THURSDAY, DECEMBER 14, 1876

McLENNAN'S STUDIES IN ANCIENT HISTORY

Studies in Ancient History, Comprising a Reprint of Primitive Marriage. By John Ferguson McLennan, M.A., LL.D., Advocate. (London : Quaritch, 1876.)

THE learned and ingenious author of "Primitive Marriage" has in this volume republished that excellent work, appending to it his paper on "Kinship in Ancient Greece," which originally appeared in the *Fortnightly Review*, and some essays, in which he discusses Bachofen's "Das Mutterrecht," Morgan's work on Relationships, Sir Henry Maine's views on the constitution of the Ancient Irish Family, and the chapter on Marriage in my "Origin of Civilisation."

Bachofen supposes that in the first stage of the development of the family marriage was unknown ; and his theory is that at length the women, being by nature nobler and more sensitive than the men, and impelled moreover by strong religious aspirations, combined to put an end to this system, and to introduce marriage. For this an appeal to force became necessary and was successful. The result of the victory, moreover, was that the women claimed and established a superiority over the men ; they were the heads of the family ; after them the children were named ; through them the rights of succession were traced, and they even exercised a political as well as a domestic supremacy. At length, however, men reasserted their original supremacy ; they reconquered the first place in the family and the state, established their right to the inheritance of property, and to confer their names upon their children. Mr. McLennan considers that Bachofen's "methods and results are equally unscientific ;" he points out that the system of inheritance through females is really no evidence of female superiority, but arises partly from marriage not being monogamous, or such as permitted the certainty of fatherhood, and partly also, he considers, from the fact of women not yet living in their husbands' houses. At the same time he fully concedes to Bachofen the honour of being the first to point out that a system of kinship through mothers only had generally preceded that through the male line.

Nor is Mr. McLennan more disposed to accept the theory of Mr. Morgan, which indeed he characterises as a "wild dream, not to say nightmare, of early institutions." Mr. McLennan supposes that in Mr. Morgan's opinion his tribal organisation was an institution intentionally designed to prevent the intermarriage of near relations. In support of this he quotes the passage in which Mr. Morgan says : "It is to be inferred that the tribal organisation was designed to work out a reformation with respect to the intermarriage of brothers and sisters."

From this and other passages a previous writer in this journal, and I myself, had supposed that Mr. Morgan regarded the steps in his system of development as arbitrary and intentional.

Nor do I even now see how we could have arrived at any other conclusion. Mr. Morgan has, however, explained that this is not his theory, and I am not therefore sure whether either Mr. McLennan or I thoroughly understand his view. The solution which he has given, however, of the origin of the family in Mr. McLennan's opinion—"failing to explain the phenomena must sink below the level of reasonable guessing, to which level indeed it must have sunk even had it explained the phenomena, if by any other set of mere conjectures the phenomena could be equally well explained."

Mr. McLennan himself considers that the earliest form of marriage (if indeed it can be so called) was that still prevalent amongst the Nairs of Malabar, in which one wife was married to several husbands not necessarily related to one another. Under this system the idea of relationship naturally took the form of kinship through females. Family property went ultimately to the daughters of their sisters ; a man's heirs being in the first place his brothers, and subsequently his sister's children. From the Nair system was, in Mr. McLennan's opinion, gradually developed (in many cases through an intermediate form anciently prevalent in Britain) the Thibetan species of polyandry in which the sons of a house took one wife between them. This change eventually introduced kinship through males. It involved the breaking up of the primitive form of the family, and led in time to the transference of the government from the mother to the father. After this the practice of monandry arose, the younger brothers making separate marriages, and thus Thibetan polyandry died out, leaving behind it the Levirate, that is to say, the obligation of brothers to marry in turn the widow of a brother deceased, a custom which the Old Testament has rendered familiar to us. The Levirate next died out, and thus the family slowly assumed the form to which we are accustomed. It will thus be seen that the keystone of Mr. McLennan's system is the practice of polyandry, which, indeed, under his theory is a necessary stage of the development of the family relationship. I cannot, however, regard polyandry as having been a general and necessary stage in human development. I have therefore suggested that individual marriage rose out of capture. That just as a warrior converted to his own use the animals which he captured, and made slaves of the men, so he made a wife of any woman whom he admired, the capture giving him a right which the other men of the tribe did not share. This view, indeed, seems so natural that I wonder it had not been before suggested ; and I observe that one or two recent writers have treated it as a recognised and well-known fact, whereas it cannot at present claim to be more than an individual theory which none of the authorities on such a question, such as Mr. Darwin, and Mr. McLennan himself, have as yet accepted. Mr. McLennan, indeed, denies that my views are "in the least degree probable," and if such a question could be decided by authority, I should at once bow to his decision. Such, however, is not the case, and I will only say that, before my work was published, I had foreseen and weighed as carefully as I was able, the objections which Mr. McLennan has now brought forward ; and that I then thought, and still think, that I have satisfactorily replied to them. The fundamental objection which Mr. McLennan urges I did not indeed expect him to allege. Like Bachofen, I commence with a time when marriage did not exist. Mr. McLennan, however, replies :—"Sir John Lubbock has not only failed to show that the initial stage of his scheme ever

existed, but has failed also to make it in any the least degree probable that it ever existed" (p. 449).

I will, in reply, content myself here with quoting one authority only for the existence of my first stage, an authority for whom I have the highest respect, namely, Mr. McLennan himself:—"I conceive," he says, "that marriage was at first unknown;" in fact, the initial state in his system is practically the same as in mine; the differences between our views lie in the subsequent stages.

In his last essay Mr. McLennan discusses Sir Henry Maine's views on the Ancient Irish Family. The question is very complex, and those who have not Sir Henry's work by their side for reference will not find this chapter very easy to follow.

The Irish family "was anciently divided into four groups known as the 'geilfine,' 'deirbfine,' 'iarfine,' and 'indfine' divisions. . . . . Within the family seventeen members were organised in four divisions, of which the junior class, known as the 'geilfine' divisions, consisted of five persons; the 'deirbfine' the second in order, the 'iarfine' the third in order, and the 'indfine' the senior of all, consisted respectively of four persons. . . . . If any person was born into the 'geilfine' division, its eldest member was promoted into the 'deirbfine,' the eldest member of the 'deirbfine' passed into the 'iarfine,' the eldest member of the 'iarfine' moved into the 'indfine,' and the eldest member of the 'indfine' passed out of the organisation altogether."

A complete family therefore would be composed as follows:—

Indfine.	Iarfine.	Deirbfine.	Geilfine.	
A_1	A_2	A_3	A_4	Fathers and brothers.
B_1	B_2	B_3	B_4	Sons and first cousins.
C_1	C_2	C_3	C_4	Grandsons and second cousins.
D_1	D_2	D_3	D_4	Great-grandsons and third cousins.
			E_4	Great-great-grandsons.

On many points, however, Mr. McLennan dissents from the views of Sir H. Maine.

Sir Henry Maine, for instance, says, "The Brehon writers speak of its (the geilfine division) consisting of a father, son, grandson, great-grandson, and great-great-grandson, which is a conceivable case of geilfine relationship, though it can scarcely be a common one." Mr. McLennan, on the contrary, thinks that "it was, actually or constructively, the only one—when the division was full—*i.e.*, when all its possible members were in being."

Again, Sir Henry Maine considers this strange arrangement to be "a monument of that power of the father which is the first and greatest landmark in the course of legal history."

Mr. McLennan entirely dissents from this, and indeed after discussing Sir Henry Maine's views with ingenuity and erudition, he concludes that the objections he has brought forward "are fatal to Sir Henry Maine's account of the system. He has failed to throw light either on its purposes or its principles. He has made no single feature of it clear in the light of Roman law, and, after all his ingenious reasonings, has left its main features as mysterious as he found them."

Whatever conclusions on these subjects may ultimately be arrived at, everyone who reads Mr. McLennan's book must feel that he brings to the inquiry an immense amount of learning, and has stated his views with great ingenuity. All students of early history will hope that he may have leisure and health to pursue his studies.

JOHN LUBBOCK

OUR BOOK SHELF

Science in Sport made Philosophy in Earnest. Edited by R. Routledge, B.Sc., F.C.S. (London: Routledge and Sons, 1877.)

THE title of this book at once recalls Dr. Paris' "Philosophy in Sport made Science in Earnest." The author, however, tells us in his preface, that the reason he has adopted so similar a title is that his original design was to re-edit Dr. Paris's well-known, but now antiquated, book; finding, however, that mere patchwork would not bring the book into harmony with the present state of science, he determed to treat the subject afresh, and the volume before us is the result of that determination. The inversion of the title is, we think, wise, though some will object to the use of the word philosophy in the sense meant by the author, and will contend that the term physics should have been employed. The graver question is whether, under any circumstances, science should be taught by sugar-sticks. Our own opinion is decidedly against all books of this kind, and there can be little doubt intelligent children prefer not being trapped into the study of any subject, but like work openly and honestly put before them. Such books as the original editions of Mrs. Marcet's "Conversations in Chemistry," or the altogether admirable "Chapters on Sound," and other little books by Miss C. A. Martineau, are the best kind of reading to put into the hands of children who wish to learn the rudiments of natural knowledge. Nevertheless, Mr. Routledge has done his work extremely well. Those who like science and a story running together, will here find a trustworthy, clear, and accurate introduction to the study of physics. W. F. B.

Mushrooms and Toadstools. By Worthington G. Smith. (London: Hardwicke and Bogue.)

THIS is a reprint in a separate form of the descriptions illustrative of two large sheets of figures of edible and poisonous fungi, with the addition of two key-plates. Not having been written and designed for separate publication, it is consequently not so complete as it might otherwise have been, and we doubt whether by itself it will prove of much service in the discrimination of good and bad fungi. Mr. Worthington Smith may be accepted as a safe and trustworthy guide, having himself suffered on one or two occasions from reckless indulgence in doubtful species; he is desirous of sparing others like sufferings, and approaches the subject fortified by experience. In conjunction with the plates this key is admirably suited to fulfil its purpose; as a separate work, we doubt whether the author himself would feel wholly satisfied. If this reprint leads to a wider acquaintance with the diagrams, which ought to find a place in every schoolroom, its reproduction in this form will fully justify the step which the publishers have taken. M. C. C.

Between the Danube and the Black Sea; or, Five Years in Bulgaria. By Henry C. Barkley, C.E. (London: John Murray, 1876.)

THIS book has not been written to take advantage of the interest in Bulgaria excited by the present crisis. Mr. Barkley really spent twelve years in Turkey—the first five commencing shortly after the Crimean war, and the other seven at a subsequent period. He was employed as an engineer in connection with a Bulgarian railway, and had ample opportunities of becoming well acquainted with the country and the people. These opportunities he took good advantage of, and in the volume before us has recorded his impressions and adventures in simple and

interesting style. It is a valuable feature that Mr. Barkley's sojourn in Turkey was not made during recent events, and his narrative is not written with a view to advocate one side or the other in the present unhappy conflict. He saw the Bulgarians in what may be called their normal condition, and had no reason to be prejudiced for or against any section of them. He saw much to condemn and a good deal to praise both in Christians and Mohammedans, but little that was praiseworthy in Turkish officials, "from the Governor-General to the hangman." The work contains much information on the Bulgarians, their characters and ways, and will be found both interesting and instructive.

The District of Bákarganj; its History and Statistics. By H. Beveridge, B.C.S. (London: Trübner and Co., 1876.)

THE publication of this work is somewhat opportune, for under the reformed, if not improved, spelling of the title, our readers will no doubt recognise the district of Backergunge, which, with other districts at the mouth of the Hooghly, was recently overwhelmed by one of the most disastrous cyclone-waves on record. Mr. Beveridge is magistrate and collector of the district, and as such has had a good opportunity of becoming acquainted with it. He has evidently also read a great deal on the subject, and the result is a work which ought to take a good place as a local history. Mr. Beveridge describes the physical features of the district, its antiquities and early history, the pergunnahs and sunderbunds, treats of Government estates, land tenures, the inhabitants, productions, and manufactures. The second part refers to the several departments of the administration, education, &c. This district, from its low-lying position at the top of the Bay of Bengal, has been peculiarly subject to the inrush of the wave which accompanies cyclones. Until the recent catastrophe the great event in the history of the district was an inundation, evidently caused by a cyclone-wave, which occurred in June, 1822. According to contemporary account, 100,000 persons lost their lives, and as many cattle; but this must pale before the recent catastrophe, and henceforth October 31, 1876, will be the black-letter day in Bákarganj. Mr. Beveridge's book will be found to contain a great deal of really valuable information, and if every district in India were treated in a similar manner, we should possess a library of information of the greatest value. The volume contains a good map of the district.

LETTERS TO THE EDITOR

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

Sea Fisheries

PROF. NEWTON has sent you a long reply to my criticism of his Address to the British Association; and although it is exceedingly inconvenient to me, as I believe it is also to him, to continue the discussion at the present time, I must ask you for space, as soon as you can spare it, to point out as briefly as I can, how little progress my friend has yet made in his subject. He was good enough to show me his reply before he sent it to you, and I then told him I should have to meet him on every point in it. Nevertheless he sent it for publication, and as the subject appears to me to be one on which the public should not be misled, I am compelled to ask space for some comments upon it.

The Minutes of Evidence given before the Commission, consisting of 61,831 questons and answers rather frighten him, and he turns to the Index for help. Without a careful study of the evidence the Index is practically useless for my friend's purpose, as I told him. And some knowledge of the habits of sea-fish will be exceedingly valuable in enabling it to be understood. The Index is not a *précis*. There is intrinsic evidence in Prof. Newton's reply that he framed it, I think I may say solely from the Index, and not from the evidence; his tables of increase and decrease, and calculations of the number of questions and answers relating to each, are obviously so, and the value of his arguments may be judged of accordingly. To show what his tables are worth I will take two or three entries in them for examination. "Bream" is the first, and that fish is said to have increased. This is the only entry relating to bream in the whole Index. Bream are common fish on very many parts of our coast; they often congregate in large numbers, sometimes on one part, sometimes on another; they happened to have been unusually numerous off Hastings just before the Commissioners were there, and the fishermen accordingly recorded the increase. But there is not the slightest reason for believing bream were then more abundant than usual on our coasts generally than the reverse. "Brill" has two entries, both in the decrease column. In one case the evidence is that of fishermen in Start Bay, who used the sean nets within half a mile of the beach, and who were furious against the Brixham trawlers for sometimes working in the Bay; the said trawlers, up to that time, and for the twelve years since, finding no falling off in the supply of brill. The other case was at Liverpool, where brill was mentioned, among other fish, by the Inspector of the Fish Market, as having diminished. He made the remarkable statement that not 1 per cent. of sea-fish of all kinds was brought to the market in 1864 compared with what had been taken there twenty years before. As the number of fishing-boats had increased during that interval, and the price of fish was, if anything, rather less, owing to the large supplies sent by railway from the east coast direct to the fishmongers'[1] shops, and not going into the market, it is very clear that if the fishermen were getting a living from the 1 per cent. in 1864, they must have been making at least 100 times as much money twenty years before, which is an absurdity. "Cod and ling" come next. Here my friend had better look to his arithmetic. There is no doubt that cod, and haddock especially had fallen off at many of the inshore fishing grounds. They are both species which have fluctuated very much in numbers in many places, the haddock in particular making its appearance in abundance for a season or two, and then becoming very scarce; or they have left places where they were abundant for years together, and again unexpectedly returned. The last report I had from the north-east coast of England, just three years ago, was that the line fishermen were doing well, and their only complaint was of the scarcity of mussels for bait. But it was along this coast that the great outcry against the trawlers arose in 1863, which led to the issue of the Sea Fisheries Commission; and a great deal of the evidence given there was such as might have been expected under the circumstances.

