it is considered that the constant mean temperature t in Formula (4), which, in consequence of the want of knowledge of the law governing the decrease of temperature, is substituted for the temperatures falling with the height h, evidently must correspond to a layer near the base,* this temperature becomes at the same time approximated to that belonging to the outer surface of the separating layer.

By virtue of the first assumption, the value ρ in Formula (4)

* T. Zöllner. *Ueber die Temperatur und physische Beschaffenheit der Sonne.* Berichte Kön, Sächs, Gesellschaft der Wissenschaften. Sitzung am 2 Juni, 1870.

† The forms of the protuberances may be divided into two characteristic groups—into the vapour- or cloud-like and into the eruptive formations. The preponderance of the one or the other type appears partly to be dependent on local conditions on the surface of the sun, partly on the time, so that at particular periods the one, at others the other, type preponderates. The striking resemblance of the cloud-like formations to terrestrial clouds is readily explained, when it is considered that the forms of our clouds are due, not to the particles of water suspended in them, but essentially to the nature and manner in which the differently heated and agitated masses of air are spread out. *The particles of aqueous vapour are, in terrestrial clouds, simply the material by means of which the above-mentioned differences between the masses of air are rendered evident to us. The glow of the incandescent masses of hydrogen is the cause of the visibility of the clouds of the protuberances.*

‡ Zeuner, Grundzüge der mechanischen Wärmetheorie. 2 Aufl 1866, p. 165.

* With regard to the increasing density of the layers of air towards the base, the temperature introduced into Formula (4) must, apart from the special law for the decrease of temperature, always agree with the temperature of a layer which lies deeper than $\frac{h}{2}$. This difference, which, as a simple calculation shows, is in general a very considerable one, seems to me to be entirely disregarded in the barometrical estimation of heights, in which, as is known, the mean temperature of the two stations is made use of, and to give a simple explanation of certain periodical phenomena which have lately been urged.

becomes identical with the analogous one in (3), and, in consequence, of the second

(2.)

$$t = t_a$$

The theoretical foundations and essential assumptions necessary for the treatment of the phenomena in question on the sun's surface having been explained, a reconstruction and simplification of the above equations, more suitable to the object in hand, may well follow.

If H denote the height to which a body with the initial velocity v on the sun's surface is hurled up in a vertical direction, then, taking the diminution of gravity into account, we have:

$$v^2 = 2gH \frac{r}{r+H}$$

or,

$$\frac{v^2}{2g} = \frac{rH}{r+H}$$

This value substituted for $\frac{v^2}{2g}$ in Equation (1) gives:

$$t_i = \frac{rHA}{xc\,(r+H)} + t_a;$$

or taking $\frac{rHA}{xc\,(r+A)} = a$, and, according to our assumption, $t_a = t$, we obtain the following for Equation (1):

$$t_i = a + t \qquad (\text{I.})$$

Further, let

$$\frac{x-1}{x} = \frac{1}{q}$$

$$\frac{p}{a_1 a} = b$$

$$\frac{g}{g_1 r_1} = m$$

The Equations (2) (3) (4) become then converted into the following:

$$\frac{t_i}{t} = \left(\frac{p_i}{p_a}\right)^{\frac{1}{q}} \qquad (\text{II.})$$

$$\sigma = b\frac{p_i}{t_i} \qquad (\text{III.})$$

$$p = p_h e^{bm\frac{rh}{(r+h)t}} \qquad (\text{IV.})$$

In addition there is obtained from these four equations by elimination the following:

$$\sigma = \frac{b\,p_h}{a+t}\left(\frac{a+t}{t}\right)^q e^{bm\frac{rh}{(r+h)t}} \qquad (\text{V.})$$

This equation, of course, expresses the density, σ, of the compressed mass of gas only as a function of the three magnitudes p_h, h, and t; if, therefore, under the assumptions made, three out of the four values considered can be determined by observation, either positively or within certain degrees, the fourth can then be determined. And in fact, partly by spectroscopic, partly by other, observations, fixed extreme values can be obtained for the magnitudes σ, p_h, and h, so that thus a limit for t, that is to say, for the outer hydrogen atmosphere in the neighbourhood of the incandescent liquid separating layer, is also obtained. This value substituted in Equation (1) the value of H being known, gives then at once a value for the inner temperature t_i, and from (III.) and (IV.) fixed values for p_i and p_a can be obtained with equal ease.

(3.)

Now to proceed, however, to the discussion of numerical values, I will commence with Formula (I.)

The lowest value which can be attached to t is evidently 0. We then obtain for the inner temperature t_i the minimum value:

$$t_i = a = \frac{rHA}{xc\,(r+H)} \qquad (5)$$

Having regard to the extreme tenuity, and therefore slight resistance, of the atmosphere even at a very moderate distance from the sun's surface, the value of H may, for the sake of simplicity, be put as equal to the mean height of the eruptive protuberances. A more detailed discussion of the conditions under which this is allowable will be given later.

Protuberances three minutes high are not of very rare occurrence; to keep, however, as close as possible within the limit of an estimation of mean value, I will assume H to be only 1· minutes.

Adopting the metre and centigrade degree as units, I take the heat equivalent A as $\frac{1}{424}$. The product xc is, according to the latest researches of Regnault, for hydrogen 3·409. According to Dulong, the value of x for hydrogen is 1·411.

The numerical value of r requires a somewhat more detailed discussion. It is, according to the preceding, the radius of the separating layer from which the protuberances break forth. There then arises the question, whether this value agrees with that of the sun's radius; that is, whether this separating layer coincides or not with the portion of the sun's luminous disc, which we have made use of for our measurements.

