THURSDAY, JUNE 6, 1895.

THE "CHALLENGER" EXPEDITION AND THE FUTURE OF OCEANOGRAPHY.

The Voyage of H.M.S. "Challenger." A Summary of the Scientific Results. (With Appendices). Two Parts. (London: Eyre and Spottiswoode, 1895.)

THE two new volumes of the *Challenger* Expedition have appeared, and with them this momentous enterprise has arrived at its final close. It is well worth our while to seize this occasion for a few words of reflection on a scientific drama, which is equally great in all its parts and dimensions, as in the effects it has produced and will go on to produce, on the progress of a group of sciences which every day grow more important in their influence on human intellect and thought.

It is nowadays a very common complaint, that specialisation in scientific pursuits threatens to do away with that character of universality that was attributed in former times to all those who busied their brains with the phenomena of nature. I can fully remember how, in my own childhood, the naturalist κατ' ἐξοχήν found his example in Alexander von Humboldt. He was credited with "knowing everything," and whoever followed some small pursuit as a naturalist, partook, in a certain degree, of the prestige the great "Naturforscher" enjoyed in all circles of the reading public. When I was studying zoology at Jena, a fellow-student of divinity asked me once, "Please tell me what is the name of those stars?" "I don't know, my dear friend, I am studying biology." "Oh, I thought you 'Naturforscher' study all the natural sciences."

I am afraid we are at present drifting far away in the opposite direction, and the general public is rather inclined to believe that each naturalist or natural philosopher lives on an island, of which he investigates only a small corner, without caring a bit for the rest of the island, and still less for other islands and whole continents. Whether we are quite as bad, I will not try to decide; certainly those happy times are far behind us when a professor of mathematics and astronomy taught also physics and medicine, or when botany, zoology, and chemistry were represented by the only professor of medicine, and all these things were taught merely by books and traditions. But even those modest cases of personal union between zoology and botany, or between geology and zoology, which not unfrequently occurred in the first half of our century, have passed away now at its close. Instead of such personal unions, we meet with, in a well-equipped university, distinct chairs for zoology, comparative anatomy, embryology, palæontology, geology, mineralogy; round each of these chairs we see gathering numbers of privatdocents and other teachers, who deliver lectures on distinct specialities of these sciences, which threaten to grow themselves again to independent divisions craving a chair for themselves. "Division of labour" is all very well; but if we do not in time prepare for better mental digestion and assimilation, the next century will live to see a new Babylonian turret; dispersion of languages will grow to such a degree, that even the inhabitants of the same scientific island will find it hard to talk to each other.

It is a consolation, under these circumstances, to see, that, along with division of labour, combination of labour takes its firm hold in the organisation of modern scientific life, and Moltke's maxim, "march separately, attack jointly," proves also useful in the peaceful battles of thought and science.

A splendid proof of this combination of labour lies before me in the numerous volumes of the *Challenger* Expedition. Physics, chemistry, geology, zoology, and botany, and all those nautical and hydrographical attainments of modern date, have combined to produce results which close a past of unwarranted belief, and open a future of new research, boundless in fertility of problems and of unknown possible effect on the human intellect and understanding.

The imagination of human kind from the beginning of historical ages, and along all its phases of development and evolution, took hold of those unknown regions of the heights of mountains as well as of the depths of the ocean. Covered by ice and snow, hidden in thick masses of clouds, out of which thunder and lightning and endless floods of rain and hail came forth, the ranges of mountains gave birth to the grandest and most appalling visions of powers, upon which the poor human individual looked aghast, against whose mighty influences he felt helpless, and whom he dreaded and revered. Every human being becomes a poet under the influence of fear and reverence. Both magnify and intensify impressions, even of the most common kind, and create combinations where the acutest observer could not discover any connection. Thus the oldest forms of religious belief, as well as the numerous forms of still existing superstitions, have peopled the tops of mountains and the depths of the seas with images of supernatural powers; the Olymp of Hellas, and the old German myths, the Hebrew Jehovah, and the rudest Paganism, found their abodes beyond the clouds, and below the waters. And who can resist the temptation of such dreams, grand and awful at once, when standing on those solitary heights of the Alps, with ice and snow, and rock and cloud around him and below him, and looking over endless ranges of peaks and valleys? Who is not struck by the image of death and destruction, when he wanders on the volcanic deserts of Etna, where there is not one leaf of grass, not one smallest insect to keep him company? And in the midst of the raging ocean, with waves dashing against the poor ship, and clouds spreading darkness around, who will refrain from images of terror created by the imagination of the boundless depths to which he has trusted his life? Will there ever come a time when the human mind replaces such emotions by the cool reflection that the minimum or the maximum of atmospheric currents and pressure causes the disturbance of equilibrium on the floods of the ocean to such a degree as to shake the balance of the floating mass of wood or iron, on which he happens to find himself, and bring its metacentre to a position which enables the water to supplant the air-filled spaces until the greater specific gravity of iron carries all away, through the lamina of the hydrosphere, down to the lithosphere, which resists further gravitational concurrence? And will ever barometer and thermometer, or the observing eye of the geologist, caught by phenomena of denudation or glacial erosion on Mont Blanc, diminish the trembling of emotion when the eye measures the enormous distances it commands from such a height? Whoever has experienced the thrilling delight of that other emotion caused by insight and discovery; whoever knows that intellectual powers can produce as much enthusiasm as artistic and æsthetical emotion, will not be haunted by the sickening dread that human imagination could become stripped naked by the impious hand of science. Whoever cares more for the Why, than for the How, will gather around the temple of science; but those gifted natures, who are impressed by colours, shapes, and situations, why shall they not go on to shake their kaleidoscope of beauty and appearance, just as much as these go on drawing invisible threads of cause and effect between old and new facts?

Let us therefore not quarrel with the natural growth of the human mind, but rather accept in delight all such actions as include a great increase of knowledge in regions where ignorance lent the hand to superstition; and so let us hail the work of those who lifted a piece of the thick veil that covered the abyssal depths of the ocean.

It will always be one of the greatest of the many merits of the late Prof. W. B. Carpenter to have given the first suggestion to the *Challenger* Expedition. Not content with asking the Council of the Royal Society to throw in its authority with the British Government to undertake a new and complete course of research for the exploration of the deep sea, he entered into direct correspondence with the First Lord of the Admiralty, and carried his point so far as to receive the answer that the Government would be prepared to give the requisite aid in furtherance of such an expedition on receipt of a formal application from the Royal Society—in consequence of which answer the Royal Society at once proceeded to take these necessary steps; and after exchanging some correspondence with the Secretary of the Admiralty, the proposal to defray the expense of such an expedition out of the public funds was brought before Parliament and "received the cordial assent of the House of Commons" in April 1872.

It is to be lamented that in the "Narrative of the Cruise," neither the proposition of the British Government nor the debate of the House of Commons are reproduced literally. It would have been of high historical interest to the general, as well as to the special, reader to know exactly the wording in which the proposition was formed, and the views and opinions with which it was received. It is, perhaps, not possible to the editor of NATURE to supply even now this omission, but yet many in the outer world would greatly desire a reprint of the day's discussion which produced results so momentous as that great and memorable expedition of the *Challenger*. In uttering this regret, I can assure the British reader that, though a foreigner, I feel deeply my share of gratitude to both Government and Parliament of Great Britain. I cannot omit this occasion to congratulate science for having her wants so well interpreted, understood, and satisfied by all those who have a share in the *Challenger* Expedition, be it the Government or Parliament, be it the officers and crew of the ship, or the scientific staff and the authors of the voluminous reports lying before me.

And I may be permitted to claim some personal license to proffer my thanks in the name of science, and especially of biological science; for at the time when Dr. Carpenter and the Royal Society asked the British Government to undertake the expedition, I was myself engaged in a collateral enterprise of similar tendency, and felt the same necessity to ask for help and assistance of the authorities of the German Government, and, in smaller degree, of the Governments of almost all civilised States and nations. A few years after the British House of Commons had "cordially assented" to the proposition of the Royal Society, and voted the funds demanded by the Admiralty, the German Reichstag passed a resolution, based on a petition of Helmholtz, Dubois-Reymond and Virchow, by which the Government of the empire was asked to grant an annual subvention of £1500 to the Zoological Station of Naples, a subvention not only continued up to this date, but four years since increased to £2000. These votes of the two great parliamentary bodies go far to disprove the old doctrine, that science and the promotion of research are to be abandoned to private enterprise, and to the favours they may meet with accidentally in raising money by public subscription, or falling in with wealthy private persons whose interest and generosity can be won over. I am afraid, if the House of Commons had not granted the necessary funds, the *Challenger* Expedition would never have taken place, and our ignorance about the many great and innumerable smaller questions connected with the deep-sea problems would be still the same as in 1872. Had not the German Reichstag voted in favour of the Zoological Station, all my personal efforts would have failed, and neither the Naples Station nor the Plymouth Laboratory, nor, perhaps, the many other imitations of "the big brother at Naples," would have had the chances with which they have met now. No; let science not be immodest and ask for all and everything from the State; but let it still less linger on and wait for the chances, growing always scarcer and scarcer, of being endowed by private source, be it public subscription or donation from wealthy men and amateurs. The number of persons combining great wealth with sufficient culture is unfortunately not on the increase; inherited wealth, which offers more chance for the acquirement of higher intellectual pursuits, is decidedly diminishing. The demand for funds for the endowment of research is doubtlessly augmenting, and the competition in the advancement of science is such, that the nation which is not ready to pay its share, will either be thrown in the background, or live like a parasite on the intellectual blood of its neighbours. How long such a parasitic existence could be protracted, remains to be seen; but certainly no great nation will deliberately accept such a disgraceful situation, the more since it cannot be doubted that each nation has its peculiar gifts and talents, which make its co-operation indispensable in the chorus of other nations and in the interest of humanity. It must be granted, that the weight of a nation in the scale of culture depends on the power and number of men of genius it has produced and goes on to produce; it may also be granted, that a genius has been known to open up his own ways and make his career through all the adversities of fate. Yet a genius needs to feed quite as much, or perhaps more than an ordinary mortal, and some think it would be economical o give him at least average chance. Would any genius have been capable of diving, on his own account, to the great depths of the Pacific? or would a genius find it possible to replace, by his own work, the ant-like activity of the Naples Station? Hardly. But let him come now and handle the innumerable data of the *Challenger's* investigations, or use the opportunities offered by a modern laboratory to give us a solution of the problem of heredity, or decide whether natural selection suffices to account for the evolution of the organic world, or whether other principles must be sought. The genius of Pasteur and Lister and Koch have opened the enormous field of research regarding the nature and effect of bacteria, and I think the world has not been the worse for France and Germany spending public money for the equipment of large laboratories to enable those geniuses to continue, in the most effective way, their labours.

