THURSDAY, SEPTEMBER 30, 1875

THE SCIENCE COMMISSION REPORT ON THE ADVANCEMENT OF SCIENCE*

IN our last article under the above head we commenced our analysis of that part of the Commissioners' Report which deals with the Administration of Science. In our present article we shall conclude our notice of the Report by stating the chief arguments and opinions of the witnesses regarding the formation of a Council of Science. Thus, following the evidence of Sir Wm. Thomson and Dr. Frankland, to which we have before referred, we find Dr. Hooker, Admiral Richards, General Strachey, Dr. Roscoe, Dr. Balfour Stewart, Dr. Sclater, Mr. De la Rue, Sir H. Rawlinson, and others in favour of a Council, while the Astronomer Royal, Prof. Owen, Lord Salisbury, and Lord Derby are opposed to its formation.

Admiral Richards, late Hydrographer to the Navy, is of opinion that the appointment of a Minister of Science and of a Council stand and fall together; and thinks "that the one would not be of very much value without the other."

Dr. Sclater's idea of the Council is as follows:—

"The heads of the different scientific institutions that are put under the control of the department of science and the minister of education might form a consultative body and be called a council of science, and that there might be certain other members added to assist them in deliberation, if it were thought necessary, such as representatives of the College of Physicians, the College of Surgeons, and of the scientific branches of the army and navy.

"Most men of science, I think, see that something of the sort is imperatively required. All lament the piecemeal way in which scientific subjects are dealt with by Government, in consequence of their being subdivided amongst all these different offices, and of there being nobody to appeal to upon a question of science, and therefore I think the proposal to establish such a Council would meet with universal acceptation amongst scientific men."

Dr. Hooker, the President of the Royal Society, gives it as his opinion "that the general proposition, that the Government should be aided by scientific persons, is an excellent one, both with respect to the administration of the existing Government scientific institutions and with respect to the occasional grants which the Government may be called upon to make for scientific objects." Like Dr. Roscoe, he thinks that the Council should not consist exclusively of scientific men.

Mr. De la Rue considers that the usual permanent staff of a secretary and assistant secretaries, as suggested by Prof. Owen, even if they were men of science, would not be sufficient; urging as a reason that science is really now so extensive that one could hardly imagine any secretary to be so intimately acquainted with every branch of science as to be able, even with the aid of his assistant secretaries, to advise, or to point out where to obtain specific information on every question which might be brought under consideration. Nor does he think the Government Grant Committee, a body regarded with favour by many witnesses, could be so modified as to render a special Council unnecessary.

Sir Henry Rawlinson regards the nomination of a Permanent Council of Science as the natural remedy for the "spasmodic" action on the part of the Government; and another Indian officer, General Strachey, gives the following important evidence:—

"The persons who are employed in the public administration are certainly as a class not amongst those who have anything deserving the name of scientific education; therefore, for a long time to come, it is not to be expected that the members of the Government, or their chief subordinates, will have any such general knowledge of science as would enable them at all satisfactorily to deal with the scientific questions which come before them. Therefore I conclude that it is absolutely essential for the Government, under any circumstances, to get advice from outside; and then comes the question as to how this advice is to be got. If there is no recognised and regularly organised body whose business it is to give advice to the Government on such subjects, then the only thing that a minister can do is to get his information from unrecognised and irresponsible authorities, persons whose opinions, perhaps, may be very valuable, but still persons of whom the public never can have any cognisance; and private advice given in that way seems to me given in the worst possible form. If, then, that form of advice is bad, how can you obtain advice of proper intrinsic value on the multifarious subjects on which it is certain to be needed by an administration really striving to advance science to the utmost, and how can you secure its being given under a sufficient sense of responsibility, and in such a way as to carry the greatest weight possible to the mind of the minister who is expected to act upon it? And here I would repeat that any specific proposal to give effect to such an idea must be made to fit into the general form of the administration; and I therefore consider that the best course would be to adopt the proposal that has been made by many persons, that there shall be some sort of council constituted to advise the responsible Government department as to its proceedings in connection with science."

He then proceeds:—

"I would take the opportunity of saying that it is a question that is open, and which I believe has been discussed, whether the Council, for instance, of the Royal Society, with or without any addition, might not be made to perform satisfactorily some or all of the functions which it has been suggested should devolve upon this Commission. But I think not. And the principal reason that I have for thinking that such a body as the Council of the Royal Society is not suitable for the purpose is, that it cannot have that specific responsibility put upon it which should be put upon a body such as I have spoken of, and that it is got together for totally different purposes and objects. The Council of the Royal Society has to manage the business of the Royal Society, and is not at all selected to advise the Government on matters connected with the advancement of science, or the application of science in the operations of the public departments."

He further points out that the Minister would have a perfect right to repudiate any scheme which the Royal Society might put forward, or any advice they gave—that he would be justified in doing so on the ground that he was not responsible for their selection.

Capt. Galton points out that "the institutions which are maintained by the State for scientific purposes are maintained upon no principle whatever with regard to their administration. You have got the British Museum under trustees, you have got South Kensington under the President of the Council, you have Kew under the Office of Works, you have the Botanic Gardens at Edinburgh, I think, under the Queen's Re-

* Concluded from p. 432.

membrancer. You have the Observatory at Edinburgh as part of the University of Edinburgh, and you have the Observatory at Greenwich under the Admiralty, besides several others. You have every possible variety of jurisdiction, and, consequently, it seems to me that you have a great waste of power; there is the School of Chemistry, and the School of Mines, and the Museum at Edinburgh, all under South Kensington Museum, and the Meteorological Department, which is partly under the Royal Society and partly under the Board of Trade. There is no possibility of getting any correlation between those different scientific bodies, and if you are to get proper unity of administration you must bring them all under one head, or to one focus. I should recommend placing them all under a scientific commission or council, and I should place that council probably under the Privy Council; but I should make it a body for administering all questions connected with all the scientific institutions, or all grants made by the Government for scientific purposes in the country, and I should give to this council the same status, with regard to its administration, or very much the same, that the Indian Council have. . . . . The parliamentary head of the department, if he differed from them in opinion as to their recommendations upon the scientific questions connected with those institutions, or any other that might be founded, should record his differences of opinion in a minute."

Dr. Siemens would "assemble the heads of departments at frequent intervals for the discussion of general questions, and would propose to add to their number such men as the president of the Royal Society, the president of the Institution of Civil Engineers, and at least one representative of the two great Universities. This Board would decide general questions appertaining to the advancement of science."

We could fill many more columns with evidence analogous to the above samples. Making due allowance for the different ways in which a new and complex question like this, compounded of scientific, political, and administrative elements, must present itself to a variety of minds trained to dissimilar pursuits and habits of thought, the almost general consensus as to the necessity of some such advising body as that proposed is most striking.

Still those who object to the creation of a Council on various grounds are not wanting, and we now glance briefly at the evidence of these witnesses.

Sir G. Airy thinks a paid Consultative Council could not do very much to assist the Government, and that the Council of the Royal Society would be the best body to which the Government could have recourse in any matters of that kind.

Prof. Owen prefers a Minister of Science, with a permanent Under-secretary and administrative staff, as in his opinion the representative of any particular branch of science on the Board would have too great an influence.

The Earl of Derby is very sceptical either as to the necessity, or as to the utility, or as to the successful working of such a Council. One objection he urges is that if matters for which the head of a department is responsible are to be referred to the Council, and if upon those matters the Council is to pronounce an authoritative opinion, the responsibility of Ministers to Parliament will be considerably lessened.

In reply to the suggestion that one function of the Council would probably be to advise the State as to the application of money for the higher teaching of science and for scientific research, and also to advise the Government with respect to any applications that may come before it for grants of money connected with science, whilst objecting to a Council, Lord Derby thinks that it is a matter which falls strictly within the province of the Minister of Education.

Lord Salisbury is opposed to a Council because he has never seen anything to lead him to believe that such a Council of Science would have anything to do; and he considers that the Government would always get better opinions on any scientific point that arises, by applying to the most distinguished scientific man in that particular branch at the time, than it would by having a set of permanent officers to give advice on such subjects.

There appears to have been before the Commission practically three solutions of the question. First, that no change should be made in the present condition of things. The Astronomer Royal is apparently the sole witness of eminence in science who seems to desire no reform in the scientific administration of the country. Secondly, that the Council of the Royal Society should be constituted the official advisers of the State—a view held generally by those who are adverse to the creation of a new Council; and third, that a Council be provided to assist the Minister charged with science and the Departments concerned with science.

The Commission arrive at the conclusion that the balance of argument and authority is in favour of the last-named arrangement, which accordingly they recommend in terms which, though general, leave no doubt that they contemplate the creation of a new official body so constituted as fairly to represent the various branches of science. We think that no unprejudiced and competent person can read the whole evidence without accepting this conclusion as undeniably sound, if not indeed absolutely unavoidable.

THE GOVERNMENT RESEARCHES IN PATHOLOGY AND MEDICINE

THE third volume of the "New Series of Reports of the Medical Officer of the Privy Council and Local Government Board," brings before us another instalment of the work paid for by the annual grant of 2,000*l.* "in aid of scientific investigations related to pathology and medicine." This grant has been actively opposed by a small minority in the House of Commons mainly upon the narrow and invidious ground that the medical profession was thereby obtaining knowledge and instruction which the medical profession ought to obtain at its own expense. "The medical profession lives upon the public; the medical profession makes use of its knowledge to extract money from the public; the grant will add to the knowledge which the medical profession uses with such object—therefore the grant is money drawn from the pockets of the public to aid in the further depletion of the pockets of the public."

Such appears to be the main inspiration of the opposition. Whoever will be careful to read the last public report, and the short but most weighty statement with which Mr. Simon introduces it to his chiefs, will see plainly that this kind of opposition is founded in misapprehension or ignorance. The information sought is such as may help to inform the State how to offer most effectual resistance to the introduction of disease from without, and to the extension of disease within. It concerns resistance to typhoid fever, small pox, and many other diseases of well recognised contagious nature, and the possibility of controlling the extension of diseases less recognised as having like nature, as for instance what Mr. Simon calls "the tubercular infection." "It aims to be a systematic study of the intimate pathology of the morbid infections, acute and chronic."

Mr. Simon in his remarks points out that much of the study involved is most elaborate and purely scientific, never immediately convertible to pecuniary profit, but perhaps, on the contrary, involving heavy cost; not pretending to immediate popular application, but addressing itself primarily to the deeper scientific requirements of the medical profession, and therefore in an extreme degree technical. Studies of this sort cannot be cultivated to any adequate extent by private medical investigators, and the scientific investigations set going by the 2,000*l.* grant have a distinctive intention to supplement the ordinary resources of private medical observation in the direction already indicated. The work connects itself with the objects of preventive rather than with the objects of curative medicine, and in addition to investigations into the ætiology of infective diseases, it includes some very elaborate research concerning normal standards, histological and chemical, of the tissues involved in the morbid infective processes.

The latest published volume, entitled "Report made to the Lords of the Council on Scientific Investigations, made under their direction, in aid of Pathology and Medicine," contains the result of five researches:—(1 Dr. Sanderson's Further Report on the Intimate Pathology of Contagion; (2) Dr. Klein's Research into the Contagium of Variola Ovina; (3) Dr. Klein's Research into the Lymphatic System and its relation to Tubercle; (4) Dr. Creighton's Anatomical Research towards the Ætiology of Cancer; (5) Dr. Thudichum's Research into the Chemical Constitution of the Brain.

Dr. Sanderson's paper is a sequel to a former Report on the nature of infecting agents or contagia, in which Chauveau's opinion, expressed in the sentence "All contagia are probably particulate," was supported. The present paper treats of the infecting agents and morbid processes in diphtheria, erysipelas, splenic fever, and relapsing fever. In relation to all of these a mass of evidence collected from many observers is adduced to show that vegetable forms are connected with the contagions or with the morbid process. In splenic fever and relapsing fever organisms of a distinctive and specific form are declared to be present in the blood; bacterium-like rods accompanying splenic fever, minute organisms to which the name of *spirilla* has been given accompanying relapsing fever. In an "addendum" some observations of Dr. Letzerich, of Bramfels, Nassau, and of Dr. Oertel, of Munich, on the inoculation of animals with diphtheric poison are reported. From these it appears that in animals receiving the poison (derived from the throat of a child) by subcutaneous injection, the characteristic affection of the throat was developed after a few hours, and that the infiltration of tissues with the same sort of micrococci as are found infiltrating them in diphtheria always occurred.

Dr. Klein's first communication relates to the contagium of Variola Ovina, and describes certain small organised forms—bacteria, micrococci, and microsphæra gathered into colonies by long filaments—as found in the lymph from vesicles. The same forms are found in cavities formed in the rête Malpighii and subjacent corium, where the pock is developed after inoculation, extending afterwards into and occupying in vast numbers the lymphatics of the corium.

Dr. Klein's second communication treats of the Lymphatic System in relation to Tubercle. It commences with a minute and original description of the microscopical anatomy of the serous membranes, and their relation to the lymphatics, and compares with this the conditions in acute and chronic inflammation, noting in particular the processes leading to the formation of new blood-vessels and lymphatics both in healthy and diseased membranes. The second part of this communication relates to the lymphatics of the lungs in health, in certain chronic inflammations, and in tubercular infection. The appearances in the lungs of guinea-pigs after the production of artificial tuberculosis and in human lungs in tuberculosis are compared. Dr. Klein comes to the conclusion that the two processes are only to a limited extent similar (a conclusion opposed to the opinions of Sanderson and Wilson Fox). According to Dr. Klein, "in artificial tuberculosis of the lung of the guinea-pig the parts first attacked are the small branches of the pulmonary artery or pulmonary vein, whereas in acute miliary tuberculosis of man the capillary blood-vessels of the alveoli seem to be the tissue from which the action of the morbid agent starts.

Dr. Creighton's paper is a very thoughtful contribution to the present knowledge of cancer. It relates some unsuccessful attempts to propagate cancer by inoculation, and a number of careful observations as to the process of formation of secondary cancerous tumours. The attention is chiefly fixed upon the epithelium in relation to hyperplastic and heteroplastic (endoplastic) growth. Dr. Creighton infers from his observations that the efficient cause of secondary tumours in the liver is the substitution of the endoplastic for the normal (or excessive but still homo- though hyper-) plastic activity of the liver cells. The operation of deeper or extraneous causes is discussed, but left undecided. Hope is expressed that aids to a decision may be obtained from the results of a systematic examination of mammary tumour now proceeding.

Dr. Thudichum's research is a study of the normal chemical constitution of the brain, undertaken to prepare the way for a study of the brain in fevers, and other morbid states and processes. The paper is very long, occupying more than half of the 247 pages of the Report, and most elaborate. Dr. Thudichum believes that he has both added to and corrected former knowledge of the chemistry of the brain. In particular he describes with careful detail a number of newly observed principles, both phosphorised and nitrogenised. Among the phosphorised, kephalin and myelin (both of which contain nitrogen, as well as phosphorus) are new, and are associated with lecithin. They are described as typical colloids, of no true solubility, of almost indefinite power of soaking up water so as to form an imperfect solution, of feeble chemical activity, of a remarkable readiness to combine with acids salts and alkalies, and to part with them on the addition of excess of water. Kephalin and myelin are stable, lecithin so unstable as to elude proper analysis. Similarly the nitrogenised bodies, cerebrine (Müller's), kerasine, and phrenosine, are colloids, but of less perfectly marked type, and less interesting natural history.