The late researches of Frankland and Lockyer, St. Claire, Deville, and Wüllner have proved that the discontinuous spectrum of hydrogen and other gases can, by increase of pressure, be converted into a bright luminous and continuous spectrum, the bright lines of the discontinuous spectrum passing through a series of very characteristic changes, on the pressure being gradually raised, which principally, as for instance by the line Hβ, consist in a widening out and increasing indefiniteness of outline.

These changes permit within certain limits an estimation of the intensity of the pressure on the spot in question, and Frankland and Lockyer have already hazarded such conclusions. They arrive at the result "that at the lower surface of the chromosphere the pressure is very far below the pressure of the earth's atmosphere."

The researches of Wüllner, I believe, allow even the conclusion *that the pressure at the base of the chromosphere, or at the outer edge of the sun's luminous disc, must lie between 50mm. and 500mm. of a mercury barometer on the earth's surface.*

According to this, the presence of dark lines on a continuous ground in the sun's spectrum no longer compels the conclusion that this continuous spectrum is caused by the incandescence of a solid or liquid body. The continuous spectrum can equally well be considered as produced by the incandescence of a strongly compressed gas.

Wüllner has, in fact, proved this for the sodium line; for in his account of the above-mentioned researches he remarks:—

"At a pressure of 1230mm. the maximum at H_a recedes still further; the whole spectrum is truly dazzling; the sodium lines appear as beautiful dark bands;* consequently, the light of hydrogen gas is sufficiently intense to produce a Fraunhofer's line in a sodium atmosphere – a proof that the light of an incandescent solid is not necessary."

From this it follows that the radius of the visible portion of the sun's disc need not be considered as identical with that of the supposed separating layer, but that the latter probably must be looked upon as situated beneath the layer where, through increased pressure, the spectrum of the hydrogen atmosphere becomes continuous. This view is strongly supported by a consideration of the phenomena of the sun's spots.

However different the views as to the nature of the sun's spots may be, almost all observers agree that the nuclei of the spots must lie deeper than the surrounding portion.† Partly from direct (De La Rue, Stewart, Loewy), partly from indirect (Faye) observations the depth is assumed to be about 8".‡

If, then, the nuclei of the sun's spots are considered as scoriaceous products of a local cooling down on an incandescent liquid surface and the penumbra as clouds of condensation, which at a certain height crown the coasts of these slag islands, the simplest assumption is, that the (according to this theory) necessarily liquid surface is identical with the surface of the separating layer in question from which the protuberances break forth. The radius r of this surface therefore, the observed semi-diameter of the sun being expressed in seconds, would be approximately—

$$r = R - 8''$$
$$r = 15'52''$$

* In consequence of the high temperature in the tubes, sodium volatilises out of the glass. At a pressure of 1000mm. the sodium lines are still luminous.

† Spoerer says, however, "We consider the spots to be cloudlike formations far above the luminous surface of the sun's body. The penumbra is simply a collection of smaller spots, through the spaces between which the luminous surface is visible above which the spot is situated." (Comp. Pogg. Ann. lxxviii. 270.)

‡ From calculation of Carrington's observations Faye finds this depth to be ·005 — ·009 of the sun's radius. (Comp. Rend. lxi. 270.)

Accepting Hansen's determination of the mean parallax of the sun, 8"·915 we obtain

$$r = 680{,}930{,}000 \text{ metres};$$

consequently,

$$8'' = 5{,}720{,}500 \text{ metres.}$$

We have accordingly, in order to get at a numerical estimation of the absolute minimum temperature in the space from which an eruption 1·5 minutes high breaks forth, to introduce the following values into Formula (5):—

$$r = 680{,}930{,}000$$
$$H = 64{,}370{,}000$$
$$A = \frac{1}{424}$$
$$xc = 3{\cdot}409$$

It is then found,

$$t_i = 49{,}690^\circ$$

If for H a double so high a value be taken, viz., the by no means rarely observed height of eruption of three minutes, a minimum value

$$t_i = 74{,}910^\circ$$

is obtained.

The question arises here, however, are we at all authorised to introduce the extreme observed heights of protuberances at once into our formulæ as values of H, in which H denotes the height to which a body hurled up from the surface of the sun would rise if there were no resistance. If in fact, and it is conclusively proved by observation, we are dealing with ascending masses of nascent hydrogen, the ascent can also take place according to the Archimedean principle, similarly to the heated masses of air, which are thereby lighter than the surrounding portions, issuing from a chimney. It is however at once manifest that both causes of motion with regard to the time in which the masses reach a certain height are essentially different. Without entering more specially into this circumstance, it is clear that the time which, in virtue of the Archimedean principle, a protuberance requires to reach a certain height H, must under all circumstances be greater than the time expended by a body thrown up with a certain initial velocity, and without resistance to the same height H.

Consequently, a possibly correct observation of the time which an ascending protuberance requires to attain a certain height may serve as a criterion, whether we have to regard this height as the result of the first cause or not, and only in the former case can this height be made use of as an integrating constituent in the above formulæ.

According to the assumptions made, the exit opening (Auströmungsöffnung) of the protuberances is situated in the incandescent liquid separating layer at a depth $h = 8''$ below the visible border of the sun's disc. The height of a protuberance from the plane of the exit-opening was expressed above by H.

Let now:

τ = the time occupied by the protuberance in passing from the opening to the height H,

τ_1 the time occupied by the protuberance in passing from the height h, i.e., from the outer border of the photosphere, to the height H,

v, the velocity at the exit opening,

v_1, the velocity at the height h.