Certainly not every whim or fancy of a learned individual can be accepted as a sufficient reason for spending public funds; some sort of a controlling apparatus will always be necessary. But in the Royal Societies, National Academies, and other learned bodies of high standard, each nation has already what is wanted, and it is understood well enough that such bodies are often even more difficult to be won over by some new rising genius than a Minister of Public Instruction or the outside public. It is, therefore, not to be anticipated that from the Scylla of nihilism in officially supporting research, one must necessarily glide down into the Charybdis of supporting whatever scheme comes out of the fervid brain of a young discoverer. But this much can be said, or repeated over and over again—for it is certainly no new truth—that the mental and intellectual productions of a nation ought not to be the last, nor the least, in their claims on the public money; and it may be maintained with all confidence, that hardly any other expense will so amply repay the budget of a nation, both materially and ideally, as the funds handed over for the promotion of research, or, in the truer expression, for the *organisation* of research.

For it is in this, that the real future lies: in organisation. Being organised, the small Japanese empire was more than a match to the tenfold bigger Chinese mass: being organised, a few British regiments can keep populations in abeyance, which, if they were equally well organised, might crush them in a moment. And to be organised, even in the intellectual sphere, means to economise natural powers and not throw away chances, which if they cannot perhaps be brought about deliberately, nevertheless can be profited by when they occur—and they occur always and everywhere.

Organisation of research, will, I do not doubt, become the special feature of the coming century. It would be well worth to provoke discussion about schemes, ways and channels, into which organised research ought to grow. Each nation may adopt its own, according to its character, habits, and prejudices. But one feature ought to be observed with them all, for it will soon become uppermost; that is, *international organisation* of those interests and productions by which all the nations may be benefited together, without being forced to arrange separately, each for itself, what more effectually and with less material and intellectual effort can be provided for all of them at once. And there can be no doubt that foremost, in this regard, stands the question: *How to reorganise, or organise at all, scientific publication?*

It cannot be doubted that the way in which we deliver over to publicity at present the results of the work of hundreds and thousands of investigators, is all but destitute of any regulating principle. Publishing in the nineteenth century resembles very much the kind of warfare practised in bygone times, when regiments were the property of single individuals, who were responsible for their equipments, nourishment, efficiency, and who entered into contracts with their men and soldiers and with states and princes. Defection on the one side, plundering on the other, were concomitant features of such arrangements, which one only need compare with the present constitution of the Prussian army to feel at once what powerful element organisation has proved to be. Why shall the most subtle of human activities, the mental and intellectual functions, not be liable to profit in the same degree by organisation? Why shall prejudice and egoism be permitted to govern with almost absolute sovereignty in the lofty regions of thought and speculation, of experiment and observation—in one word, of research? Organisation is not pedantry, discipline not slavery, genius no direct contradiction to order and measure. Originality and individualism will neither be sacrificed nor diminished, if certain rules are observed in bringing the results of investigation to public knowledge, and a better, more economical, and more effective system of reporting and recording is adopted, with the intention to facilitate the communication of valuable scientific results over the greatest possible circles of competent readers. It is true that the all-powerful *vis inertiæ* will go far in opposing any serious attempt of reorganisation in this department; but, as I remarked at the commencement of this article, unless we put hands and shoulders to the work, we shall unavoidably arrive soon at a state of chaotic confusion, where the worse elements may be conspicuous, and valuable productions at times be choked among mediocrity.

It would lead me too far away from the direct subject of this article to develop here any scheme of better arrangement for scientific publication; and if I am not mistaken, the feeling that such arrangements ought to be found and to be universally introduced, is spreading rapidly among competent and conscientious men of science. Let these soon unite and form national and international centres for the organisation of scientific publication—a more wholesome influence on the progress of science and research can hardly be imagined nowadays.

The two new and last volumes of the "*Challenger* Report" are the work of Mr. Murray, the true soul of the expedition, to whom science owes a great debt of gratitude for his never-ceasing care and toil, and for his talent and amiability, with which he undertook the great burden of superintending the publications of the expedition, besides himself adding most remarkably to the vast amount of new knowledge regarding the deep sea.

In the "Editorial Notes" to these two volumes, Mr. Murray has some paragraphs on the whole expedition so characteristic that I think it right to repeat them here to every reader who does not happen to lay his hands on the volumes themselves. Mr. Murray, after having given an account of how in general the collections and the reports on them were disposed, adds the following :—

"From beginning to end the history of the *Challenger* Expedition is simply a record of continuous and diligent work. There were few opportunities for brilliant exploits during the voyage. The daily and hourly magnetic and meteorologic observations, the handling of the ship during the tedious deep-sea investigations, the work connected with the boat excursions and expeditions on land, in addition to the usual operations of the marine surveyor and navigator, all demanded from the naval officers and seamen an amount of care and attention far surpassing what is required during an ordinary commission of one of Her Majesty's ships. The labour connected with preserving, cataloguing, and packing the biological and other collections on board ship was enormous, so also was that involved in their subsequent examination on the return of the expedition and their distribution to specialists in many parts of the world. All this was, however, accomplished with success, and the typical collections have now been deposited without any mishap in the British Museum. The majority of the authors of the special memoirs have spent years in the examination of the collections and in the preparation of their manuscript and illustrations for the press, without other remuneration than either a copy of the *Challenger* publications or a small honorarium to cover the outlay necessitated by their researches. The payments of the civilian staff have been very moderate, and in my own case, at least, have not covered actual expenditure in connection with the work of the expedition.

"The great difficulty in carrying through an undertaking of this nature arises from considerations of time. The researches of the specialist tend ever to become more elaborate : in no case were the authors of the larger special reports able to terminate their work within the original estimates as to time and bulk. The limitations in reference to expenditure imposed on me by the Government often rendered it imperative to curtail the investigations, and to cut out matter from the memoirs when, in other circumstances, I would gladly have fallen in with the views of contributors and collaborators. The researches and publications connected with the expedition might have been extended in several directions with advantage to science had the allotted time and funds permitted ; as it is, a few collections have not been thoroughly examined, and some observations have not been fully discussed.

"In June, 1872, I was appointed one of the naturalists of the *Challenger* when the expedition was being fitted out. During the past twenty-three years my time has been wholly taken up with the work of the expedition and in the study of those subjects which the expedition was organised to investigate. The direction of the whole of the work connected with the publication of the scientific results passed unexpectedly into my hands, and I have done my best in the circumstances to place on permanent record a trustworthy account of the labours of this famous expedition. It has been my earnest endeavour to complete the publications in a manner worthy of the naval position and scientific reputation of this great empire. Notwithstanding the troubles, personal sacrifices and regrets necessarily connected with the work, it has been a pleasure and an honour to have taken part in explorations and researches which mark the greatest advance in the knowledge of our planet since the celebrated geographical discoveries of the fifteenth and sixteenth centuries."

It is hardly possible to speak in a more truthful, simple, and dignified manner of one's life's work than here Mr. Murray speaks of the work and the difficulties that beset the *Challenger* Expedition, "cujus pars magna fuit." He might have used quite other language, and have felt sure to meet the full acknowledgment of his contemporaries, and nobody will certainly dispute him the proud sentence with which he finishes the above account. There can hardly be any doubt about the epoch-making importance of the *Challenger* expedition, and if in the first letter of Dr. Carpenter to the Royal Society attention is drawn to an article in this journal (NATURE, vol. iv. p. 107, 1871), in which it was stated that the Governments of Germany, Sweden, and the United States were preparing to dispatch ships to various parts of the ocean, expressly fitted for deep-sea exploration, and the question put forward, whether Great Britain should not step in to do her share in such work, I think it might well be urged now, after Great Britain having done her work in the most unparalleled way, that other nations might continue and profit by the experience of the *Challenger*. Such expeditions may be undertaken by deliberately dividing the task of filling the gaps and lacunes left by the *Challenger*, one nation taking the Atlantic, the other the Indian, a third the Pacific, and a fourth especially the Antarctic Sea for its investigation and exploration. A large basis has been laid by the *Challenger*, capable of bearing any superstructure to be erected on it. Let France and Germany, the United States and Russia take up this work after a mutual understanding, let Sweden or Norway explore once more the North Polar Sea, Italy the Red Sea, and let international organisation add a second chapter to oceanography, after the first has been so well worked out by Great Britain.