It will be difficult to treat the next entry seriously, but I will try to do so. In a table professing to show the increase or decrease of the fishes with which our markets are supplied, he notes one particular kind as having decreased, and he counts the two instances as helping to prove that our sea fisheries are on the high road to ruin. Those persons who have even but a very slight acquaintance with sea fishing will be rather surprised to hear that the fish whose end is approaching is nothing less than the "dogfish!" one whose utter extermination would gladden the hearts of the fishermen from the Shetlands to the Land's End, and from Dingle to Dover. Hordes of these predatory and mischievous fishes roam round the coasts of the British Islands; sometimes they swarm in one place, sometimes in another. In the herring season the destruction they have caused to both fish and nets has been such that the fishing where they were has been almost entirely stopped for nights together, and the long-liners also suffer severely from them. They are said to have seriously interrupted the Yarmouth herring fishery this year; and the official report for 1875 from the coastguard at Killibegs, in Ireland, states that the dogfish had so much increased that Donegal Bay had, on several occasions, been apparently cleared of fish by them, and that the nets were constantly full of them. It may be well said that happy is the country whose dogfish are decreasing! Would that I could believe they were becoming scarce in our own!

Prof. Newton has evidently been taken in by a heading in the Index, thus—"Dogfish, consumption of." The explanation is as amusing as it is simple. In Morecambe Bay some of the fishermen catch the dogfish, and after skinning it and removing the head and tail, send it under the name of "Darwen Salmon" to the Blackburn and Preston weavers, who are the only persons who will buy it. This is the only case I ever heard of in which the hated dogfish was not knocked on the head and thrown overboard whenever there was a chance of doing so.

Had I space at command I could enlarge on these tables, but

I think I have said enough to show how much dependence can be placed on them.

We now come to the "process of extinction of animals." Here my ingenious friend should be on safe ground. His argument is, as I understand it, that a species first becomes scarce on the borders of the locality or area where it is found in most abundance. Let me apply this rule to the herring. That fish is only met with when it shows tolerably near the coast—on what I presume he would call the "borders." Can he tell us where the herrings go after what is called the herring season, when they disappear—where the great herring centre is? Fishermen have a vague way of answering this question by saying that they go into deep water, which strictly means somewhere out of sight. Has Prof. Newton evolved more than that from his inner consciousness in his study at Cambridge? That he had given five minutes' attention to the practical study of the habits of the herring—to its life-history, before he gave his Address to the British Association at Glasgow, I venture to doubt very much. The apparent abundance of any fish depends very much on the success of the fishery for it; but many things may and do interfere with that success. It is only within the last very few years that evidence, apparently of great importance, has been obtained, pointing to a relation of the temperature of the sea to the depth at which the herrings swim, and consequently to the chances of their coming within reach of the floating drift-nets, or otherwise. No one can say, with any confidence, why fish are taken abundantly by drift-net or line in one week, or even day, and very few in the next. It may appear very well for Prof. Newton to talk of the consideration of our sea-fisheries being "fraught with unusual difficulties," but he disposes of the question as if it were one of no difficulty, by saying, in his Address, that "it is highly necessary to impose some limitation upon them"—the sea-fisheries. This implies, as plainly as can be, that, notwithstanding the "unusual difficulties," he has quite made up his mind on the subject.

Again, he says, "I have found that the grumblers among the fishermen commonly assign some specific cause for their complaints, be that cause real or imaginary. Their assignment of any cause is purely a matter of opinion with them." Is not Prof. Newton aware that very many of the fishermen who gave evidence before the Commissioners stated these matters of opinion simply as matters of fact? The history of the trawling question is quite sufficient to prove this. He then says, "Their statements [those of the fishermen] as to the increase or decrease of fishes relate to a matter of fact within their knowledge." Does he know the meaning of "conflicting evidence," for which he gives a separate column in his tables? If one fisherman says fishes are increasing in a district and another says they are decreasing, are they both to be considered as speaking of a matter of *fact* within their knowledge?

In the matter of the extent of the inquiry under the Sea Fisheries Commission, I must keep my friend to the point. My stated objection to Prof. Newton's address was that in it he clearly spoke of a decline as a fact, and of the Sea Fisheries Commissioners being unable to find any remedy for it—"There was nothing to be done with our Sea Fisheries but to leave things alone." No possible refuge under the expressions in his Address will prevent this meaning being obvious.

I now come to this statement:—"It is quite compatible with an increased *supply* of fish that there may be an actual decrease in the *stock* of fishes." It may be so; but as, at all events, in the important drift-net and line-fisheries, the capture of the fish depends entirely on their chancing to come in the way of the nets or baits, and there is no hunting them down as there is with game on land, the probability of such a case does not appear to be very great. Of course, Prof. Newton will admit that the converse proposition is equally sound—that a decreased supply of fish may be compatible with an increased stock in the sea. There may be millions of herrings in the British seas without our fishermen finding them out; in fact, there must be, for it is only during a certain season in each year that the fishermen meet with them, and, as they will themselves admit, "luck" has then a great deal to do with it.

The "practical mischief likely to result" from such agitation as his Address might stir up is this—the fishermen may fear some new regulation for the size of mesh they use in particular fisheries, and be afraid to order new nets of the old pattern when they are urgently wanted, as was the case in 1863; for a mile and a quarter of herring nets, or two miles and a half of mackerel nets, are very much more costly articles than the comparatively small nets used for salmon-fishing in the rivers. The salmon fishers, as it is, are sufficiently bothered with inquiries and alterations of bye-laws.

Prof. Newton can find no new facts in my book on "Deep-Sea Fishing," supporting my belief that our sea fisheries are not falling off; but I may mention that I have shown from official returns that since the Commissioners reported, there has been a vast increase in the number of the most important class of fishing-boats, used respectively for the trawl, drift, and line-fisheries; and I believe most people will consider that when, for years in succession, more money is invested by the fishermen generally in boats and gear—by the persons who Prof. Newton says know more about fishing matters than he does—and the general supply to the markets does not fall off, but the contrary, there is not much evidence of the stock of fishes in the sea becoming exhausted. That the fisheries at particular places have fluctuated is well known, and if we knew more about what affects the movements of fish, this might perhaps be explained.

The reason why the fishermen were so much in favour of the Sea Birds' Preservation Act was simply because the congregation of sea birds over the shoals of fish often told the fishermen when their fishing was likely to be successful. Prof. Newton, I believe, was anxious to prevent certain species of sea-bird having the protection of the Act, but, unless I am greatly mistaken, he was strongly in favour of the general principle of the Bill. What particular species of fish is devoured by any particular species of protected sea-bird does not affect my argument; but I may suggest that the protected gulls are surface-feeders, and mackerel, herrings, pilchards, and sprats, besides plenty of young fishes of other species, are commonly considered as surface-swimmers. The collection of gulls over the shoals of fish is spoken of by the fishermen on many parts of our coast as one of the "appearances of fish."

The last point to be considered is Prof. Baird's Report on the American Sea Fisheries. Prof. Newton says the decrease of these fisheries on the Atlantic coast of America is therein treated as a fact beyond denial. He is quite correct here; but he adds that "'overfishing' is unquestionably assigned as the chief cause of that decrease."

I have Prof. Baird's Report for 1872 and 1873 now before me. I must content myself with a few extracts from it. Among other fishes he mentions, Prof. Baird gives a good notice of the alewife or fresh-water herring, and speaks of how valuable it has been in attracting the deep-sea fishes to the shores; he points out how the alewife has diminished, owing to the dams in the rivers preventing the alewives proceeding up to their spawning grounds some way from the sea, this fish having much the same habits as the salmon and the shad. As a consequence the myriads of young fish which used formerly to come down to the sea are now not to be met with at the mouths of the rivers. He goes on to say, at page 12:—"It would, therefore, appear that while the river fisheries have been depreciated or destroyed by means of dams or exhaustive fishing, the codfish have disappeared in equal ratio. *This is not, however, for the same reason, as they are taken only by the line, at a rate more than compensated by the natural fecundity of the fish.*" (The italics are mine.) At p. 60 Prof. Baird says, after speaking of the value of the alewife as food for salmon, trout, and other freshwater fishes: "It is in another still more important connection that we should consider the alewife. It is well known that within the last thirty or forty years the fisheries of cod, haddock, and hake along our coast have measurably diminished, and in some places ceased entirely. . . . Various causes have been assigned for this condition of things, and among others, the alleged diminution of the sea-herring. After a careful consideration of the subject, however, I am strongly inclined to believe that it is due to the diminution, and in many instances to the extermination of the alewives" (or freshwater herring). . . . . "We know that the alewife is particularly attractive as a bait, especially for cod and mackerel. . . . The coincidence, at least, in the erection of the dams and the enormous diminution in the number of alewives, and the decadence of the in-shore cod-fishery, is certainly very remarkable. It is probable, also, that the mackerel fisheries have suffered in the same way, as these fish find in the young menhaden and alewives an attractive bait."

Unless Prof. Baird has in a later report expressed an entire change of opinion, I think I am justified in believing that Prof. Newton is as unable to comprehend the question of the American sea-fisheries as he is that of our own.

My friend has told me in his usual good-humoured manner that neither of us will ever convince the other, and I am cer-

tainly indisposed to throw away any more time on this discussion; but I may remind those who are interested in it that the question really lies between Prof. Newton and the Royal Commissioners. On the one hand, Prof. Newton, with no practical acquaintance with the subject, knowing nothing of the witnesses or of the circumstances under which they gave their evidence, but taking the Index to the evidence as his sole guide, so completely satisfied himself that the Commissioners had arrived at an erroneous conclusion in believing our sea-fisheries were flourishing that he brought their delinquencies before the British Association. On the other hand, Prof. Huxley (who somehow has obtained the character of thoroughly working out any subject he takes up, so far as he has the means of doing so) and the other Commissioners themselves visited the several fishery stations, ascertained beforehand the nature of the disputes and complaints among the fishermen, and examined and cross-examined the various witnesses. They spent many days separately and together in considering the evidence on the several subjects of their report, and unanimously agreed to this among other conclusions:—

"The total supply of fish obtained upon the coasts of the United Kingdom has not diminished of late years, but has increased; and it admits of further augmentation to an extent the limits of which are not indicated by any evidence we have been able to obtain."

E. W. H. HOLDSWORTH

Athenæum Club, November 25

Examinations in Science

MAY I beg you will introduce to the notice of your readers a grievance urgently in need of a remedy? The grievance is considerable; the remedy simple; and if scientific men will not make it their concern, nobody else will.

A short time ago, when the competitive examination controversy waxed warm, I ventured to enter somewhat fully into the present haphazard system of awarding marks to candidates who competed for posts in the public service. My object then was twofold:—(*a*) to bring facts and figures to bear against the erroneous statements of a few theorists who unfortunately were able to command a great deal of public attention; (*b*) to get fair play for all examinees. My object now, however, is to warn those who advocate the advancement of scientific instruction, that the present faulty method of conducting public examinations (in some quarters at least) tends far more to the depression than to the encouragement of scientific study. Destitute myself of scientific knowledge, and bound to no particular curriculum of instruction, I am obviously not writing from the point of view of a partisan; and if I have joined in the lament of scientific men that insufficient consideration is given in most schools to the teaching of science, it is simply because there are good grounds for the conviction that the higher education of this country is too one-sided.

The point I wish to raise is not whether the grammar and philosophy of science contribute to the training and stimulating of the youthful mind in a greater or less proportion than the grammar and ornaments of the Latin and Greek languages; nor whether so-called technical instruction is being properly administered or injudiciously shelved; but I am asking whether scientific teaching, so however little it be, is adequately encouraged by scientific men in the persons of their public examiners?

Judging from the issues of certain examinations, the candidates for which are drawn from the leading schools, I am satisfied that it is not.

By dint only of considerable pressure are candidates induced nowadays to carry on their school course in science for an additional year or two, so general is the conviction among them that they are merely gambling for marks and that the object of much honest labour will not be attained. In fact, for the particular purposes they have in view, they run the double risk of wasting their time and burning their fingers.

It may, of course, be urged that the ends of science are not furthered by youths who aspire to touch only a modest limit in view of qualifying for public employment; but surely as much may be said of almost every other branch of study. And if this is really the opinion entertained by science examiners, it would be better at once to expunge all scientific subjects from the Government programmes.

But candidates and teachers are concerned only with the rules and regulations that are actually current, and that wiser men have made; and their grievance is that there *is a greater element of uncertainty in the awards issued for science than for any other subject.* Instead of estimating the various science subjects as fractional parts of a wide and comprehensive programme, and of dispensing marks on a fixed and definite plan whereby a given quota of proficiency shall be made to carry the same *relative* weight as a given quota of proficiency in other branches, it would seem that examiners, who, by the way, are constantly being changed, regard their own branch as a distinct entity—set up their own standard of excellence for the nonce—and distribute basket after basket of ducks' eggs among all who fail to reach a very advanced qualifying minimum, forgetting perhaps that meanwhile the classical candidate is receiving his modest or substantial reward according to the character of his work. The position indeed would be pretty much the same as if a classical examiner should announce that no candidate would be entitled to a single mark who did not write a faultless copy of Greek verses! I am prepared to show that this ideal standard has varied to the extent of 50 per cent. in two successive years; nay, more, that science candidates have suffered a loss of 50 per cent. in their marks after an additional year's reading under the best teaching that money can purchase. A case occurred some months ago of a youth who, having won the Huxley and Balfour prizes in Edinburgh, entered his name for an open competitive examination in London. He obtained 64 out of 1,000 marks in his two branches of science, at a moment when from 500 to 600 marks out of a total of 1,500 were being showered upon the classical men. At this particular ordeal "Chemistry" chanced to receive decent recognition, but as this youth's tastes happened to run in another direction he was ignominiously defeated.

Any number of such cases may be enumerated, but perhaps I have said enough to prove that a real grievance does exist.

The remedy is obvious: either to induce the authorities to strike out the words "Natural Science" from their list of subjects, or to arrange for the formation of a committee of science examiners who will devise some plan for fixing, as nearly as possible, a uniform standard, and for distributing marks on equitable principles, after consultation with the classical, mathematical, and other examiners. In default of this I do not hesitate to say that examinees will continue to be trifled with at the most important crisis of their lives; for at these public examinations it is no longer a question whether they gain a scholarship or improve their position at school—it is a question of their future career.

I have yet to state the main point. Setting aside the fact of hardship and injustice, it may be asked how far the present independent and very summary system of dealing with batches of schoolboys can possibly cripple the cause of the technicists who are anxious to press forward the teaching of science. My reply is that science candidates, heartsick with disappointment, will fall out of the ranks and will induce others not to enter them; the belief will rapidly gain ground in the schools that science is "a mistake;" and there is abundant reason for supposing that many a schoolmaster will be only too willing to endorse this opinion. I contend that our public examiners wield the thong that lashes the schools into action, and that we are only just beginning to get fair play for what are called "modern" subjects, but that unless our science examiners apportion their marks in a more just and consistent manner they will simply drive all science candidates "bag and baggage" out of the field. In other words, they will virtually be paying a premium to the schoolmasters for neglecting to carry out the very objects they are clamouring for.

W. BAPTISTE SCOONES

Garrick Chambers, Garrick Street,
December 12

The Rocks of Charnwood Forest

MAY I be allowed a short space in reply to Prof. Hull's courteous reference to my letter on the Charnwood rocks, for I fear that I have failed to make two points in that sufficiently clear? One was, that as the Borrowdale series of the Lake District and the (Lower) Cambrian series of North Wales are *equally* azoic, no correspondence in time with the latter could be inferred for the azoic Charnwood rocks. The argument from absence of fossils surely tells as much one way as the other; indeed, having regard to the similar petrological conditions of the Borrowdale and Charnwood rocks, I think it is slightly in favour of their correspondence. The other point was, that as Prof. Sedgwick's term Cambrian included the Cambrian and Lower Silurian of the survey, his authority could not be quoted in favour of the (Lower) Cambrian age of the Charnwood rocks any more than of their correspondence with the Borrowdale series, unless it could be

shown (I am not aware that it can) that he had definitely correlated them with (Lower) Cambrian beds. T. G. BONNEY

St. John's College, Cambridge, December 11

Self-Fertilisation in Flowers

DR. MÜLLER (NATURE, vol. xiv. p. 571) and Prof. Asa Gray (vol. xv. p. 24) reflect on your abstract of my verbal remarks (vol. xiv. p. 475) on Browallia in a way not particularly complimentary to me. Prof. Gray admits having read the full report, and yet fails to notice that "February 8," is there given as the date of my remarks. Had he not overlooked this, he would not have wondered that I did not see "Hymenoptera and Lepidoptera of various sorts" visiting them. As reported in the *Proceedings* of the Society, I exhibited fresh specimens in fruit at the meeting of that date, which is about mid-winter with us, when these insects are at rest. The plants were of course grown under glass, and when I say "Browallia is not visited by insects, yet seeds abundantly," I am referring naturally to the experience I am describing. If one be justified in taking an unguarded expression, or even a whole sentence, without any regard to the subject matter of its connection, we might have as many "theories" in science as there are sects in religion, all founded on isolated "texts" in Scripture. It is remarkable that in a paper in which Prof. Gray is commenting on hasty observations, in another he should have overlooked a fact like this. I do not say Browallia is never visited by insects, but I do say that they do not visit them *under such circumstances as I was describing*.