In his summary Dr. Thudichum, speaking of the phosphorised bodies, remarks that "we have therefore here a diversity of affinities such as is not possessed by any other class of chemical compounds in nature at present known; and the exercise of these affinities being greatly influenced by the mass of reagent and the mass of water which may be present, the interchange of affinities may produce a perfectly incalculable number of states of the phosphorised and consequently of brain matter. This power of answering to any qualitative and quantitative influence by reciprocal quality or quantity we may term the state of *labile equilibrium;* it foreshadows on the chemical side the remarkable properties which nerve matter exhibits in regard of its vital functions."

The volume now under consideration has been preceded by two volumes, containing a first and second report by Dr. Klein, on the Lymphatic System and its relation to Tubercle, a report by Dr. Sanderson on the Infective Products of Inflammation, and by Dr. Thudichum on Chemical Changes in cases of Typhus. Reports are now in course of preparation by Dr. Baxter on Disinfectants, by Dr. Sanderson on the Febrile Process and on Infective Inflammations, by Dr. Thudichum on the Chemical Constitution of the Brain, by Dr. Creighton on Anatomical Studies with reference to Cancer, by Dr. Klein on the Contagium of Enteric Fever. The whole represents four years' work, for which 8,000*l*. has been voted. The value and importance of all this work in relation to the welfare of the community, as a contribution in aid of preventive medicine, cannot be doubted by any careful reader of the record. Nor, after even a superficial reading of the record can there be doubt but that the work is of a kind which can only be set going by such means as public grants, since it involves a special training and a special devotion inconsistent with the earning of livelihood by other direct or incidental means. The grant is on the evidence justified.

But there are other aspects of the work which claim a serious regard. The department of the Government concerned in protecting the country from the invasions of contagious disease, whether represented by Minister of Health or principal medical officer, needs in all things to be fully informed of the latest discoveries in pathology, hygiene, and therapeutics. Of such minister or officer the body of scientific men whose work is here recorded, together with others who are engaged in sanitary investigations and inspections under the central authority—men like Drs. Seaton and Buchanan and Mr. J. N. Radcliffe—constitute a body of advisers or council representing the most advanced knowledge bearing upon the public health. They constitute a council to which the minister or officer may refer for latest knowledge when legislation is concerned, or for practical advice when action has to be taken. They are, in fact, at this moment practically such a council. In the Science Commission Report on the Advancement of Science, the formation of a similar council as adviser of a Minister of Science is advocated. We would suggest that we have in what we have stated an excellent illustration of the principle proposed, with a wider application, in the Science Commission Report.

THE INFLUENCE OF THE PRESSURE OF THE ATMOSPHERE ON HUMAN LIFE

Influence de la pression de l'air sur la vie de l'homme. Par D. Jourdanet. 2 vols. (Paris: Masson, 1874.)

AFTER having practised medicine for six years on the borders of the Gulf of Mexico, and rendered himself familiar with the diseases and conditions of life of the inhabitants of low levels, M. Jourdanet removed to the elevated plateau of Anahuac—more than 2,000 metres above the sea level. Here, as might have been anticipated, he found the pathological conditions different, but to his surprise he discovered that the differences were not simply such as result from temperature, or are paralleled in places of lower level and higher latitude, but presented peculiarities which he conceived to be dependent on the elevation of the situation alone. A residence of twenty years in the locality enabled him to confirm this idea and to prove that, while the blood of the inhabitants presented no poverty of corpuscles, the corpuscles themselves were deficient in oxygen, on account, as he believed, of the too feeble pressure of the atmosphere in these high regions. This led him to undertake the study of the whole question of the influence of the atmospheric pressure on health, and to call to his aid M. Paul Bert, Professor of Physiology at the Sorbonne, by means of whose experiments he believes himself to have arrived at some definite results. These, with every other possible point of interest connected with the subject, he now presents us with, in two large and beautifully illustrated volumes; leaving, however, the details of the physiological experiments to be published in a forthcoming work by M. Bert himself.

The question so fully discussed by M. Jourdanet is certainly of very great interest, and, in spite of previous observations and opinions on the therapeutic action of compressed air and on the possible limits of life in regard to height and other similar points, it is also of some novelty as treated by him.

According to M. Jourdanet the pressure of the atmosphere has not always been as small as it is now; and assuming, what is probably true, that a greater pressure would involve greater heat, he would account in this way for the warm periods known to have existed in Tertiary times. This leads him to make an hypothesis as regards the cause of the glacial epoch, the occurrence of which would be contrary to the above theory; but it is not an hypothesis that could recommend itself to geologists. The glacial epoch arose, he imagines, in this way: by some sudden convulsions the crust of the earth was torn open, and prodigious quantities of gas and vapour driven up, which forced up the atmosphere to a prodigious height, where it was chilled and its vapour condensed, which fell in diluvial torrents, leaving the air so free from vapour that radiation took place at an enormous rate, chilling the earth and causing the glacial epoch! He also concludes that on account of the too great density of air in the plain, man must have made his first appearance on elevated plateaux, and he accounts in this way for the veneration of high places among the early races. These and similar speculations, though they may sometimes amuse, do not detract from the real merit of the work in more determinable matters.

We reach the substantial part when we come to the experiments of M. Bert, of which the results are here given. Small animals were placed in chambers of various capacities, which were then filled by the same absolute quantity of air, necessarily at various pressures; when the animals were dead, the remaining air was analysed, and it was found that in the larger vessels the proportion of oxygen was greater, and this proportion was such that the total amount of oxygen left was proportional to the capacity of the chamber. The animals died as soon as the oxygen by itself was reduced to a density of 4 per cent. of what it would be if the whole chamber were filled with it at the normal pressure—the amount being thus independent of the quantities of the other gases present. This being true for any sized chamber, it follows we may suppose the chamber indefinitely large; and an animal would die in the open air if the oxygen should have less pressure by itself than 4 per cent. of 76 millimetres. Taking the air to have its ordinary 21 per cent. of oxygen, these experiments would appear to prove that life is impossible in air of less pressure than 14·5 mm. The proportion of oxygen, however, seems to be much less than that which is ordinarily supposed to be small enough to produce asphyxia. Further experiments were performed, pointing to the same result. Dogs were so fastened that they could breathe only from a bag of limited size, and from time to time the air in the bag and the blood of the dog were analysed, and it was found that the oxygen in both decreased *simultaneously*, though not at the same rate. These and similar experiments, together with the fact ascertained by M. Jourdanet, that the blood of Mexican dogs contains a less proportion than usual of oxygen, are the proofs offered that the blood cannot be sufficiently oxygenised for health without a certain amount of atmospheric pressure.

In all these experiments, however, no allowance is made for the possibility of the human lungs accommodating themselves in time to the smaller pressure, so as to enable the blood to take up a sufficiency of oxygen; and this objection is seen by M. Jourdanet, who, after giving an interesting account of the various evils that have befallen noted climbers, discusses the question whether an increased number of respirations, or an enlargement of the thorax, could counteract the effect of the rarity of the air. As to the first, numerous experiments on himself during his residence in Mexico have enabled him to verify the law given by Lehmann, that the carbonic acid expired is in part constant and in part only variable with the number and magnitude of the respirations; and he calculates from hence that, in order to counteract the loss of pressure and diminution of oxygen by increased respiration, it would require twenty-four ordinary respirations per minute, which of course the Mexicans do not make. As to the size of the thorax, which has been stated by Forbes to be larger in the inhabitants of these high regions, he objects that this statement was made on too restricted data, and that Coindet has found that it does not generally hold true. Whatever may be the truth on this point, the explanation which M. Jourdanet offers of the result of the low pressure on the temperature of the body cannot be considered satisfactory. He considers that, as a general rule, the temperature of Mexicans is not below the average, although their surface temperature often is, and that the loss of heat which would arise from the more easy radiation and the lower oxygenisation of their blood is prevented by "the repose of their functions," while their respiratory organs are specially modified so as to be capable of "exceptional exercise." The latter would require proof, and as to the former, although the body must lose temperature by the exact amount of work done on external objects, "a care to avoid every effort" would prevent the body doing work upon *itself*, and less heat would therefore be produced. The "apathy" of the Mexicans and other inhabitants of high levels must have another cause than this.

M. Jourdanet's work ranges over a wide field, discussing, without much plan, various points in connection with the climate of plateaux. Thus, in one chapter he attempts to prove, by statistics of population, that the low pestilential area round the Gulf of Mexico is more healthy than the elevated table-land, the former having increased six per thousand and the latter three per thousand in forty-seven years; that the decadence of the Peruvian race is due to the influence of the atmosphere, without apparently seeing the obvious objection that they must have *risen* under the same influence, since they are a very ancient race; that the mental and physical work of the Mexicans is below that of the inhabitants of the plains; and then he discusses the extreme height at which it is possible to live permanently, which he places between 4,000 and 5,000 metres. This variety makes the book very readable, but, in spite of its large size, the arguments on many points are too brief to be convincing.

The second volume is engaged in discussing the influence of atmospheric pressure on disease, M. Jourdanet being "convinced that the true nature of exterior influences is far better seen in the maladies caused by them than in the health which they favour." This portion of the work has a principally medical interest, although some of the results of his experience may be usefully mentioned. He finds that consumption is rare in Mexico, and is principally confined to the poorer classes who have insufficient nourishment, which he explains by their feebly oxygenised blood being unfavourable to the development of the disease;—typhus fever, on the contrary, finds there its most suitable subjects, as do other inflammatory disorders, while yellow and intermittent fevers are almost unknown.

The elevation of the country where these observations were made, and concerning which M. Jourdanet's conclusions have been arrived at, is 2,000 metres and over; and the climate of these places he speaks of as "climats d'altitude;" while intermediate heights he characterises as "climats de montagne;" to which latter he also devotes a few chapters. These, however, are of less interest as not embodying the results of his personal observations, but being a discussion of the various well-known moral, mental, and physical characteristics of mountaineers. To these follow chapters on the influence of mountain travelling on health, and detailed experiences of the application of artificial rarefaction of the air in disease.

With the desire of making the work as complete on the subject as possible, the author has compiled a large part of it from well-known writers, and recapitulates much that is of everyday observation; and these parts have naturally less interest than those which deal directly with his Mexican experiences. The whole of the facts, however, which bear upon the question discussed are conveniently collected together and put in an interesting form for the perusal of the general reader, for whom, however, much of it has too medical an aspect.

OUR BOOK SHELF

The Royal Tiger of Bengal: his Life and Death. By J. Fayrer, M.D. (London: J. and A. Churchill.)

IN this small work Dr. Fayrer gives a popular description of the zoological relationships, anatomical structure, geographical distribution and habits of the tiger. Accounts are also introduced of tiger-hunts, which well exemplify the dangers to be feared and the precautions to be taken in the pursuit of that large game, which even under the most favourable circumstances cannot be followed without a great amount of risk. The author's considerable Indian experience gives great weight to his opinions on many of these points, especially with reference to the nature of the wounds inflicted by the enraged creature.

Anatomically Dr. Fayrer brings to our notice a point in the disposition of the claw-bearing or ungual phalanges of the digits in the cat-tribe, which is not without interest. In the fore-limbs, as is well known, these bones, when the claws are fully retracted, bend extremely backwards in order to allow of the claws themselves being protected during progression. To so great an extent is this retraction carried, according to Prof. Owen, that the bone passes back to the side of the second phalanx in the same way that the blade of a clasp-knife may be said to do the same with reference to each lateral portion of the handle. In the hind limb of the tiger, Prof. Owen remarks that they are retracted in a different direction, "viz., directly upon, not by the sides of the second phalanges, and the elastic ligaments are differently disposed." Dr. Fayrer finds that in the smaller Felidæ, as the Ocelot, the hind claws are constructed and retracted on exactly the same principle as the fore. Such being the case, either the tiger differs from its smaller congeners, or Prof. Owen is wrong. Till Dr. Fayrer proves the latter, we prefer to assume that the former is the case.

"Contrary to custom, I propose to give him (the tiger) precedence of the lion. He is generally described as inferior, though nearly equal, to the so-called king of beasts; but in size, strength, activity, and beauty he really surpasses him; and therefore, though he may neither be so courageous nor so dignified, he is entitled to the first place—at all events in India." Thus says our author, and many of his descriptions fully exemplify all the animal's best points. Nevertheless, though he may be slightly greater in length, and is perhaps more active, we considerably doubt his greater strength, and as the work before us fully proves, we cannot say of him as a recent writer tells us of the lion, that "it should always be recollected, before meddling with lions, that if you do come to close quarters with them death is the probable result," the tiger having a much less dignified habit, an example or two of which we quote with reference to a case in the Madras Presidency, where a sportsman wounded the creature more than once. "It charged and seized him by the loins on one side, gave him a fierce shake or two, dropped him, and then seized him on the other side, repeated the shaking and again dropping, left him and disappeared." In a second instance a military man, "a most distinguished soldier and sportsman, when following a wounded tiger on foot in the long grass, was suddenly seized and carried off by the animal he was seeking. He managed, however, to effect his escape without having received any serious injury, and rejoined his companions, who had deemed him lost."

When so acute an observer as the late Mr. Edward Blyth, with his great experience, expresses uncertainty as to whether the lion or the tiger is the larger animal, we may be certain that there is no great difference either way. Dr. Fayrer tells us, "I have been informed by Indian sportsmen of reliability, that they have seen and killed tigers over twelve feet in length." In none of the special instances he mentions, in which careful measurements were made, did the length exceed ten feet by more than an inch. We quite coincide with the author in looking with doubt on Buffon's statement that one has attained the length of fifteen feet.

For further information on the above and kindred points with reference to the Royal Tiger of Bengal, we cannot do better than recommend the reader to glance through the small work under review.

An Introduction to Animal Physiology. By E. Tulley Newton, F.G.S. (Mumby's "Science and Art Department" series of Text Books.)

IN more than one of the Science Primers which we have lately had occasion to look through and notice, it has been painfully apparent that the author is not nearly so well grounded in the subject he is endeavouring to teach as even some of his probable pupils. Some write on human physiology without having studied human anatomy; others even do not know their physiology. The author of the work before us is not one of these. It is accurate, and therefore reliable. The descriptions are precise and clear. The limits of space within which the author is confined have, in some of his descriptions, made it necessary for him to sacrifice clearness to a certain extent, but this cannot be avoided. A novel feature of the work is the addition to each chapter of a practical section, in which directions are fully given for study, by the student himself, of the more simple physiological and anatomical points referred to. These directions are particularly clear, and if carefully worked out by everyone who reads the book, will be found to lead to a sound knowledge of the first principles of physiological science. The illustrations, which are numerous, though mostly to be found elsewhere, are well selected, and sufficiently large to be distinct.

Abstracts and Results of Magnetical and Meteorological Observations at the Magnetic Observatory, Toronto, Canada, from 1841 *to* 1871. (Toronto, 1875).