Nevertheless, whatever important results may be arrived at by such repeated expeditions, embodying both principles—division of labour and combination of results—the future of oceanography requires still other means of research. Whenever a new domain of science is opened up, either by the isolated work of a discovering genius, such as Pasteur and Koch, or by combination of rarely found chances, such as the *Challenger* Expedition offered, the immediate consequence is that specialisation sets in to work out all the different chapters of the new doctrine, enlarging the basis, multiplying the parts, drawing new conclusions, correcting old ones—in short, bringing about a detailed colonisation of the newly-discovered intellectual areas. But no oceanic or African colony can live and prosper nowadays without well-established communication with its motherland ; no haphazard visits of travellers can supplant the permanent and systematic exploitation that alone provides those conditions of life which make a colony prosper. And the same holds good for intellectual colonising, and especially for problems of oceanography.

If we look over the fifty volumes of the "*Challenger* Reports," we see, at once, that the lion's share belongs to biology. More than nine-tenths of them are purely biological, and almost all the others include some important biological elements. It is therefore hardly wrong to suppose that the future of oceanography will lie with biology, and with its ways and means for increasing our knowledge. The problems of biology, of course, are extremely varied, and many of them may be studied in every inland university. Not so the problems of marine biology, for which the last twenty years have established the utter necessity of laboratories near the sea-shore. Here we are only in the beginning of a movement, which will go far to increase our knowledge of the conditions of marine life.

If the establishment of marine laboratories on different parts of the Mediterranean and on both sides of the Atlantic—not to speak of the North Sea and the Baltic—have proved a necessity: if already, both in Japan and in California, the coasts of the Pacific have been provided with such scientific outposts, it cannot fail that, by-and-by, Africa, Australia, and the Polynesian Archipelago will also have their biological stations. It is a great pleasure to me to be able to state here, that a small beginning is being made at Ralum in Neu Pommern (alias New Britain), the neighbour island of New Guinea, from whence numerous specimens of *Nautilus pompilius* have lately been procured. An intelligent and enthusiastic German planter, Mr. Parkinson, living since many years on that island, visited me a year ago in Naples, and offered spontaneously his help and services to establish a small station on his own land. According to his views, locality and climate will favour such a plan, and as there is every six weeks a steamer of the North German Lloyd from Ralum to Singapore, and soon perhaps another one to Sydney, the possibilities of a tropical archipelago station are given. The Naples Station has undertaken to provide the scientific equipment of its infant brother at the Antipodes, and my friend Major Alex. Henry Davis, from Syracuse (New York), who, already helped so much to establish lasting and fruitful relations between the United States and the Naples Station, has again stepped forward to provide for the first pecuniary wants of the Papua Station. Let us hope that this small beginning will reap some fruits, and the more so, as Mr. Arthur Willey, well known by his work on the development and morphology of the Tunicates and Amphioxus, has gone there as first pioneer of biology to study the development of *Nautilus pompilius*. His impressions have been as yet very favourable, and he thinks that the fauna of New Britain will amply repay every sacrifice of Mr. Parkinson and Major Davis. If the local authorities of New South Wales, or Victoria, or New Zealand, would find it worth their while to help to a laboratory in Port Jackson, or somewhere else in Australia: if in the Cape Colony somebody would do as Mr. Parkinson has done—numerous problems thrown open by the work of the *Challenger* would make progress, and the threads of biological study would draw nearer and nearer to encircle the most distant parts of the oceans.

But the greatest stroke would come, if one nation or an international combination would present biology and oceanography with a steamer, expressly built for purposes of such research as the *Challenger* performed. In the year 1884, I attempted something of the kind by forming a committee of influential men in Germany for the purpose of collecting £15,000 to £20,000, with which to build a yacht large enough to go round the globe, and serving as a floating biological laboratory. Of course it was not the sum of money wanted for the construction of such a ship which constituted the main difficulty of the scheme, though I failed even in that from reasons which had nothing to do with the scheme itself. The true difficulties lie in the extraordinary great regular expenses in commissioning such a ship, as every owner of an ocean yacht understands. Of course I was also prepared for that, and have no doubt that my plans would have answered, at least to some extent, but I was compelled to recognise the truth of the old proverb, "qui trop embrasse mal étreint." I do not know whether I shall yet be able to return to the attack; it seems rather unlikely, but it is my firm conviction that this scheme is, if not the only one which will permit us to conquer the battlefield, at any rate the chief means to enlarge our knowledge in oceanography, and will and must therefore be executed in no distant future.

Such a ship ought not to be continuously crossing the oceans; on the contrary, its best services would be rendered by giving it the chance to thoroughly investigate distant areas for distinct problems. Give such a ship the commission to study in the greatest possible detail, and in a comparative way, life and formation of the coral reefs in the Indian Ocean; let it be stationary for months together on the most favourable spots for such a study; prepare a scientific staff of specialists for the work, land them where the best opportunities for a transient establishment of a small laboratory is to be got, assist them by as many hands of the crew as can be spared, help them by the steam-pinnace on board, use the diving dress as well as native divers, and study for hours under water the construction and the destruction of the reef, apply all kinds of dredging and surface-fishing at day and night, have well-trained laboratory servants for the preservation alive and in alcohol of such organisms as are required for further study,—in short, do as if a well-appointed laboratory were transported to Polynesia; and be sure that results will ensue which by no other contrivances can possibly be obtained, especially if the ship be under no restrictions, and can stay in any one spot as long as may be requisite.

For it is the great drawback of the usual men-of-war expeditions, that they are only allowed a few days or weeks to remain at the same locality. There are so many other objects, to which it is necessary to give full attention, that they are always driven away from the work when the preliminary difficulties are just overcome. Science must be sovereign on board, the scientific leader must be absolute for determining the course to take and the time to remain. Discipline on board the ship must, of course, be handled by the captain or his officers, but the general dispositions of the work must remain with the scientific leader. That alone already would make a great difference in such an expedition from all those antecedent, and though very often the naval captains of expeditions for scientific purposes might well enough be transformed also into scientific leaders, nevertheless they are dependent on orders from home, and cannot always understand the importance of embryological, physiological, or other specialist work, for which they have to stay a month or two longer in the same harbour.

Again, the scientific staff must be selected with greatest care in regard to technical and personal accomplishments. If the staff is not varied enough, and does not include men of different attainments, many opportunities for investigation must be lost for want of previous knowledge on the side of the naturalists on board. On the other hand, nothing is more difficult than to live together for months, or even years, on board a ship, for men not well trained to such existence, except where the composition of the staff is made with a sharp eye for compatibility and incompatibility of character. Especially the scientific leader must be a man of imposing personality rather than of special scientific competence, for it will fall to his share to dictate in every case where conflicting tendencies threaten to do away with social harmony.

But though all this may be considered to offer considerable difficulties in the way of execution, nevertheless the future for oceanography will belong to such floating biological stations, and the time is perhaps not so far distant, when more than one of them will cross the oceans, and supersede completely the now adopted system of single-handed expeditions of young naturalists. The necessity for such expeditions is doubtless existing, in so far as it is better to try the solution of problems regarding the tropics by travelling alone than by staying at home. And no doubt very many geographical, ethnographical, geological problems have been greatly advanced by competent travellers, and will furthermore be advanced in the same way. Collections of animals and plants have been made, mostly terrestrial, and the systematic part of biology has had its due share. But all more complicated studies, such as require more technical appliances and preparations, remain in the background, for the same reason which has forced us already in Europe to establish well-organised morphological, physiological and chemical laboratories, both inland and on the sea-shore. And if we cannot go on without them in Europe, where the general conditions for biological research are so much more advantageous, we must certainly have them, if we wish to advance our knowledge of tropical, terrestrial and marine organisms.

Botany enjoys already some advantages through the botanical gardens in Ceylon and Java, and it is to be hoped that the British and the Dutch authorities will use their exceptional opportunities in both places to establish some sort of regulations for their use by the botanists of all nations. May it not be possible to enlarge these botanical gardens by adding also some facilities for research of animal morphology? The Zoological Station at Naples has a special part prepared and equipped for morphological and physiological botany; in the first place, of course, for marine algæ, but any other sort of botanical study, for which Naples offers opportunities, might be undertaken there, and already a valuable work on the cultivation of figs has been greatly assisted by the Zoological Station. No doubt every naturalist who travels in Ceylon or the Sunda Archipelago receives the most valuable advice and assistance by Messrs. Trimen and Treub, and perhaps these most competent gentlemen would be the first to advocate a larger endowment of their establishments in the sense just now indicated; science and research would be certainly greatly benefited by it.