Then assuming the first cause, and disregarding the decrease in the intensity of gravity (g) we obtain the following equations:—

$$\tau = \sqrt{\frac{2H}{g}} \qquad \tau_1 = \sqrt{\frac{2(H-h)}{g}}$$

$$v = \sqrt{2gH} \qquad v_1 = \sqrt{2g(H-h)}$$

Then making

$$H = 64{,}370{,}000 \text{ m.}$$
$$h = 5{,}722{,}600 \text{ m.}$$
$$g = 274{\cdot}3 \text{ m.}$$

we have

$$\tau = 11 \text{ min. } 25 \text{ sec.} \qquad \tau_1 = 10 \text{ min. } 54 \text{ sec.}$$
$$v = 187{,}900 \text{ mm.} = 25{\cdot}32 \text{ geogr. miles.}$$
$$v_1 = 179{,}400 \text{ mm.} = 34{\cdot}17 \quad ,, \quad ,,$$

If, therefore, we observe a velocity of ascent of the quoted magnitude, we are entitled to make use of the height obtained by the protuberance in the above time in our equations. I have often observed such a rapidity of evolution, and annex a sketch of a protuberance, the observed velocity of ascent of which agreed well with the value above found.

With respect to the enormous initial velocities of motion, Lockyer has by his magnificent observations of the change in refrangibility of the light arrived directly at results of exactly the same order.

Lockyer,* during the short period of observations of this nature, found 40 and 120 English miles per second as maximum values for the velocities of vertical and horizontal gas currents in the chromosphere. The above values expressed in English miles gave,

$$v = 123{\cdot}1 \text{ English miles}, \quad v_1 = 117{\cdot}7 \text{ English miles},$$

and agree, therefore, with Lockyer's values.

But movements of such magnitude pre-suppose, necessarily, according to the mechanical theory of heat, differences of temperature of 40,690° C. for hydrogen.

We shall, accordingly, be able to ascertain the actual temperature if we can succeed in determining the temperature, t, of the outer hydrogen atmosphere at a certain spot. Why this temperature is taken as agreeing approximately with the temperature in the vicinity of the exit opening has already been discussed.

(4.)

An extreme value for t is obtained by discussion of Eq. V. This equation is:—

$$\sigma = \frac{b p_h}{a+t}\left(\frac{a+t}{t}\right)^{q} e^{\,bm\frac{rh}{(r+h)t}}$$

The density σ of the included mass of gas is in this expressed as function of the three magnitudes p_h, h and t. I shall now show that σ must not exceed a certain value, by which the value of t is also indirectly fixed within a certain limit, since the magnitude $p_h + h$ are determined within certain limits by the observations already quoted.

Stress has already been laid upon the fact that the explanation of the eruptive protuberances necessarily requires the assumption of a separating layer which separates the space from which the eruptions break forth from that into which they discharge. Only by such a separating layer are the requisite differences of pressure made possible.

With regard to the physical constitution of the separating layer the further assumption must necessarily be made, that it consist of a substance other than in a gaseous condition. It can therefore only be liquid or solid. If, having regard to the high temperature, we exclude the solid condition, there then only remains the assumption, that the separating layer consists of *an incandescent liquid.*

With respect to the inner masses of hydrogen bounded by this layer, two assumptions seem on superficial considerations possible, viz.:

1. The whole interior of the sun is filled with incandescent hydrogen: the sun therefore resembles a vast hydrogen bubble surrounded by an incandescent liquid envelope.

2. The hydrogen masses which burst forth during the eruptions are local accumulations in vesicular spaces, which form in the surface layers of an incandescent liquid mass, and which break through their envelope in consequence of the increasing tension of the included gas.

Under the first assumption a stable equilibrium could only exist when the sp. gr. of the liquid boundary layer is lower than that of the layer of gas directly beneath. The density of a ball of gas, the particles of which obey the laws of Newton and Mariotte, increases, however, from the exterior to the interior, consequently the sp. gr. of the separating layer must necessarily be lower than the mean sp. gr. of the sun; if, on the other hand, the mean sp. gr. of the sun be taken as the extreme sp. gr. of the liquid separating layer, this value would at the same time involve the assumption that all the deeper layers, therefore the layer of gas immediately below, possess the same sp. gr.

The interior of the sun would then no more consist of a gas, but of an incompressible liquid. All these properties are evidently a necessary consequence of the assumption, that the sp. gr. σ of the compressed mass of gas which breaks forth during eruptions attains its maximum value, viz., that of the sun's mean sp. gr.

Then in this case the first assumption is changed into the second, viz., that the sun consists of an incompressible liquid, in which local accumulations of incandescent hydrogen masses form near the surface, which on the necessary differences of pressure burst forth from the hollows containing them as eruptive protuberances.

However small the hollows may be assumed to be in special

* Proceedings R.S. No. 115 (1869). Comp. Rend. lxix. 123.

cases, the sp. gr. of the enclosed gas masses must not be taken as higher than that of the enclosing liquid, since otherwise the compressed gas masses would, in virtue of the Archimedean principle, sink down into the interior of the sun.

The sp. gr. of the sun is, according to the latest determinations,

1·46.

Substituting this value for σ and for a (in Formula V.), the above found value 40,690, also for h the value 8″ in metres, we obtain for the extreme values p_h = 0·050 m. above given, the following values of t:

for p_h = 0.500 m. t = 29,500°
for p_h = 0·050 m. t = 26,000°,

therefore in mean, t = 27,700°.

On differentiating Equation (5) by t, the differential quotient $\frac{d\sigma}{dt}$ is negative. From this follows that the values found above for t are also minimum values.