IN this thick pamphlet of 249 pages, Professor Kingston gives the results of an elaborate, able, and discriminative discussion of the magnetical and meteorological observations made at Toronto during the thirty-one years ending with 1871, in a series of fifty-one tables. To these are appended the daily observations from January 1863 to December 1871. While all the results of the observations, devised and carried out with so much care, and extending over so long a period, are of very great value, we would point to the wind observations as regards the diurnal changes, but particularly in their relations to differences of temperature, pressure, humidity, and cloud, and to light, moderate, and heavy falls of rain and snow respectively, as affording, from the fulness and originality with which they are discussed, much valuable information on many intricate points which it would be difficult if not impossible to find elsewhere. The influence of Lake Ontario is seen in the diurnal changes of the wind, which in July is nearly S. from 10 A.M. to 3 P.M., W. at 5 P.M., nearly N. at midnight, about which it remains till 9 A.M., when it rapidly shifts to S.W., and ultimately to S. at 10 A.M. From October to March, when storms are most frequent, the greatest depression of the barometer and increase of vapour occur with winds from N.E. to S.S.E., and the greatest rise of the barometer and diminution of vapour with winds from W. to N.N.W. On the other hand, in summer the greatest depression of the barometer occurs with winds from E.N.E. to E.S.E., but the greatest increase of vapour with winds from E.S.E. to S.S.W. Most of the light falls of rain occur with winds from N.E. by S. to W., and of snow with winds from S.W. by N. to N.E.; most of the moderate falls of rain with winds from N.E. to S.S.W., and of snow with winds from N.N.W. to S.E.; and most of the heavy falls of rain with winds from N.E. to S.S.E., and of snow from N. to E.S.E. The important bearing of these facts on the question of North American storms as well as on the climate of no inconsiderable portion of that continent is evident. Tables II. and XX. giving by interpolation-formulæ the mean temperatures and mean pressures of different days of the year, while of very slight scientific value, may be found to be useful in a meteorological office, but a simpler and in everyway more preferable table of normal daily values for pressure and temperature could be constructed from the arithmetic means of the thirty-one years' observations treated by Bloxam's method of averages.

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.*]

"Tone" and "Overtone"

IN the very favourable estimate of the work I have done in my translation of Helmholtz, in your number for Sept. 23, I am aken rather severely to task for my use of "Sensations of *Tone*" on my title-page, and my refusal to use the expression *overtones* in the body of the work. The title was long a matter of anxious consideration to me, and I have not yet seen my way to improving it. True, practical musicians, physiologists, and artists have each their own, very different, technical meanings for *tone*. The two last generally use it without an article, and in the singular; but musicians are accustomed to speak of "*a* tone," or of *several* tones, when they allude to musical intervals. In common speech, however, all three agree with the outside world in speaking of a "loud and soft, gentle and angry *tone* of voice," of a "fine-*toned* instrument," of the "splendid or miserable *tone* produced by a violinist," of the "magnificent *tones* of the organ." That is, we are all accustomed to use *tone*, as I have done on my title, for "a musical quality of sound." I know no other single word in English which expresses the same conception. In the original German, Prof. Helmholtz (and after him Prof. Tyndall) endeavours to use *tone* for a "simple tone" only. Neither have contrived to be consistent in so doing. I have had to correct the text several times in my translation on this very point, and instead of using *tone* for "simple tone" only, which is a new conception, and *clang* (in English, a *din*) for "compound musical tone," which is also a new and not an easy conception, I have invariably used the word *tone* (except when distinguished by a capital letter—thus, *Tone*, for the interval) in the usual general sense of the word, and distinguished the particular cases by the prefix "simple" or "compound." It seems to me that this is not so much "a little waywardness" on my part, as a desire for scientific accuracy.

As to "overtones," it is well known to those who, like my reviewer, are acquainted with the work in the original, that Helmholtz's expression "Obertöne" is a mere contraction for "Obertheiltöne" or "Oberparzialtöne," both of which terms he not unfrequently uses, and these are literally rendered by my "upper partial tones." Waiving my strong linguistic objection to the term "overtones" as an English word, my scientific justification for not using it in my translation must be sought for in the fact that even the German "Obertöne" has led Prof. Helmholtz himself not unfrequently to its inaccurate use for "partial tones" simply, including the lowest partial tone, which the word was especially invented to exclude. Singularly enough, even my reviewer has many times fallen into the same error (NATURE, p. 451, col. 2) in speaking of the "overtones" of a pianoforte string. Thus he says, "the first six overtones are all audible," which is not correct; but he means "the *lowest* partial tone and first *five* of the *upper* partial tones," or briefly "the first six partial tones," which *is* correct. Again, he says, "the seventh and ninth (overtone) which are *inharmonious*, &c., which is not correct, for the *seventh* and *ninth over*tones are the *eighth* and *tenth* partial tones, and are perfectly harmonious; but he meant the seventh and ninth partial tones. Again, he cites from p. 126 of my translation, the relative force of the first six "partial tones," as they are there called, but refers the table to the first six "*over*tones," which is altogether incorrect. Now if such men as Helmholtz, who invented the term, and as my reviewer, who uses it familiarly, can be led by it into what with them are mere inaccuracies of *expression*, must we not look to the utmost confusion of *thought* among persons to whom the whole subject is new, and who employ the term with a very vague or loose conception of its meaning? In point of fact, many such cases have come to my notice. Hence, again, I cannot agree to think that my deliberate rejection of the word "overtones" is "the chief fault" or "a blot on the translation," but rather submit that it is a consistent endeavour to attain scientific accuracy of expression, and avoid confusion of thought.

I thank the reviewer for his generally favourable estimate, gladly accepting his rectification of the accidental Germanism "the musically beautiful" for "the beautiful in music," and I apologise for the length of this communication on the ground that it is not a merely personal vindication,

Sept. 25 ALEXANDER J. ELLIS

Colours of Heated Metals

I HAVE just watched the casting in gun-metal, in an engineering establishment in this town, of what is intended to be the rudder-post of a large vessel, which when completed will weigh about three tons. As the casting was a simple one, it was accomplished very quickly, and as the contents of the huge four-ton ladle were emptied into the mould, the dazzling stream of the metal flowed in a large volume over its lip. Brilliantly glossy it appeared as it broke through the folds of thin dross with which its surface was encrusted; and this it did at the lip of the vessel, while fold after fold of the encrusting pellicle was swept down the stream, and left behind it a straight or ragged edge of the thin film, from underneath which the metal welled out for a moment with an appearance on the surface of perfectly transparent purity. The appearance was a deception arising from the strong bluish-green colour of the light emitted by the pure surface of the metal, which I have never seen exhibited under similar circumstances by melted iron or steel. It extended also for only a short distance from the encrusting edge, the green colour soon passing into white, or paler green, where exposure to the air enveloped the metal again in a rapidly increasing film of oxides that tarnish its surface and render the stream white, or nearly so, in every part, excepting in a bluish-green ring, or border where the fresh metal made its appearance, and flowed over in a beautifully coloured stream from the mouth of the ladle. The strongest patches of the colour there were transient, the film of oxide apparently soon thickening enough to eclipse it, and by connecting itself to the broken edge of the thin film in the pot to tear away another fold, when the characteristic greenish glow of the metal immediately presented itself along the freshly-broken edge. I had watched and thus interpreted this beautifully varied play of natural colours in the molten stream for some time before it occurred to me that the peculiar hue of the freshly-exposed surface of the metal, glowing as it does with the brightness of what in the black film of oxide appears as white heat, is no other than the very colour of the heated metal which the theory of exchanges would lead us to expect. For as the colour of gun-metal in a cold state is yellow, the selective absorption of its surface in that condition must be exercised chiefly upon rays occupying the blue portion of the spectrum, and consequently in the heated state these rays are emitted in excess; or if the heat is sufficiently intense to produce them largely, as in the melted metal, where the thin films of oxide on its surface glow with perfect whiteness, the metal itself must shine with bluish, or it may be with geenish-blue light, if the heat is only high enough to make the excess of green rays very strongly visible. If this should be, as I suppose, the real explanation of the very curious appearance of depth of a certain tint of colour, contrasting strongly in some parts of the melted stream by its greenish hue with the surrounding redder lights, according as the natural tinted appearance of the vivid metal is effaced or diluted by the floating films of white-hot oxides in lines and parts of the stream depending on the surface-flow, and suggesting in some degree the idea of a transparent cascade, and even from its colour of a waterfall, the process often repeated in large foundries of running gun-metal into large castings presents an instance of well-defined action of the law of exchanges which must be constantly witnessed and noted inquiringly by daily observers, and which certainly presents, if a different and more natural explanation can be given of its origin, to eyes unaccustomed and unprepared to receive it, a somewhat surprising and otherwise unaccountable appearance. In gun-metal, when the proportion of zinc introduced is very small, the coating of the melted surface by copper oxide is comparatively slow, and in melted brass it might not be possible, from the rapid oxidation of zinc upon the surface, successfully to observe the same phenomenon. In order to render melted copper fluid enough for casting, a small proportion of alloy sufficient to give it almost the colour of brass is required to be mixed with it, and large pourings of the pure metal cannot commonly be made; but perhaps in small castings of this metal, and probably also in those of gold, opportunities would present themselves similar to that which I have here attempted to describe, of verifying the same general law of radiation connecting together the qualities of luminosity and absorption in the surfaces of highly coloured metals.

Newcastle-on-Tyne, Sept. 20 A. S. HERSCHEL

Changes of Level in the Island of Savaii

WHILE feeling some diffidence about setting myself in opposition to so careful an observer as the Rev. S. J. Whitmee (NATURE vol. xii., p. 291), I cannot allow his statements in regard to changes of level in the island of Savaii, Samoan group, to pass altogether unchallenged. In the month of June 1874 I spent some weeks on the island, during which time I travelled around nearly the whole of it on foot. Though not a scientific observer, I was on the look-out for indications of change of level along the coast, and it is my decided opinion that such indications are quite as little apparent in Savaii as in Upolu. Mr. Whitmee, whom I had the pleasure of meeting on the island, directed my attention to what he believed to be a line of upheaved cliffs a couple of hundred yards back from the sea, near Tufu, on the south side of the island. On examining the place, after parting from Mr. Whitmee, I particularly observed that the floor of volcanic rock at the base of the cliffs bore exactly the appearance of lava that had cooled in the open air. The creases and ripples left on the surface of the lava in cooling were distinctly visible, which could not have been the case if the rock had ever been exposed to the action of the waves. No doubt was left on my mind that the floor of volcanic rock between the base of the cliffs and the sea was at one time on a level with the top of the cliffs, and that it had broken away and sunk several feet, from some cause which I do not attempt to explain.

I brought away the impression that Savaii was at one time much more fully supplied with barrier reefs than at present, and that recent lava-flows had extended the island out beyond the reef. So far as my observations extended, where reefs do exist they are terminated by points or capes of volcanic rock, looking as if the lava had overflowed and cut off the reef.

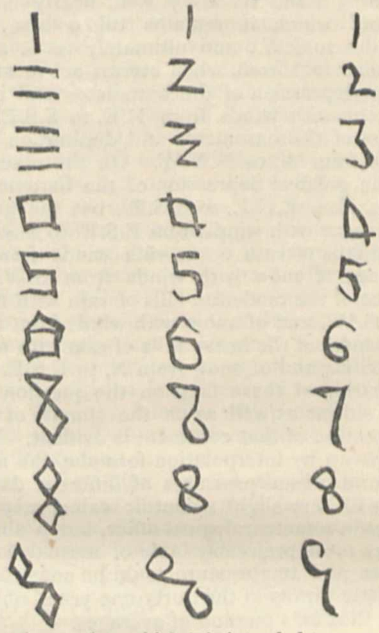
One circumstance almost, if not quite, fatal to the theory that Savaii has been upheaved in whole or in part in recent times, is that nowhere are there any signs of coral *in situ* above the sea-level. In this respect it is very different from the island of Rarotonga, in the Hervey group, which has most unquestionably been upheaved several feet, at least on the south side. There the barrier reef is altogether out of water, and what was once the enclosed lagoon is in some places dry land.

In regard to the absence of barrier reefs in front of lava-flows, I venture to suggest that it is more likely to be caused by the depth of the water or by the recency of the lava-flow than by any effect of existing submarine volcanic action on the coral insect.

San Francisco, Sept. 7 RICHARD WEBB

Origin of the Numerals

HAVING never met with any explanation of the origin of the numerals, or rather of the figures symbolising them, perhaps I am right in supposing that nothing satisfactory is known of it. In that case the following may be interesting to your readers. The first column contains the original figures, each containing as



many lines as the number which it is intended to represent. The other columns show the transitions likely to result from quick writing.

W. DONISTHORPE

17, Porchester Terrace, W.

Pugnacity of Rabbits and Hares

I HAVE occasion just now to keep over thirty Himalayan rabbits in an outhouse. A short time ago it was observed that some of these rabbits had been attacked and slightly bitten by rats. Next day the person who feeds the rabbits observed, upon entering the outhouse, that nearly all the inmates were congregated in one corner, and upon going to ascertain the cause, found one rat dead and another so much injured that it could scarcely run. Both rats were of an unusually large size, and their bodies were much mangled by the rabbits' teeth.

I never before knew that domestic rabbits would fight with any carnivorous antagonist. That wild rabbits never do so I infer from having several times seen ferrets turn out, from the most crowded burrow in a warren, young stoats and weasels not more than four inches long.

It is evident that the show-fight instinct cannot have been developed in Himalayan rabbits by means of natural selection, but it is no less evident that if it ever arose in wild rabbits it would be preserved and intensified by such means. And in this connection I should like to ask any of your readers who may be able to supply information upon the point, whether there is any difference between the hares of Great Britain and those of the Continent with regard to pugnacity. I have been assured by Germans that in their country a hare will fight a good-sized dog rather than run, and that it is dangerous to handle a wounded individual. I do not know, however, whether or not to trust these statements, and as there appear to be very few examples of local varieties of instincts, it is desirable that anyone who can should either confirm or deny this curious instance.

Dunskaith, Ross-shire GEORGE J. ROMANES

OUR ASTRONOMICAL COLUMN

"35 CAMELOPARDI," B.A.C. 1924.—The principal component of this double star is not included either amongst the certain or suspected variables in Professor Schönfeld's last catalogue, but there would appear to be sufficient evidence of change to justify its being placed in the former class. Variability was suspected by the Baron Dembowski from his own estimates of magnitude 1865-

68 (A.N. 1810), and the following are almost decisive of fluctuation through about two magnitudes, so that at times the star will be visible to the naked eye, and at others fairly beyond unassisted vision.

As lower estimates we have Argelander 1842 January 25—8 mag., and Radcliffe Obs. 1870 February 22—7·5 mag.

As higher estimates we find, Flamsteed, 1696 January—5½, Lalande (in Fedorenko's Catalogue), 1790 February—5·6, Dembowski, 1868 February 2—5·5, and Radcliffe Obs., 1872 March 9—6·0.