All these dreams and perspectives are opened up before us when we are looking over the enormous mass of new facts and new material for study brought together by the *Challenger*. And to think that there were only four naturalists and one chemist on board all the years long, and one of the naturalists died during the expedition! It is, I think, only right to remember here that two others of the gentlemen of the civilian staff so heavily overtaxed their strength with the often surely very monotonous, and always very hard work, that their health broke down soon after their return, and they fell victims to their enthusiasm. If it is only right to pay the highest possible respect to Mr. Murray for his extraordinary power of work, talent for administration and competence in dealing with the special problems of deep-sea deposits, and if we gladly recognise the excellent work done by Mr. Buchanan, I think nobody will be so ready as these two gentlemen to join here in thankful remembrance of the share of work that fell to their late companions, Sir Wyville Thomson, Prof. Moseley, and Dr. von Willemöes-Suhm. And may it be once more permitted to the writer of these lines, who by right or wrong claims some special title for it as a sort of international official of biological science, to utter the thanks of science to the officers and men of the *Challenger*, and to the Admiralty, and to the British Government and Parliament, and to the whole British nation for having set the example to the world of one of the grandest and most successful scientific expeditions that ever has been, and most likely for considerable time to come will be, started.

ANTON DOHRN.

OUR BOOK SHELF.

Horses, Asses, Zebras, Mules, and Mule Breeding. By W. B. Tegetmeier, F.Z.S., and C. L. Sutherland, F.Z.S. (London: Horace Cox, 1895.)

THE first portion of the title of this interesting work is somewhat misleading, for with the exception of some half-dozen pages which deal mainly with the distinctions between the horse and the other species of the genus, and a description of the supposed new species known as Prejevalsky's Horse, the book entirely relates to asses, zebras, and mules. None of the varieties of the horse which have been produced during the period of its long domestication are referred to. We mention this fact in case the general reader should infer from the title of the work that it was a treatise on the multitudinous domestic varieties of the horse which exist in nearly every quarter of the globe.

The volume is conveniently divided into two parts. Part i. is chiefly of zoological interest, and contains very complete and accurate descriptions of the existing species of the genus known to modern zoologists under the name of Equus, including, in addition to Prejevalsky's Horse, an account of the still more recently discovered Grevy's Zebra. The engravings which illustrate the letterpress are particularly good, and will greatly assist the student in his endeavour to master the peculiarities of each species. It concludes with a chapter on the hybrids which may be produced by crossing the horse with the other species of the genus Equus.

Attention should be directed to an assertion on the part of the authors that a remarkable and noticeable difference exists in the period of gestation of the mare and ass. The duration of gestation in the mare is well known to be eleven months, and it has generally been assumed that it was similar in the ass and zebra. The authors, however, emphatically assert that in asses and zebras it usually exceeds twelve months; one of them, Mr. Sutherland, who is well known as an extensive breeder of mules, quotes from his stud-book eight instances of the period of gestation in the ass, the result in six cases of a single service, the period varying from 358 to 385 days. It seems strange that such a marked difference should have hitherto escaped mention in all previously published works.

Part ii. is devoted exclusively to mules and mule breeding, and is replete with valuable and exhaustive information on these subjects. The authors strenuously deny the existence of fertility in either the male or female mule, affirming that abnormal lactation not unfrequently occurs in female mules, when milk is secreted in great abundance, and that the foals which they are observed to be suckling are in reality the foals of other animals which the mules have adopted. With regard to the oft-quoted instance of a mule in the Acclimitisation Gardens in Paris, which has produced foals when mated both with the horse and ass, the writers doubt whether the animal is a mule, and assume that she is an ordinary mare, whose female parent was influenced by a first alliance, as is so often the case in dogs and other animals. If their contention is correct, the mule may still aptly be described as "an animal of no ancestry and with no hope of posterity."

The writers are enthusiastic, nay even fulsome in their praise of this hybrid, and bitterly lament the lack of appreciation in which it is held in Great Britain as compared with America and some European States. "In endurance," say the authors, "capability of hard labour, economy in keep, longevity, and freedom from disease, mules far surpass horses." Into so controversial a matter this is not the place to enter, and we must content ourselves with the belief that so plain and oftentimes so ugly an animal as the mule will never supplant to any great extent, in this country at least, the beautiful and graceful varieties of the horse of which Englishmen are naturally so proud.

To any of our readers who are interested in the subject of mule breeding, this work may be heartily recommended; and, in conclusion, we feel bound to compliment in the highest terms all who have been instrumental in its production. W. F. G.

The Moon. By T. Gwyn Elger, F.R.A.S. Pp. 174. (London: George Philip and Son, 1895.)

IN this latest work on the moon, from the pen of one of the foremost of British selenographers, the most noteworthy feature is the excellent chart, eighteen inches in diameter; this is given in four quadrants, but it can also be obtained complete and separately. All the named formations are distinctly shown, and the names of the more important are very clearly printed on the map itself. The greater part of the text resolves itself into a descriptive index to the map; but though this appears in rather stereotyped fashion, it embodies a good deal of information which has been gleaned by the author during many years of observation. An introduction of forty pages deals with lunar phenomena generally, and includes numerous hints which will be of use to the observer. Mr. Elger objects most emphatically to our satellite being spoken of as a changeless world, and justifies his position by stating that volcanic outbursts, producing mountains as large as the Monte Nuovo, might occur in many parts of the moon without the world being any the wiser. Though possessing little of novelty, and not appealing to the general reader, the book and map together constitute a handy work of reference which observers of experience, as well as beginners, will be glad to have by them. A few details as to the phenomena to be observed during eclipses of the moon, might have been included with advantage.

Algebra. Part i. By M. H. Senior. (Oldham: D. W. Bardsley.)

KINDERGARTEN methods of teaching are now applied to most subjects. In this small book of fifty pages, the author endeavours to make algebra interesting to young students by associating the abstract symbols with concrete objects. The novel features of the book are the explanation of brackets, the exercises on factors, short methods of multiplication and division, the elucidation of signs, and the numerous practical examples.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Argon and Dissociation.

THE discovery of the new substance argon, by Lord Rayleigh, has given rise to a difficulty which, it is thought by some, shows that the periodic law of Mendelejeff has not that generality which has been attached to it by chemists during the last few years.

According to Lord Rayleigh's determination, the density of argon is 19·9 (H=1), making the atomic weight 39·8, as the molecules are shown to have no internal energy of the same order as their energy of translation, and hence to be monatomic. Argon with this atomic weight cannot possibly find a place in the natural classification. If its atomic weight were less than 39·1 (the atomic weight of potassium), argon would fall in the VIIIth or interperiodic group in Lothar Meyer's table; and its properties, so far as they have been investigated, would harmonise with this position.

The determination of the vapour density of iodine by V. Meyer, Crafts and Meier, and others, has shown that at temperatures below 1000° C. the gas consists of diatomic molecules, while above this dissociation takes place, and above 1500° C. we have the dissociation complete, and the molecules are monatomic.

Why, then, cannot we have a similar behaviour in the case of argon?

If argon at low temperatures (somewhere near its critical point) consisted of diatomic molecules, which dissociate as the temperature rises, the difficulty of the position of argon would be removed. Thus, suppose at the temperature at which 19·9 was determined as the density of argon, the dissociation has proceeded so far that 5 per cent. of the molecules remain diatomic; the average molecular weight would be 39·8, but we should have two kinds of molecules, monatomic and diatomic, and the atomic weight under these supposed conditions would be

$$\frac{39 \cdot 8 \times 100}{105} = 37 \cdot 9.$$

The ratio of the specific heats, at constant pressure and constant volume, taking 1·4 for this ratio for a gas with diatomic molecules, and $\frac{5}{3}$ for a gas with monatomic molecules, would be for argon, on the above supposition,

$$\frac{95 \times \frac{5}{3} + 5 \times 1 \cdot 4}{100} = 1 \cdot 65.$$

This value agrees very well with the values (1–16—1–65) determined for argon.

This explanation reconciles argon with the natural classification; and as yet no facts have been published in opposition to it.

If this hypothesis be true it could be easily verified, for at temperatures, not much higher than that at which the vapour density determinations were made, the dissociation would be complete; and hence the vapour density in agreement with a molecular weight about 38; and also at lower temperatures than that at which the vapour density has been determined the gas would not obey Charles' law; for the recombination of the single atoms to form diatomic molecules, and possibly molecules containing a greater number of atoms, would cause a contraction greater than that due merely to the cooling of the gas according to the ordinary law. PENRY VAUGHAN BEVAN.

Melbourne University, April 18.

PROF. BEVAN ascribes to me work done conjointly with Prof. Ramsay. An addendum to our paper (see *Proc. Roy. Soc.*) contains an account of experiments by Prof. Ramsay, especially directed to examine the question raised.

It has turned out that the gas possesses the same value of $\frac{pv}{T}$ as hydrogen, and that the value of this expression is not altered between − 90° and + 250°. The most trustworthy determination of the ratio of specific heats gives the number 1·65; but much dependence is not to be placed on the accurate value of the second decimal. Very concordant determinations of density gave as a mean number 19·90.

Argon, therefore, shows no sign of association on cooling, nor of dissociation on heating, as Prof. Bevan thinks it might.

RAYLEIGH.

Terrestrial Helium (?).