From the mean value of t for the temperature of the sun's atmosphere the value of p_h is found = 0·180 m. These values will be those made use of in the following calculations.

It may be noticed in connection with the high numbers obtained for the temperature values, that they are about eight times higher than the temperatures of combustion of a mixture of detonating gas as found by Bunsen, and that iron must permanently exist in a gaseous condition in the sun's atmosphere.

With the above value for t = 27,700° we obtain from Formula (I.) for the inner temperature

$$t_i = 68{,}400$$

substituting these two values of t and t_i in Formula (II.) we have

$$\frac{p_i}{p_a} = 22{\cdot}1$$

i.e., the pressure in the interior of the space from which the protuberances break forth is 22·1 times greater than the pressure on the surface of the liquid separating layer. Further substituting the value for t in Formula (IV.) and assuming as before the value of h to be 8″, we have

$$\frac{p_a}{p_h} = 766{,}000$$

as the relation of the pressure on the fluid surface of the sun to the pressure at the height h, where the hydrogen spectrum, in consequence of the pressure, begins to become continuous.

Substituting for p_h the above value of 0·180 m. mercury, we have

p_a = 184,000 atmospheres,

and consequently for p_i = 4,070,000 „

If the depth be calculated at which in the interior of the liquid mass of the sun which has a sp. gr. of 1·46, and simply as the result of the hydrostatic pressure, this maximum pressure of p_i would be attained, it is found that this would occur at a depth of 139 geographical miles below the surface, *i.e.*, at a depth of about 1·46 arc seconds, or $\frac{1}{658}$ of the sun's semidiameter.

Even if the liquid condition be put quite out of question, and, under assumption of a much larger atmospheric envelope of hydrogen, the depth in it be calculated, at which the atmospheric pressure becomes equal to the inner pressure p_i, it is found that even assuming a temperature of 68,400°, that depth is only 27″ below the visible edge of the sun's disc, or about $\frac{1}{36}$ of the sun's apparent semidiameter.

This circumstance shows how rapidly the pressure must increase towards the interior of the sun's body, and thus justifies the assumption that in the interior of the sun, even at such high temperatures, the permanent gases, for example, hydrogen, can only exist in an incandescent liquid condition.

(5.)

A surprising result is obtained if, under the assumption of a nitrogen or oxygen atmosphere of equal weight and temperature to the hydrogen atmosphere above considered, the pressure be calculated which is reached in those atmospheres at heights at which the hydrogen spectrum commences to become continuous. If at a depth of 8″ below the visible edge of the sun's disc, *i.e.*, a the riwean of the supposed separating layer, the pressure of the three atmospheres of hydrogen, oxygen, and nitrogen be assumed as equal, and that p_a = 184,000 atmospheres, a value which from the above, corresponds to the assumed value of p_h. The following values are obtained for the pressures at the temperature above found t = 27,700° on the surface of the sun's *visible* disc in the three atmospheres :—

Hydrogen p_h = 180 millimetres.
Nitrogen p_h = $323\frac{1}{1071}$ „
Oxygen p_h = $124\frac{1}{1088}$ „

It follows from these that, the assumptions made, the quantities of the two latter gases are, in proportion to the quantity of hydrogen in that layer in which the spectrum of the latter commences to be continuous, infinitely small. This would, as is evident, also be the case if the weights of the two atmospheres were assumed to be many million times greater, although having regard to the specific gravity, a 14-times smaller weight of nitrogen and a 16-times smaller weight of oxygen would suffice, in order that under the assumed conditions the density of these two gases should coincide at the base with that of hydrogen.

According to our former considerations, the sun's mean specific gravity would also in this case have to be assumed as the maximum value of the density at the base of these atmospheres, and it is easy to calculate, with the help of Formula 3, and the known specific gravities of oxygen and nitrogen, how high the weights of these two atmospheres would have to be assumed in order to attain this maximum value.

As result is obtained, that the weight of the oxygen atmosphere could only amount to ·56, that of the nitrogen atmosphere to ·64 of the weight of the existing hydrogen atmosphere.

If therefore the simultaneous existence of these three gases on the sun's surface be assumed, and the influence of atmospheric motion be disregarded, the rays emitted by the continuous spectrum of the hydrogen layers would, on their path to our eyes, pass through so small a number of incandescent nitrogen and oxygen particles, that the absorption caused thereby is a vanishing one, and therefore, as is in fact the case, the presence of oxygen and nitrogen in the sun's spectrum could not be demonstrable by dark lines.

Although the motion of the gases is active in lessening the differences just considered, the existence of the chromosphere proves clearly the slight influence of this action in consequence of the great intensity of gravity, and the considerable height of the layer considered (compare Formula 4).

In order, however to explain through the circumstance indicated the absence of lines in the sun's spectrum of two bodies of such universal distribution on the earth as nitrogen and oxygen, the very slight emissive power of the permanent gases in proportion to that of volatilised bodies must also be taken into consideration. If the emissive power of different gases at the same temperature for rays of the same refrangibility be referred to equal very minute weights of these gases,* the before-quoted experiment of Wüllner's, in which the small amount of sodium volatilised in the Geissler's tube emitted more light than the hydrogen gas under a pressure of 1,000mm., gives a beautiful proo of the extraordinary difference of emissive—and consequently according to the theorem of Kirchhoff of absorptive—power of different gases at the same temperature. Only by consid ration of this circumstance is the contradiction removed, which could be deduced against the above explanation of the absence of nitrogen and oxygen lines from the fact that in the sun's spectrum the lines of bodies are present whose vapour densities, as a consequence of their simple relation to the atomic weights, must be much higher than the density of oxygen and nitrogen.