It does not occur in the *Uranometria*, but is B.A.C. 1924, and there very properly removed from Camelopardus, to which it could only have been originally assigned by a mistake. It belongs to Auriga, though it is hardly, as the Bedford *Cycle* tells us, "in the Waggoner's eye."

THE DOUBLE STAR Σ 2120.—M. Camille Flammarion sends us some remarks on this object, to which allusion was made in NATURE, vol. xi. p. 147. Identifying it with No. 89 of Sir W. Herschel's Class III., M. Flammarion thinks the early observation tends to establish the binary character of the star, notwithstanding the measures from 1829 to 1873 may be represented by rectilinear motion. We shall revert to this subject next week.

THE MINOR PLANETS.—The elements of No. 148 have been calculated by M. Bossert and Herr V. Knorre; the orbit is one of the most inclined to the ecliptic (26°).—No. 136, Austria, was recovered at the Observatory of Berlin on the 6th of the present month. Dike and Camilla, with one or two others, are still adrift.

THE AUGUST METEORS.—As previously stated, the systematic course of observation of the meteors of the August period, organised by the French Scientific Association, has this year been attended with considerable success, the atmospheric conditions on the nights of the 9th, 10th, and 11th having been as favourable as possible at many of the stations. The greatest number was observed during the night between the 10th and 11th, but this number varies much in the different accounts so far published by M. Leverrier. The Lisbon observers would appear to have recorded the greatest number, 1,227 meteors having been noted between 10h. and 15h. 25m., when the sky clouded. A table of more than forty tracks, exactly noted, appears in the Paris "Bulletin International" of Sept. 23, the co-ordinates of the points of commencement and extinction being expressed in right ascension and declination, with the corresponding mean times. At Avignon, on the same night, 858 meteors were recorded between 8h. 35m. and 15h. 40m. At Bordeaux M. Lespiault remarked that four-fifths of the meteors seen were Perseids, generally very small, though in a few cases they had considerable brightness and left trains. At Dijon, on a mean of the three nights' observations, the radiant was fixed approximately in R.A. 37°, and polar distance 45°, and in addition to this point, two secondary radiants were detected, one in R.A. 320°·4, N.P.D. 91°·8, and the other in R.A. 331°·0, and N.P.D. 90°·0. With respect to these it is remarked that although, by the means, these coordinates appeared to be confused together, yet for each night the points of radiation were very distinct, the meteors of the first group appearing to be directed towards the second radiant, and those of the second group towards the first. At Rouen, 500 meteor-tracks were entered upon the charts, the invariable direction being from Perseus. At the Observatory of Palermo, Prof. Tacchini and M. Delisa made numerous determinations of the position of the radiant from August 9-12 inclusive, the mean of the whole being in R.A. 2h. 50m.9, N.P.D. 36° 51′, but when the points are laid down on a chart it is seen that they are comprised in a very narrow ellipse, a circumstance to which Prof. Tacchini has already drawn attention.

M. Wolf, in reporting the results of this year's observations, considers that the phenomenon advances rapidly towards a very brilliant maximum; the next year will enable us to judge if this maximum has been attained, and it may then be possible, he thinks, to determine the period of revolution of a swarm of meteors, which, though now extended far along the orbit, still presents a very marked region of condensation. On the contrary, M. Wolf observes, the November shower has so nearly ceased, passing now almost unperceived, that it may be unnecessary to call upon observers, who have previously co-operated in this class of observations, to expose themselves again to the possible severity of the nights at that season.

THE CLINICAL LABORATORIES ANNEXED TO THE PARIS HOSPITALS

THE first and typical clinical laboratory was created at the Hôtel-Dieu, by private exertions, a very few months after the time when blood had been running so freely on the pavement of the great city. It was organised at the expense of two doctors, who had shared the disappointments and dangers of those troubled times.

Dr. Liouville, a nephew of the celebrated academician who edited for so many years the *Annals of Mathematics*, having learned by his travels, before the Franco-German war, that Prussia and other German powers had established special laboratories at Berlin and other large cities for promoting physiological researches in the Universities, resolved to introduce establishments of that description in his native land, but under a different system. He laid his ideas before Dr. Behier, one of the most popular professors of the Faculty who adhered to the scheme, and lent all his influence and patronage to bring physical and chemical instruments to the very bedside of the patients at the hospitals.

The intention of these two distinguished physicians was not only to open an institution where physiological science might be promoted as it is at Berlin and Vienna, but to place under the hands of practitioners ready means for enlarging the degree of accuracy of their diagnoses. At a moment's notice an able microscopist armed with a powerful instrument is to answer any question put for ascertaining the composition of humours, the nature of abnormal secretions, &c. A competent chemist, well acquainted with the properties of reagents, is ready to make an analysis of blood, of virus, of medicaments, of urine, of *excreta*, suspected poisonous matters, &c. The use of the spectroscope was not so general at the time as to call for the service of a spectroscopist, but the utility of the speciality even then was made apparent to MM. Behier and Liouville.

These operations can be done daily for the instruction of the students following the daily practice of the hospital.

When the patient dies, his autopsy being carefully made, it can be shown whether the diagnosis was true, or whether the fatal result was due to some uncontrollable circumstance. The unhappy inmate whom science and humanity were powerless to save, is turned into an object of instruction, so that human knowledge may be enlarged and other sufferers cured under similar circumstances. The laboratory was also open from the time of its infancy to foreign men of science or to practitioners wishing to investigate any points connected with their patient.

To the Hôtel-Dieu Laboratory was annexed a "chenil," where a number of rabbits and the like are constantly bred and kept in an excellent state of health. These animals are destined to be employed in testing the efficacy of new medicines to be tried, if proved innocuous, on the patients. In cases of poisoning, the localisation of toxic substances is ascertained, as well as the symptoms of death, and in some cases antidotes are administered for testing their restorative power. They may be considered as living instruments for exploring and extending scientifically the scope of *Pharmacology*.

The results obtained by the two learned associates were so rapid and so unquestionable, that in 1872 their laboratory at the Hôtel-Dieu was declared to be an establishment of public utility.

A few weeks afterwards the Commissioner of the Budget of the National Assembly having paid a visit to the Hôtel-Dieu, inserted in his report a clause asking support for the then existing establishment, at the expense of the Government, and the extension of the system to other Paris hospitals. A sum of 32,000 francs was voted without opposition, and three laboratories were opened, one at La Pitié, the second at the Charité, and the third at the Clinical Hospital. The reports of the *Commission de Budget* were succesively presented by M. Beulé, the ex-Minister of the Interior, and, after he had met his untimely death, by the present sub-Minister of Justice, M. Bardoux, who both of them asked for *frais de premier établissement.* A sum of 90,000 francs was voted, partly by the Versailles National Assembly and partly by the Municipal Council of Paris.

Dr. Liouville was appointed the chief of the Hôtel-Dieu Laboratory; Dr. Carnhill, an anatomist universally known by his researches on the diseases of the liver, was appointed the chief of the *La Charité* Laboratory.

In one of the first sittings of the last session the Municipal Council decided that a large pavilion on the northern part of the New Hôtel-Dieu, now building, should be reserved for the clinical laboratory. No money is to be spared in order to procure the most important instruments which can be designed for chemical or medico-physical observations, either in the way of galvanic batteries, microscopes, spectroscopes, &c. A clinical laboratory will also be established in the new hospital to be inaugurated at the end of next November, which will be one of the most extensive in Paris.

NOTE ON HÆMATITE INDIAN AXES FROM WEST VIRGINIA, U.S.A.

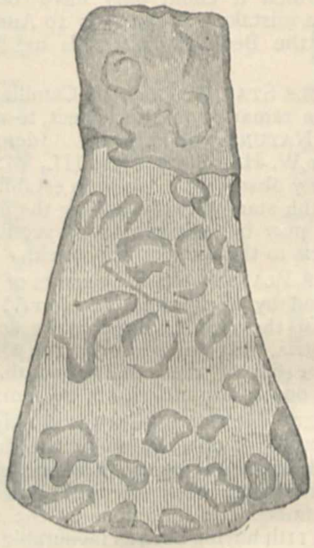
THROUGH the kindness of Horace Fisk, of Trenton, and Major Jed. Hotchkiss, of Staunton, Va., I have been able to procure two specimens of hæmatite iron ore hatchets, of aboriginal manufacture. They possess great interest from the fact of being very similar to native copper axes, characteristic of the "finds" of relics of "mound builders." The specimens, one of which is here figured, have unquestionably been hammered out cold, and shaped from a fragment of the ore, without the aid of fire in previously refining the mass. The specimen figured measures five inches and a quarter in length, by three inches in breadth at the cutting end. The opposite end is square, nearly two inches in width, and somewhat thinner than the broader portion of the implement, which is nowhere of greater thickness than one-fourth of an inch.

The entire surface still shows the hammer marks made in shaping the hatchet, even to the edge, which now shows no trace of grinding or polish; but this may have been obliterated by the rust; but I am inclined to believe from close inspection of both specimens, that the edge originally was a hammered one, and not a ground one; making the specimen more nearly allied to the "clipped" jasper hatchets than polished (ground) porphyry axes.

The accompanying specimen is four-and-a-half inches in length, by two in breadth, is nearly uniform in thickness about three-sixteenths of an inch, and has a well-defined edge, which from its slightly wavy outline, and slight variation in width, I believe to be a hammered, and not a ground or polished edge.

Two other specimens, similar to these, were found with them, and are now in the calimat of Major Hotchkiss, who informs me that the series of four were found under an uprooted tree, on an Indian trail, at the Forks of Kelley's and Rich Creek, Gauley Mt., Tayette Co., West Va.

It has been suggested that the use of hæmatite for paint among our Indians may have led to its employment for other purposes ("Flint Chips," by E. T. Stevens, p. 553), and this is no doubt true, inasmuch as small irregular fragments of this mineral were often utilised, if the shape would at all permit, as arrow heads. Among the thousands of arrow-heads gathered in New Jersey, I have not met with one of iron ore that has been worked into any of the various patterns of flint points; but from graves, associated with others, I have found fragments of the ore, and once, of native copper, of such shape and size, and so placed, that they were evidently arrow-heads.



A curious form of "relic," known here as a "plummet," occasionally occurs, made of iron-ore. One such is figured in the "American Naturalist," vol. vi., p. 643, Fig. 132. This specimen "is made of iron ore, ground down and polished until it is almost as smooth as glass." As such plummets are found in the western mounds, as well as on the surface of the ground throughout the Atlantic coast States, and are always polished, it seems fair to presume that a cutting instrument of such hard material would undoubtedly be polished and ground, if, at the time of its manufacture, grinding was known or practised among the aborigines in fashioning their various weapons and instruments.

When we consider that these iron hatchets were found in a locality once thickly populated by Indians, and probably frequently visited, if not occupied, by the mound-builders, and now yield, on search, an abundance of ordinary stone implements of every grade of workmanship and variety of pattern, it seems at least probable that the specimens in question were not fashioned at a time when the polishing and grinding of weapons was customary, but earlier, as the labour of beating so hard a material into its present shape would doubtless be supplemented by polishing, if the additional value given to an implement by the operation had been recognised.

As the writer has already endeavoured to show, through an extensive series of New Jersey specimens (NATURE, vol. xi., p. 215), that the ruder chipped implements of "our native rocks" are older than the more elaborate jasper and porphyry specimens, so I consider these hammered iron hatchets to be of an earlier age than either the polished iron plummets of the mound-builders, or ground axes of the Indians.

CHARLES C. ABBOTT

Trenton, New Jersey, U.S.A.

DOHRN ON THE ORIGIN OF THE VERTEBRATA AND ON THE PRINCIPLE OF SUCCESSION OF FUNCTIONS

THE introduction of the doctrine of Descent into the study of organic phenomena has opened the flood-gates of speculation, of hypothesis, and theory. Probably, with very few exceptions, this is regarded with regret and impatience by zoologists and botanists, even though staunch Darwinian converts, who had made any name in biology in the period anterior to the publication of Mr. Darwin's work on the "Origin of Species." Those were the days of a reaction brought about by the fantastic imaginings of Oken and his school; and the naturalists brought up in those days cannot rid themselves of a dread of speculation which has become as much an organic part of their nervous systems as has the fear of precipices, bricklayers' ladders, and of the mythological personages of their childhood, to most men. It remains for the present and later generations who will be brought up, not to fear, but to use speculation, to turn fully to account the immense engine of research which Mr. Darwin has placed in their hands. We see, in fact, no reason for refusing to welcome any number of hypotheses and theories on biological topics: let every one make his suggestion—the more ingenious and original the better—and let it be taken for what it is worth. If in its author's or another naturalist's hands it should lead to the discovery of new facts—if it should in a more or less modified form be established as true—it will bring thanks and honour to its promoter. If, on the other hand, it should lead to nothing, should be tested and found neither true nor suggestive of truth, it will fall to the ground quietly enough, and do no harm to anybody. This, be it said, applies only to the publication of such hypotheses within the scientific area—a totally different and a very grave responsibility is incurred when an author represents a hypothesis as an established doctrine, and appeals to the support of an uninstructed public. The fact is that we have acquired this freedom of speculation as compared with the proscription of it in the pre-Darwinian period, through the circumstance that biological theory has passed from the theological to the scientific form. To-day—no matter who its author—a speculation as to the mode of development of this or that group of animals and the significance of this or that organ, may be verified or rejected; no one will attach undue value to it until this process has been gone through. Formerly it was not possible to test such speculations; we had in fact no link by which organic phenomena were made part of the whole series of phenomena of which science takes cognisance, and biology had no foundation in the so-called experimental sciences. Hence speculations were liable (as in theological discussion) to be launched by authorities, and to be received not as speculation, but as something like *inspiration*, by disciples; and on the other hand to be rigorously and almost puritanically tabooed by a constantly increasing number who, refusing to occupy themselves with these vain imaginings, endeavoured to keep the facts pure and undefiled, waiting for the coming of an interpreter—who was realised in Mr. Darwin. The doctrine of organic evolution as elaborated by Mr. Darwin and his immediate successors has provided us with a proper scientific framework, and we can now proceed to build on that by the legitimate methods of modern inductive science. It will be some time before biology fully emerges from its theological form; at least another generation must pass; and in the mean time we must expect the continuation of special claims on the part of authorities to advance speculative doctrines *ex cathedrâ*; and on the other hand a lingering antagonism to all speculation, even to that which makes no pretension to authority, on the part of those who have imbibed the horror of fantastic "Natur-philosophie" and of dogmatic pretensions.