PROF. PASCHEN and I have lately made a careful determination of the wave-length of the strong yellow line emitted by cleveite when heated in a Plücker tube. We owe the mineral to the kindness of Prof. Rinne. My large Rowland concave grating of 6·5 metre radius, clearly shows the yellow line to be double. Its less refrangible component is much weaker, but comes out quite bright, when the stronger one is brilliant. We photographed the two lines together with the second order of the spark spectrum of iron. There are a number of iron lines on each side that are included in Rowland's list of standard wave-lengths (*Phil. Mag.*, July 1893). From these we interpolated the wave-lengths of the yellow lines by micrometric measurement. Three different plates taken on different days gave us:

Strong component.	Weak component.
5875·894	5876·216
5875·874	5876·206
5875·880	5876·196
Mean 5875·883	Mean 5876·206

We think an error of more than 0·025 very improbable.

Now Rowland's determination of D_3 (*Phil. Mag.*, July 1893) is:—

5875·982

the result of three series of measurements which he believes to be accurate to 0·02.

The difference between this value and the wave-length of the strong component is much too large to be accounted for by an error of observation.

We do not therefore agree with the conclusion, drawn by Mr. Crookes, that the unknown element helium causing the line D_3 to appear in the solar spectrum is identical with the gas in cleveite, *unless D_3 is shown to be double*. Perhaps Prof. Rowland will tell us if this might have escaped his notice. From his note on D_3 in *Phil. Mag.*, July 1893, it appears that D_3 cannot have been so wide as to include both lines, because he would then not have considered his determination accurate to 0·02. As for dispersion, one may see in his table of solar spectrum wave-lengths that he has frequently measured three and even four lines in an interval as large as the one between the components.

Hannover Techn. Hochschule, May 16. C. RUNGE.

The Origin of the Cultivated Cineraria.

I HAD hoped that it would not be necessary for me to say anything more upon this subject. But Mr. Bateson's last letter seems to require a few remarks on my part.

I confess that I find it very difficult to follow his train of arguments. All I can do is to restate once more my original position, and endeavour to see how far Mr. Bateson has been successful in impugning it. I am sorry that Mr. Bateson thinks I have "treated" him "to some hard words," though I confess he seems to me, in that matter, quite able to take care of himself.

I asserted then (*a*) that the cultivated Cineraria only differs from the wild form, putting colour changes aside, in dimensional differences. I believe that in saying this I am expressing the deliberate opinion of the Kew staff, the members of which, such is human nature, would have no hesitation in disagreeing with their chief, if they thought otherwise. To this point I do not understand that Mr. Bateson advances any serious objection.

Secondly (*b*) I asserted that these dimensional differences had been gradually accumulated. To this I understand Mr. Bateson demurs, though I fail to see that he has brought forward a particle of evidence to prove the contrary.

Now for Mr. Bateson's own position. He asserts, in common with other authorities, that the modern Cineraria is of hybrid origin. I have arrived at an opposite conclusion. And here I may quote the support of Dr. Masters, F.R.S., the well-known editor of the *Gardeners' Chronicle*, who in that paper for January 24, 1891, p. 108, states:—"Carnations and Picotees, again, which originate from one species, vary from seed but not from buds; and the same may be said of the Cineraria, the offspring of one species."

Mr. Bateson complains that I do not give "any specific answer" to the historical evidence. I thought I had made it sufficiently clear in my last letter that: (*a*) I doubted its value for scientific purposes; (*b*) I set it aside as irrelevant on account of the impossibility of proving the descent of the modern Cineraria from its supposed ancestors. Both Prof. Weldon and I have shown that the historical evidence can be handled both ways. But I prefer to set it aside altogether in the face of objective facts.

Mr. Bateson's next step is one to which I most seriously demur. He transforms a proposition of mine into terms to which I could not assent, and then proceeds to attack it. He makes me say that "to improve a plant the only safe way is by selecting," &c. I absolutely never said anything of the kind. "Improve" in horticulture is a word of large connotation. I confined myself to the production of dimensional changes, and I believe that what I said was in accordance with horticultural experience.

To demolish my position, Mr. Bateson has to get over the fact, which seems to me incontestable, that there is no essential morphological difference between the cultivated Cineraria and the wild *C. cruenta*. To do this he trots out the Himalayan rabbit. I cannot but admire his courage. What possible analogy can there be in the two cases? Two "breeds" of rabbits are crossed and produce a third *different from either*. If the modern Cineraria is of hybrid origin, then it has eliminated traces of all but one of its parents. The principle of economy of hypothesis makes me slow to believe this. Anyhow the Cineraria has clearly not produced anything analogous to a Himalayan rabbit which differs from both its parents.

As to Mr. Darwin's account of the origin of the Cineraria, I must frankly take the responsibility. I have no doubt he worked with ordinary garden kinds. He wrote to me for information as to their origin. At the time I was entirely ignorant of the subject. I wrote to Mr. Thomas Moore, who was considered the best authority on such matters, and he sent me the traditional account. I passed it on to Mr. Darwin, with the opinion, no doubt, that I thought the information trustworthy. So I am afraid Mr. Bateson is only appealing in this case from Philip sober to Philip drunk; *i.e.* from my own considered opinion to my unconsidered one.

I will now wind up all I have to say on the subject with a few miscellaneous remarks.

There can be no two opinions as to the importance of the study, from the point of view of organic evolution, of the changes which can be brought about in plants under cultivation. But it must be conducted with scientific precision. This discussion will not have been fruitless if it directs attention to the subject. A beginning has already been made. M. Bornet has worked on the genus *Cistus* at Antibes, and has reconstructed some of the forms, as to the origin of which there was only "historical evidence," described and figured by Sweet. My friend Count Solms-Laubach is engaged on the cultivated forms of *Fuchsia*, and I am quite sure that any results he arrives at may be accepted with implicit confidence. As he has asked me for species of *Cineraria*, I hope he may look into this matter also.

I must repeat my caution as to the danger of accepting horticultural evidence as to hybridity. I will give a few recent instances. I could easily give a long list with chapter and verse for each.

(*a*) *Thuya filiformis* was long considered to be a hybrid between *Juniperus virginiana* and a *Thuya*. It is now known to be a "growth-stage" of *Thuya orientalis*. The history is discussed by Sir Joseph Hooker in the *Gardeners' Chronicle* for June 22, 1861, pp. 575, 576. It affords a delightful commentary on the hybridisation fallacy and the value of "historical evidence."

(*b*) Some years ago we received at Kew bulbs of what professed to be a hybrid between *Amaryllis Belladonna* and *Brunsvigia Josephinæ*. When it flowered, it was evident that it was no hybrid at all, but only a very fine form of the former species. This is rarely propagated from seed. In this particular case seminal variation had come into play with corresponding dimensional change. The hybrid origin is recorded in the *Gardeners' Chronicle* for September 4, 1875, p. 302. It will, no doubt, be dug out hereafter as "historical evidence."

(*c*) The last number of the *Gardeners' Chronicle* (June 1, 1895,

p. 692) affords a striking instance. Hybrid Cypripediums are of considerable pecuniary value. One recently exhibited at the Royal Horticultural Society was at once denounced as no hybrid at all, but a merely seminal variation. The possessor has fears that it will "add one more to the long list of doubtful crosses by which auctioneer and purchaser are alike misled."

Notwithstanding the Himalayan rabbit, I am afraid botanists will continue to refuse to accept hybrid origin on historical evidence unless there is palpable objective proof of the fact.

There are two additional bits of evidence, to which, however, I do not attach great weight, but which may be recorded to complete the story. It is, at any rate, agreed that the Cineraria originated from the Canaries. I have already pointed out that De Candolle divided the wild Canarian species into shrubby and herbaceous. I do not believe that they are mixed in the modern Cineraria, which remains entirely herbaceous. Now, Schultz-Bipontinus, who described the Canarian species for Webb and Berthelot, relegates the shrubby species to *Senecio*, and the herbaceous to *Doronicum*. Though this is not now sustained, it shows that the two groups are not very closely related, and diminishes the probability of their freely intercrossing.

On the other hand, *Cineraria cruenta* and the modern *Cineraria* cross with the greatest facility. In fact, if you grow the two together it is almost impossible to keep the wild species true. I have no doubt that in a short time we shall be able to combine the pleasing habit of the wild plant with the fine colour of the modern strains. All this does not surprise one, as to me they are all essentially the same thing.

I must add one word more. I cannot but think that there is a growing danger nowadays of a pseudo-biology growing up for the especial use of evolutionists. This is not the first time by many that I have been so unlucky as to come into collision with it. Long ago I pointed out in these pages that biology is not a deductive science, and for the present, at any rate, theory must be adjusted to facts, not facts to theory.

W. T. THISELTON-DYER.

Royal Gardens, Kew, June 1.