Of the *fact* there is no doubt, of the *interpretation* there may be many opinions; and no one respects an opinion by Prof. Gray, when he carefully considers it, more highly than I. Yet I would respectfully submit, that even though an insect were as careful to avoid the "brush" which almost closes the throat, though it were able to be as careful in finding the chink as Dr. Gray was in his manipulations with the hog's bristle, the obstruction of the mouth in the way it is cannot surely be claimed as an arrangement in favour of cross-fertilisation.

Dr. Müller seems to believe that I do not know that "many flowers have recourse to self-fertilisation when not visited by insects." If he will examine the *Proceedings* of the Am. Association for 1875, p. 247, he will find that I have given him the credit of the observation, and the fact itself such consideration I thought it in justice entitled to. The impression which Dr. Müller's expression warrants, that he has not had the opportunity of reading the numerous observations I have placed on record during the last few years, in relation to this and kindred topics, fully excuses him in my mind for his sharp comments.

THOMAS MEEHAM

Germantown, Philadelphia, Nov. 21

On Supersaturated Solutions

IN a paper communicated to the Royal Society last May I described some experiments to show that the open air and the air of ordinary rooms do not generally contain crystals of the various salts which form supersaturated solutions. It has been remarked to me that I did not give the strength of the solutions, so that doubt might arise as to whether the results would hold good for very strong solutions:—The following experiments set that question at rest. I made a very strong solution of sodium sulphate which threw down abundance of anhydrous salt on boiling. When cold a good half inch of anhydrous salt remained at the bottom of the test-tube. Took this into my garden, which is near Bristol. Took up some of the solution in a clean pipette and put drops on the leaves of peonies, which were very dusty, on geraniums, on moss, on the stone coping of Bath oolite, and on the painted woodwork of the railings and garden door. Not a single drop crystallised. Made a drop set quite solid by dropping in earth with the fingers. N.B.—I had been at work with the salt for some time and crystals were probably adhering to my finger. Earth not touched inactive. The drops sank into the moss slowly, remaining quite liquid. Those on the stone were soon absorbed and dried up on the surface; fresh drops put on these remained liquid. Smeared a drop repeatedly with the finger which had been cleansed; inactive, as fresh drops remained liquid on it. Drop on flower-pot, inactive, smeared with finger; when dry inactive to fresh drops. These and other drops on the flower-pot slowly formed a film of 7-atom salt. Stirred the solution with a dry twig picked off the ground, inactive. The drops on the leaves all slowly evaporated, giving the 7-atom salt. Finally, made some of the drops and the original solution crystallise, to prove that they were really supersaturated. These experiments were made both in sun and shade. Weather dry. The test-tube was left open the whole time.

On another occasion I took a flask of sodium sulphate containing a large quantity of the 7-atom salt into the garden in the evening. Put drops on a flower-pot; one only crystallised. Put a lump of dry earth into one drop, and added more solution; did not crystallise. Made a little mud pie by breaking this up with the pipette, inactive; pipette repeatedly inactive in the solution after touching this. Brought a crystal to the earth; crystallised at once all through the mass.

Clifton College J. G. GRENFELL

KARL ERNST VON BAER

SCIENCE has sustained a great loss by the death of Dr. Karl Ernst von Baer, the eminent biologist; he died at Dorpat on November 29, in his eighty-fifth year. Von Baer was born in Esthonia on February 29, 1792, and while yet at the gymnasium became an earnest student of botany. He studied medicine at Dorpat in 1810-14, whence he proceeded to Vienna for the study of clinical medicine, to Würzburg, where he gave special attention to comparative anatomy, and to Berlin, where he studied magnetism, electricity, crystallography, and geology. In 1817 he went to Königsberg as prosector to Prof. Burdach, and two years later he became professor of zoology at the same university. In 1826 he succeeded Burdach in the chair of anatomy, accepted an invitation in 1829 from the St. Petersburg Academy, but returned to Königsberg the following year. A few years later, in 1834 he was again invited to St. Petersburg, where he became one of the most active members not only of the Academy, but also of the Geographical and Economical Societies. Von Baer's writings, marked by philosophic depth, are, on account of their orderly and clear exposition, as attractive as they are generally intelligible. The subject of the origin and development of organic bodies, which had special attractions for him, he did much to clear up. The foundation of his eminence he laid in Königsberg, where he published in 1827 his "Briefe über die Enstehung des Eies," which was soon followed by the important works "Entwickelungsgeschichte der Thiere," and "Geschichte der Entwickelung der Fische." These works, which are yet of great value, have earned for their author the title of Father of Comparative Embryology.

In the summer of 1837 von Baer made a journey of exploration from Archangel to Novaya Zemlya, and his report is still one of the most valuable sources of information upon that island. In 1851 his attention was attracted to the immense Russian fisheries and the irrational methods used. During 1851-6 he investigated the fisheries of Lake Peipus, the Gulf of Finland, and the Caspian Sea, publishing the results of his investigations in a great work in 1859. The name of Baer is connected with more than one improvement in the fisheries, and some important additions were made to the trade, thanks to his efforts. His remarkable work, "Kaspische Studien," has had no rival. It would be impossible to enumerate the various subjects upon which he has thrown clear light in his writings. The laws of excavation of river-beds, the navigability of the Arctic seas, the steppes and forests of Southern Russia, the Glacial period, the Siberian mammoths, the potato disease, were at various times treated by him, and in each department von Baer opened out new and extensive fields of inquiry. His acquirements in zoology, comparative anatomy, embryology, physiology, and anthropology are well known; moreover ethnography, the early history of mankind, archæology, and the science of language will count him among their most eminent students. In his later years, besides various anthropological papers, he published an autobiography (which appeared soon after the fiftieth anniversary—1864—of his scientific career), his "Reden," and "Kleine Aufsätze vermischten Inhalts"

(1864–75). The very valuable publication he undertook along with M. Helmersen, "Beiträge zur Kenntniss des russichen Reichs," numbers twenty-six volumes, and continued to appear until within the last few years. Von Baer continued to work up to the very last, and he has left behind him a large quantity of manuscripts and unfinished works.

Von Baer was undoubtedly one of the most accomplished investigators of the present century. Haeckel speaks of him thus:—"If among living scientific investigators there is one who justly enjoys universal honour and respect it is Karl Ernst Baer; and if classical and in the best sense natural philosophical writers will admire a Coryphæus of to-day, an unsurpassed example of exact observation and philosophic reflexion, let them go to the 'Entwickelungsgeschichte' of this head master of our science." Helmersen speaks of the late biologist as follows in the *St. Petersburger Zeitung*:—"With Baer departs a man such as is rarely met with in any century, a genial man of science and research, endowed with a penetrating critical intellect, with unusual faculty of observation, with perseverance and energy in work. The earth and its inhabitants were the great field of his research, and he brought to his work not only a deep philosophic training, but also an equipment of the profoundest knowledge in several departments of natural science which few of the great spirits of our time have possessed. This great, comprehensive, but profound knowledge, which he to the day of his death continued to increase and turn to use, combined with the determination to trace things to their ultimate grounds and by means of keen and unprejudiced, clearly arranged, and thoughtful observations to discover the truths and the laws of nature, stamp all his works with a monumental character which they will preserve for all time. The widely-known name of Baer is written in large lettters in the book of science and its history."

We hear that a subscription will be opened among all the scientific bodies of which von Baer was a member for the founding of a scholarship in his name, or for any other scientific purpose worthy of the name of the great natural philosopher.

DAVID FORBES

AT the comparatively early age of forty-eight the busy life of Mr. David Forbes has been brought to a close. Like his distinguished brother Edward, he has been unexpectedly cut off before much of the immense mass of knowledge he had acquired has been put in a form to be of use to others. He was always looking forward to a time of less active occupation, when he might devote his principal attention to putting on record the results of his many years' investigations. What there may be in the piles of manuscript he has left that will be available for use, there has not yet been time to ascertain. For the last five years the most important papers he wrote were the half-yearly reports for the Iron and Steel Institute, but among his earlier papers there will be recollected "The Relation of Silurian and Metamorphic Rocks in the South of Norway," and "The Geology of Bolivia and South Peru." Alluding to his connection with the Iron and Steel Institute, the organ of that society has just written:—"In his capacity of Foreign Secretary he has, almost from the foundation of the institute, rendered most essential service, and has in no inconsiderable degree contributed to that rapid prosperity which has characterised its operations. His exhaustive reports on the foreign iron and steel industries which appeared in the *Journal* were most valuable, as they embraced everything going on in connection with the iron trade all over the world. The wonderful linguistic accomplishments of Mr. Forbes enabled him to deal easily with the publications of all countries where iron and steel is made. His name was so well known abroad that the leading people connected with the technological features of ironmaking most readily furnished full details of what was going on in each country; and through his influence mainly the institute speedily assumed a recognised position abroad."

Mr. Forbes joined the Geological Society in 1853, and since February, 1871, has been one of the secretaries. He was also a Fellow of the Chemical Society. In June, 1856, he was elected a Fellow of the Royal Society. He had travelled extensively in many parts of the world. All the family of the Manx Forbeses have been great travellers. Dr. Wilson, in his memoir of Edward Forbes, has mentioned many of his relatives who died out of Europe. Mr. David Forbes, as a consulting engineer, had an extensive practice, and was often summoned abroad. His death occurred at his house on Tuesday, December 5, and on Monday, the 11th, his remains were laid in the Kensal Green Cemetery, in the presence of the Presidents of the Geological and Chemical Societies and many scientific friends.

THE GLACIATION OF THE SHETLAND ISLES

IN the *Geological Magazine* for May and June, 1870, my colleague, Dr. Croll, first pointed out that the Scotch and Scandinavian ice-sheets probably united on the floor of the North Sea, and thence moved northwards towards the Atlantic. He was led to this conclusion by a consideration of the peculiar direction of the striæ in Caithness, in Shetland, and the Faroe Isles, as well as by the occurrence of marine shells in the boulder clay of the northern parts of Caithness. He showed that the enormous *mer de glace* which pressed out on all sides from Scandinavia forced its way close to the Scotch coastline, and in virtue of its greater size produced a slight deflection of the Scotch ice, causing it to over-ride portions of the main land. He stated that in all likelihood both the Shetland and the Faroe Isles were over-topped by the Scandinavian ice in its onward march towards the Atlantic.

During a recent traverse in Shetland I obtained evidence which tends to strengthen this remarkable theory. In the north island of Unst, the direction of the striæ, the boulders on the surface, and the stones in the till, clearly indicate that this island was glaciated by a mass of ice moving from east to west. The proofs of continental glaciation, which are comparatively clear in the north, are obscured in a great measure in the main island by the effects of a local ice-sheet. The nature of the boulder clay, as well as the trend of the striæ in various localities, show that the movement of this local sheet was influenced by the general features of the country. In addition to these markings, however, others were found which could not have been produced by ice shedding off the land in the ordinary way. These cross the island, regardless of its physical features, and are often at right angles to the newer set. Lastly, the wide distribution of morainic matter with groups of moraines indicate the gradual disappearance of the local ice-sheet and the presence of small glaciers, where the ground presented favourable conditions for their development.

The islands are dotted over with small lochs; the most of these lie in peat or drift, while others occupy true rock basins. The singular absence of marine terraces ought not to escape notice, as bearing on the recent geological history of these islands, since the voes or sea-lochs are admirably adapted for their preservation.

These observations will be described in detail in a forthcoming paper before the Geological Society.

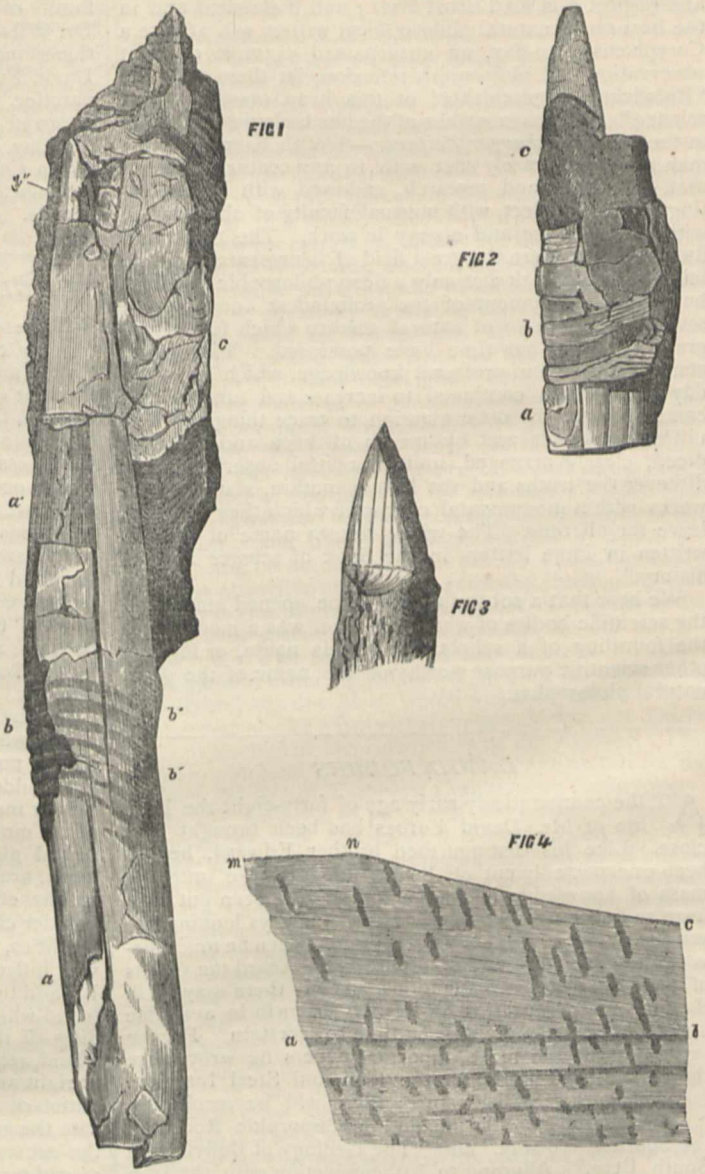
JOHN HORNE

Geological Survey of Scotland, Nairn, N.B.,
November 29

PRIMÆVAL SWITZERLAND[1]

THE veteran Swiss professor, Dr. Oswald Heer, is not more distinguished for his ability and indefatigable industry in original research than he is for his brilliant powers of popular exposition. His admirable work, "The Primæval World of Switzerland," of which both German and French editions have already appeared, has been so favourably received, alike by the scientific and the general public, that we are happy to be able to announce the publication of it in the form of an English translation, adorned with the whole of those numerous and excellent illustrations which contributed so greatly to the value of the book as it was originally issued. A work like the present, in which accuracy of scientific detail is in no degree sacrificed to its main design—that, namely, of producing a succession of lively descriptions leading up to clearly-enunciated generalisations—must be largely dependent not only on the literary skill of its translator, but on his competence for dealing in an intelligent manner with the various branches of natural history treated of; and when we state that the interpreter of Prof. Heer's views to English students is so erudite a naturalist as Mr. W. S. Dallas, the Assistant Secretary of the Geological Society, we have said enough to predispose our readers in favour of the present translation. Nor does a careful perusal of the work serve to disappoint the high expectations we have been naturally led to entertain with regard to it, for both editor and translator have evidently performed their respective tasks in a most skilful and conscientious manner. Neither in respect of accuracy or of elegance do we notice any very serious failures; under the former category, indeed, we only feel called upon to draw attention to a little confusion which exists in some parts of the work with respect to the English and German measures; and, under the latter, to what appears to us to be the rather awkward adoption of the third person, which, however suitable for abstracts or reviews of the writings of an author, seems somewhat out of place when employed in a full translation of one of his works.