To those who belong to neither of these sections, it is worth while pointing out that even the most careful observation and recording of phenomena in the absence from the observer's mind of some theory or speculation which shall, so to say, sharpen his wits and keep his eyes open, is likely to be of the very smallest value. It cannot be too strongly asserted that in observing a complicated phenomenon—such as an organic structure or series of structures—the investigator is only likely to see what he has already imagined *may be* there; the chances are greatly against his detecting an arrangement or a mode of development of which he had previously no suspicion. Though cases of unforeseen discovery do occur, yet it may be safely stated that, as far as all but the most patent and macroscopic appearances are concerned, the observations of no predecessor should be trusted by an investigator beyond the limit which is given by the hypotheses which are known to have been present to that predecessor's mind. In fact, a man can only expect to get answers from Nature to specific questions; she will not give him unsolicited information, nor make a voluntary statement, however attentive the listener. Hence the value and legitimacy of speculations, even *ad nauseam*, on such matters as the pedigree of animals and plants. When advanced, with due knowledge of ascertained facts, they suggest to the embryologist, to the palæontologist, and the anatomist, a number of possibilities which he holds before him as so many questions to be answered by the material of his studies. It is true that it is desirable in a high degree that the person who frames a hypothesis should also himself be active in using it in a practical way, and indeed if he is not, he may find no one who will take the trouble to bear it in mind. Therefore, one must admit the generosity of those who now-a-days make a present of their speculations to scientific *confrères*, and undertake the part of the profound thinker, whilst assigning to others the more practical task of verification and elaboration. For, since the days of scientific inspiration are past, but little credit will attach to the launchers of hypotheses, and more and more to those who destroy them, either by showing their error or by transubstantiating them, in demonstrating that which was supposed, actually to be. It is Darwin whose name we associate with the doctrine of evolution—not Lamarck's, nor Goethe's, nor Wells', nor Freke's.

These remarks are a necessary prelude to the consideration of the bold speculations with which Dr. Anton Dohrn, the founder of the zoological station of Naples, known also for some interesting observations on the development of Crustacea, has recently astonished the zoological world in his "Ursprung der Wirbelthiere und Princip des Functionswechsels." The necessary sequence of the general acceptance of Darwin's theory of the origin of species by descent and natural selection has been an attempt to establish the pedigree of the animal kingdom, and to indicate the degrees of consanguinity among the different members of it known to us. In the first attempts in this direction no one can doubt that errors and vagaries of all kinds must occur. It is only when naturalists have fairly set themselves to the task and made some few false starts that we can expect to see anything like a just appreciation of the methods to be pursued, of the difficulties to be encountered, and of the fallacies to be avoided. We are obliged to admit that the first attempts in the way of constructing the pedigree have been influenced, as they were likely to be, by the remnants of old notions and by the lack of a perfectly unprejudiced appreciation of the question in hand. The pamphlet of Dr. Dohrn comes opportunely enough to insist upon one or two important considerations which have been neglected; and even though, by an excess of antagonism to prevailing prejudice, Dr. Dohrn may be led to oppose exaggeration to exaggeration, we cannot the less feel that there is sound sense and truth in the general purport of his views.

In the pre-Darwinian period naturalists looked upon the series of classes and orders of the animal kingdom as a more or less branched ascending series. The effort in nearly all classifications was to distinguish the lower from the higher and to place the groups in their supposed order of merit, as competitors for the highest rank of organisation. This has led—now that Darwinism is accepted—to a tacit assumption that the order of "degree of organisation" which was worked out in the pre-Darwinian era, is necessarily the order of historical development; that consequently the lower forms of any group which are existing to day, are nearer to the ancestral forms of that group than are the more highly organised forms.

Whilst an exception has been made to this unreasoned and unchallenged assumption in favour of the parasitic forms for which the term "retrogressive development" has been coined, it does not appear to have occurred to any prominent naturalist, at any rate it has not been prominently maintained, that the "retrogressive development" which all so readily admit for parasites, may be a very general phenomenon, as widely or more widely diffused as that of "progressive development." To have insisted on this possibility even to an excess (of which more below) is the merit of Dr. Anton Dohrn. Dr. Dohrn has arrived at an appreciation of the possibilities of degradation or retrogressive development, by divesting himself of all preconceived notions and of all respect for authorities. In his pamphlet he grapples courteously, but fearlessly enough, with Von Baer, Darwin, Haeckel, Gegenbaur, and for the matter of that by implication with almost every zoologist of note.

We claim for him, first of all, full liberty to do this and to launch the hypothesis of general retrogressive development as a competitor with that of universal progressive development. It requires but a few words of explanation and an example, for which Dr. Dohrn has selected the possible relations of the Ascidians to the Vertebrata, to show that retrogressive development is not only a possibility, but *must* be going on and has been going on—on a very large scale—and in any doubtful case is as much entitled to consideration as the hypothesis of progressive development. A less important portion of the pamphlet is that which precedes the development of the author's Hypothesis of Degradation, and illustrates the application of what he calls the "principle" of the Succession of Functions. To put it in the form of a hypothesis it comes to this:—"Organs do not arise *de novo* in organisms, but are formed by the gradual change of function and accompanying change of structure of pre-existing organs." That this is true, or at any rate that it is the hypothesis which, according to the "principle of uniformity," must be preferred to its converse, namely, "that organs *are* formed *de novo*" must be admitted by everyone. In fact, most of Dr. Dohrn's readers will feel that there really is not much novelty in this proposition, since it is already involved in the doctrine of homologies to a very large extent. Dr. Dohrn admits this in his pamphlet, but we conceive that his view differs from that implied in the generally recognised doctrine of homologies, in that the latter is not absolute; it would merely assert that *many* or *some* organs do not arise *de novo*, but are formed by the gradual change of function and accompanying change of structure of pre-existing organs. Dr. Dohrn raises this into a hypothesis of *universal* application, and proposes to apply it stringently in speculations as to the genealogical relationships of organisms. He illustrates its application in an attempt to explain the genealogical affinities and mode of development of Ascidians, Amphioxus, Lampreys, and Sharks. We are very much disposed to believe that here, as in his advocacy of the hypothesis of degradation, Dr. Dohrn has grasped and emphasised a truth which has been floating before the eyes of other people but has not been appreciated at anything like its real importance by them. We believe that the hypotheses of degradation and of continued homologies put before naturalists in the present pamphlet will have a very important and powerful influence on the rapidly progressing reconstruction of the animal pedigree with which so many zoologists are busy.

At the same time it is necessary to point out that the particular speculative conclusions at which Dr. Dohrn arrives as to the new Vertebrate mouth which has replaced the ancestor's mouth as well as the new Ascidian mouth, which has done the same thing—further, the conclusion as to the secondary character of the Vertebrates' anus, and the development of Vertebrate gill-slits from segment organs and of Vertebrate limbs from annelidan gill-supports—all this and more besides is ingenious and healthy hypothesis, but has no value unless Dr. Dohrn or some one else (which is not a thing he should rely upon) will bring it to bear upon the facts and seek to establish it by new observations. We must confess that although we are inclined to entertain some of Dr. Dohrn's suggestions as hypotheses, yet we feel that he has given us rather a large supply, which, in justice to his reputation as an observer, he should hasten to balance by a fair amount of new investigation. Such a speculation as that which he gives us relative to the origin of Vertebrates, can from his hands only be regarded as a sort of programme or announcement of the work which he intends to do during the next decade at the Zoological Station. We shall look most anxiously for the first instalment of results.

Lastly, we shall not shrink from pointing out that Dr. Dohrn urges the hypothesis of degradation to a degree which would be regrettable were it quite evident that he is serious and not merely anxious to engage the attention of his reader by letting imagination have its full swing. Supposing, says Dr. Dohrn, that the Ascidians are the degenerate descendants of a half-worm- half-fish-like ancestor—and the mere consideration of their individual development is enough to make this probable—then we have to admit an amount of degeneration which covers very wide possibilities. For the compound Ascidians, with their various encrusting species, are included in the series; and, moreover, many forms which have ceased in their individual development to give any indication of the affinities which are indicated by the larvæ of other forms. If so large, so abundant, and varied a group can thus take its rise by degeneration, what is to prevent the simpler worms from having originated in the same way? Why may not the Cœlenterata have acquired histological and general simplification in a parallel manner by degeneration accompanying a fixed life? And the Protozoa, the whole series of unicellular animals, why are they not to be considered as degenerated from multicellular forms by a process of simplification? In fact, in a few sentences Dr. Dohrn suggests doubts which land him in a theory which is almost identical with that of Aristotle.

"Thus then," he says, "the animal kingdom has quite a new aspect for us when we look at it from the point of view developed in this essay. Instead of having before us a large mass of forms which from the first commencement of organic life have made little or no progress, whilst a few favoured stems have developed themselves to the highest perfection, we obtain the conception of one single stem, which bore within itself the germ of all other higher, highest but also lowest forms, whose descendants on the one hand in thought and fancy embrace the universe and recognise themselves within the universe as individualities, whilst others lead a senseless inert existence and give rise to the belief that a non-living nature might be able now or at any time to originate such things." Finally, the author argues that the development of this single stem is not to be assigned to either chance or to chemico-physical, but to an "Entwickelungs-gesetz" yet to be discovered. This, we confess, is to us a disappointing termination to a clever and spirited essay. Surely Dr. Dohrn would not expect a scientific man to understand by the word "chance" anything but a periphrasis for the operation of hidden cause. And what can he expect any law of development to be, if not an expression of the operation of chemico-physical causes?

As to the original form under which life made its first appearance, Dr. Dohrn's words would almost lead to the impression that he believes in the creation of a "type-form" something like the Cherubim, with an account of which Archdeacon Freeman favoured Section D of the British Association when it met at Exeter in 1869. His language is, however, sufficiently vague to warrant the supposition that, as an orthodox physical philosopher, he holds the doctrine of the evolution of organic forms subject to the larger doctrine of general evolution, and consequently we may suppose that he would hold that the single stem which has blossomed in man, and from which all other forms have descended by retrograde development, *did* take its origin from simple protoplasm, which had naturally been evolved from carbon compounds. If the animal pedigree did originate from these very simple beginnings, we suppose Dr. Dohrn would say that all trace of them is gone, what is simple *now* in the way of organisms is not the simplicity of the original stock, but a simplicity attained by degeneration. We do not see any reason to accept this hypothesis of *universal* degradation (man alone being excepted from its influence), any more than we can see reason to accept the competing hypothesis of *universal* progress. We are very strongly inclined to think that neither hypothesis can have the whole field to itself. We should expect to find in some directions progress, in others retrogression.

The extent to which each of these processes has gone on in past ages in connection with the family history of the animal kingdom is the great problem for zoological research. E. R. L.

THE NEW METAL GALLIUM

THE discovery, by M. Lecoq de Boisbaudran, of a supposed new element in a blende from the Pierrefite mine in the Argeles Valley, Pyrenees, was made known in our "Notes" of last week. This element, which the discoverer proposes to name *Gallium*, has revealed itself by the following chemical reactions:—

The oxide, or possibly suboxide, is precipitated by metallic zinc from a solution containing chlorides and sulphates.

In a mixture of the chlorides of the new metal and of zinc, ammonia throws down the new element first if added in a quantity insufficient to precipitate the whole of the metals present. Nearly the whole of the gallium is thus thrown down in the first fraction.

Under conditions competent to peroxidise the new metal, the oxide is soluble in excess of ammonia.

Ammonium sulphydrate produces a precipitate insoluble in an excess of the reagent. The sulphide appears to be white.

Sulphuretted hydrogen produces a precipitate in presence of ammonium acetate and excess of acetic acid. In presence of zinc salts the new substance concentrates itself in the sulphides first deposited, but six fractional precipitations were requisite to remove the greatest part of the zinc sulphide. In presence of hydrochloric acid no precipitate is formed.

The oxide, like that of zinc, dissolves in excess of ammonium carbonate.

The salts of gallium are readily precipitated in the cold by barium carbonate.

The chloride may be frequently evaporated with great excess of *aqua regia* without undergoing any loss by volatilisation.

When hydrated zinc chloride containing a trace of the new substance is heated to the point when zinc oxychloride begins to form, the gallium remains in an insoluble condition, possibly as oxychloride.

The quantity of the substance procured was too small to attempt its isolation. Some drops of zinc chloride solution in which the new metal had been concentrated were examined spectroscopically by the electric spark. The spectrum is composed chiefly of a violet line about wave-length 417, and a feeble line about 404.

In his communication to the French Academy, the author states that he obtained the first indications of the new metal on Friday, Aug. 27. It is to be hoped that a good supply of the mineral will be procurable, so that the new element may be isolated, its atomic weight determined, and its reactions studied in detail. This now makes the fifth terrestrial element which the spectroscope has been instrumental in bringing to light.

R. MELDOLA

UNPUBLISHED LETTERS OF GILBERT WHITE

AT the meeting of the Norfolk and Norwich Naturalists' Society, held on the 28th inst., the secretary read an interesting series of ten unpublished letters, written by Gilbert White, of Selborne, to Robert Marsham, F.R.S., of Stratton Strawless, Norfolk, and communicated by the Rev. H. P. Marsham, great-grandson of the latter. The letters, which are dated between August 13, 1790, and June 15, 1793, are excellent examples of Gilbert White's delightfully discursive style, their contents being of a very varied nature. Mr. Marsham, to whom they were addressed, was a great planter, and communicated his experiments on growing trees to the Royal Society; the beauty and great size of the timber at Stratton bear testimony at the present day to his judgment and successful treatment. As might be expected, under these circumstances, a large portion of the correspondence is devoted to forest-trees, the love for which was shared in an almost equal degree by both correspondents. The "Indications of Spring," of which Mr. Marsham left such a remarkable register, and which have been continued by his family, with one slight interruption, from the year 1736 to the present time (see "Philosophical Transactions" for 1789, and the "Transactions" of this Society for 1874-5), of course form an annual topic, as well as the rainfall; but perhaps the most valuable part of the correspondence is the gossip about birds, some of which is of very great interest. On the 30th October, 1792, Marsham writes to White: "My man has just shot me a bird which was flying about my house; I am confident I have never seen its likeness before." On reference to his Willoughby, he declares it to be "the Wall-creeper, or Spider-catcher," and a description, endorsed by him on one of White's letters, as well as a manuscript note in his copy of Willoughby's "Ornithology," still in the possession of the Marsham family, places it beyond doubt that the bird was a veritable *Tichodroma muraria*. White, after saying he is persuaded that the bird is the "very *Certhia muraria*," continues: "You will have the satisfaction of introducing a new bird of which future ornithologists will say, 'Found at Stratton, in Norfolk, by that painful and accurate naturalist, Robert Marsham, Esq.,'"—a prophesy which, after an interval of eighty-two years, will at length be fulfilled. Nearly a whole letter is devoted to an extract from an unpublished "Natural History of Gibraltar," by Gilbert White's brother, the Rev. John White, who resided many years on the "Rock." By this it is shown that John White, who went to reside there in 1756, soon discovered the Crag Swallow

(*Cotyle rupestris*) to be distinct from the Sand Martin, for which it was then mistaken. He gives an interesting account of its habits, and names it *Hirundo hyemalis*, from its great abundance at Gibraltar in the winter months. The last letter of the series, dated June 15, 1793, has a special interest attached to it from the fact that it was written only eleven days before the death of this estimable man and ardent naturalist. The whole of this interesting series will be published in the Transactions of the Society, and it is hoped, through the kindness of Prof. Bell, in whose hands they now are, that Marsham's letters to White may be added.