MR. BATESON now admits that *some* named varieties of *Cineraria* may have arisen from pure-bred *C. cruenta*, or from plants believed to be pure-bred. He holds that these have become extinct, while Mr. Dyer believes the hybrids to have disappeared. I have never attempted to discuss this question, and shall not do so now. I wish only to justify my interpretation of the passages I quoted against Mr. Bateson:—

(1) Mrs. Loudon begins the article quoted by both of us with these words: "Most of the purple *Cinerarias* are varieties, or hybrids, of *C. cruenta*." She then goes on to say that in or about 1827 (the year in which he recommended the growth of pure-bred *C. cruenta* "for the production of fine double and single varieties"), Drummond, of Cork, produced certain hybrids; while since his time other hybrids had been made. She then, in a new paragraph, says: "Some of the most beautiful *Cinerarias* now in our greenhouses have been raised by Messrs. Henderson . . . particularly *C. Hendersonii* and the King, both raised from seeds of *C. cruenta*"; and a line or two further: "Two new ones have lately been raised, of remarkably clear and brilliant colours, apparently from *C. cruenta*, named Queen Victoria and Prince Albert," &c.

It will be seen that the general statement, with which the article begins, declares "*most purple Cinerarias*" to be "*either varieties or hybrids*" of *C. cruenta*. Of others, and of those Cinerarias (such as "the King") which are not purple, nothing is said. This general statement is illustrated by examples, first of hybrids, next of pure-bred varieties.

In discussing the examples of pure-bred forms, Mr. Bateson omits to notice "Queen Victoria" and "Prince Albert," and discusses only *Hendersonii* and "the King." He believes Mrs. Loudon, in saying that these were "raised from seeds of *C. cruenta*," to mean simply that *C. cruenta* was the female parent, the male being unknown, or unnamed. I do not know what degree of inaccuracy Mr. Bateson is willing to attribute to Mrs. Loudon; but in the writings of serious botanists a "seed" means the fertilised product of two elements, the ovule and the pollen grain; and therefore the "seed" of *C. cruenta* means the product of two parents, both of which belonged to this species.

Mr. Bateson says that six or seven years after writing the passage in question, Mrs. Loudon speaks of *C. Hendersonii* and the King as "hybrids." This simply shows that she changed her mind; and although it may affect the value of her opinion as evidence, it does not alter the plain meaning of her words in 1842.

(2) The only author whom I quoted as asserting the pure-bred origin of *C. Hendersonii* and the King was Mrs. Loudon. It is true that in two other articles quoted by Mr. Bateson these plants are called hybrids. I did not allude to this matter in my first letter, because I hoped Mr. Bateson would himself see the folly of attributing to these articles any definite meaning whatever. It will suffice to consider one of them.

In the earlier article, describing *C. Waterhousiana* (*Paxton's Mag. Bot.* iv. p. 219), that plant itself is called a "variety," although it is said to be the offspring of specifically distinct parents. On p. 221, *C. Hendersonii* is alluded to in these words: "The following are the names of some of the hybrids raised and cultivated by Messrs. Henderson . . . *C. cruenta var. Hendersonii*, *formosa*, &c." Both these passages are meaningless, if the words "hybrid" and "variety" are construed strictly. If they are not to be so construed, and they evidently cannot be, then I was justified in ignoring the passages, for they prove nothing but the incompetence of their author.

On the other hand, the passage which I did quote from this article is at least intelligible; and it asserts that *C. cruenta* "may be regarded as *the parent*"—which means, if it means anything, the *only* parent—"of many of those beautiful varieties so successfuly cultivated by Messrs. Henderson," &c. This passage Mr. Bateson does not consider in his reply to me.

The second article (*Paxton's Mag.*, 1842, p. 125) in which the King is called a hybrid, uses the word in the same loose fashion, and it would be as easy as unprofitable to quote other passages in which the same plants are called now "varieties" and now "hybrids."

Enough has been said to show that Mr. Bateson's original evidence does in fact bear the interpretation I put upon it; and further, that the words "variety" and "hybrid" are so loosely employed by early writers that their records are often of little value. Stories of hybridism and sporting are frequently brought forward on such evidence; so that I have thought it worth while to examine the case for one such story, as stated by its advocate. Having done this, my interest in the matter ends, and I do not propose to speak further upon it.

W. F. R. WELDON.

University College, London, May 31.

Some Bibliological Discoveries in Terrestrial Magnetism.

IN a letter on the above subject, by Dr. L. G. Bauer, published in NATURE of May 23 last, I read as follows: "I find it asserted that the Frenchman, L. J. Duperrey, was the first (1836) to construct 'magnetic meridians' for the whole earth, *i.e.* those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle." The writer then remarks that the honour of first introducing this method is due to Thomas Yeates, an Englishman, in 1817.

This is hardly correct, as I possess a coloured map of the Northern Hemisphere with the "magnetic meridians" as described shown upon it of an earlier date. The title of the map is:

"To George Washington,
"President of the United States of America,
"This Magnetic Atlas or Variation Chart is humbly inscribed by John Churchman."

As Washington died in December 1799, it is evident that John Churchman has a prior claim to being the first to construct "magnetic meridians."

ETTRICK W. CREAK.

London, May 31.

Effects of Earthquake in Sumatra.

ON May 17, 1892, an unusually severe earthquake was felt through nearly the whole of North Sumatra; most severely shaken was the district between the Dolok Lubuk Raja and the Gunung Talamau (Ophir). Serious landslips occurred in many parts of the mountains, especially near the summit and along the slopes of the Gunung Merapi, a volcano 2145 metres high in the residency Tapanuli. On inspection it was found that the safety of a brick pillar, erected on its most elevated point by the triangulation service, was endangered by part of the crater having been destroyed. At three metres distant from the original pillar, as much as the narrow ridge would allow, a new pillar was built, the top of which was made level with that of the original one. The measurements made in order to fix the position of this new pillar showed such differences with the original measurements, that these could only be explained by a displacement of the original pillar. As, however, neither fissures nor local disturbances of the ground could be observed, new measurements were

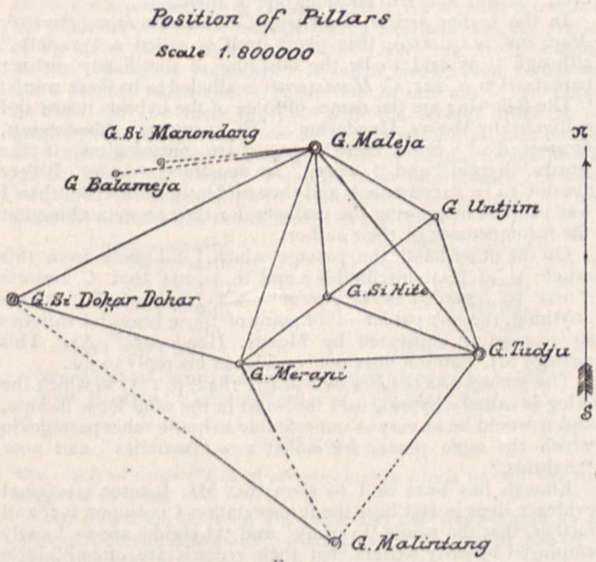


FIG. 1.

made from all the surrounding positions, and it was proved that a displacement of several more pillars had taken place.

Fig. 1 shows the position of the pillars before the earthquake; Fig. 2, their displacement by the earthquake. A detailed description of these measurements was published in the *Natuurkundig Tijdschrift*, vol. iv. part 3, by Captain Muller, the chief of the triangulation party. The longest distance over

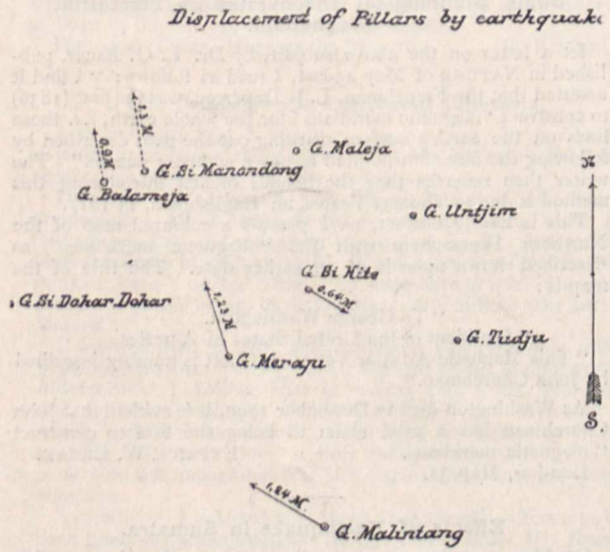


FIG. 2.

which a displacement was proved to have taken place was between the Gunung Malintang and the Dolok Balameja, or 53 kilometres. Captain Muller, however, has no doubt that if a new survey were carried on more southward, a displacement of more pillars—that is, a contortion of the surface over a larger area—would be found to have taken place.

Malang, April 14. TH. DELPRAT.

Instinct-Impulse.

THE note published in NATURE under date of April 18, in reference to my article in the April number of *Mind*, leads me to think that it may be well to explain my reasons for adopting the terminology there and elsewhere used by me, and which the writer of the note calls in question. I do so with the hope that this explanation may lead towards that "consensus of opinion on psychological nomenclature" that the writer of the note thinks is at present impossible.

The word "instinct," as my critic states, is generally applied "to the manifestation of particular activities." In other words, it is used by the biologist in an *objective* study of activities in animals, when he is not dealing with the nature of the conscious states coincident with these activities. It is thus, too, that I employ the word; but I have extended its use to cover certain manifestations of activities that do not take a large place in the considerations of the biologist, but that, nevertheless, appear to me to be of the same general nature as those "manifestations of particular activities" to which the word "instinct" is by current agreement applied.