To that numerous section of our countrymen who regard "the playground of Europe" as a place only for fashionable lounging or purposeless climbing, Prof. Heer's work may well be commended as opening up new, and to many perhaps, unsuspected sources of enjoyment during their holiday tours. Those who will take the trouble to master the contents of these two pleasantly written volumes—a task demanding no great preparation of preliminary studies—will be in a position to appreciate and follow with ever increasing interest the discussion of those numerous important geological problems, to the solution of which no country in the world affords more important materials than Switzerland. Aided by the carefully arranged collections of rocks and fossils which exist in the museums of all the larger Swiss towns, the tourist would find the means of enabling himself to vividly realise and almost live among the wonders of long past geological periods; and by personal contact with the actual evidences of geological change, his scientific knowledge and convictions would acquire a reality and solidity, which no amount of work in the library could ever communicate to them. Those who will adopt this plan will soon find aroused within their minds an interest and enthusiasm, which will prevent them from ever finding their holiday tour dull; and will be amply rewarded thereby for the necessary preliminary labour. Phlegmatic, indeed, must be the individual who does not find his pulses stirred as he follows in the work before us the life-like delineation by word-painting of the characteristics of ancient worlds, or who does not find the desire awakened in his mind to witness for himself some of the phenomena here described; for even the most indifferent reader can-



Rods of fir-wood exhibiting marks of cutting and binding, rom the lignites of Wetzikon.

not fail to catch a portion of the enthusiasm which everywhere glows in Prof. Heer's eloquent pages. But such feelings are only a very feeble echo indeed of the pleasures experienced by the student who has the courage to enter himself within the veil, and to look upon nature and her mysteries face to face.

We should, however, be doing Prof. Heer an injustice if we referred to his book as being only a popular guide to the geology of Switzerland. To the man of science

[1] "The Primæval World of Switzerland." With 360 Illustrations. By Prof. Heer, of the University of Zurich. Edited by James Heywood, M.A., F.R.S., President of the Statistical Society. Two vols. 8vo. (London: Longmans, Green, and Co., 1876.)

the work will be no less valuable than to the tourist and general reader, for in it he will find an authoritative *résumé* of the results of multifarious studies by one of the most eminent of living palæophytologists; results which otherwise he would be compelled to search for in numerous scattered papers and bulky monographs. Just such a sketch of the general geology of Switzerland as is contained in the work before us, is indeed especially welcome at the present time, from the fact that Studer's admirable "Geologie der Schweiz," is so far behind date. The main features of the Carboniferous, Saliferous, Lias, Jurassic, and Cretaceous formations as displayed in Switzerland, are all very clearly described in Prof. Heer's book; but it is of course in respect to the Miocene—to the elucidation of the characters of the fauna and flora of which his own admirable researches have been more especially devoted—that our author's detailed observations and inferences are possessed of the greatest value and interest. Prof. Heer's general conclusions on such subjects as the physical evolution of our globe, the changes of climate during former geological periods, and the doctrine of descent and Darwinism, are also worthy of the most serious attention.

We shall not here stop to discuss how far the identification of species of plants, by their leaves alone, is safe or defensible on the part of the palæophytologist. Some botanists, especially in this country, have adopted very extreme views with regard to the work of Heer and others;

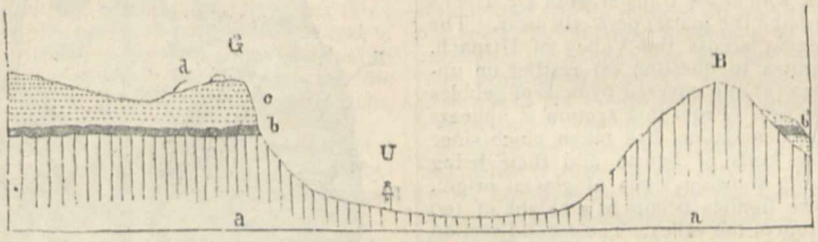


FIG. 5.—Ideal Section of the valley of Uznach (the vertical scale is to the horizontal as 8 : 1). G, Gubel; U, Utznach; B, Lower Buchberg.

demanding that fossil plants, like recent ones, should only be named after an opportunity has been found for studying their organs of fructification. But the geologist may with justice object to such a limitation, that it would practically be almost as fatal to the pursuit of his inquiries, as a demand from malacologists that no conclusions should be based on the shelly coverings of molluscs, or from comparative anatomists, that we should reject all identifications based on portions of the skeleton of the vertebrates. It is surely better to make the best of the imperfect materials which we possess—guarding ourselves meanwhile at every point with cautious reservations—rather than to reject it altogether because of its lack of completeness.

Opinions, too, may differ as to the propriety and value of those rather fanciful delineations of scenery in the ancient geological periods, which are so frequently introduced in French scientific treatises. But in the case of "The Primæval World of Switzerland" all such criticism is disarmed by the fact that, while a means of arresting the interest of the general reader has been supplied by these rather questionable "landscapes," the real wants of the student have by no means been lost sight of, but are very liberally provided for in numerous other plates and woodcuts of truly scientific character and accuracy.

No time, perhaps, could possibly be chosen as more opportune for the appearance of this work than the pre-

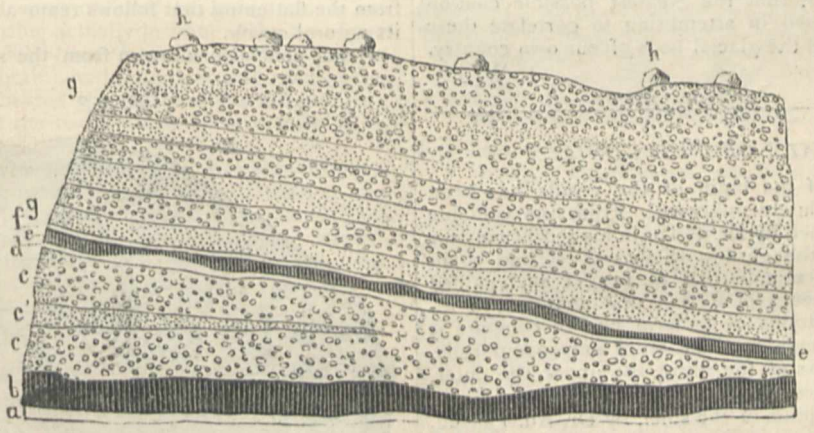


FIG. 6.—Section of the paper-coal or lignite deposit and pebble-beds, at Dürnten.

sent, for attention has recently been very generally drawn to the discovery of certain articles of human workmanship in Switzerland which seem to throw far back the date of the appearance of man upon the globe, and to make him contemporaneous with a portion at least of the Glacial period of that country. The editor of this work has, we think, acted most judiciously in appending to Dr. Heer's book, which contains ample details concerning the characters and relations of the beds which have yielded these interesting relics, a translation of Prof. Rütimeyer's memoir in the *Archiv für Anthropologie* for 1875, which describes the objects themselves, and we cannot more appropriately close this article than by a brief reference to the facts of the case, as detailed in the work before us, illustrating them by several woodcuts borrowed from the same source.

That the relics in question are of artificial origin, there can be scarcely the smallest room for doubt. They consist of a number of rods lying side by side in a block of lignite from Wetzikon, in the Canton of Zurich; these rods are of fir-wood, they are converted into true lignite perfectly similar to the surrounding matrix, and are flattened and crushed like the remains of plants, constituting the mass. Careful examination of them shows that the point of one of these rods has been artificially cut (Fig. 1, *a*″ and Fig. 3), and that it has been bound round with

some flexible material (Fig 1, *b*, *b'*). A second rod exhibits its longitudinally fibrous woody body bound round transversely with a different bark (Fig. 2, *b*). Prof. Schwendauer, who has made a microscopical examination of these interesting rods, confirms the fact of their having been subjected to artificial treatment. He supplies in Fig. 4 an enlarged view, showing how the artificially-produced section cuts across the structure of the wood. We can hardly doubt that we have here portions of a piece of the rude basket-work, the construction of which is among the earliest practised of the arts of savage peoples.

With regard to the mode of occurrence of the Wetzikon lignite deposit, in which these singular remains were found, two woodcuts, which we transfer from Dr. Heer's book, will suffice to make the matter perfectly clear. The first is an ideal section across the Valley of Utznach, which shows the lignites in question (*b*) resting on upturned Miocene strata (*a*) and covered by beds of pebbles (*c*) and erratic blocks (*d*). From this section it appears that a vast amount of denudation has taken place since the formation of the beds of lignite and their being covered up by deposits showing signs of glacial origin, for the outcrop of the lignites occurs at a height of 100 yards above the bottom of the valley. The second section shows the nature of the stratified materials, sand, loam, and pebble-beds (*c*, *d*, *f*, *g*) with which the lignites (*b*, *e*) are interstratified and covered—a number of erratic blocks (*h*), evidently derived from the Alps, surmounting the whole mass.

That these lignites of Wetzikon with their relics of human workmanship are of great antiquity there is the plainest proof; that they are, however, of more recent date than the principal mass of the glacially-derived materials occurring in the great Swiss valley is rendered clear by their undoubted superposition to these deposits, which is seen at a number of different points; but that moraine matter and erratic blocks have been deposited *above* them, either by glaciers or icebergs, there seems to be no room for doubting. We would venture to suggest, in conclusion, however, that the greatest possible caution ought to be exercised in attempting to correlate these Alpine deposits with the glacial beds of our own country.

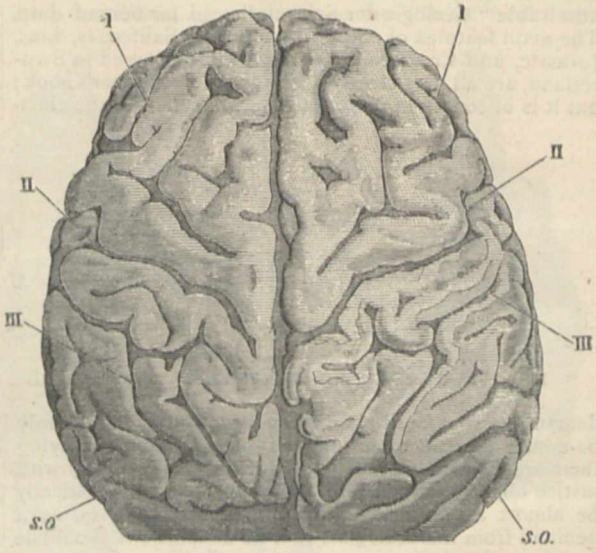
J. W. J.

THE BRAIN OF THE GORILLA

THE anatomy of the brain of the gorilla has been hitherto absolutely unknown. From the valuable photographs published by Dr. Bolau in his recent memoir on the anatomy of the gorilla, which was referred to in last week's issue of this journal (p. 127), I am enabled to give a brief account of its external anatomy, to reproduce the illustrations of its form, and to compare it with the brain of man and the other anthropomorpha. There are three views of the brain, the upper, the outer, and the inner surfaces, figures of which are here given, and a careful description of the sulci, by Dr. Ad. Pansch, is appended.

When seen from above the brain presents a broad ovoid figure, the greatest transverse diameter opposite the supra-marginal convolutions, and very nearly two-thirds of its length from the anterior extremity; the frontal lobes are broad, and show a remarkable approximation to the square form of the human brain. In the lateral view it has moderate depth, the arching of the upper surface is but slight, and the highest point would seem to be about midway between the centre of its length and the broadest part. The dimensions are given, length = 100 mm., breadth = 87 mm., and the depth = 70 mm.; but the last certainly includes the cerebellum, for which an allowance of one-fifth may very properly be made, which will reduce the depth to 56. In the orang the three proportions are respectively 100, 78, and 50; in the chimpanzee 100, 84, and 66; in the bushwoman 100, 77, and 62. The breadth of the gorilla's brain here is notable, but in connection with this it may be pointed out that the bushwoman has as great a relative breadth of brain as the European, in whom the numbers are 100, 77, and 69, and

Fig. 1.

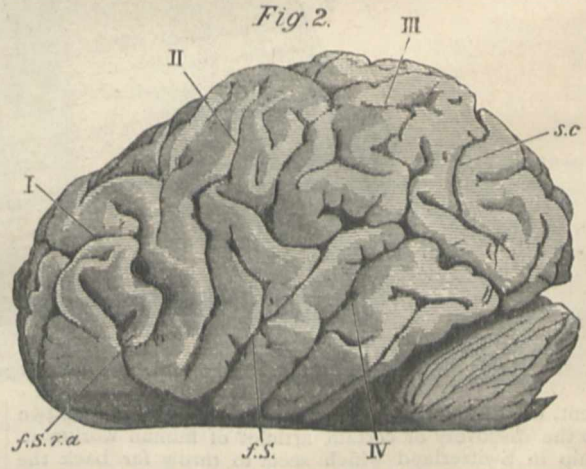


Upper view of the brain of the Gorilla. I. Sulcus, præ-central; II. Fissure of Rolando; III. Intra-parietal sulcus; *s.o.* External perpendicular fissure.

that the great and more valuable contrast is to be found in the depth; in the case of the orang (from Dr. Rolleston's paper) it would seem that it must be too low, probably from the flattening that follows removal of the brain from its natural cavity.

Hence conclusions drawn from the shape of the brain

Fig. 2.



Outer surface of the brain of the Gorilla. I. II. III. *s.o.* as before; IV. Parallel fissure; *f.s.* Sylvian fissure, posterior branch; *f.s.r.a.* Ant. branch of the same.

itself are from this very circumstance liable to error, and for this purpose casts of the interior of the cranium are the only reliable guides. Referring to those in the Hunterian Museum, that of the gorilla as compared with man is seen to be characterised by want of height, flatness of the vertex, and narrowed frontal obes; compared with

the orang, in the latter the frontal lobes are more compressed, giving a pointed form to the frontal extremity, and the occipital lobes are larger and more rounded, so that the figure is pear-shaped rather than ovoid ; but the vault is decidedly more lofty and better arched ; also the orbital concavity is less marked in the orang, so that any deficiency in the lateral development of the frontal lobes might very well be compensated by their downward extension. The chimpanzee and gorilla, however, exhibit a very great resemblance in shape and proportions, though the former has somewhat more compressed frontal lobes, a greater development of the occipital region, and apparently greater width, so that the cast looks more globular than that of the gorilla.

The *corpus callosum* is of good length, but rather thin ; its proportion, taking the length of the brain as 100, is 41, and its average thickness appears about one-twelfth of that ; in the chimpanzee the length is 39 and the thickness one-eleventh ; in the orang, 44 ; and in man, 40 ; thickness, one-thirteenth.

The convolutions are strongly marked ; in a general view they are slightly more subdivided than in the chimpanzee, but in complexity and asymmetry the orang exceeds the gorilla to about the same extent.

Outer Surface of the Hemisphere.—The posterior branch of the Sylvian fissure extends upwards and ends in the usual bifurcation, nearly at the junction of the middle and posterior thirds of the hemisphere, and almost half of the height from the lower margin ; judged by this, the fissure is more oblique than in man, less so than in either orang or chimpanzee. The short anterior limb is very faintly marked in front of the insula, but its ending is distinct, bifurcated, on the outer side of the frontal lobe. The insula has its fore part uncovered in the bottom of the fissure. The external parieto-occipital fissure travels over the outer surface to within a very short distance of the lower margin of the hemisphere ; its hinder margin, prolonged forwards, gives rise to a convex operculum in about its lower two-thirds, very much resembling that of the chimpanzee, although somewhat more sinuous. The fissure of Rolando is very oblique, the lower end is remarkably forward, being actually in front of the tip of the temporo-sphenoidal lobe (probably part of this is due to the position of the brain), and the upper end reaches the longitudinal fissure behind the centre of the hemisphere ; the angle formed by the two fissures is very little more than a right-angle, 95°.