From these considererations, partly directly and partly indirectly through a longer series of conclusions, a detailed exposition of which I reserve for another occasion, the following result :—

1. From the absence of lines in the spectrum of a star shining in its own light, the absence of the corresponding element must not be inferred.

2. The layer in which the reversal of the spectrum occurs is different for each element—the higher the vapour density, and the lower the emissive power of the element, the nearer it is situated to the centre of the star.

3. For different stars, under otherwise similar conditions, this layer lies the nearer to the centre the greater the intensity of gravity.

4. The distances of the reversing layers of the separate elements, both from the centre of the star and from each other, increase with an increase of temperature.

5. The spectra of different stars are under otherwise similar conditions the more rich in lines, the lower their temperature and the greater their mass.

6. The great difference of intensity of the dark lines in the sun's spectrum and other fixed stars does not depend only on the differences of absorptive power, but also on the depths at which the reversal of the respective spectra takes place.

In conclusion, I would offer a few remarks on the application of the observations carried out on rarefied gases to the heavenly bodies. Lecoq de Boisbandrau* has recently pointed out, with

* Compt. Rend. lxx. p. 1091.

reference to Wüllner's investigation on the variability of spectra at different pressures and temperatures, that the results obtained must only be applied with the greatest care to the conditions of pressure of the sun's atmosphere, as the changes in the spectra are due far more to temperature than to pressure. But even under the assumption that this conjecture should become verified by special experiments, this circumstance would influence the results brought forward in this communication but in a slight degree. For the nature of the function (Formula 5) which served us in determining the temperature of the atmosphere is such that the pressure p_h under which the hydrogen spectrum becomes continuous may be varied within very wide limits without thereby causing any considerable alterations of the requisite temperature. Thus it was shown above that, by introducing the extremes of the pressure assumed which were in the proportion of 1 : 10, the temperature values resulting were only in the proportion of 1 : 1·5.

Nevertheless, the separation of the influences which pressure and temperature exercise on the nature of the spectrum of luminous gases must be regarded as a problem the solution of which is of the highest importance for astrophysics.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

Section A.—Mathematical and Physical Science

Rainfall: its Variation with Elevations of the Gauge.—Mr. Charles Chambers, F.R.S. The fact is well known to meteorologists that the quantities of rain received in gauges placed at different heights above the ground diminish as the elevation of the gauge increases. Several attempts have been made to explain this phenomenon, but none of them are so satisfactory as to discourage the search for other causes that may contribute substantially or mainly to its production. Hence the submission for the consideration of the British Association of this further attempt. One of the principal causes of rain is undoubtedly the transfer, effected by winds, of air charged with moisture in a warm damp district to a colder region, where the vapour it contains is partially condensed. The temperature of the lower as well as of the higher horizontal strata of the atmosphere being reduced by this transfer, it may fairly be inferred that condensation of vapour may also occur in the lower as well as in the higher horizontal strata. The rain caught by a gauge at any given elevation will therefore be the sum of the condensations in all the strata above it, and thus the lower a gauge be placed the greater will be the quantity of rain received by it. Again, it is known by observation, that there is at all times a greater or less difference of electrical tension between the atmosphere and the surface of the ground. If then—in accordance with the views of Prof. Andrews as to the continuity of the liquid and gaseous states of matter, from which it follows that the change of other physical properties must also be continuous—we regard the particles of vapour suspended in the electric bodies in relation to the dielectric principal constituents of the atmosphere, they will be polarised by induction from the ground. This polarisation will give rise to an attraction between every particle and the neighbouring particles above and below it; and being stronger in the particles near the ground than in those more remote, the tendency of the particles to coalesce—which will increase, by their mutual induction, as two neighbours approach each other—will be greatest near the ground. Thus it may be, each particle gathering to itself its neighbours in succession till their united density exceeds that of the atmosphere generally, some rain drops are formed, and that in greatest abundance near the ground. If this be the true cause of any substantial part of the phenomena in question, then, as the variation in intensity of electrical polarisation of the particles will vary with the height most rapidly near the ground, so the variation in the rainfall near the ground should be more rapid than at a greater elevation, and such is indeed the fact. Also, if the idea be correct, it will probably serve to explain other phenomena which it was not specially conceived to meet; and so it does. For, first, it requires that the rainfall over even ground, where the electrical tension is relatively weak, should be less than over similarly situated forest land, where at the tops of the trees, ends of branches, and edges of leaves, the tension is high, and this is in accordance with observation. And, secondly, the tension being relatively high at the tops of the elevations of a mountainous district, the rainfall should be greater there than in the neighbouring plains; this, again, is borne out by observation. Further, at the commencement of a passing thunderstorm, a sudden heavy shower of rain will often fall for a few moments and then suddenly cease. May not this arise from the approach, by the agency of opposite wind currents, of detached masses of differently charged clouds, the process just described of formation of rain drops going on rapidly in each mass as the two come near each other, and stopping when, by a flash of lightning between them, the two masses are brought into the same electrical condition? An experimental test of this idea would be to repeat Dalton's measures of the pressure of vapour in the vacuum space of a mercurial barometer tube—filling that space with air and a little water, and compare the values found when the mercury was charged with electricity and when not so charged. If in the former case a less pressure was found, we might conclude that the particles of vapour are really susceptible of electric induction, and the amount of difference existing would enable us to estimate whether the attractions of the particles upon each other were strong enough to cause the formation of rain-drops hypothetically attributed to them above.

* A perfect transparency of the gas mass to all rays emitted by itself is here assumed, a supposition which is the nearer the truth the smaller the weights compared.