NOTES

DURING the last week there has been a goodly talk about education, and Mr. Cross has come to the front in a most unexpected manner, while the modern English Cardinal has been acting as his foil. Cambridge, too, in the shape of Mr. James Stuart, has been active at Nottingham, and the world thinks that the University is active. The truth is, however, that the University is too poor to do anything, and that the Colleges are simply looking on while a private benefactor is providing both with those means of teaching which third-rate institutions on the Continent have possessed to a greater or less extent any time during the present century. Mr. Cross not only foreshadows compulsion, but he shows that we have now a Minister who knows the difference between Education and Instruction. "It is not mere book learning that I am talking of. That is not the object of these schools. It is the school discipline, the training of the mind of the child, the teaching him how to teach himself, the self-control and the self-respect which he gets at school, which do more for him than all the book learning that you put into his head." The Cardinal, on the other hand, defines "Secular Education" as "secular knowledge," and then adds: "Education means the full possession and understanding and enjoyment of the inheritance of faith, which the child has by virtue of his regeneration in baptism." It is clear that the Cardinal, if he means anything, confounds instruction with education as successfully as ninety-nine out of every hundred who talk on the subject confound education with instruction.

AT a meeting of the Entomological Club of the American Association for the Advancement of Science, Mr. C. V. Riley, the secretary, read a paper on "Locusts as Food," in which he gave his own experience in cooking and eating them. On one occasion he ate nothing else for a whole day. He found them to have an agreeable nutty flavour, and especially recommended them deprived of their legs and wing-cases, and fried in butter, and also spoke very highly of a soup made from them. He referred to John the Baptist, who had often been pitied for the scantiness of his fare, locusts and wild honey, and expressed his opinion that he was rather to be envied than otherwise. The writer regarded it as absurd that parties should actually die of starvation, as some had done in the districts where this locust plague had prevailed, while surrounded by such an abundance of nutritious and palatable food.

FROM different settlements on the West Coast of Africa young living gorillas have several times been shipped for Europe under auspices apparently the most favourable. On one occasion, about six years ago, a Dutch merchant at St. Paul de Loanda took the trouble to keep a young male in company with a black boy for some considerable time on the coast, and when the two had become good friends, took passages for them both to Holland. The animal only survived a fortnight from the date of its embarcation, dying rather suddenly, as most others seem to have done, from a kind of depression or home-sickness, not from any well-marked disease. No gorilla, exported as such, has reached Europe alive. Quite recently, within the last month or so, one destined for Hamburg arrived within two days of its journey's end, when it shared the fate of its predecessors. This specimen was, immediately after its death, placed into spirit, and will, we believe, form the subject of a monograph by Dr. Bolau, of the Zoological Museum of Hamburg, from whom we may expect the settlement of several important and doubtful points in the anatomy of the greatest of the anthropoid apes. In about the year 1852, in one of Wombwell's travelling menageries, there was exhibited for some months a monkey very like a chimpanzee. The animal was expert at tricks, and was clad in a grotesque costume. From a daguerreotype photograph in the possession of Mr. A. D. Bartlett, resident superintendent of the Zoological Gardens in Regent's Park, that gentleman was enabled to identify the specimen as one of a young gorilla, and not a chimpanzee. Its face was dark, its arms and legs proportionately larger, its ears very much smaller, and the distance between the eyes greater than in the chimpanzee. A still more interesting instance of the same kind has, however, recently occurred. For the last two years there has been a female "chimpanzee" at the Zoological Gardens at Dresden, named Mafota, which has attracted considerable atention. She was purchased by Herr Schöpff, the Director of the Dresden Gardens, in a very unpromising condition, being much denuded of hair, and covered with an unhealthy skin eruption. Since the animal has been under Herr Schöpff's skilful care, it has become quite a different creature. It has grown very rapidly; surprisingly so. The hair now forms an abundant covering, and the skin is in a perfectly healthy condition. It is quite tame with its keepers, whose boots it is in the habit of taking off and replacing for the amusement of visitors. It performs many other tricks, showing great intelligence. Herr Carl Nissle, an artist, we believe, whilst studying the figure and movements of Mafota, became rapidly impressed with the idea that she is not a chimpanzee at all. Her great size, the numerous black spots on the naked skin of the face, which in the chimpanzee is simply flesh-colour, the black instead of pink hands, the slight webbing between the fingers, and the different expression, with a broader nose, all led him to the conviction that she is a gorilla. He carefully studied the stuffed specimens of the gorilla and chimpanzee, both at Berlin and Lubeck, and, what is more, has had the opportunity of seeing the new Hamburg spirit specimen above referred to. These all confirmed his surmise, towards the complete verification of which we have the affirmative opinion of Prof. R. Hartmann, prosector to the Anatomical Museum of Berlin. So there is strong reason for the belief that Mafota is a gorilla, the first living specimen recognised as such in this continent.

THE following are the hours of the various Introductory Lectures at the London Medical Schools, which will be delivered to-morrow (Oct. 1st), with the names of the respective lecturers:—

HOSPITAL.	LECTURER.	HOUR.
Charing Cross	Mr. Fairlie Clarke	4 P.M.
St. George's	Dr. Barnes	4 ,,
Guy's	Dr. Stevenson	2 ,,
King's College	Dr. Curnow	4 ,,
London	Dr. B. Woodman	3 ,,
St. Mary's	Dr. Randall	3·30
Middlesex	Mr. Lowne	3 ,,
St. Thomas's	Dr. Payne	3 ,,
University College	Dr. Corfield	3 ,,
Westminster	Mr. R. Davy	3 ,,

DR. JAMES BELL PETTIGREW, F.R.S., Lecturer on the Institutes of Medicine at the Royal College of Surgeons, Edinburgh, has been appointed to the Chair of Medicine in the University of St. Andrews, vacant by the death of the late Dr. Oswald Home Bell.

THE following is a list of candidates who have been successful in obtaining Royal Exhibitions of 50*l.* per annum each for three years, and free admission to the course of instruction at the Royal School of Mines, London, and the Royal College of Science in Dublin:—1. School of Mines: John Gray, 21, engineer, Strichen, N.B.; Frederick G. Mills, 14, student, London; Thomas E. Holgate, 20, farmer, Blackburn. 2. College of Science: C. C. Hutchinson, 21, engineer, Leeds; Henry Hatfield, 20, student, Stockport; Thomas Whittaker, 18, clerk, Accrington.

THE term of office of the present Lord Rector of Aberdeen University—Professor Huxley—having nearly expired, the students are already looking out for a successor. Mr. M. E. Grant Duff, M.P., Dr. W. B. Carpenter, Mr. Robert Lowe, and Dr. Alexander Russel, editor of the *Scotsman*, are proposed for election. A report in the *Times* states that the feeling of the majority seems to be in favour of Dr. Carpenter.

THE preliminary North-west African Expedition is expected to leave England for the coast of Africa early in November. General Sir Arthur Cotton and several scientific gentlemen are expected to accompany it. The object in view is to make a survey of the coast of Africa opposite the Canary Islands for the purpose of finding a suitable position for a harbour and commercial and missionary station; to enter into commercial arrangements with the native tribes, and to inquire into their present means of commerce, and the resources of the countries through which it is proposed to pass. To examine as far as practicable the sand bar across the mouth of the River Belta, which it is supposed keeps back the waters of the Atlantic Ocean from flowing into the dry bed of the ancient inland sea, to obtain levels and other necessary information. Mr. Mackenzie, the director of the party, expects to get the friendly support of the most powerful chief of the tribes on the north-west coast of Africa.

THE celebration of the fiftieth anniversary of the opening of the first railway between Stockport and Darlington is attracting the notice of the French papers. A curious fact connected with French railways is that Baron Charles Dupin, who published his celebrated work on Great Britain in 1826, described railways at full length, but abstained from saying a word about motive-power. Baron Dupin, a great geometer and mechanician, declared to the Institute that locomotives could never move, owing to the weakness of their hold on the rails, and that the use of horses could not be dispensed with. Baron Charles Dupin's reputation was so great that the truth of the statement was taken for granted, and in the École des Ponts et Chaussées, the public institution where State engineers are educated at the expense of the Government, in a course of lectures given after 1830, it was said that horses could never be dispensed with. The advantages of locomotion were lectured upon in a free institution which was opened at that time, called the École Centrale des Arts et Manufactures. The professor was the celebrated railway engineer, Perdonnet. Arago was opposed to the boring of tunnels as endangering the health of travellers, owing to the great cold which he anticipated would be felt.

M. LEVERRIER has addressed a circular to the Presidents of the Meteorological Commissions of the departments with reference to the Meteorological Atlas in course of publication for the years 1872, 1873, and 1874. It is intended that this important work shall contain instructions relative to meteorological observations and tables for their reduction: a discussion of thunderstorms which have occurred in the different river-basins as well as over France generally; a *résumé* of the observations made during the three years at the departmental stations; hail charts; the rainfall for the whole of France, by M. Belgrand; and lastly, a series of memoirs on special subjects by French and foreign meteorologists. The price for the large or folio volume will be only eight shillings, representing the price of paper and printing, the printing being undertaken by the Government, and the compilation having been done by the Meteorological Service at the Observatory. The number of copies printed being necessarily limited, persons wishing to purchase the work are required to send a money order to the Secretary of the Association Française, 11, Quai Voltaire, Paris.

DR. GUSTAVUS HINRICHS, Director of the Laboratory of the Iowa State University, Iowa city, has issued a circular, dated August 1875, with the view of organising a system of rainfall observation for the whole of the State of Iowa. He is confident of a start with one rain-gauge in each county of the State, and hopes in a few years to secure the erection of four or five gauges in each county. Printed forms on addressed postal cards will be issued to the observers, who are requested to mail them on the 1st, 11th, and 21st of each month. Thrice a month Dr. Hinrichs will prepare a statement of the rainfall of Iowa for the corresponding ten days, comparing it at the same time with past averages, and forward it to the daily press for publication. Other States will doubtless soon follow the example.

THE Upsala Observatory has published a Circular (No. 6) giving an elaborate discussion by Dr. Cronwall, of the observations made over Sweden to determine the annual periods of the duration of ice. The six coloured maps, which illustrate the paper, showing, by lines passing through equal times and periods, the beginning, end, and number of days' continuance of the ice over the different districts of the country during the winters of 1871-72 and 1872-73, are valuable contributions to the climatology of Sweden. Their great value lies in illustrating in a precise as well as striking manner the influence of its adjoining seas, its lakes, its mountains and lesser elevations, and latitude, in determining the times of occurrence and termination of this element of the climate of Sweden. These discussions, begun by Dr. Hildebrandsson for the winter of 1870-71, cannot fail to be of great benefit to agricultural and other public interests.

SINCE our last issue we have received telegraphic intelligence of frightful floods and consequent loss of property in Texas. At Indianola the storm began on the 15th. The east wind which prevailed next morning increased to a gale. The water soon became six feet deep in the streets. On the 17th the wind veered to the north-west. The waves became chopped. The houses were washed away or tumbled down. Toward the morning of the 18th the wind lulled and the water receded; wind veered to the north. When daylight broke an awful destruction became visible. The town could not be recognised. The ruin was almost total. Seventy bodies were found in a brief period and buried. Men and women were discovered who had floated on doors or anything obtainable. Some were imprisoned beneath roofs. Hundreds had miraculous escapes. The loss of life may reach 200. Every business house but five has been destroyed. Every pilot but one has been drowned. The city of Sabine has been submerged and greatly damaged, but without loss of life. Matagorda, at the entrance of Matagorda Bay, has been swept away; but two houses are standing. Cedar Lake is also destroyed. All the inhabitants are reported lost at East Bay. In a village containing twenty-eight people, all but five are lost.

A CORRESPONDENT of the *Daily News*, writing from Christiania, says: "I translate the following from the *Finmarken-post*, a newspaper published in Europe's northernmost city—Hammerfest:—' On the 3rd instant arrived at Hammerfest the schooner *Regina*, Capt. Gundersen, belonging to the firm of O. J. Finckenhagen, from a voyage in the Arctic regions and the north coast of Nova Zembla. Capt. Gundersen discovered in Nova Zembla a journal, kept by the Dutch Arctic voyager, Barent, apparently giving an account of his doings from the 1st of June to the 29th August, 1580, as far as Capt. Gundersen was able to make out, being unacquainted with Dutch and Dutch writing of 300 years ago. The paper is in excellent preservation, and the writing distinct. Barent passed the winter 1596-97 in the Arctic regions. This journal, therefore, relating presumably to 1580, will give no information of his stay, but will, nevertheless, be of great interest.'"

WE learn from *Harper's Weekly* that the Kirtland School of Natural Sciences, established in Cleveland, Ohio, for summer instruction in natural history, concluded its course on the 9th of August last. The school consisted of twenty members, of whom thirteen were ladies, and lasted for five weeks, during which time gratuitous instruction was given by lectures and otherwise, and short excursions were made in connection with the subjects of study. Dr. Newberry, Prof. Theodore B. Comstock, Prof. Albert Tuttle, and Dr. William K. Brooks were the instructors. The operations of the school were mainly conducted by Prof. Comstock. Facilities were extended by railroad and steamboat companies in the transportation of the school and in various interesting excursions.

IN a recent number of the *Philadelphia American Times*, Dr. W. W. Keen proposes the employment of a solution of chloral as a preservative for objects of anatomy and natural history, its special advantage being said to be that the colour of objects is perfectly preserved, and all the parts retain their natural consistency, at the same time that no special precaution is necessary in stoppering the bottles containing the preparations. It is used by injecting it into the blood-vessels, or by immersion.

IN a recent number of the *Journal de la Société centrale d'Horticulture de France*, there is an article by M. Ch. Royer, "On the Causes of the Sleep of Flowers." The sleep of flowers has been attributed to various causes, including heat, light, moisture, dilatation of the epidermis of the inside of the perianth, contraction of the outside of the perianth, &c. The writer of the article in question endeavours to prove that expansion of the flowers in the morning is due to a turgescence of the parenchyma of the flower, brought abcut by heat, certainly; but the same agent indirectly causes the same flowers to close up again, after the disappearance of the swelling through evaporation. This, he contends, accounts for the early closing of flowers under a high temperature, or in dry soils. We have always understood that this phenomenon was governed by the hygrometrical conditions of the atmosphere.

THE *Revue des Eaux et des Forêts*, 1875, gives some statistics of the constituents of the forests of Denmark. The beech is now the most universal, having gradually succeeded in displacing the oak and pine. Next in order are the birch, alder, aspen, hazel, &c. Although at a very remote period pines appear to have formed the principal forests of Denmark, they are not now indigenous, nor have they been for many centuries; indeed, they do not thrive when introduced. According to the celebrated Danish geologist, M. Forchhausmer, the beech grows best in the formation which he calls *argile caillouteuse*, or *argiles à blocs erratiques*; whereas the oak prefers the *sable caillouteuse*, or *sable à blocs erratiques*. An examination of the vegetable remains in the bogs so common in Denmark reveals the fact that the earliest forests were composed of pines, followed by the sessile-fruited variety of the oak, now to a great extent superseded by the beech, &c. It is supposed that the pine forests flourished during the *âge de la pierre à éclats*; and the oak was at its greatest development at the commencement of the bronze age.