The length of the hemisphere being 100, the distances in a horizontal line from the anterior extremity to the upper end of the fissure of Rolando, *i.e.*, the extreme length of frontal lobe, being *a*, thence to the parieto-occipital fissure, length of parietal lobe, *b*, and from that to the hinder extremity, occipital lobe, *c*, we get—

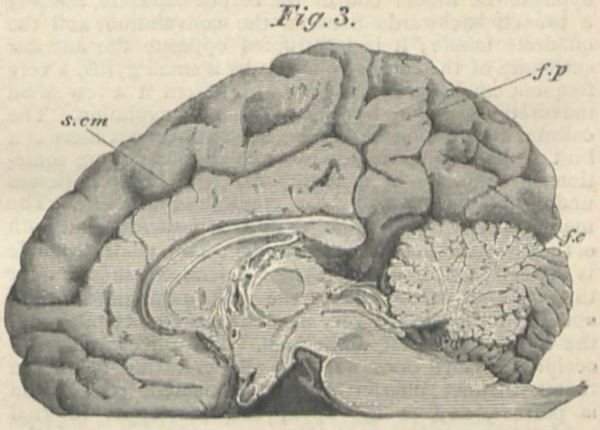
	a.	*b.*	*c.*
Gorilla	57	29	14
Chimpanzee ...	49	28	23
Orang	52	27	21
Bushwoman ...	65	17·5	17·5
European ...	57	23	30

The length of the frontal and the smallness of the occipital lobes are especially noteworthy.

The convolutions of the frontal lobe present their typical arrangement : the ascending frontal is very simple, following in easy curves the fissure of Rolando, and marked only by one slight indentation opposite the superior frontal sulcus ; it is bounded in front for the lower half by the præ-central sulcus (Ecker) from which the distinct and strongly-bent inferior frontal sulcus runs forwards to the tip of the lobe. The superior frontal sulcus has its characteristic T shape, the top of the T being placed vertically in front of the upper half of the foregoing convolution, and the second limb sent forwards almost straight for about two-thirds of the distance to the anterior extremity between the upper and middle frontal convolutions. Of the three horizontal gyri, the upper springs by a narrow root from the ascending frontal close to the margin, and shows plainly the indications of the longitudinal division into two ; the middle is much narrower, connected to the ascending by a pedicle between the præ-central and superior frontal sulci ; and the inferior frontal is well developed, arching over the anterior limb of the Sylvian fissure, and considerably folded. Compared with the chimpanzee, the upper is narrow, the middle and lower larger and more subdivided. The orang throughout the whole lobe exhibits a greater richness of convolution.

The convolutions of the parietal lobe are very definitely and strictly marked off, and at the same time they are decidedly the most developed of the whole brain, far exceeding the chimpanzee, and not inferior to the orang. The intra-parietal sulcus, springing from the Sylvian fissure near its hinder end, runs forwards and then upwards, round the front of the supra-marginal lobule, parallel to the fissure of Rolando, as far as the centre of that, when it turns backwards at an obtuse angle, continues, approaching slightly the longitudinal fissure, and debouches into the external parieto-occipital fissure ; from the angle which it forms, the customary prolongation, giving off two or three smaller branches, is sent upwards

Fig. 3.



Inner surface of the brain of the Gorilla. *f.p.* internal parieto-occipital fissure ; *f.c.* Calcarine fissure ; *s.cm.* Calloso-marginal ssure.

in continuation of the ascending portion, and dividing the ascending parietal convolution from the parietal lobule almost completely ; the interruption to this fissure about its bend which is seen so often in human brains I have observed only in the orang. The ascending parietal convolution is much more developed than the ascending frontal, and it presents a marked indication of a longitudinal fission ; the lower end has the usual triangular expansion split into two (superior marginal convolution of Gratiolet). The parietal lobule is large and divided into an outer and an inner portion, the latter showing further subdivisions, in this condition approaching more to the human brain than either orang or chimpanzee. The supra-marginal lobule is more developed than in either chimpanzee or orang, and divided into three portions by a triradiate sulcus, but its proportion and the amplitude of its gyri are much inferior to the human brain. The angular convolution springs from the upper end of the supra-marginal lobule by a narrow bent piece ; the descending branch here differs from the chimpanzee and orang in being cut off from the middle temporo-sphenoidal convolution and running backwards into the middle occipital convolution, constituting the anterior boundary of the external parieto-occipital fissure.

The occipital obe is by no means richly convoluted,

the three gyri are marked off, the upper being broad and the lower narrow.

The temporo-sphenoidal lobe presents nothing remarkable; the parallel fissure is continuous and simple, running up behind into the angular convolution, where it is cleft, one branch extending downwards parallel to the lower part of the external parieto-occipital fissure, and cutting off the middle temporo-sphenoidal gyrus from the descending ramus of the angular and the second annectent convolutions. The upper convolution is simple, the middle is broader and more folded, the inferior is separated by a well-marked sulcus from the middle.

The first external annectent gyrus is seen issuing from under cover of the operculum, and passing forwards and inwards to the parietal lobule; this is an approach to the orang, in which the gyrus is normally superficial, and an advance on the chimpanzee, in which it occurs only at times. The second does not appear at all; the third is to be recognised nearer the lower margin of the hemisphere and below the lower end of the external parieto-occipital fissure, but the extension of the parallel fissure separates it superficially from the middle temporo-sphenoidal convolution; below this the small fourth appears, uniting the lower occipital and third temporo-sphenoidal convolutions.

Inner Surface of the Hemisphere.—The calloso-marginal fissure pursues its usual course and turns upwards opposite the hinder end of the corpus callosum, sending a branch backwards between the convolution and the quadrate lobule; it is interrupted opposite the anterior extremity of the corpus callosum by a small gyrus, a very frequent condition in human brains; from it a few small indentations pass up into the marginal convolution. The callosal convolution is simple: at the fore part there is a hint of the longitudinal division which obtains here sometimes in man, and it is more broken up when it passes under and is joined by the quadrilateral lobule. The marginal convolution is larger and more divided, but both of these are simpler than in the orang. The quadrate lobule is divided into about four small gyri, and is much larger than in the orang, where the calloso-marginal fissure opens into the surface very near the parieto-occipital, and the lobule is almost obliterated. The internal parieto-occipital fissure does not join the calcarine below, so that a distinct inferior internal annectent convolution is present, and at the upper end the upper internal annectent convolution can be seen coming out of the fissure and joining the upper posterior angle of the quadrate lobule. The calcarine fissure is usual, so also is the fissure of the hippocampus. The occipital lobule is divided into three gyri transversely by two furrows running the upper from the parieto-occipital, and the lower from the calcarine fissures nearly across; this is in marked contrast with the arrangement of the human brain where the gyri run from apex to base, being subject, however, to great variety. The gyri on the under surface of the occipital and temporo-sphenoidal lobes cannot be seen.

The resemblance between this brain and the chimpanzee's is striking both in its shape and the arrangement of the convolutions, so much so that Gratiolet's description of the latter would serve also for many parts of the gorilla's brain. The chief points of difference between the two are mainly in favour of the gorilla, *e.g.*, the greater length and breadth of the frontal lobe, a greater development of the middle and lower frontal and of the parietal convolutions, especially of the supra-marginal lobule and the appearance of the first external annectent gyrus. On the other hand the chimpanzee appears to have some advantage in the important point of greater vertical height. On the whole, the comparison seems to indicate a development of the chimpanzee type of brain and to give it a higher rank than that.

In one particular character it approaches the orang, the partial appearance of the annectent convolution, but the differences in shape, the more perfect operculum, the lesser complication of the frontal and occipital convolutions and the greater symmetry far outweigh the resemblances and denote its proper position as with the chimpanzee, although somewhat nearer the orang than that.

Gratiolet placed the gorilla with the baboons by reason of its elliptical form and the supposed want of development of the frontal and great excess of the occipital lobes; but we see now that of all the anthropomorpha the gorilla is characterised by the most extensive frontal lobe and smallest occipital; in addition to which the richness of the convolutions and the breadth of the frontal region also separate it farther from the baboons than the chimpanzee.

It is certainly open to great doubt whether this diminution of the occipital lobe is at all an ascensive step in the cerebral conformation, in fact, the comparison of the respective proportions in the bushwoman and the European points distinctly in the opposite direction; and it is to be noticed that the great relative length of the frontal lobe is entirely due to this dwarfing of the occipital, for the proportion of the frontal to the parietal is no greater in the gorilla than in the others; and the highest type is to be sought rather in the co-ordinate development of all the lobes and not in the predominance of any one; so that regarded by this standard the gorilla's brain shows one marked feature of inferiority.

It should be remarked that Dr. Pansch, whose judgment must carry great weight, is disposed to regard the divergences from the chimpanzee as sufficient to establish a distinct type of brain in the gorilla.

In the comparisons above instituted, the brain of the bushwoman so carefully and elaborately described by Mr. Marshall in the *Philosophical Transactions* for 1864, that of the chimpanzee described with photographs by the same author in the *Natural History Review* for 1861, and that of the orang in the same journal by Prof. Rolleston have been taken as standards, supplemented by reference to the specimens in the Hunterian Museum.

G. D. THANE

MUSEUM SPECIMENS FOR TEACHING PURPOSES[1]

IT is now generally admitted that a thorough and practically useful knowledge of the form and other properties of natural bodies can only be acquired by an examination of such bodies themselves. The difference between knowing a thing by description only and knowing it from personal acquaintance need scarcely be insisted on.

All teaching, therefore, of the physical properties, especially the form, texture, colour, and relation to one another of the component parts of any natural object, whether organised or inorganic, should be illustrated by reference to the object itself. The more completely the student is enabled to examine it, the more thorough will his knowledge of it be.

In the Department of Biology, which is that to which my remarks must now be limited, very much valuable and practical teaching can certainly be given without the possession of a museum or a single permanent preparation. The commoner and easily accessible animals and plants furnish materials for study and demonstration, which, when done with, can be thrown away and replaced as occasion requires. But it is often desirable to preserve specimens for a considerable time or permanently, either on account of their intrinsic rarity, causing difficulties in procuring them when needed, or on account of the labour and skill which may have been expended upon their proper display, and which it is not desirable to have wasted.

Hence the necessity for museums as most important adjuncts in connection with all establishments for teaching biology.

[1] Lecture at the Loan Collection of Scientific Apparatus, South Kensington, July 26, 1876, by Prof. W. H. Flower, F.R.S., Conservator of the Museum of the Royal College of Surgeons of England.

It has been suggested to me on this occasion to give a few hints as to the class of objects best adapted for display in such museums, and the best method of preserving them, especially illustrated by the specimens contributed to the Loan Collection, and having been a collector of specimens and a curator of some kind of museum all my life, at all events since I was nine years old, I do not speak without some experience. As the time at my disposal will be limited, I shall say nothing of the preparation and preservation of specimens intended for microscopical investigation and demonstration, as that is a special branch of the subject, on which there are numerous excellent treatises, and which is considered in other lectures of the present course. I shall therefore confine myself to objects visible to the unassisted eye, and mainly to those derived from the animal kingdom, as having come most fully within the scope of my own experience.

Whether it has arisen from a mistaken impression that museum specimens are scarcely legitimate objects for this exhibition, or whether from the too general neglect on the part of those in charge of collections, of expenditure of labour, ingenuity, skill, and taste, in effecting improvements in arranging, displaying, and preserving the objects under their care, this department of the Exhibition is, on the whole, not very satisfactorily represented, and, compared with some others, puts in rather an insignificant appearance. And yet properly preserved and displayed specimens are essentially "scientific apparatus;" the preparing of such specimens is a most valuable aid to the cultivation of biology, and it is to be regretted that such an opportunity as that now presented of comparing the merits of different processes and of different materials used in the art, has not been more fully taken advantage of.

In considering the subject of museum specimens, it will be convenient to treat, first, of the methods of preservation in a dry state, afterwards of the methods of preserving animals or parts of animals in some kind of fluid, and lastly, to speak of the reproduction, by means of casts and models, of such objects as cannot be conveniently preserved in other ways.

The first method is applicable only to certain parts or tissues of the animal body; either those like the bones of vertebrates, shells of molluscs, chitinous integuments of articulata, &c., which are in their natural condition so hard and dry, that they undergo no material change when completely deprived of all the water contained in their substance, or those like skins, hollow viscera, &c., which may be kept in shape until they are dry by filling them with some stuffing material, or distending them with air. Attempts have frequently been made to preserve soft and fleshy tissues, as the muscles, in a dry state, but by all the processes hitherto adopted they eventually lose so much of their form and substance as to be of little value as representations of actual and natural objects. Such parts of the body, and the whole of the soft-bodied invertebrates, can only be successfully preserved in a fluid medium.

The best known and most generally practised method of preservation in a dry state is that intended to give an exact idea of the whole animal when living, by means of its skin, and the various tegumentary appendages, as fur, feathers, scales, horns, &c., attached to it, removed from the body, and then mounted by means of internal supports and paddings, or "stuffed," as it is familiarly called. The art of preserving animals in this manner is called "taxidermy." Although an art of really great importance for the study of natural history, and one essential, in fact, to its proper diffusion among the masses, it is almost unrepresented in the present collection, the only exceptions being among the "Apparatus for Instruction in Physical Science, contributed by the Committee of the Pedagogical Museum of Russia." The examples sent by this committee, though extremely interesting as illustrations of an admirable system of practical school teaching, and quite equal to the average level seen in most public museums, are of no especial merit as works of art, or as showing improvement over the ordinary methods. That this level should be so low is extremely to be regretted; but as long as curators of museums are contented to fill their cases with wretched and repulsive caricatures of mammals and birds, out of all natural proportion, shrunken here and bloated there, and in impossible attitudes, it will be difficult to get it raised. There may be seen occasionally, especially in continental museums and in private collections, where amateurs of artistic taste and good knowledge of natural history have devoted themselves to the subject, examples enough to show that an animal can be converted after death, by a proper application of taxidermy, into a real life-like representation of the original, perfect in form, proportions, and attitude, and almost, if not quite as valuable for conveying information on these points as the living creature itself.

The injurious effect of a low standard of perfection in one branch of art upon another is curiously seen in the drawings of birds often introduced into pictures by some of our most accomplished artists. I could point out in the present Royal Academy exhibition several examples of birds introduced into landscapes, and therefore evidently intended to be representations of living and moving creatures, carefully copied from miserable specimens of "stuffing" of the lowest order. The fact is that taxidermy is an art, resembling that of the painter, or rather the sculptor; it requires natural genius as well as great cultivation. One of the obstacles to its improvement seems to be that few people have knowledge enough of the subject to judge of the execution of the taxidermist as they do of the painter or sculptor. And yet to curators of natural history museums this knowledge should be indispensable. But then they must give up the conventional low standard of payment for "bird stuffing" which now prevails. The artist should be able to devote far more time to the manipulation of each subject than at present, and, moreover, be able to compensate himself for the time he must spend in the study of the anatomy of the dead, and of the form, attitudes, and manners of the living. I have often thought that if a Landseer or a Wolf could have devoted himself to taxidermy, what glorious specimens we should have, and how different then would be the effect of a visit to the "bird gallery" of one of our great museums to that which it now produces. How much of nature would then be learned while admiring the art! And why should this not be? Simply because no one, at least no one in charge of a public museum, thinks of paying for a stuffed bird more than some ridiculously inadequate sum. It may be said that our natural history museums have not funds for such a purpose. If so it is of course a subject to be regretted, and ought to be remedied; but it is not exactly the case. A few really good specimens are far better than an infinity of bad ones. Let the same amount of money, judiciously laid out on skill and labour, now expended on a hundred specimens be concentrated on ten, and a far more valuable and instructive museum will be produced. The remaining specimens for completing the series for advanced students of the subject, should be kept as skins in drawers, in which state they are in every respect preferable to badly-stuffed specimens. They can be handled or examined without damage, and they do not mislead or disgust.

Next to the skin, the part of vertebrate animals most commonly preserved is the skeleton, the bones being, in fact, the most imperishable and easily preserved of all the tissues. The facilities, therefore for the study of osteology are very great, and it has especial importance in comparison with that of any other system, inasmuch as large numbers of animals, all in fact of those not at present existing on the earth, can be known to us by little else than the form of their bones.

These remarks, however, only apply to the skeleton in its *ossified* state, when the bone-tissue is so strongly impregnated with salts of lime, as to resemble, in its properties, rather a mineral than an animal substance. Many of the most important problems of anatomy relating to the skeleton, either the adult skeleton of the lower vertebrates, or the developing skeleton of higher forms, can only be worked out on fresh specimens or wet preparations. The ossified bones, which alone constitute what is popularly called the skeleton, can be studied best from dried specimens.