Section C.—Geology

On the Mountain Limestone of Flintshire and part of Denbighshire.—Mr. G. H. Morton. Minute details of the physical structure of the region, and lists of the fossils, showed that these beds have been erroneously referred to the Millstone Grit, and that they were really Mountain Limestone, the shales and sandstones being intercalated among the typical rocks. The white limestone was an ancient coral reef, with the organisms exquisitely preserved. Mr. Hughes protested against co-relating with the Yorkshire beds, while Mr. Bailey supported the opinions of the author.

On the formation of Swallow-holes, or Pits with vertical Sides, in Mountain Limestone.—Mr. L. C. Miall. The author distinguished between cavities formed by direct excavation and those produced by subsidence of part of the roof of a cavern. The curious pits near the Buttertubs Pass at the head of Swaledale, were regarded as typical of the first kind, and their appearance and mode of formation were described, especially the vertical fluted sides and the isolated fluted pillars, which were ascribed to the action of dropping water, aided by pebbles. A basin is first formed upon a ledge of rock, and as the excavation proceeds it produces a semi-cylindrical scar, with sharp ridges upon the face of the limestone wall, as if cut by a gauge. The presence of a thick surface-covering of alluvium or drift was necessary to absorb and retain the rainfall, and to distribute it slowly and regularly. The limestone of a bare plateau furnishes fissures in great variety, but they are not true swallow-holes. Regular and well-marked joints were also necessary to the production of fissures, as they permitted the ready escape of the waters of erosion. The texture of mountain limestone, and its power of receiving and retaining sharp impressions, gave the peculiar features to the swallow-holes excavated in it. Some swallow-holes were due to the subsidence of an undermined crust. These frequently lie in a line, sometimes in a ring round a hill-side. A particular description of some near Ripon was given, and the testimony of eye-witnesses as to their sudden appearance was quoted. Swallow-holes are often disguised by surface accumulations. Many conical hollows in drift are probably due to concealed cavities of subsidence.

On the Stratigraphical Distribution of the British Fossil Gasteropoda.—Mr. J. L. Lobley. This was the third of a series of reports by the author on British fossil mollusca. By the help of diagrams were shown the distribution of the species, and the range, increment, decrement, and maximum development of the genera, families, and orders of the *Gasteropoda*. The cainozoic deposits contain the greatest number of genera and sub-genera, though they are numerous also in both the mesozoic and palæozoic rocks. A large number of genera and sub-genera are characteristic of single formations, and these are especially numerous in the carboniferous limestone, the lower lias, the middle Eocene, and the older and newer Pliocene. Details of the range and of the distribution of species of each of the families of *Gasteropoda* were given, and the large groups or orders were similarly passed in review. The remarkable contrast between the distribution of the species of *Holostomata* and *Siphonostomata* was pointed out, the former being very largely developed in each of the three great divisions of the stratified rocks, while the other and more highly organised order is absent from the palæozoic, has only a few species in the Mesozoic, but is largely represented in the cainozoic formations. The distribution of the species without reference to generic family or ordinal divisions showed that the maximum number occurs in middle Eocene strata, from which beds 420 species have been described. The total number recorded in the paper exceeds 3000, but a great number of these are recurrent. The author insisted, in concluding, that as the recognised formations were of different values, as they had been unequally explored, as many formations were wanting in Britain, as the organic remains in the different formations had not been subjected to a uniformly rigid scrutiny, and as the British area, compared with the whole world, was so small, only the most general conclusions could be drawn from this investigation as to the progress of life on the globe.

On the Glacial and Post-Glacial Beds in the Neighbourhood of Llandudno.—Mr. H. F. Hall. The necessity for a more exact definition of boulder clay, and for discarding a name which is made to include a series of beds formed under very different conditions, was insisted upon. The section at Llandudno exhibited a base bed formed by the action of an ice-cap covering the whole land down to the sea level, which ground together the different materials of the bed. The overlying beds lie unconformably on this base bed, and show land and water conditions connected with a much more genial climate. From the section, the author concluded that colour is no criterion for deciding as to the base bed, as it varies in each district with the underlying rock; that the materials of the base bed are obtained from the rocks of the neighbourhood; that this bed was the result of the pressure and passage of land-ice disintegrating the surface of the land which it capped; that to this bed, which is invariably denuded and has the superposed beds lying unconformably upon it, the name "Boulder-clay" should be restricted; and that the red clay, over the sands and gravels which overlie the base bed, is variable in colour and constituents, showing a change which produced extensive denudation in more northern regions, the materials being spread over the sea bottom mixed with pebbles and boulders, which fell from melting or stranded icebergs and ice-flows. The author said there was a hope of being able to co-relate the beds of the eastern districts with those he had described.