AMONGST the several ameliorations which are in preparation at the Ministry of Public Instruction in France, is the remodelling of the *baccalaureat* in a manner which is likely to benefit the study of medicine and the spread of the study of science. The *baccalaureat* of sciences is to be required as formerly from students in medicine; but after having passed a general examination for their first *baccalaureat* they will be examined in a second *baccalaureat* of *sciences physiques*, which includes not only physics, but general notions of botany, zoology, mineralogy, &c. The general *baccalaureat* is common to students in medicine and in mathematics, the students of the latter branch having to pass a special examination of their own entitled *Baccalaureat des Sciences Mathematiques*.

WE believe that the Belgian Government is about to establish tide gauges on the Escault, and to undertake complete researches on the tides and currents of the coasts of Belgium generally. Prof. Van Rysselberghe, the inventor of the self-recording meteorograph, to which we have already called attention, has been attached to the Hydrographic Department, with a view of aiding in these researches.

WE would direct the attention of our biological readers to a translation from the *Berliner Klinische Wochenschrift*, in the current number of the now monthly *London Medical Record*, of a paper by Dr. Scheele, of Dantzig, on two cases of complete transposition of the viscera, together with valuable observations and references on the subject generally.

AN interesting ceremony recently took place at Estagel, a small country town in the Department of the Pyrenees, where the great Arago was born. The local authorities and an immense number of people have celebrated the tenth anniversary of the erection of a statue of that astronomer. No scientific speaker was present, and Arago was merely eulogised in general terms for his science as well as for his patriotism.

THE vanilla plant has lately been attacked by a disease which has greatly interfered with its cultivation. Chemistry has been brought to bear in the production of a new substance from which the "vanilla essence" is produced. Messrs. Hartig and Kubel, two German chemists, have found in the *cambium* of conifers a species of resin which, after certain processes, produces an aroma exactly similar to that of the vanilla, and which possesses the same composition as that of the true vanilla essence itself. This *pseudo* vanilla is sold largely in Germany for the real article; its price is about two-thirds that of the true vanilla essence.

ON Tuesday last there was a private view of the works of the Westminster Aquarium and Winter Garden. From their unfinished state it was not possible to form an accurate idea of the contemplated arrangements, but the considerable area already occupied or to be covered with buildings struck everybody. At the luncheon subsequently given the Managing Director made a speech, in which much was said about science and intellectual enjoyment. Undoubtedly the Company will have a powerful engine at its disposal either for instruction or amusement.

NO. 3, vol. iv., of the Proceedings of the Geological Association, contains, besides pleasant descriptions of some excursions, the following papers:—"On the deposits now forming in British seas," by G. A. Lebour; "Notes on specimens of Phosphorate from the Department of the Lot, France," by F. W. Rudler; "A probable origin of the perforation in sharks' teeth, from the Crag," by H. A. Burrows; and "On the conditions of animal life in the Deep Sea bottom," by Dr. W. B. Carpenter.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus*) from India, presented by Mr. Turnbull; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Knight; two Common Wolves (*Canis lupus*) from Russia, presented by Mr. Charles Bell; a Chinese Mynah (*Acridotheres cristatellus*) from China, presented by Mr. J. R. France; two Rattlesnakes (*Crotalus durissus*) from N. America, a Long-nosed Crocodile (*Crocodilus cataphractes*) from W. Africa, received in exchange; five Russell's Vipers (*Vipera russelli*) born in the Gardens.

SOME LECTURE NOTES UPON METEORITES

NOWHERE in the "Cosmos" does Alexander von Humboldt show more vividly his keen appreciation of all the grander operations of nature than in those passages in which he discusses the subject of meteors, and in which he gives us a forecast of the connection of those striking and still not entirely explained phenomena with other celestial spectacles, such as the apparition of comets and the fall of meteorites.

Thus Humboldt dwells with a lingering interest on the subject of the meteoric showers which in their grandest form, on at least one, and generally on some successive Novembers in every generation, and in a less brilliant degree on every 10th of August, illuminate the sky with countless lines of momentary light. And while bringing the occurrence of these swarms of meteors with much vividness before our eyes, he treats them as a special form of the same display presented by the single meteor, that, gliding down the sky, leaves its thread of light to illuminate a few degrees of the great arc described on the dome of heaven by the meteor; nor does he hesitate to link these phenomena into one series with those larger meteoroids that we call fireballs, and which sometimes light up the whole heavens, and may occasionally be seen over half a continent. And we may go on with Humboldt to connect with these greater meteors a class of still more striking phenomena accompanying the descent generally out of a dark cloud when seen in daylight, or with a bright flame, when seen by night, of meteoric stones, heralded by sounds as of thunder.

Within the last few years the cases of recurring periods of meteoric showers have been considerably multiplied, while these and the comets have been recognised by astronomers as belonging to the same order of celestial objects: and we are now enabled to group the whole of the phenomena we are considering under a single category with a confidence far greater than that on which Humboldt built his surmise regarding them.

It is with the meteoric bodies that fall from out of a cloud when seen by day, and in fiery mass where the light can be distinguished, and accompanied by detonations like cannon, that we are going more immediately to deal here; and it may be well therefore, without recalling the descriptions that may be found in many treatises of some of the more familiar meteoric falls, such as those of L'Aigle and of Braunau, to recount the evidence of eye-witnesses of these events on other occasions. The following is a contemporary account of the fall of a shower of stones in the county of Limerick, at Adare, on Sept. 10, 1813:—

"Friday morning, the 10th September, 1813, being very calm and serene, and the sky clear, about 9 o'clock, a cloud appeared in the east, and very soon after I heard eleven distinct reports, appearing to proceed thence, somewhat resembling the discharge of heavy artillery. Immediately after this followed a considerable noise not unlike the beating of a large drum, which was succeeded by an uproar resembling the continued discharge of musketry in line. The sky above the place whence this noise appeared to issue became darkened and very much disturbed, making a hissing noise, and from thence appeared to issue with great violence different masses of matter, which directed their course with great velocity in a horizontal direction towards the west. One of these was observed to descend; it fell to the earth, and sank into it more than a foot and a half, on the lands of Scagh, in the neighbourhood of Patrick's Well, in the county of Limerick. It was immediately dug up, and I have been informed by those that were present, and on whom I could rely, that it was then warm, and had a sulphurous smell. It weighed about 17 lbs., and had no appearance of having been fractured in any part, for the whole of its surface was uniformly smooth and black, as if affected by sulphur or gunpowder. Six or seven more of the same kind of masses, but smaller, and fractured, as if shattered from each other or from larger ones, descended at the same time with great velocity in different places between the lands of Scagh and the village of Adare. One more very large mass passed with great rapidity and considerable noise at a small distance from me; it came to the ground on the lands of Brasky, and penetrated a very hard and dry earth about two feet. This was not taken up for two days; it appeared to be fractured in many places, and weighed about 65 lbs.! Its shape was rather round, but irregular. It cannot be ascertained whether the small fragments which came down at the same time corresponded with the fractures of this large stone in shape or number, but the unfractured part of the surface has the same appearance as the one first mentioned. There fell also at the same time, on the lands of Faha, another stone, which does not appear to have been part of or separated from any other mass; its skin is smooth and blackish, of the same appearance with the first-mentioned; it weighed above 74 lbs.; its shape was very irregular. This stone is in my possession, and, for its volume, is very heavy.

"There was no flash of lightning at the time of, or immediately before, or after the explosion; the day continued very calm and serene, was rather close and sultry, and without wind or rain. It is about three miles in a direct line from the lands of Brasky, where the very large stone descended, to the place where the small ones fell in Adare, and all the others fell intermediately; but they appeared to descend horizontally, and as if discharged from a bomb and scattered in the air."

The next account is that of a stone that fell at Durala, or Dooralla, on February 18, 1815.

Extracts from a Letter from Capt. G. Bird.

"Loodiana, April 5, 1815.

"On the 18th February last, some people who were at work in a field about half a mile distant from the village of Dooralla were suddenly alarmed by the explosion of what they conceived to be a large cannon, 'the report being louder than that of any other gun they had ever heard,' which report was succeeded by a rushing noise like that of a cannon ball in its greatest force. When looking towards the quarter whence the noise proceeded, they perceived a large black body in the air, apparently moving directly towards them, but, passing with inconceivable velocity, buried itself in the earth at the distance of about 60 paces from the spot where they stood. The Brahmins of the village, hearing of it, proceeded to the spot with tools for digging it up. They found the surface broken, and the fresh earth and sand thrown about to a considerable distance, and at the depth of rather more than 5 ft. in a soil of mingled sand and loam they found the stone, which they cannot doubt was what actually fell, being altogether unlike anything known in that part of the country. The Brahmins conveyed it to the village, covered it with wreaths of flowers, and started a subscription for the purpose of raising a small temple over it. It fell on the 18th of February, about mid-day, in a field near the village of Dooralla, which lies about lat. 30° 20', long. 76° 41', within the territory belonging to the Pattialah Rajah, 16 or 17 miles from Umballa, and 80 from Loodiana. The day was very clear and serene, and, as usual at that season of the year, not a cloud was to be seen, nor was there in the temperature of the air anything to engage their attention; the thermometer of course may be stated about 68° in the shade. The report was heard in all the circumjacent towns and villages, to the distance of 20 coss, or 25 miles, from Dooralla. The Rajah having been led to consider it as a messenger of ill omen, according to my wish gave immediate orders for its conveyance to Loodiana, but with positive injunctions that it should not approach his place of residence. It weighs rather more than 25 lbs., and is covered with a pellicle thinner than a wafer, of a black sulphureous crust, though it emits no smell of sulphur that I can discover. It is an ill-shapen triangle, and from one of the corners a piece has been broken off, either in its fall or by the instruments when taken out of the ground. This fracture discloses a view of the interior, in which iron pyrites and nickel are distinctly visible. No Hindoo ventures to approach it but with closed hands in apparent devotion, so awful a matter is it in their eyes."

This aërolite was brought from India by Lieut.-Col. Pennington, and presented to the Hon. East India Company. It is now in the British Museum.

The next description is that of the fall of a stone at Manegaum, in Kandeish, on June 29, 1843. The account is given by two Hindoo eye-witnesses:—

"On the day the aërolite fell we were both seated, about 3 o'clock in the afternoon, on the outskirts of the village, in a shed belonging to Ranoo Patel. There was at the time no rain, but heavy clouds towards the northward. There had been several claps of thunder for two hours previously, and some lightning. Suddenly, while we were seated in the shed, several very heavy claps of thunder occurred in quick succession, accompanied with lightning, on which we both went out to look around us, when, in the middle of a heavy clap, we saw a stone fall to the ground in a slanting direction from north to south, preceded by a flash of lightning. It fell about 50 paces from us. On going up to it we found that it had indented itself some four or five inches into the ground; it was broken in pieces, and, as far as we could judge, appeared to be about fifteen inches long, and three inches in diameter, of an oblong shape, somewhat similar to a Chouthe grain measure; it was of a black vitreous colour outside, and of a greyish yellow inside; it was then of a mouldy texture, and hardened to the consistence of the present specimens afterwards.

"Only one stone fell. No rain had fallen for eight days previously, nor did it for four days after the fall of the stone. It had been warm all day before, but not much more so than usual. From mid-day till the time the stone fell (3 P.M.) it was very cloudy towards the northward; after its fall the thunder ceased, and the clouds cleared away. No stone of a similar description had ever fallen near our village before. The pieces of the stone were immediately after carried off by the country people. Our village is situated on the banks of the small river, the Poorma. There are no hills in its vicinity, the nearest being 3 coss (or 6 miles) off."

Finally, we may extract from the contemporary notices published in the United States, the more remarkable circumstances attending the fall of a great number of aërolites at New Concord, U.S.A.:—

"About fifteen minutes before one o'clock, May 1, 1860, the people of South-eastern Ohio and North-eastern Virginia were startled by a loud noise. . . . . The area over which the explosion was heard was probably not less than 150 miles in diameter. . . . . An examination of all the different directions leads to the conclusion that the central point from which the sound emanated was near the southern part of Noble County, Ohio. . . . .

"Twenty-three distinct detonations were heard, after which the sounds became blended together, and were compared to the rattling fire of an awkward squad of soldiers, and by others to the roar of a railway train. These sounds, with their reverberations, are thought to have continued for two minutes. The last sounds seemed to come from a point in the south-east, 45° below zenith. The result of this cannonading was the falling of a large number of stony meteorites upon an area of about ten miles long by three wide. The sky was cloudy, but some of the stones were seen first as 'black specs,' then as 'black birds,' and finally falling to the ground. A few were picked up within twenty or thirty minutes. The warmest was no warmer than if it had lain on the ground exposed to the sun's rays. They penetrated the earth from 2 ft. to 3 ft. The largest stone, which weighed 103 lbs., struck the earth at the foot of a large oak tree, and after cutting off two roots, one 5 in. in diameter, and grazing a third root, it descended 2 ft. 10 in. into hard clay. This stone was found resting under a root which was not cut off. This would seemingly imply that it entered the earth obliquely. It is said that other stones which fell in soft ground entered the earth at a similar angle. They must have been flying in a north-west direction. This fact, added to the other facts, that the detonations heard at New Concord came lower and lower from the zenith toward the south-east, and that the area upon which the stones fell extends with its longer axis in a south-east and north-west direction, would imply that the orbit of the meteor, of which these stones are fragments, extended from south-east to north-west. This conclusion is confirmed by the many witnesses who saw at the time a luminous body moving in the same direction. It is a fact of some interest that the larger stones were carried by the orbital force further than the small ones, and were found scattered upon the north-west end of the area referred to.

. . . .

"Prof. Evans computes, from data supplied by several reliable witnesses, the altitude of the meteor when first seen to range between thirty-seven and forty-four miles.

. . . .

"A train accompanying the stones is described as a cone, having its base upon a fire-ball. As seen from near Parkersburg its length was estimated at twelve times the diameter of the ball. The part next the base appeared as a white flame, but not so bright as to render the outline of the ball indistinct. About half way toward the apex it faded into a steel blue.

. . . .

"Near McConnellsville several boys observed a huge stone descend to the earth which they averred looked like a red ball, leaving a line of smoke in its wake." McConnellsville is twenty-five miles south of Concord.

. . . .

Another observer at Berlin saw a ball of fire flying in a northerly direction with great velocity. It appeared as white as melted iron, and left a bright streak of fire behind it which soon faded into a white vapour. This remained more than a minute, when it became crooked and disappeared. Berlin is about 80 miles south-west of Concord.

Now, these and other descriptions of similar events witnessed by people in different parts of the world substantially agree. In some minute circumstances they naturally differ, as doubtless do also the events themselves or the conditions under which they are witnessed. The appearance of a cloud at a great elevation, its rapid motion, the emanation from it of masses of matter ultimately falling to the earth, the association with these appearances of a fiery light forming a splendid spectacle that lights up the heavens by night and in twilight, and is often also seen by day; the trail that follows the great meteoroid mass, and lingers on the air in the form of a long-drawn film of cloud that remains luminous by night for some short period after the passage of the luminous ball or cone,—are phenomena to which witness is borne in many cases besides the last above recorded. Testimony is also concurrent on the loudness and repetition of detonations that accompany these phenomena, irrespectively of their multiplication by the effect of echo. In the case of a group of meteorites that fell at Butsura, in India (near Goruckpore), on May 12, 1861, we have evidence of three different explosions.