An osteological collection for teaching purposes should contain a certain number of mounted skeletons of the most characteristic types of vertebrate animals; *i.e.*, skeletons with all the bones joined together in their natural relations, and placed in such an attitude as the animal ordinarily assumes when alive.

In this way the student acquires a general idea of the construction of the framework of the body, the proportions and relative positions of the various parts. But in such skeletons much that is important to know is inaccessible to examination. One bone more or less overlies and hides another, and the articular ends, or those parts that come in contact with each other at the joints, are entirely concealed. Although general comparisons of form and proportion can be made with other skeletons, detailed comparisons of bone with bone are impossible. This applies more especially to skeletons articulated upon the plan almost universally adopted until the last few years, in which all the parts are immovably fixed to each other. It is to a large extent obviated by the method I shall refer to presently, but still, for the complete study of osteology, it is very desirable

indeed essential, to have at hand separate parts of skeletons, or individual bones, which may be kept in boxes, drawers, cases, or in any way found to be most convenient. Entire skeletons with the bones separated occupy very little space in boxes, and the most characteristic parts may be selected and mounted in the way I shall presently indicate.

Everyone in charge of a biological museum, however small, should be familiar with the mode of preparing skeletons. I can only indicate the outlines of the process, for in this, as in every other part of the work of making anatomical preparations, a few practical lessons from a person already an adept, and a little experience and observation will do more than any description. When the principles are known, the details can be carried out with such modifications and improvements for each individual case, as the skill and ingenuity of the operator can suggest. With regard to museum specimens generally, the question is frequently asked how such or such a preparation is made, and an answer is expected in a few words, which will enable the questioner to do the same himself. This is much as if a novice who had never handled a brush were to ask an artist how he had painted his picture and expect that a few simple directions would put him on a level with the master. Preparation-making is an art which can only be acquired by labour and perseverance, superadded to some natural qualifications not possessed in an equal degree by all.

To return to the bones, as in many respects the simplest kind of preparations. There is a popular notion that skeletons are made by putting animals into ant-hills. So I have been told over and over again ever since I was a child. I must, however, say that I have never actually seen, or even heard of a skeleton really made in this way, though ants, doubtless, especially in hot countries, will make short work of the flesh of an animal's body, leaving at least all the larger bones untouched. But we must adopt some safer and more universally applicable method of proceeding. Another common idea is that some "chemical" substance is necessary to steep them in for dissolving the soft parts, and I am often asked "What acid do you use for this purpose?" when a little reflection would have shown that the bones would be the first parts to disappear under the influence of such a menstruum. No—water—pure water, is the only thing required in preparing bones and skeletons in the great majority of cases, and in the proper use of the water the art of "macerating," as it is called, chiefly consists.

This process is nothing more or less than placing bones in water and leaving them undisturbed until putrefaction of all the flesh and blood remaining on and around them and within the hollows and small cavities of their interior, takes place, and these soft parts entirely lose their form and structure and become converted into liquids and gases mingled with the water or escaped from its surface; so that when the bones are removed and well washed, nothing remains but the comparatively indestructible true osseous tissue, which, when dried, is hard, clean, and without smell.

Maceration consists, then, essentially in the destruction of the soft tissues by putrefaction, and certain circumstances are essential or favourable to the success of the process. In the first place, the water should not be too abundant in proportion to the amount of animal matter to be destroyed. Then it should never be changed or disturbed until the process is completed. The surface should be exposed to the air, and the loss from evaporation supplied from time to time. The temperature should be uniform and elevated. Cold checks the process; freezing arrests it altogether. If the heat is too great the bones are often greasy and discoloured, as when they are prepared by boiling. It is to the fact that the process varies in rapidity according to so many circumstances that the chief practical difficulty, which is to know when it is completed, is due. If the bones are taken out too soon, unless they are returned immediately to the same water, a check takes place in their preparation. To estimate the necessary time is a matter acquired only by practice and knowledge of the surrounding circumstances. Much will depend upon the size of the bones, small bones macerating much more rapidly than large ones; also upon their condition, if fresh, they macerate far more quickly than if they have been previously dried (as is the case with skeletons sent from abroad in a rough state), or if they have been kept in spirit or any other preservative solution.

When the bones are to be removed, the water must be carefully poured off through a hair seive, and all the solid matter which remains at the bottom of the jar must be carefully searched from any of the smaller bones which might otherwise be lost. They are then removed to clean water, frequently changed for several days, well washed with a brush if necessary, and dried, if possible, in the sun.

The process of maceration is necessarily attended with disagreeable smells. As long as it continues the surface of the water slowly emits gases; but the worst is when the water is stirred up by pouring it off to remove the bones. Hence it should be carried on in the open air, or what is far better, in a building isolated for the purpose, and in which the temperature may be kept uniform. When maceration has to be conducted among dwellings, it is necessary to be very careful not to disturb the vessels, and to put some disinfectant, as chloride of lime, into them the day before the contents are taken out. This will obviate most of the usual disagreeable effects, and if not used in too great a quantity, will not cause any material damage to the bones. But chloride of lime, when used too freely, is a dangerous agent; it destroys the gelatinous portion of the osseous tissue (which of course is not removed in maceration) and leaves the bones white, chalky, and friable. After proper maceration no chemical bleaching is required. Exposure to sunlight or alternate sun and rain for some months is generally good, especially for large solid bones, though this may be carried too far, as the intensely white, cracked, porous and fragile condition of osseous fragments which have been lying long on moors or hill-sides, shows. Bones are not naturally of a pure white colour, but have a delicate yellowish or creamy tint like that of ivory.

Several substitutes for the process of maceration in water are occasionally adopted under special circumstances.

1. Boiling. This process has the advantage of rapidity, but is seldom resorted to except when absolutely necessary (as in the case of the celebrated skeleton of the "Irish giant" in the Hunterian Museum), as the fatty matter in the medullary cavity is melted and pervades the whole osseous tissue, and generally leaves the bones discoloured and greasy, as may be seen in most of those that have been cooked for the table.

2. Burying in the ground may be resorted to when there are no conveniences for maceration, but it is even a slower process. The effect upon the bones is the same, but they are nearly always stained brown by the colouring matter in the soil, and the small ones are apt to get lost.

3. It has lately occurred to me, following out a suggestion of Mr. Seymour Haden's in his excellent letters, entitled "Earth to Earth," relating to the best mode of disposing of the dead, to clean bones by burying them in a basket of charcoal, and though the experiments are not quite complete, they promise excellent results, especially as all the disagreeable odour of maceration is entirely obviated, and the process may even be carried on in inhabited rooms without any inconvenience.

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

A New Star in Cygnus.—On November 24, at 5h. 41m. P.M., the director of the Observatory at Athens, Prof. Schmidt, remarked a star of the third magnitude not far from ρ Cygni, which was not visible on November 20, the last clear evening previous. Its position from observations with the refractor was found to be in R.A. 21h. 36m. 50·5s., N.P.D. 47° 40′ 34″ for the beginning of the present year. At midnight its light was more intense than that of η Pegasi, which is rated a third magnitude by Argelander, and very yellow.

Direct intimation of this discovery was given by Prof. Schmidt to M. Leverrier, and the Paris *Bulletin International* of December 6 contains the few particulars concerning this star which the generally unfavourable weather up to that date had permitted to be put upon record. M. Paul Henry estimated it of the fifth magnitude, so that as in the cases of the similar suddenly-visible stars of 1848 and 1866, it would appear to have remained but a very short time at a maximum. He considered the colour "greenish, almost blue" by comparison with Lalande 42,304, not far distant. M. Cornu examined it on December 2 with a spectroscope applied to the great equatorial, though only during a short break; the spectrum was chiefly formed of bright lines, and consequently proceeded probably from a vapour or incandescent gas. On the same evening, but under conditions equally unfavourable, M. Cazin made similar observations with a spectroscope on the 9-inch Foucault equatorial, and with the

same result. On December 5 M. Cornu succeeded in making several measures, though still much interrupted by clouds; the *Bulletin* states:—"Il a constaté la presence des trois lignes de l'hydrogène, C, F et λ = 434 (échelle des longueurs d'onde); la raie D, du sodium, la raie b du magnésium et deux autres λ = 535 et λ = 503. La première parait coïncider avec la raie 1474 (échelle de Kirchhoff) ou λ = 532 observée pendant les éclipses dans la couronne solaire; ce qui ferait peut-être penser que la raie notée comme correspondant au sodium pourrait être celle de l'élément solaire appelé hélium."

There is a slight confusion about the declination of this star, which, according to the lithographed *Bulletin*, M. Paul Henry made three minutes less than Prof. Schmidt, while the declination, as reduced by the latter to 1855·0, differs more than a minute from his declination for 1876·0, correctly carried back.

The nearest catalogue star is one 9·2 m. = + 42°, No. 4,184, in the sixth volume of the Bonn Observations, $\Delta\alpha = -24{\cdot}6$s., $\Delta\delta = +4'\,3''$, according to Prof. Schmidt's place. We find no star in the position of the new one, in the *Durchmusterung*, nor in Lalande, d'Agelet, Bode, Bessel, &c., nor Harding's Atlas.

The remarkable star of 1866 (T Coronæ Borealis) descended to the limit of unaided vision in ten days from its discovery by Mr. J. Birmingham, of Millbrook, Tuam, on the night of May 12, when it appears to have become suddenly visible as a star of the second magnitude: it is now a little over the eleventh magnitude in Bessel's scale extended.

The similar object of 1848, detected by Mr. Hind on the morning of April 28, then of the sixth magnitude, and certainly less than the ninth on April 4 and 5, attained its maximum about May 7, and at that time was a little brighter than 20 Ophiuchi, rated a fifth magnitude by Argelander. The maximum brilliancy assigned to this star in Schönfeld's last catalogue is one magnitude too low. It continued visible without the telescope to the end of May. Last summer it was not over the thirteenth magnitude.

[By observations at Mr. Bishop's observatory, Twickenham, on the 12th inst., the position of the new star for 1876·0 is in R.A. 21h. 36m. 50·35s. N.P.D. 47° 43′ 4″; Prof. Schmidt's declination is in error. The star was of the seventh magnitude and colourless; the sky, however, very indifferent.]

THE OPPOSITION OF MARS, 1877.—In addition to the stars observed by Bessel, which are mapped on the Astronomer-Royal's Chart in the *Monthly Notices* of the Royal Astronomical Society, the following lie near the path of the planet at this opposition:—

1. An uncatalogued star of the ninth magnitude, the place of which for 1877·0 is in R.A. 23h. 19m. 55·3s., N.P.D. 101° 21′ 49″; the planet in conjunction with this star, August 25·292, G.M.T. 5′ 14″ north.

2. Lalande, 45504 – 7½ mag.; mean place, 1877·0, in R.A. 23h. 8m. 56·1s., N.P.D. 102° 14′ 0″; the planet in conjunction with this star, September 5·224, 3′ 10″ north.

On September 6·415 Mars will be in conjunction with, and 2′ 15″, north of a tenth magnitude, the mean place of which is in R.A. 23h. 7m. 46·4s., N.P.D. 102° 18′ 6″.

There is every reason to expect that this favourable opposition of Mars will be very completely observed with the view to another determination of the solar parallax.

NOTES

FOR the erection of a monument to Linnæus 36,000 crowns have been received. The monument will be erected in Stockholm, and will be unveiled on January 10, 1878, the hundredth anniversary of the death of the great naturalist.

THE inauguration of the Liebig monument in the new promenade at Darmstadt, will shortly take place. Pupils, friends, and admirers of Liebig are invited to be present on the occasion.

M. DE LESSEPS presented to the Academy of Sciences at its last sitting, the final report of Capt. Roudaire, who has returned from Tunis after having completed his survey of the Algero-Tunisian depression. The project is now quite complete and ready for execution. All the trigonometrical measurements have been taken, and the preliminary steps for making an inland sea have been considered. A commission, of which M. de Lesseps is a member, and will most probably be the referee, was appointed by the President, Admiral Paris. The opposition offered by some influential members of the Academy is now considered as being quite at an end.

FROM the *Tour du Monde* we learn that an American company proposes to introduce fur seals from Alaska into Lake Superior. The temperature of the lake is considered to be sufficiently cold for the purpose, and the company hopes to obtain from Congress and the Canadian Parliament an Act protecting the creatures from slaughter for twenty years, after which time it is supposed that they will be sufficiently acclimatised and numerous to form subjects of sport.

NEWS has been received from Gen. Nansauty, the adventurous observer who has located himself near the top of the Pic du Midi for the purpose of taking meteorological observations during the winter. He and his companions have been made comfortable and secure, the only thing wanting being a telegraph to connect the Pic du Midi with Toulouse as Puy-de-Dôme has been with Clermont. This will very likely be the work of next year. Up to the end of last week the weather was very mild and almost no snow had been observed.

AT the Arctic meeting of the Geographical Society on Tuesday night, honoured by the presence of the Prince of Wales, Sir George Nares and the other officers of the Expedition met with a deservedly enthusiastic reception. Addresses were given by Sir George Nares and Captains Stephenson and Markham, in which details were given of the work of the Expedition. Sir G. Nares gave a clear account of the currents of the Atlantic and Pacific in their bearing on the condition of the ice in the Arctic regions. We may now consider the Polar basin, he stated, as a locked-up bay continuing out of the narrowed North Atlantic Channel, with a warm stream of water constantly pouring into it between Spitzbergen and Norway, and a cold icy one as constantly running out between Spitzbergen and Greenland, and also through the very narrow straits between Greenland and America; the first conveying an enormous source of heat towards the north, the latter causing the intense cold of Canada and that on the east coast of Greenland and North America. In the Polar Sea, near the inflow of the warm water, we should naturally expect to meet the lightest ice and an early season; near the outlets the heaviest ice. And such is found to be the case. Heavy ice has been traced all the way from Behring Straits eastward to Bank's Land, and from there, west of Prince Patrick Island, to Ireland's Eye, from which point it is lost; for the sledging parties under Admirals Richards and Osborn, journeying along the north shores of the Parry Islands, found light ice. It is therefore concluded with certainty that some protecting land exists to the northward. From the *Alert's* winter quarters the heavy Polar ice was traced by Aldrich for one-third of the distance towards Ireland's Eye, leaving 400 miles still unknown; to the eastward, Beaumont proved that it extends for 100 miles, leaving about 500 miles still unexplored between his farthest and the farthest of the Greenland Arctic Expedition under Koldewey. We have now a distinct knowledge of the nature of the ice in the Polar Sea. Whether that sea extends to the Pole or across the Pole, we cannot, according to Sir George Nares, be absolutely certain, but by reasoning, we may safely predict that a very

broad opening exists north of Cape Columbia, and Sir G. Nares firmly believes that it extends at least as far as the Pole. During winter there appears to be a compact mass of ice in the Polar basin, the thinner portions of which are melted by summer heat, leaving open lanes or pools of water, giving sufficient scope for the broken-up permanent ice-mass to drive backwards and forwards according to the wind or current, its main course being towards the channels of outlet, by which a small portion escapes. By the end of September the increasing frost cements together the struggling masses, and young ice forms on the open spaces to about seven feet thick. Thus the pack covering the Polar Sea is a collection of separate pieces of ice, movable among each other during the summer like the pebbles and boulders in a river bed, each piece grinding against its neighbour as the whole body jostles its way along, slowly pressing forwards towards the outlet between Spitzbergen and Greenland. Sir George Nares showed that by calculation an extremely small proportion of the Polar ice can escape by the various channels, and that much of what remains may easily be at least 100 years old. Many other valuable observations were made on Polar questions by Sir George, and Captains Stephenson and Markham read interesting papers on "Arctic Winter Experiences" and "Sledge Travelling."