Remarks on the Fossils from the Railway Cutting at Huyton.—Mr. W. Carruthers. The great value of this collection, made by the Rev. H. Higgins, depended as much upon the comparatively limited number of species met with as on the fine state of preservation in which they occurred. It was possible to arrive at considerable—in some cases absolute—certainty as to the different parts of the same species. Of the four species of *Calamites*, the materials existed in the specimens from Huyton for reconstructing the entire plant of at least one. The roots, long considered to be a distinct plant under the name *Pinnularia*, were present in great abundance. It had a delicate fistular stem of the type described by Professor Williamson at a previous meeting of the section, but of great size. The scars of the fallen branches were shown in several specimens as well as the foliage, which was preserved in the early bud condition, as well as in its fully developed state. Several fruits showing the structure of the cone, described by the author under the name *Volkmannia binneyi*, but with differences that were at least of specific value. A cone having the structure of that described by Professor Williamson probably belonged to *Calamites longifolius*, with the foliage of which it was associated in these beds. Specimens of *Sphenophyllum* were exhibited and referred to *Calamites*. The light thrown on the structure of *Lepidodendron* by the specimens was then dwelt on, and especially two undescribed cones—one long and slender, with a single sporangium on each scale, the other short and having two sporangia on each scale. The stem and foliage of *Flabellaria*—a palm-like lycopodiaceous genus—occurred among the fossils, as well as several species of beautifully-preserved ferns. Two specimens of insect remains had also been found—the one by Messrs. Clementshaw and Smith, young gentlemen whose interest in natural science was due to the revival of those studies in our great schools, and whose personal efforts had greatly contributed to its advancement at Rugby. Professor Williamson contended that the interpretation he had given on a former occasion of the structure of the stems of *Calamites* was more in accordance with the hundreds of specimens he had examined than those just expressed; but, in reply, Mr. Carruthers maintained, on structural grounds, the correctness of the views he had expounded.

Section D.—Biology

Department of Zoology and Botany

The Secretary read a paper by Dr. J. E. Gray, F.R.S., *On the Whalebone Whales of the Southern Hemisphere.* The author remarked that formerly the number of Cetacea was believed to be very limited, and that each species was supposed to have a very extensive geographical distribution. At one time, even, the hunchback of the Cape of Good Hope was supposed to be the same species as the whale of the North Sea. The author gave a list of the true whales, or Balænidæ, the hump-backed whales, and the Physalidæ. Five species of the first group were described, three of the second, and one of the last group. Reference was also made to three apparently different forms of Finne whales, known only from having been seen swimming.

Dr. Cunningham read a paper *On the Terrestrial and Marine Fauna of the Strait of Magellan and Western Patagonia.*

Professor Van Beneden read some notes "*Sur les Parasites*"—One frequently finds described under the title of "Parasites" animals which do not demand more than a place to live on, and do not live at the expense of their neighbour, such, for example, as the Adamsia by the side of the Pagurus. Some of these do not completely enjoy their liberty, as the Coronula on the whale. This type I would designate under the name of Oikasitæ, whereas those which are perfectly free I would designate as Coinositæ. The true Parasites may also be divided into groups: those that have no communication with the exterior are the Xenositæ, these, like the Cysticercus, are possibly only transitory forms: others, having arrived at the end of their journey, live in the open passages of organs, occupying themselves with reproduction, and these I would designate Nostocitæ; and lastly, those which appear to stray by the way, without a hope of arriving at the end of their journey, and indeed only by chance returning to the good road, such as the vesicular and agamic worms which frequent the flesh of carnivores, I would call Planositæ.

Professor Van Beneden exhibited a specimen of a species of Echinorhynchus, apparently new, lent to him for exhibition by Dr. John Barker of Dublin.

On Brackish-water Foraminifera.—Mr. H. B. Brady. The author described a form of Foraminifera from a fresh-water pond, some five or six miles from the sea, and while describing in addition a large number of new species from brackish water, he also alluded to the fact that he had met with some Foraminifera whose tests had withstood the action of acids. Without wishing positively to assert the absolute presence of chlorophyll granules, as occurring in some species, he might yet mention that he had found traces of it in the test of some of the forms he had examined.

On a stock-form of the parasitic Flat-worms.—Mr. E. Ray Lankester. This worm was found parasitic in *Tubifex rivulorum* from the Thames. It had the form of a fluke with very mobile head, no alimentary tube, a very elaborate vascular system, and simple generative organs. A small mobile tail was attached to one end of the worm at the opening of the vascular system. This tail was only paralleled by that of the *Cercariæ* or larvæ of Flukes. A worm known as Caryophyllœus, which lives in carp and tench, was stated to be exactly like the new worm in respect of its mobile head, wrongly held to be the tail by Emile Blanchard. The tailed form *Uroscolex*, Mr. Lankester considered to be the larva of *Caryophyllœus*, and hence we have in this simple worm a representative of the common ancestors of all the Trematods and Cestoids. Mr. Lankester said he was informed by Professor Van Beneden, that last year Caryophyllœus had been shown to have a six-hooked embryo.

On Worms from Thames mud.—Mr. E. Ray Lankester. The author showed that the tons of red worms which are the only non-microscopic tenants of the foul parts of the Thames at London, consist of three distinct species, and a natural hybrid between two also occurs, as he demonstrated from minute study of their characters. The species are *Tubifex rivulorum*, *T. umbellatus* and *Limnodrilus*, sp. *incert.*; the last very abundant. Mr. Lankester then mentioned the gregarinæ of these worms, and the discovery of their pseudo-naviculæ having long stiff processes, so that they run into the worms like pins, and in this way penetrate into previously uninfested worms. The formation of the spermatophors of this group of Annelids (the Oligochæta) discovered by Mr. Lankester—Professor Claparède having mistaken them for Opalinoid parasites—was also detailed.

Department of Anatomy and Physiology

On the Embryonal Development of the Hæmatozöon (Bilharzia).—Dr. Cobbold. After commencing with a general description of this remarkable parasite, Dr. Cobbold proceeded to notice the manner in which the larvæ escaped from the eggs; and also their subsequent activity and remarkable alterations of form and structure. He had obtained ample evidence of the existence of a complicated water-vascular system, similar to that described by Dr. Guido Wagener, as occurring in the larva of *Diploditcus*. The prevalence of the Bilharzia disorder in Egypt and at the Cape was well known; and it had recently been suggested by Dr. Aitkin, that these parasites had some connection with the so-called Delhi boils. He refrained from entering into professional details in this matter: but stated that he had performed a large number of feeding experiments on small fishes, crustaceæ, and molluscs, with the view of putting the question of injection beyond the region of mere conjecture. Dr. Cobbold added that he had obtained for a month past about 10,000 eggs of Bilharzia daily, from a case under his care.