Now, for some parts of the phenomena thus recorded we can offer satisfactory explanations, though of other parts of them the explanations hitherto offered may seem not quite so complete.

First, we have the enormous velocity with which such a body comes into our atmosphere, sufficient in some cases to bear the meteorite through the distance from London to Edinburgh in as many seconds as an express train takes hours; and where the body enters our atmosphere that medium is so rare that we can hardly conceive it presenting any resistance; yet even at that enormous elevation—certainly in many cases as much as forty miles above the earth, where the meteor enters this fine atmosphere—there cannot be a doubt that the atmospheric resistance at once called into play is sufficient to impede the body that enters it with so enormous a velocity. And by virtue of a principle which is now an axiom of science, this arresting of the velocity of the meteorite means calling into activity intense heat that is largely imparted to the meteorite itself—heat, in fact, that is proportional to the velocity for which it is exchanged.

Now, these meteoric masses must often come into our atmosphere, not individually, but in swarms. From the rapidity with which the heat is developed, and partly also as a consequence of the low conductivity for heat of the stony masses, their surface only has time to experience the effects in the few seconds of transit, and therefore only the surface fuses; and, as a consequence of this fusion, there arises a sort of spray of meteoric dust flung off from the meteorite or from the meteoric swarm; and this forms a cloud, such as may be seen lingering on the track of almost any large meteor that is visible by daylight. To the material nature of such a cloud as it rests, or rather, though rapidly falling, seems to rest, poised in the air, the writer can bear personal testimony, having witnessed it in the train of a fine meteor many years ago, about sunset. When the ordinary clouds had long ceased to be tinted by the rays of the evening sun, as in the after-glow on the Alps, the long line of meteoric cloud became lit up with rose-tinted hues, and bending into a curve towards the east before an upper current of air, offered proof beyond question of the material nature of this cloud, and at the same time of its great elevation and the fine state of division of its dust-like particles, which undoubtedly resulted from the disintegration of the meteoric mass in its passage through the air. The same cloud of dust is often visible as a luminous trail by night, in consequence partly of its retaining its incandescence for a certain time, but probably also in part from the phosphorescence of its material. We are thus able to offer an undoubtedly true explanation of one part of the spectacle.

The existence in the crust of a meteorite of projecting particles of unoxidised meteoric iron, and, in the case of the Busti meteorite, of calcium sulphide unaltered, is explained by the momentary character of the process which during the flight of the meteorite perpetually removes the outer surface and exposes a fresh one, which, however, is always screened by a protecting glaze of fused silicate from the immediate action of the air, so long as there is velocity enough left to the mass thus to fuse and to throw off in its wake fresh portions of its surface; while in the later stage of its flight the glaze accumulates into a denser crust highly charged with magnetic iron oxide, mainly the result of the oxidation of the iron of the silicates.

The cause or causes of the explosions are more difficult to demonstrate. They have been accounted for in two separate ways, which, though different, are not inconsistent, and are both probably involved in a complete explanation of the disruption and detonations. Why should a meteorite explode with a repor which could be heard forty or fifty miles away? Nay, why should it explode at all?

One answer is this. The aërolite comes into our atmosphere from regions in which the temperature, "the cold of space," may range as low as 140° below zero Centigrade; and though the mass, from the absorption of solar heat, would possess a temperature much above this, it would nevertheless be intensely cold, and consequently more brittle than at ordinary temperatures; and hence, on its entering our atmosphere, the heat it instantaneously acquires on its outer portion expands this, and tends to tear it away, so as to dissever the exterior from the interior, which continues to be relatively contracted by the intensity of the cold which the aërolite brings with it from space. The consequence is, first, that little bits of the stone spring out all over it, leaving those curious little holes or pit marks which are characteristic of a meteorite; and eveiy now and then, as the heat penetrates, larger masses split away, of which interesting evidence is afforded by the meteorite, for instance, that fell at Butsura on May 12, 1861. Fragments of this stone were picked up three or four miles apart; and by supplementing them by a small piece modelled to fill up one lacuna, one is able to build up again with much certainty the original meteorite, or at least the portion of it represented by the fragments of it which were found. Important portions of this stone are in the British Museum, presented some years ago by the liberality of that invaluable institution, the Asiatic Society of Calcutta. Now, it is remarkable that these fragments, which in other respects fit perfectly together, are, even on the faces of junction, now coated with a black crust. On the other hand, another of these fragments not thus coated fits like the former to a part of the meteorite that was found some miles away rom it, and is also not incrusted at the surface of fracture. Hence we can assert that this aërolite acquired after coming into our atmosphere a scoriated and blackened surface or incrustation. The first explosion drove the fragments first alluded to asunder, and these became at once incrusted on their broken surfaces; but others that were separated afterwards, probably on the last of the three explosions, had not sufficient velocity left to cause their incrustation in the same manner as was the case with the fragments previously severed. Now, this successive incrustation of the fragments of the meteorite confirms the idea that the disruption of the mass, and the explosions heard for so vast a distance as Goruckpore (some sixty miles), are parts of the same convulsion; and sixty miles is by no means an uncommon distance for the sound of such a meteoric explosion to be heard.

The late W. von Haidinger (to whom we are indebted for a collation of the facts and for valuable suggestions bearing on this subject) threw out the notion that what really produced the detonation was not the disruption of the mass (which he held not to be a sufficient cause for so loud a report) so much as the collapse of the air into a vacuum which, after following the meteorite as it pursued its rapid course, suddenly ceased to exist as the velocity of the meteorite became practically reduced to zero.

But it still would remain to be explained why at one time more than another this collapse of the vacuum should take place, or how it could be repeated; of this, however, a sufficient explanation would seem to be afforded by the actual bursting asunder of the meteorite from the cause before assigned, since this explosion, by disturbing the conditions on which the persistence of the vacuum depends, would permit the collapse of the air and consequent detonation.

(*To be continued.*)

*OBSERVATIONS ON A REMARKABLE FORMATION OF CLOUD AT THE ISLE OF SKYE**

THE resistance offered by the earth's surface to the wind is known to reduce its velocity and to cause deviations in its direction both horizontal and vertical, as well as to retard the progress of the storm itself. This friction to which aërial currents are subjected is probably least for a surface of water such as the sea—greater for plains of loose sand, where, as in the Nubian deserts, lofty sand pillars are produced—and greater still where the surface is immoveable, as in the case of solid land; but the greatest resistance of all is due to the obstruction offered by rugged hills and lofty mountain-ranges.

In an account of the Morayshire easterly storm of September 1871; published in the *Scottish Meteorological Journal*, I suggested that the great amount of rainfall which fell on that occasion at and near the Morayshire coast, and on the sea-coasts of the counties of Fife and East Lothian which also fronted this storm, was due to the sudden increase of friction which the wind encountered when it reached the land. The in-shore stream of air being checked by the unyielding nature of the shore, even though it was, as in this case, of no great elevation, would form a pillow of obstructed or perhaps nearly stationary air, which would produce vertical deflection on the strong currents coming in from the sea. The stream of air thus projected upwards to a height where the temperature is lower would be condensed into vapour and rain.

This sudden change of resistance to in-shore winds is probably one of the causes of the well-known peculiarity of seaside climates.

On the 27th July last, about 11.30 A.M., when in the steamer of the Northern Lighthouses off the Sound of Harris, I saw a beautiful example of the genesis of clouds—due, however, not to

a low foreshore, but to hills of about 900 feet high. The sky was perfectly clear, with a steady but very slight breeze from the S.W., which came straight upon the south-western extremity of the Island of Skye, distant about twelve miles from the ship. A small portion of the most southerly projection of the island, which was considerably lower than the more inland parts, was perfectly free from vapour, but at a short distance inland from the shore, there was an abrupt face of hill, from the top of which there rose a very slender column of white vapour which gradually expanded as it ascended into the air, presenting exactly the appearance of the escape of steam from the spiracle of a volcano. The cloud thus formed not only extended as far as the northern extremity of Skye—itself a distance of twenty-eight miles—but was visible as a well-defined stratum of cloud for a long distance beyond Skye, so that its whole length must have considerably exceeded forty miles, beyond which distance it became more diffuse and attenuated. Had I not known to the contrary, I should undoubtedly have believed that what I saw was due to volcanic eruption.

The vapour caused by the lower temperature of the atmosphere at the level of the top of the bluff face was obviously carried away by the breeze gradually as it was formed, thus producing by a continuous process of generation the long extent of cloud which I have described. This fact shows that clouds may be due to deflections produced by irregularities on the earth's surface far remote from the place where we actually see them. I may mention, in proof of the steady nature of the breeze and of the entire absence of any vertical disturbance in

* By Thomas Stevenson, F.R.S.E.

the atmosphere, that later in the day we traced the smoke from the steamer's funnel for a distance of nearly fifteen miles.

The accompanying woodcut is from a sketch which I made on board the vessel at the time, and I doubt not will be interesting to your readers.

SCIENTIFIC SERIALS

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, August 15.—This number contains a description, with diagrams, of Theorell's printing meteorograph, a very ingenious instrument, likely to be of much service in meteorology. It differs from other meteorographs in this, that instead of tracing curves, which have to be afterwards translated into figures, it prints the figures at once, thus saving much future trouble. One of the three already made has been in use at the Royal Observatory of Vienna since September 1874, and has been so adapted as to record, by electric communication, the state of the following instruments, placed in any situation: anemometer, vane, wet and dry thermometers, and barometer, once in every quarter of an hour. The moving force is a galvanic current connected with a clock. Dr. Theorell's account of the instrument referring to the plates will be continued in the next number of the *Zeitschrift*. In the "Kleinere Mittheilungen" Prof. Hoffmann, of Giessen, compares the sum of the daily maxima of solar radiation in several years with the time of the flowering of certain plants. His results in 1875 bear out his expectations derived from four previous years' observations, 1866-69, and in certain cases his forecast of the time of flowering was nearly correct.—There is besides a paper by Dr. Schreiber on a new registering air thermometer; also a letter from Mr. Ferrel on the theory of storms.

Jahrbuch der Kais.-kön. Geologischen Reichs-Anstalt, Band 24, heft iv.—Nearly all this part of the *Jahrbuch* is occupied by the second part of Dr. Guido Stache's elaborate memoir on the Palæozoic regions of the eastern Alps. In this part he summarises all that is known respecting the geology of the western slopes (Cadoric Alps) of the area embraced in his review.—The only other paper is one by M. V. Lipold—"Explanation of the geological map of the environs of Idria, in Carniola." A coloured map and plate of horizontal sections accompany the paper.—In Dr. Tschermak's "Mineralogische Mittheilungen" Dr. R. v. Drasche concludes his paper, entitled "Petrographic-geological Observations on the West Coast of Spitzbergen." The editor describes the *Labradorite* of Verespatak; and a notice of two other minerals, *Famatinite* and *Wapplerite*, is given by A. Frenzel.

THE *Boletin de la Academia Nacional de Ciencias exactas en la Universitad de Cordova* (*South America*), Entrega iii., 1874, contains some papers of interest. We note the following:—On the chemical composition of the water of the La Plata River, by Señor Kyle.—On the formation of saline deposits, by D. Fred. Schickendantz.—On the chemical and physical action which took place in the formation of the pampas of Cordova, by Dr. A. Doering.—Critical notices on some entomological publications, by Dr. D. C. Berg.

THE *Annali di Chimica applicata alla Medecina* (August) contain the following papers of note:—On salicylic acid, by Dr. D. Gibertini.—Note on chloral-santonine, by C. Pavesi.—On the health of smokers, by Dr. Bertherand.—On the substitution of iron shot for lead shot for the purpose of cleaning bottles in hospitals, barracks, &c., by Sig. Fordos.—On the comparison of human milk with cows' milk with regard to the nutrition of infants, by Ph. Biedert.—A number of papers of minor interest.

SOCIETIES AND ACADEMIES

VIENNA

Imperial Academy of Sciences, July 15.—On the solubility of calcic chloride in water, by H. Hammerle.—On the decrease in the temperature of the maximum of density of water through pressure, by C. Puschl.—On the system of vessels of the tube-bones, with notes on the structure and development of bones, by C. Langer.—Researches on the capacity of gas-mixtures for conducting heat, by J. Plank.—On the theory of the composite eyes and the seeing of motions, by Dr. S. Exner.—On the graduation of induction apparatus, by Dr. E. Fleischl.—Researches on the motion of the imbibition-water in wood and in the membrane of the vegetable cell, by Prof. Wiesner.—On the morphology and biology of Lenticellæ, by G. Haberlandt.—Meteorological observations made at Hohe Warte, near Vienna.

July 22.—(Last meeting before holidays).—Remarks on the variations in the velocity of light passing through quartz which is subjected to pressure, by J. Merten.—The Crustacea, Pygnogonida, and Tunicata of the Austro-Hungarian North Polar Expedition, by C. Heller.—On the finer structure of bone substance, by Prof. von Ebner.—On the construction of the reflection goniometer, by Prof. von Lang.—(The next meeting will take place on Oct. 14.)

K.K. Geologische Reichsanstalt, May 31.—Report from Dr. O. Lenz on his travels in Africa.—On the occurrence of marine petrefacts in the Ostrau layers, by D. Stur.——On the coal deposits of Drenovec, by Dr. R. Hörnes.

June 30.—On the Island of Kos, by Dr. M. Neumayer.—On fresh-water strata amongst the Sarmatic deposits near the Sea of Marmora, by Dr. R. Hörnes.—On the landslip near Unterstein, on the Salzburg-Tyrol Railway, by H. Wolf.

July 31.—On some fossil plants from India, by O. Feistmantel. On the formation of the terra rossa, by Th. Fuchs.—On mountain folds, by the same.—On secondary infiltrations of carbonate of lime into loose and porous formations, by the same.—Report by D. Stur on his travels in Silesia.—On the fauna of the Schliers of Ottnang, in Upper Austria, by R. Hörnes.

STOCKHOLM

Kongl. Vetenskaps Akademiens Förhandlingar, March 10.—The following papers were read:—Genera et species Lithobioidarium disposuit, by A. Stuxberg.—Review of all Lithobioidæ hitherto known in North America, by the same.—Report on the bryological researches in Norway during 1874, by C. Hartman.—On the moss flora of Lulea (Lappmark), by P. J. Hellbom.—On the observation of two crossing rainbows, by O. Gumaelius, with some remarks on the same, by R. Rubenson.

April 14.—On the marine Entomostraca collected during the Swedish Scientific Exhibition to Spitzbergen, by W. Lilljeborg. —On the formation of the smaller bays, of the river valleys, of lakes, and of sea banks, by A. Helland.

GÖTTINGEN

Nachrichten von der königl. Gesellschaft der Wissenschaften, Aug. 7.—The following papers were read:—On lens fibres, by Prof. J. Henle.—On the linear differential equations of the second order which possess algebraic integrals, and on a new application of the "invariant" theory, by Prof. L. Fuchs.

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ERRATA.—Vol. xii. p. 455, col. 1, line 8 from bottom, for "time t" read "very small time t." P. 463, col. 1, line 21 from bottom, for "$2_n + 2$" read "$2n + 2$."