DR. PETERMANN has sent us a long letter on the English Arctic Expedition, which he has addressed to the President of the Geographical Society. Dr. Petermann says he has made himself acquainted with the history of every Arctic and Antarctic expedition that has ever been undertaken, and it appears to him "there never was a more able and heroic expedition than that of Capt. Nares. Capt. Nares's expedition may be said to have '*finished*,' as it were, a great portion, say one-third, of the Arctic regions. . . . From Smith Sound to Behring Strait, the region of the Palæocrystic Sea, our knowledge is entirely due to British enterprise and perseverance." Petermann thinks Sir George Nares has exploded the fallacy of the continuous navigability of the Smith Sound route, and that it required the greatest moral courage to return with results diametrically opposed to what was expected. He thinks that had he been able to stay another winter and gone round to East Greenland, he would also have "finished" the Pole. Petermann thinks it has been a triumph of seamanship that the commander has been able to bring back the two ships safe and sound, and that if our "enlightened and liberal Government remains true to the English way of doing things, in a complete way, and not by half-measures, it is to be hoped that these vessels will once more be sent out by a more promising route." He then refers to the six routes to the pole, advocating the Novaya Zemlya and the East Greenland routes. He believes there is a great open sea all along Northern Siberia, and states that Prof. Nordenskjöld intends in 1878 to sail right across from Norway to Behring Strait. Petermann believes that a high latitude could easily be obtained along the west coast of Franz-Josef Land, and maintains that no proper attempt has been made since Parry's journey in 1827 to push north beyond Spitzbergen. But of all the routes that by East Greenland is the one which he advocates most strongly. He maintains that throughout the summer the East Greenland coast is almost free of ice, and even in winter there is a strong outward drift. He firmly believes that an expedition, like that which has just returned, would have no difficulty in sailing direct north, crossing the pole, and coming out at Behring Strait. These views are based on the observations of the whaling captain, David Gray, and on the known drift of the Polar currents. The well-established current by the Smith Sound route brings down much ice, but much more of the Palæocrystic ice must escape by the wide opening between East Greenland and Spitzbergen. The ice-drift must leave an open space behind, and there is therefore good reason to believe that in the Polar region will be found an open sea. Petermann is convinced that Sir G. Nares, with the *Alert* and *Discovery*, could steam right to the pole by this route, probably in one season. He thinks it possible, moreover, that East Greenland and Franz-Josef Land may approach each other towards the pole, and still maintains the prolongation of Greenland across to Behring Strait, in, however, it appears to us, a somewhat modified and less objectionable form. He considers the central region to be divided into two nearly equal areas of land or islands, the one extending from the shore of East Greenland in about 20° W. long. over Baffin's Bay, Parry Island, and Point Burrow, Behring Strait, and Cape Yakan, in about 176° E. long. ; the other half thence all along the Siberian Coast, over Franz-Josef Land, Spitzbergen, to East Greenland. These two regions are in all respects distinct. In the two former, or western, the land prevails, in the latter, sea. "It is not at all unlikely," Petermann states, "that Eskimos will yet be found right under the North Pole." A Swedish and a Dutch expedition have, he assures us, been decided on. He has no hope of anything being done, meantime, to carry out Weyprecht's scheme. The mass of data collected by various expeditions has not yet been half worked out. He still maintains : "It might be done, and England ought to do it." We have endeavoured to give the drift of Dr. Peterma nn's letter, without comment.

DR. PETERMANN has sent us Nos. 123–5, of his valuable series of papers, "Geographie und Erforschung der Polar Regionen." In these he discusses the results of Nordenskjöld's recent expedition, of the Siberian expedition of Finsch, Brehm, and Ziel, and those of the English expedition. As prefatory to an abstract of Nares's report, he describes and discusses some of the observations of previous expeditions, all tending to show the impracticability of the Smith Sound route, and the probability of an open Polar Sea. The papers are accompanied by three maps illustrating the observations and theories referred to, and whatever may be thought of the latter, are a valuable contribution to the polar question.

M. W. DE FONVIELLE writes to us protesting against any scheme of Arctic exploration by means of balloons in the present state of the aeronautical art. No such scheme is practicable without some certain means of directing a balloon and insuring its progress in any direction independently of air-currents ; and as no method of steering and propulsion has yet been invented that merits consideration, discussion of the subject on existing bases is totally unscientific.

THE German Arctic Exploration Society has received a telegram from the Ob Expedition dated Jenisseisk, the 5th instant, announcing that Captain Wiggins has found a good harbour in the Podarala Bay, where he remained several weeks ; he also discovered a large island north of the mouth of the Ob, and a new land route to the Jenissei. He found the water way up the Jenissei to Nurjaha good.

ABOUT two years ago the Registrar-General discontinued the practice of sending, free of charge, to the London Medical Officers of Health, copies of the returns of deaths made by the local registrars. Among the three vestries that still protest against this change for what is public property, is that of St. George's, Hanover Square. For the last two years, therefore, the Medical Officer of Health for St. George's, Dr. Corfield, has been without these returns for his district, and the result is shown in his fourth annual report, just issued. It appears that during the year, according to the Registrar-General's returns, out of ninety-three deaths from scarlet fever, diphtheria and fever, only six were reported to the sanitary inspector. "We had no knowledge," Dr. Corfield states, "of where the other eighty-seven deaths occurred, or what precautions were taken to prevent

the spread of the diseases by means of infected bedding, clothing, &c. This shows, I think, quite clearly, that it is necessary that in some way or another I should be supplied with the mortality returns, and especially with details of the deaths from infectious diseases. As regards the cases which do not prove fatal, there is at present no official way of obtaining information about them." Dr. Corfield has stated so clearly the ill effects of the present arrangement as to show that there is evidently urgent need for an alteration.

AT the Hamburg meeting of the German Naturalists, Prof. Virchow gave an address on the present position of anthropology. In introducing his subject he pointed out that anthropology, although one of the youngest of the sciences, already occupies as advanced a position as many of the older branches of study; and also that the races possessed of least ability are also those lowest in the scale of culture. To judge of the capacity for culture from the signs of it which exist is only admissible in the case of people who are surrounded by great intellectual activity, but not in the case of entirely isolated people. Thus the Australian Papuas by no means stand so low as has hitherto been believed. On this fact Virchow founds an important doctrine, which opposes the idea that such people must necessarily fade away when they come into contact with civilisation. Virchow thinks that the extinction of such races is rather to be ascribed to the barbarousness of Europeans and to the incapacity of the latter to educate the former. There is yet no evidence that uncivilised races must become extinct, which indeed is contradicted by the history of Europeans themselves. If the civilised people of the present day be considered as the product of a higher development, we cannot regard the possibility of such a development as a cause of the extinction of such people as are now on the same platform of culture which we ourselves once occupied. In relation to this Virchow spoke of the value of a systematic observation, of collections of skulls weapons, clothing, also of evidences of intellectual activity, religious ideas, language, &c., and urged the important services that might be rendered in this respect by captains both in the navy and in the merchant service. No time is to be lost; every day is of value, since many of the lower races are rapidly becoming extinct. Virchow showed the utility of such observations by reference to the German peoples, of whose anthropology we as yet know almost nothing. Osteological remains, to be of any scientific value, should be seen in the place where they are found. Virchow spoke of the value of the statistics recently taken as to the colour of the hair, eyes, and skin of German children, to which we have already referred.

THE Siberian explorers, Brehm and Finsch, have arrived at St. Petersburg, after a most successful journey.

THE following extracts of a letter from Dr. Miklucho Maclay, dated July 3, Maclay Coast (New Guinea), appear in the *Golos*, November 28:—"I landed on June 28; the natives received me in a very friendly way, and were not at all astonished to see me amongst them again; they said they had been long awaiting my arrival, as I had said when leaving them I would soon return. Since the departure of the *Isumrud* in December, 1872, no ship had visited the shore. Three or four months after that time there was an earthquake on the island, which destroyed many villages in the highlands. Talking with my old acquaintances, I was astonished to hear how many Russian words they had retained since my first stay; they pronounce the words perfectly, and have introduced them in common use. Hundreds of natives from the neighbouring settlements helped in building me a house. I have brought for the Papuas various seeds of useful plants and of fruit-trees, and I hope they will grow as well as the Indian corn I left here in 1872. I expect soon to make some excursions in the highlands." The captain of the *Sea Bird*, who transmitted this letter, adds that Dr. Maclay was perfectly well on board the ship during the cruise and on his landing.

THE Omsk correspondent of the Irkutsk newspaper *Siberia*, writes under date September 27, that M. Potanin's expedition in Mongolia (NATURE, vol. xiv., p. 534) has been stopped by the Chinese authorities at Toolty, a short way beyond the Chinese frontier. Arriving at Toolty, the members of the expedition expressed their wish to make a visit to the Governor of the place. Being told that he resided in a convent some miles from the town, they proceeded thither, but on their arrival they were informed that the Governor was not at home, and that the visit must be made next morning. On going next morning to the convent they were met at the doors by a hostile crowd, which soon began to throw stones at them, seriously injuring two persons. The authorities then appeared and took the members of the expedition into custody, but released them next day, announcing that they could not answer for their safety if they insisted on proceeding further.

SOME very important geological discoveries have been made by Prof. Hartt and his assistants in Brazil. Working over again the region explored by Prof. Theodore B. Comstock in 1870, and by Messrs. Hartt and Derby in 1871, they have extended the Devonian down 1,000 feet below the beds discovered by Messrs. Smith and Comstock. The lower beds are Oriskany, with the characteristic North American fossils, as well as some others (seventy-five species in all) which have undoubted Devonian affinities. Carboniferous beds were also discovered to the northward, making a complete section from the base of the Devonian to recent beds in the lower Amazonian Valley.

PROF. TYNDALL has accepted the office of President of the Midland Institute for 1877.

THE professors of the new School of Anthropology established in Paris under the patronage of the Faculty of Medicine and the Municipal Council, intend to organise excursions. The first of these was made last Sunday to the St. Germains Museum of National Antiquities, of which M. Mortillet is the sub-director. He explained to a large number of visitors the arrangement of the collections and the scientific value attached to the several curiosities which are exhibited.

M. LEVERRIER has organised meteorological warnings for St. Cloud, Clichy, Boulogne, and Levallais Perret, and will gradually extend them to the suburban districts. He will give a lecture at Boulogne to the several delegates of Communes for the purpose of teaching them the use of the meteorological maps published daily by the Observatory.

ON the 16th of next month, at the scientific meeting of the Zoological Society, we are informed that Captain Feilden, Naturalist to the Arctic Expedition, will exhibit the birds obtained by him in the regions he has so recently traversed.

AT the last meeting of the French Geographical Society, it was intimated that M. Jules Trebeau had been sent to explore French Guiana, which although a part of the territory of a civilised nation is almost untrodden by explorers. M. Trebeau will proceed by the river Maroni, up to its source, with a party of three French and ten negroes, and will return by the Oyapok or Amazon, according to circumstances.

"NIPHON and its Antiquities" is the title of a pamphlet by Mr. W. C. Borlase, in which from personal investigation during a stay of some weeks in Japan, and the study of the best works on the subject, the author gives an interesting summary of what is known of the ethnology, mythology, and religions of the Japanese. The pamphlet is published by Brendon and Son, Plymouth.

In a paper in the *Cleveland* (U.S.) *Herald*, November 14, entitled "Archæological Frauds," Col. Whittlesey examines some of the recent so-called prehistoric finds in some parts of the United States, and comes to the conclusion that most of them are extremely suspicious; among these is the well-known Grave Creek inscription.

The New York Aquarium, some account of which we gave recently, is publishing a fortnightly journal. Of course it is essentially popular, but, while keeping an eye to the success of the aquarium, it gives considerable information concerning its inhabitants.

We have received from New South Wales several papers which show that there is a creditable amount of activity in connection with science in that colony. We recently published a brief account of two meetings of the Royal Society of the colony, and from the rules, list of members, and other documents in connection with that body, which have been sent us, we have hopes that it will become an important centre of scientific influence and culture. We believe that the recent development of the Society is greatly due to the energy of the hon. secretary, Prof. Liversidge. "New South Wales; its Progress and Resources," is the title of a paper prepared by the authority of the Commissioners for the Philadelphia Exhibition, and giving, in brief space, an interesting account of the rapid progress of the colony. Along with this is a mineral map of New South Wales, showing the localities of the principal minerals, the back being utilised for the tabulation of some important statistics. As text to this map is a long and valuable paper by Prof. Liversidge giving a complete account of the minerals of the colony. Finally, we have by the same gentleman, a "Report of the Sugar-Cane Disease in the May River District, Queensland."

We noticed some time ago the opening of Mr. Rooke Pennington's local museum at Castleton, in Derbyshire. We are informed that the result has been a great impetus to scientific study in that and the neighbouring Peak villages, and that a course of lectures has been arranged in connection with the museum. The first lecture will be delivered by Mr. Ralph Betley, F.G.S., who will take as a subject "Water." Mr. C. E. De Rance will give the second.

A very valuable and interesting collection of silver ores from Chili and Bolivia was sold by auction the other day by Mr. Stevens at his sale rooms in King Street, Covent Garden. Some of the specimens realised very high prices, one piece of red silver about the size of an orange being bought by Mr. H. Ludlam for 200*l.*, another about a quarter the size, with very perfect crystals, was secured by Mr. Bryce M. Wright, the mineralogist, for 100 guineas, and the remainder, comprising about 100 small specimens, were sold at proportionate prices; we believe Mr. Wright bought nearly a third of the collection, which was probably one of the finest ever brought to England.

This year Dr. Hermann Müller was accompanied to the Alps by one of his pupils, Ed. Gaffron, who collected, prepared, and carefully mounted fine specimens of all those Alpine flowers which Dr. Müller has observed and described, or will describe, in Nature, in his articles on the Fertilisation of Flowers by Insects. Twenty complete collections have been made, and the young collector is anxious to sell them in order to raise funds to accompany Dr. Müller next summer. No doubt a number of our readers will desire to possess such a collection, which may be obtained by writing to Eduard Gaffron, Realschule, Lippstadt. The price, we believe, of a single collection is fifteen shillings.

The additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus ruber*) from West Africa, presented by Mr. J. W. Feather; an Australian Crane (*Grus australasiana*) from Australia, presented by Mr. H. Roberts; two Crested Guinea Fowls (*Numida cristata*) from West Africa, presented by Mr. Daniel R. Ratcliff; two King Parrakeets (*Aprosmictus scapulatus*) from New South Wales, presented by Miss E. Rigby; a Short-eared Owl (*Otus brachyotus*), European, presented by Mr. W. R. Stanley; a Snowy Owl (*Nyctea nivea*), European, presented by Mr. John Kendall; a Brown Capuchin (*Cebus fatuellus*) from South-east Brazil; a Kinkajou (*Cercoleptes caudivolvulus*) from South America, a Royal Python (*Python regius*), a West African Python (*Python sebæ*) from West Africa, deposited.

SOCIETIES AND ACADEMIES

London

Linnean Society, December 7.—Mr. G. Bentham, vice-president, in the chair.—Mr. Francis Day read Part I. of the "Geographical Distribution of the Fresh-water Fishes of India." This contribution aims towards solving the vexed question of whether the fauna of Hindostan is mostly African or Malayan. The author first separates the true fresh-water species from those which enter rivers from the sea for breeding or predacious purposes. Out of nine families of Spiny-rayed fish (Acanthopterygians), only two are likewise found in the African region, but one of these is in Madagascar, which is doubtfully African; the other is also found in the Malay Archipelago, which possesses representatives of eight out of nine families. Each of the forty-five known species is then followed out, and the author considers that the Indian and Malayan fauna (of the group in question) are essentially identical, whereas the species are scarcely represented in Africa. The fresh-water fishes of Ceylon, the Andamans and Nicobars, he believes, are also strictly Indian, whilst as these fishes cannot be spread except by line of fresh-water communication, it thus appears highly probable that these islands were at one period connected to the continent of India. Moreover, certain forms exist in Malabar which are absent from the rest of India, but reappear in the regions of Chittagong or Siam.—Mr. J. G. Baker gave the substance of an exhaustive memoir on a general systematic arrangement of the Iridaceæ (the Iris family). Nearly all the Iridaceæ inhabit temperate regions, and may be grown successfully in the open air in this country. Some are among our most familiar garden genera—for instance, the Crocus, the Iris, and the Gladiolus. Altogether about 700 species and sixty-five genera are now recognised. In his present classification the structure of the perianth mainly guides the author to adopt three primary divisions—(1) Ixieæ, (2) Irideæ, and (3) Gladioleæ, the above common garden plants serving respectively as typical examples of these groups. The three divisions in question are again subdivided into—(*a*) Those having *bulbs with free stamens*; (*b*) Those having *bulbs with monadelphous stamens*; (*c*) Those *wanting bulbs, but with free stamens*: (*d*) Those also *devoid of bulbs with monadelphous stamens*. As regards distribution, 312 genera are found at the Cape; in Europe and North Africa, 94; Temperate Asia, 89; Tropical America, 82; Tropical Africa, 56; South America, 34; Australia, 31; and Polynesia, 1.—The Rev. W. A. Leighton communicated a description of eleven new British Lichens, seven of these belonging to the genus *Lecidea*, one to *Odontotrema*, and three to *Verrucaria*.—The Chairman passed some remarks on a folio treatise concerning the structure and culture of the quinine-bearing trees (*Cinchona*) in our East Indian Plantations, by I. E. Howard, F.R.S.—Mr. T. Christy exhibited specimens of the so-called Black Coral (*Antipathes*) from the Philippines, referring to its commercial value.—Thirteen gentlemen were elected Fellows of the Society.