Dr. Cobbold exhibited the heart of a dog filled with Hæmatozoa causing the animal's death. He had received the specimen from Mr. Robert Swinhoe, H.B.M. Consul at Amoy, China, accompanied by a note from the donor, stating that the dog "died on the 18th of April, 1869, at Shanghai, after three days of great suffering." Hitherto, following the authority of M. Bohe-Moorea Diesing and other systematists, he had been accustomed to regard this form of entozoon as the species called *Spiroptera sanguinolenta*; but, in the author's opinion, this view would have to be changed. He hoped, before long, to be able, by further investigation, to set this point at rest. The presence of entozoa in the heart and blood-vessels of animals and man is much more common than is supposed. Thus, MM. Grube and Delafond, who examined 480 dogs, found Filariæ present in nearly 5 per cent. Most of these parasites, however, were of microscopic size; being probably the brood of the species marked *Filaria sanguinis* in Dr. Cobbold's list. They estimated that these verminiferous dogs severally harboured from 11,000 to 224,000 of these larval hæmatozoa.

Note on Methæmoglobin.—Mr. E. Ray Lankester. It was shown that carbonic acid, when passed through a solution of oxyhæmoglobin, gave rise to two new bands in addition to those of the oxyhæmoglobin itself. This was the nitrite-hæmoglobin of Dr. Gamgee, and the brown cruorine of Mr. Sorby, also identical with methæmoglobin as described by Preyer. Addition of a minute quantity of acetic acid to this solution caused the disappearance of the oxyhæmoglobin bands and intensification of the two new bands, which are those of what really was originally called methæmoglobin by Hoppe Seyler. It can be formed by the passage of CO_2 alone if a weak solution of hæmoglobin is used, as was done by Heynsius, who mistook this product for hæmatin. Its band in red is not identical with that of hæmatin as supposed by Hoppe Seyler and Heynsius, and all previous observers, including Hoppe Seyler, Preyer, Gamgee, and Sorby have missed the second band in blue (the fourth of the mixture of oxyhæmoglobin and methæmoglobin) now figured and described. It was shown that no separation of an albumen accompanies the change of hæmoglobin into methæmoglobin, whilst hæmatin results solely from a splitting up of the hæmoglobin into it and an albumen.

The action of certain Vapours and Gases on the red Blood Corpuscles.—Mr. E. Ray Lankester. These experiments were made with Stricker's gas chamber, which enables the observer to study gradual changes, caused by gaseous reagents, as to the change of form caused by atmospheric air in the red corpuscle of the frog, which had been acted on first by CO_2 as observed by Stricker, was shown to be equally produced by hydrogen, or by carbonic oxide, or by diminution of pressure, hence it was simply to be ascribed to the diffusion of the carbonic acid. The action of cyanogen gas, carbonic oxide, alcohol vapour, chloroform vapour, and especially of ammonia vapour, was described. A distinction was insisted on between mere definition of the nucleus—as caused by some agents—and granulation of the nucleus. The normal living frog's red corpuscles was inferred to be *usually* free from any appearance of definition of the nucleus, and to be devoid of an envelope or æcoid, though owing its form to a remarkable condition of tension, which was readily destroyed by physical agents.

On the Relations of Fins of Fishes to one another.—Professor Humphry.

Department of Ethnology and Anthropology

The Pre-Turkish Frontagers of Persia.—Mr. H. Howorth. In continuation of the previous paper the author showed with the assistance of Vivien St. Martin, Thomas, Prinsep, &c., that after the first century, the Indo-Scyths were called Kouschauk by the Armenians, Koneichang by the Chinese, that their great king Kanichka who was a convert to Buddhism, and introduced that religion into China and Thibet, was, with his people, previously a fire-worshipper, and that the form of Mithraism, which was introduced at Rome by Pompey and derived by him, in the first instance, from the Parthians, was the original religion of the Massagetæ and the Indo-Scyths.

On the decay of the power of the Indo-Scyths, the remains of the nation were conquered by the Avares or White Huns, and are called by Procopius, Priscu, and Cosmas, White Huns, and Ephtalitæ, and by the Persians Hainthelah. The etymology of these names shows they were the Yuetchi or Massagetæ, led and governed by a caste of Huns.

In latter days these White Huns are to be identified with the Khazars, the ancestors of the Circassians. Thus the Circassians are proved to be lineal descendants of the Massagetæ. That the Circassians are allied to the Thibetans was long ago showed by Mr. Hodgson in the Journal of the Asiatic Society. This is the first time their genealogy as a race has been clearly traced out, and it opens up a new light on Asiatic Ethnography.

On the Manx of the Isle of Man.—Dr. King.

Section G.—Mechanical Science

On Ashton and Storey's Steam-Power Meter.—Mr. Ashton. The apparatus described in this paper, as its name implies, shows at all times the measure of the power developed by the steam engine to which it is applied, and registers the aggregate of that power during any required period of time. The mechanisim is very like that of a well-balanced chronometer. The whole of the indicating mechanism is very light, and mounted so as to move with great freedom; and the power required to work it is exceedingly small. Its indications would be especially valuable in the case of steamships. The apparatus has been in practical use about a year.

In the discussion which followed this paper the invention was very highly praised.

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Errata.—Page 464, second column, line 30, for "monodont" read "homodont"; line 35, for "but its analogue in front has" read "but, unlike its analogue in front, has."