What I claim is that the actions of one who is carried away by imitation, and the work of the philanthropist and of the artist, when objectively viewed, appear as "manifestations of particular activities," just as much as do the actions that go with self-defence and tribal protection, with care of the young, with nest-building, with migration, &c., and that therefore the term instinct, if applied to one set of such activities, may be applied to all.

If it be held that the objection to the extension of the use of the term lies in the fact that the activities that I speak of as due to the "imitation instinct," the "benevolent instincts" and the "art instincts" are not sufficiently *particular*, then I must answer that the fixedness of the actions involved is in all cases of instinct only relative; that this relative fixedness varies with the different instincts. In the self-preservative reactions, for example, we are able to predict the blow at the enemy, whilst the very varied actions by the animal mother in securing the safety of her young are unpredictable; but who hesitates to speak of the maternal "instincts"?

The word "instinct" then, in my view, should be used to indicate the manifestations of those animal activities which, when we consider them objectively, we see to have become emphasised because of racial values; of these values the acting animal (even if he be a man) may have no cognisance whatever. This is the usual use of the word, and there seems to me to be no scientific demand for any change in this usage.

On the other hand, I have suggested that we use the term "instinct feelings" to indicate the conscious coincidents of the animal activities that we call instinctive; and I have endeavoured to show that where these instinct actions are relatively fixed and forceful, then their coincident "instinct feelings" gain names, and form the class of psychic states known as the "emotions."

Furthermore, I object to the use of the word "impulse" in the description of these activities, as my critic suggests its employment, especially when they are objectively considered; for the word "impulse" is in general used to indicate those phases of consciousness which are produced by the *inhibition* of instinctive activities that have been stimulated by the presence of the objective condition that usually calls them out, but which for one reason or another are not at once realised. This, indeed, is the way in which the word is usually employed, not only by the psychologist, but in common speech as well. We speak of having an impulse to strike an enemy, not when we do strike him, but when the instinct to strike is held in check. What is more, I think this word "impulse" should be employed in this sense only; for the requirements of science do not demand its use with any other signification. I have discussed this matter of the nature of impulse rather fully at pp. 272, &c., in my book, "Pain, Pleasure, and Æsthetics," to which the writer of the above-mentioned note refers. HENRY RUTGERS MARSHALL.

New York, May 2.

THE term "instinctive" should, in my judgment, be applied to those activities which are congenital and which are also relatively definite; the term "instinct" being reserved for the subjective and affective condition of the performance of instinctive activities. Where the definiteness is the result of individual acquisition the term "instinctive" should not be applied, though it is so used by Prof. Wundt and others. The modern controversy as to the inheritance of acquired characters seems to render insistence on the congenital element advisable. Undoubtedly there is an inherited tendency to imitation; but from the nature of the case, the activity performed through imitation is not congenitally definite.

With Mr. Marshall's statements concerning impulse I cannot agree. If we say in common speech that "the instinct to strike is held in check," we also say that the impulse to strike is held in check. The control of our lower impulses is an important part of our moral life; but the contention that the impulses are "produced by the inhibition," is open to serious criticism.

THE WRITER OF THE NOTE.

RECENT EXCAVATIONS AT THE PYRAMIDS OF DAHSHUR.

FEW sources have supplied more facts for the study of anthropology than the Egyptian tombs, and the most important necropolis of Egypt is situated south-east of Cairo, close to the remains of ancient Memphis.

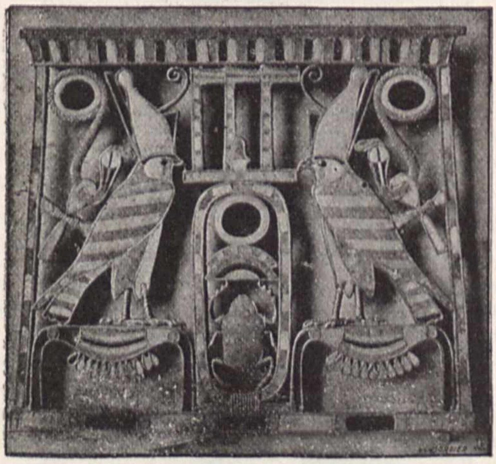


FIG. 1.—Pectoral belonging to Usertsen II. (Found March 7, 1894.)

This stretches from the village of Abou-Roash on the north to that of Mêdûm on the south, about a distance of twenty-five miles.

To the south, and at the end of the great chain of pyramids, are those of Dahshûr, of which four are of stone and two of brick. Up till 1892 the history of two of these still remained to be unravelled, but in that year a large party of excavators, headed by M. de Morgan, set out, and succeeded in opening up both these pyramids. It is to this interesting work we wish to draw attention, for it marks an important step in Egyptology, indicating some of the earliest applications of science in one direction known to us, while ancient art is at the same time illustrated. M. de Morgan has recently given an account of his explorations in *Le Monde Moderne*, and we are indebted to the courtesy of the Editor of that magazine for the illustrations of the finds.

The two pyramids are of brick, and covered with a layer of limestone; each one was surrounded by a brick wall, which showed the limits of land reserved for the use of the royal family. Round this was an avenue, left out of respect to the descendants of the gods; then came the tombs of the great people connected with the court. From inscriptions found, there is every reason to believe that these two pyramids belonged to Usertsen III. and Amen-em-hāt III., both of the Twelfth Dynasty. On the north side of the more northerly one are the tombs of some princesses, four among them more important than the rest.

These tombs have been plundered, for, owing to the Egyptian custom of burying jewels with their dead, the pyramids have ever been a favourite resort of robbers; and thus it is that some of the tombs are in great disorder, which causes much hindrance to the scientific research now being carried on, more especially as many documents have been carried away. Still, the plunderers have not stripped them entirely, and the remaining documents and treasures have been a most important clue to finding out the dates of the pyramids and the history of the people they entomb.

This spoliation of the tombs, continued by each successive generation, was not stopped till the celebrated Mariette founded the "Service for the Conservation of Monuments in Egypt."

Amongst the most interesting and perfect pieces of jewellery found are three pectorals. They were found in the princesses' tomb, and had been hidden in the soil in order, no doubt, to deceive the plunderers.

Fig. 1, the first one unearthed, has in the centre the cartouch of Usertsen II., held by two hawks, which bear the crown of Lower and Upper Egypt. The signs of the cartouch are made of cornelian, lapis-lazuli, and turquoise, set in gold; the other figures are likewise set with precious stones. The other two pectorals are similarly executed. The first (Fig. 2) represents two men, each in the act of striking with a club an Asiatic captive who they are holding by the hair. In the centre is the double cartouch of the king, and on each side the emblem of life, out of which protrude two arms holding a flabellum. Above them all is an eagle with outspread

FIG. 2.—Pectoral belonging to Amen-em-hāt III. (Found March 8, 1894.)

wings, having in its claws the symbols of eternal life and stability. The second one (Fig. 3) has similarly an eagle with outspread wings, and beneath it is the cartouch of Usertsen III. To the right and left is a sphinx with the head of a hawk, on which are the feathers of Ammon; each is standing on a captive, whilst in front of each kneels an interceding Asiatic prisoner.

The workmanship of these jewels is wonderful. The perfection with which the precious stones are set, and, moreover, the delicacy and freshness of the whole, makes it hard to believe them five thousand years old. The work shows how far science dates back, and is evidence that in the case of the Egyptians, the further we look back, the higher we find their culture.

It is a curious fact that when we compare these jewels with those of a later period, we should find them far superior in workmanship; but so it is, for those of the time of the Ramessids are but an imperfect edition of the more ancient ones, not nearly so artistic, nor yet so well finished off.

When the excavations were continued, five large barges were brought to light; it was not till the work had continued some time that the royal apartments were found, so cleverly were they hidden.

The pyramid of the south is the most southerly royal monument of the Memphite necropolis. Traces are still to be found of a wall round it, and similarly situated as

FIG. 3.—Pectoral belonging to Usertsen III.

the princesses' tombs at the pyramid of the north; here, too, we find a gallery of twelve vaults or tombs, of which only two contain mummies, one being King Ra-Fou-Ab, and the other a princess, Queen Noub-Hotep.

Near the king's sarcophagus is a small chamber, in which were a quantity of broken vases and chests, and in a great wooden tabernacle was a statue of the *double* of the deceased (Fig. 4), painted grey, representing a young man of fifteen or sixteen. It is made of hard wood, almost black, and is admirably done; every muscle and vein are perfectly placed, and specialists have certified its veracity. It is a fine piece of Egyptian sculpture, of which only four good specimens have descended to us. Some savants have endeavoured to classify what has been found into certain schools, but this is scarcely advantageous till more has been collected.

The well leading to the princesses' tomb is about 13 ft. deep. At the bottom is a vaulted brick passage, which formerly ended in a wall. As was suspected, the wall being removed revealed a vault containing a flagstone, on which were water-jars, pieces of embalmed meat, and other offerings, also two cases, containing many things pertaining to the toilet. No inscriptions were found until the flagstone was removed, and a coffin brought to light, on which were many texts relating to the name and title of the princess. As this tomb is so similar to that of King Ra-Fou-Ab, and is so closely situated, it is supposed that the princess was his wife; but nothing has been found to confirm her marriage with him.