Zoological Society, December 5.—Dr. E. Hamilton, vice president, in the chair.—A letter was read from Count T. Salvadori, announcing that a new species of Paradise-bird, of the genus *Drepanornis*, had been discovered near the most inland point of Geelvink Bay, New Guinea.—A communication was read from Mr. Andrew Anderson, containing some corrections of and additions to previous papers on the Raptorial Birds of North Western India.—Mr. Francis Day read a paper on the fishes collected by the Yarkand Mission, in 1873, to which the late Dr. Stoliczka was attached as naturalist. The paper gave an outline sketch of the fresh-water fishes of Hindustan, Afghanistan, Western Turkestan, Yarkand, Tibet, and Cashmere. The author showed that the principal fishes of Yarkand belong to a

local group of carps, termed "Hill Barbels, or *Schizothoracinæ*," by McClelland: that this group is almost restricted to cold and elevated regions, spreading to the most eastern portion of Western Turkestan, Afghanistan, and along the western slopes of the Himalayas to China; and that these forms are entirely distinct from the carps of the plains to the south of the Himalayas.—A communication was read from Mr. Martin Jacoby giving the descriptions of new genera and species of Phytophagous Coleoptera.—A communication was read from Dr. A. Günther, F.R.S., containing the description of a new species of lizard from Asia Minor, which he proposed to name *Zootoca danfordi* after Mr. C. G. Danford, its discoverer.—Dr. Günther communicated a paper by Mr. W. Ferguson, of Colombo, containing the description of a new species of snake of the genus *Aspidura* from Ceylon, for which the name of *A. guentheri* was proposed.

Geological Society, November 22.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—The following communications were read:—On the pre-Cambrian (or Dimetian) rocks of St. David's, by Henry Hicks, F.G.S. Referring to the ridge of pre-Cambrian rocks, which he described in a former paper as running down the St. David's promontory, and as previously supposed to consist of intrusive syenite and felstone, the author stated that he had now found it to be composed exclusively of altered sedimentary rocks of earlier date than the Cambrian deposits, the conglomerates at the base of which are chiefly made up of pebbles derived from these rocks. Recent investigations had led him to the conclusion that the main ridge was composed of two distinct and decidedly unconformable formations, the older of which, composed of quartzites and altered shales and limestones, constituting the centre of the ridge, has a north-west and south-east strike, and dips at a very high angle; whilst the newer series, consisting of altered shales, and having at its base a conglomerate composed of pebbles of the older rock, has a strike nearly at right angles to that of the latter. For the former he proposed the name of Dimetian, and for the latter that of Pebidian. The author indicated the points of resemblance between these pre-Cambrian rocks and the Laurentian of Canada, the Malvern rocks, and others in Scotland and elsewhere, but thought it safer at present to abstain from attempting any definite correlation of them. The exposure of the older, or Dimetian series, led the author to ascribe to those rocks a thicknesss of at least 15,000 feet; the upper, or Pebidian rocks, which flank both sides of the old ridge through a great portion of its length, are apparently of considerably less thickness, but they are in most parts more or less concealed by Cambrian deposits overlying them unconformably. Running nearly parallel with Ramsey Sound is another large mass of the author's Pebidian rocks, and at the south-western extremity of Ramsay Island they compose a bold hill almost 400 feet high, and on the east side of this a fault, with a downthrow of at least 14,000 feet, has brought the Arenig beds into contact with the pre-Cambrian rocks.—On the fossil vertebrates of Spain, by Prof. Salvador Calderon, communicated by the President.

Anthropological Institute, November 28.—Col. A. Lane Fox, F.R.S., president, in the chair—An Indian hammock from the city of Mexico, weapons from Perak and British Guiana and a Bosjesman's skull were exhibited.—The President, by permission of Messrs. Bollin and Feuardent, exhibited some terra cotta figures from Tanagra, Bœotia, and read some notes thereon. Mr. Hyde Clarke and others joined in the discussion.—Papers on the Lapplanders, by A. von Humboldt von Horck, and on the tribes of British Guiana, by the Rev. D. Harper, were also read.

Royal Microscopical Society, December 6.—H. C. Sorby, F.R.S., president, in the chair.—The President exhibited a *facsimile* of Jancen's microscope, made by permission of the Dutch Government from the original, exhibited at the South Kensington Loan Collection.—A paper by the Rev. W. H. Dallinger was read by the Secretary, on *Navicula crassinervis*, *N. rhomboides* and *Frustulia saxonica*, as test objects, in which, after referring at some length to the recent discussion upon the subject of their identity or difference, he expressed his belief that they were all specimens of *Rhomboides*, differing only as to size; and in support of his opinion, a number of very beautifully executed drawings were exhibited to the meeting, showing the microscopical appearance of the diatoms in question under a magnifying power of 800 diameters.

Institution of Civil Engineers, November 28.—Mr. George Robert Stephenson, president, in the chair.—The paper read was on the chalk water system, by Mr. J. Lucas.

CAMBRIDGE

Philosophical Society, November 20.—Mr. O. Fisher read a communication on the effect of convective currents on the distribution of heat in a bore-hole. This paper was supplementary to one read by the author on November 29, 1875. The temperatures obtained in the boring at Speremberg, near Berlin, which attained a depth of upwards of 4,000 feet, were reduced to a mean law by Prof. Mohr, of Bonn, and had been already shown by the author to conform closely to those expressed by a parabolic curve, having its axis horizontal, and its vertex at a depth of 5,171 feet expressed by the equation—

$$v = -\frac{251}{10^8}x^2 + 0{\cdot}012982\,x + 7{\cdot}1817,$$

v being the temperature expressed in Reaumur's scale, and x the depth. An elaborate account of the observations taken in the Speremberg boring has been given by Dunker in a paper entitled "Ueber die Benutzung tiefer Bohrlöcher zur Ermittelung der Temperatur des Erdkörpers, und die desshalb in dem Bohrloche zu Speremberg auf Steinsalz angestellten Beobachtungen." The temperature curve within the earth's crust being believed to be a straight line expressing an increase of 1° Fahr. for 60 feet of descent, it was shown that the departure from this law in a large bore-hole (in the present case a foot in diameter) might be accounted for by vertical currents. At the surface of the water its temperature, from radiation and evaporation, will nearly coincide with that of the upper bed of rock. As we descend, the tendency will be for the currents to warm the water in the upper part of the bore-hole above the normal temperature of the rock, and to cool it in the lower part below the temperature of the rock. At a certain depth the temperatures of the water and rock would be the same. By Dunker's table this appears to have occurred at the depth of about 200 feet. A diagram representing graphically the effects mentioned will appear, accompanied by the proper explanations, in the forthcoming number of the Society's *Proceedings*.—Prof. Hughes then gave a criticism of the evidence for pre-glacial man, which we have already noted.

MANCHESTER

Literary and Philosophical Society, October 31.—Rev. William Gaskell, M.A., in the chair.—Remarks on the general affections of the barometer noticed by Mr. J. A. Broun, by Prof. B. Stewart, LL.D., F.R.S. Mr. J. A. Broun has found as an experimental fact that simultaneous variations of the barometric pressure occur at such distant portions of the globe as to lead to the inference that the whole globe is thus affected, from which Mr. Broun infers that some other force besides gravity is concerned in these phenomena. We know as a matter of fact that there are causes at work which give rise to electrical separation, although we may not know the precise nature of these causes. Thus evaporation goes on from the surface of the sea and of the land. Changes take place in the amount of aqueous vapour held by the air, and also probably in the molecular state of this aqueous vapour. But although we may not be able to point to the specific actions which produce electrical separation, we know that such separation implies a one-sidedness or heterogeneity; and since gravity will presumably act differently on the two things, we may probably suppose that one of the constituents which have caused this electrical separation may have a tendency to mount upwards in the atmosphere, while the other may have a tendency to move downwards. For instance, if evaporation from the surface of the earth or sea be one cause of this electrical separation, we might imagine the land or sea to become electrified in one way, while the vapour electrified in the other direction might mount in the air, owing to its being specifically lighter. In fine, whatever be the cause of the electrical separation, we may presumably suppose that the one constituent will either remain below or find its way downwards, while the other, carrying with it its peculiar electricity, will mount upwards. Now, may not the earth be regarded as a Leyden jar, the sea and earth forming one coating, and the upper, rarer, and hence electrically conducting strata of air forming the other coating; and will not the tendency of the action above named be to charge the upper coating with one kind of electricity and the under with another? Such a process would, of course, be continually going on, while on the other hand the earth, regarded as a Leyden jar, would, by means of thunderstorms, and possibly by other means, be continually discharging itself. Next, let us suppose that by some extensive local circumstances a greater than usual electrical separation and charging of the earth jar has

been going on. The effect of this local cause would, however, not be local, but would contribute to increase the charge of the earth as a whole—as one jar in fact, so that the earth as a whole might, for a short period, be increasing its charge—the local charging causes being in excess of the local discharges. Next, would not this excessive charge appear to increase the barometric pressure of the air over the whole earth? On the other hand we may imagine the discharging influences to be sometimes in excess of the charging causes, and then the electrical separation of the earth jar would diminish, and the barometric pressure of the air appear to diminish also. These remarks are put forward not as a formal theory, but rather with the view of inviting discussion. In considering a fact such as that brought forward by Mr. Broun, we must first endeavour to explain it by the operation of some known cause. I have therefore introduced a force which we know to exist, and a mode of operation which is not at first sight improbable. It may be thought that electrical separation can hardly be great enough to produce a sensible barometrical difference. Let this be proved, and a point will be gained by the dismissal of what seems at first sight a possible hypothesis. Meanwhile—to bring these remarks to a practical issue—might it not be well to examine the records of atmospherical electricity corresponding to the dates of Mr. Broun's observations with the view of ascertaining whether Mr. Broun's results are in any way connected with the electrical state of the earth's envelopes?

GENEVA

Physical and Natural History Society, October 5.—Prof. Plantamour communicated to the Society the continuation of his investigations into the climate of Geneva, dealing especially with winds, cloudiness, and rain. As to winds, he has obtained for the last fifteen years a confirmation of the previous results, viz., that the local winds at Geneva play an important part in consequence of the action of the lake; the land breeze at the lake giving a south wind, prevails in the warm season, especially during the morning and evening hours. Generally the south wind prevails from April to October, while the *bise*, or north wind, prevails from November to March. With regard to cloudiness, M. Plantamour has established that the daily variation changes in a notable manner with the season. In winter the minimum of nebulosity occurs at 4 P.M., while in summer it is the afternoon hours that are most overcast. In winter it is the night cold which is the principal cause of clouds; in summer the afternoon heat. December is the most cloudy month; July and August are the clearest months, with difference from one year to another. Examining next the period of fifty years from 1826 to 1875, in reference to the quantity of rainfall, M. Plantamour arranges the years into very dry, dry, wet, and very wet, according to the proportion of rain which they have given, as compared with the mean. It follows from this classification that years of different qualities succeed each other in an order entirely irregular; that a year may be followed as likely by a similar as by a different or very different year in point of humidity. However, there are series of years in which the rainy prevail, and others in which the dry prevail. Thus, from 1826 to 1837 there were eight dry years and four wet; from 1838 to 1856, fourteen wet years and five dry years; from 1857 to 1865, six dry years and three wet. Still there is no regularity, no periodicity in the return of these dry and wet epochs, nothing especially which we may connect with the eleven-year period of sun-spots.

PARIS

Academy of Sciences, December 4.—Vice-Admiral Paris in the chair.—The following papers were read:—On a note of P. Secchi relative to the formation of hail, by M. Faye. He is surprised at P. Secchi giving out as new the idea of trombes with vertical axes bringing down cold air from the upper regions. He urges the sun-spot analogy.—Indices of a new genus of edentate mammifera, fossil in the Saint Ouen Eocene deposits, by M. Gervais. The peculiarities of some of the fragments (chiefly a calcaneum, and metatarsal or metacarpal bones) are described. The animal seems to have been like *Macrotherium* and *Ancylotherium* in some respects, unlike in others. M. Gervais names the genus *Pernatherium*, and the species *P. rugosum*, in allusion to the wrinkles on many of its bones.—Preparation of alcohol by means of sugar contained in the leaves of beets, by M. Pierre. He calculates that 34 kilogrammes of juice of the leaves may yield 0·198 litre of absolute alcohol the leaves of 1 hectare about 173 litres. At the moment of removal the leaves contained nearly 350 kilogrammes of sugar per hectare.—M. de Lesseps, presenting a *brochure* on Africa and the Geographical Conference at Brussels, explained the scheme of the King of Belgium of an international association for opening and civilising Central Africa.—On the employment of iodide of potassium in lead colic and paralysis, according to M. Melsens' method, by M. Jacobs. The patient takes 1 gramme a day, and increases the dose by 1 gramme up to 6, 8, 10, 12, or 15 grammes, then returning gradually to the initial dose. The more iodide he can bear the sooner is he cured.—Researches on the devitrification of vitreous rocks, by M. Meunier. M. Levy's facts do not seem to him to furnish any argument against the production of crystalline rocks at expense of vitreous rocks, by way of devitrification.—Results obtained by the decortication of vine stocks, by M. Sabaté.—Report on the experiments made by the Paris-Lyons-Mediterranean Company for combating phylloxera, by M. Marion. They commend sulphide of carbon and sulpho-carbonates, which should be applied when the products of the winter eggs have descended to the roots, *i.e.*, about July. The old phylloxera of the roots is thus treated as well.—Scale of platinised iridium (4 m.) of the International Geodesic Association, by M. Matthey. Observations on this communication, by M. H. Sainte-Claire Deville, also by MM. Tresca and Dumas. M. Tresca remarked on the high density, about 21·50. If it contained only 10 per cent. of iridium, $\frac{1}{1000}$ of rhodium, and $\frac{1}{1000}$ of ruthenium, the manufacture of metals of platinum must have been greatly improved since 1872.—Observation of a new star in the constellation of Cygnus, by M. Schmidt, director of the Athens Observatory.—Observations of the planet (169) Zelia, discovered at the Observatory of Paris, Sept. 28, 1876, by MM. Henry.—On the application of the methods of mathematical physics to the study of bodies terminated by cyclides, by M. Darboux.—New method for studying calorific spectra, by M. Aymonnet. With a constant heat-source, one may, even with a pretty large aperture of thermopile, find approximately the quantities of heat in spectral parts smaller than the aperture. Thus, suppose the pile is 1 mm. in aperture, you advance it (say) two-tenths of a millimetre at a time, noting each time the portion of spectrum passed from and the new part embraced. This is the principle of the method; by which M. Aymonnet gets some interesting results. The minima present a remarkable periodicity, and are displaced when the temperature of the source varies or a liquid screen is interposed.—Productions of carbonate of soda by the action of chloride of sodium in solution on carbonates of lime and magnesia, in presence of vegetable matters, by M. Pichard.—Researches on sensation compared with motion, by M. Richet. He formulates this general law as applicable both to muscles and to sensitive nerve centres: the number of excitations necessary to produce a perception or a motion is inversely proportional to the intensity and frequency of these excitations.—Experimental researches on the cardiac, vascular and respiratory effects of painful affections, by M. Franck. The immediate effect on the heat is always a stoppage or retardation of the beats.—On the form and reciprocal relations of the cellular elements of loose connective tissue, by M. Renaut.—Habits of fishes; the gourami and its nest, by M. Carbonnier.

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