FIG. 4.—Statue of double of the King Ra-Fou-Ab. (Found April 16, 1894.)

Although a great deal has been done, it will require many years of hard work to open up all the tombs in these Dahshûr necropolis; but general interest has now been awakened, thanks to those who have been the means of making us acquainted with the preceding facts; the results of future action will be followed by many.

NOTES.

PROF. CORNU, the Vice-President of the Paris Academy of Sciences, is now in England, and will deliver the discourse at the Royal Institution to-morrow evening. On Tuesday evening he was entertained by the members of the Athenæum Club who are members of the Institut de France, either as Associates or Correspondants. There were present, representing the Académie des Sciences, Lord Kelvin (Associate), Sir H. Gilbert, Mr. Huggins, Mr. Lockyer, Admiral Sir G. H. Richards, and Mr. Sylvester (Correspondants); representing the Académie des Inscriptions, Sir J. Evans and Sir E. Maunde Thompson; representing the Académie des Beaux Arts, Mr. Herkomer. Letters of regret for unavoidable absence were read from Mr. Frankland and Sir Joseph Lister, Associates of the Académie des Sciences; and Sir J. Hooker, Lord Rayleigh, Sir A. Geikie, Dr. Williamson, and Sir H. Roscoe, Correspondants; Académie des Beaux Arts, Sir J. Millais, Mr. Alma-Tadema, Sir E. Burne-Jones; Académie des Sciences Morales et Politiques, Mr. Goschen, Mr. Bryce, Mr. Lecky, and Sir F. Pollock.

MR. HERBERT SPENCER has been created by the German Emperor a foreign Knight of the Order Pour le Mérite. Another mark of the esteem in which he is held is his election as an Honorary Member of the Vienna Academy of Sciences.

SIR ARCHIBALD GEIKIE has just been elected a Corresponding Member of the same Academy.

DR. BACKLUND has been appointed Director of the Pulkova Observatory, and Dr. Hermann Struve will succeed the late Dr. C. F. W. Peters as Director of the Königsberg Observatory.

IT is noted in *Science* that Deputy Surgeon-General J. S. Billings will shortly leave the Army Medical Museum, of which he is curator, and the Library of the Surgeon-General's Office, of which he is librarian, having accepted the chair of Hygiene in the University of Pennsylvania. Dr. Billings hopes to complete his work on the final volume of the great Index Catalogue before his retirement.

DR. JOHN ANTHONY, whose name is familiar to many workers in microscopy, died at Birmingham on Monday, at eighty-one years of age.

THE death is announced of Prof. Franz Ernst Neumann, Honorary President of the Physikalisch-Ökonomische Gesellschaft at Königsberg. Prof. Neumann died on May 23 at the advanced age of ninety-seven, having been born September 11, 1798. He was eminent in the department of mathematical physics, and was elected a foreign member of the Royal Society of London in 1862.

AMONG other deaths of scientific men abroad, we notice that of Dr. John Byron, well-known for his bacteriological researches. He was bacteriologist in the Loomis Laboratory, and lecturer on bacteriology in the University Medical School of New York. Dr. Byron is believed to have contracted the disease of which he died, by inhaling tubercle bacilli while carrying out some experiments. The deaths are also announced of Dr. O. Reich, at Berlin; Dr. F. Müller, the zoologist, at Basel; and Brigadier-General Charles Sutherland, formerly Surgeon-General of the United States Army, at Washington.

THE Harveian Oration will be delivered at Edinburgh on June 28, by Dr. Yellowlees.

THE Secretary of State for the Home Department has requested the following gentlemen to inquire into and report on the manufacture, filling, and use of gas cylinders:—Prof. C. V. Boys, Prof. H. B. Dixon, Dr. A. Dupré, the Rev. F. J. Smith, and Prof. W. C. Unwin. Mr. Robert F. Reynard, of the Home Office, will act as secretary.

ACTING under the Wild Birds Protection Act, 1894, notice has been given by the Home Secretary, that the taking or destroying of the eggs of the "barn owl, brown or wood owl, long-eared owl, short-eared owl, common buzzard, merlin, kestrel, goldfinch, black-headed gull, peregrine falcon, kingfisher, dotterel, raven, heron, bittern, woodcock, dipper or water ouzel, and golden plover," is prohibited in any part of the county of Westmorland.

THE preliminary programme for the sixty-third annual meeting of the British Medical Association, to be held in London from July 30 to August 2, is given in the *British Medical Journal*. The President, Sir J. Russell Reynolds, will deliver his address on July 30. The Address in Medicine will be delivered by Sir William Broadbent on the following day. Mr. Jonathan Hutchinson, F.R.S., will give the Address in Surgery on Thursday, August 1, and the Address in Physiology will be given by Prof. E. A. Schafer at the concluding meeting on August 2.

AT the annual general meeting of the Institution of Civil Engineers, held last week, Sir B. Baker was elected President, and Mr. J. Wolfe Barry, C.B., Mr. W. H. Preece, C.B., Sir Douglas Fox, and Mr. James Mansergh Vice-Presidents. The members of the Council are Dr. W. Anderson, Mr. Alex. R. Binnie, Mr. W. R. Galbraith, Mr. J. H. Greathead, Mr. J. C. Hawkshaw, Mr. C. Hawksley, Dr. John Hopkinson, Dr. Alex. B. W. Kennedy, Sir G. L. Molesworth, Sir Andrew Noble, Sir E. J. Reed, Mr. W. Shelford, Mr. F. W. Webb, Sir W. H. White, and Sir E. Leader Williams.

WE have received from Dr. P. Bergholz, Director of the Meteorological Observatory at Bremen, the results of the hourly observations made during the year 1894, with rainfall values obtained from four stations in the suburbs. This observatory forms part of the regular German meteorological service, and the results are therefore given in the form recommended by recent congresses; but in addition to the prescribed observations the work contains other valuable information, *e.g.* phenological observations, and the dates of freezing and clearing of the Weser since 1818. This table shows that the most prolonged frosts during that period were in 1844-5, 1846-7, 1857-8, and 1870-1. In each case the Weser was frozen over for two months or upwards. We observe, however, that the publication of the data is to be discontinued, as that river is now kept free for navigation by artificial means. A graphical representation of the principal meteorological results gives a ready means of comparing the characteristics of the different months.

THE Egyptian Government have published an important paper on the climate of Cairo and Alexandria, based on observations taken between 1886 and 1890, and discussed by Dr. Engel, chief of the Statistical Service. The work contains a number of tables and diagrams, together with introductory text, from which we extract a few of the results obtained. At Cairo, the mean annual temperature for the five years was 70°·3, the absolute maximum being 118°·2 on June 13, 1886, and the lowest 33°·8 on January 1, 1890. The average yearly number of rainy days was twenty-four, and the amount 1·2 inch only. At Alexandria the mean temperature was 68°·5, the absolute maximum being 100°·6, on May 10, 1889, and the minimum 43°·9, on January 22, 1889. The average number of rainy days was forty, and the amount 8·2 inches. The principal difference in the climate of the two places consists in the diurnal and seasonal variations of temperature. Cairo is much the hotter of the two places in summer, but cooler than Alexandria in the winter; and the differences in the extreme temperatures are much greater at

Cairo, both as regards days and seasons. Relative humidity varies much more at Cairo than at Alexandria, but it is much lower at Cairo in summer, and a little higher in winter than at Alexandria; while, on the contrary, the absolute humidity varies much more at Alexandria, being very high in summer and considerably greater than at Cairo. Both places enjoy a large amount of sunshine, but fog occurs occasionally, more particularly at Cairo in the early morning.

A most important contribution to the study of the formation of dolomite is made by M. C. Klement, in the *Bull. Soc. Belge Géol. Paléontol. et Hydrol.* After describing the history of theories of dolomite, the author calls attention to the frequent occurrence of dolomite in the form of coral-reefs, as observed by Dupont in the Devonian, by Richthofen and Mojsisovics in the Trias, and by Dana in the recent raised reefs of Metia in the Pacific. He points out that while in the chemical experiments that have been made with a view of dolomitising carbonate of lime, *calcite* has always been operated on, the substance of coral has been shown by Sorby to be probably *aragonite*. The author has therefore carried out a large series of experiments on the action of the constituents of sea-water (particularly magnesium sulphate) on aragonite, the results of which are given at full length. From these he finds (1) that a solution of magnesium sulphate, in the presence of sodium chloride, and at a temperature of 60° C. or more, decomposes aragonite with formation of a magnesium carbonate the exact composition of which is difficult to determine, owing to the impossibility of isolating it from the residual aragonite; (2) that this action increases with the *rise of temperature*, and with the *concentration* of the solution, and is greatly diminished by the absence of sodium chloride; (3) that recent coral is attacked by magnesium sulphate just as mineral aragonite is; and (4) that the lagoons of modern coral-reefs afford all the conditions of temperature, saturation, &c., necessary for the production of magnesium carbonate in the manner of his experiments. While recognising, therefore, that dolomites may have been formed in more ways than one, M. Klement concludes that one of the most usual ways of formation of dolomite in nature has been the action of heated and concentrated sea-water in coral-lagoons on the aragonite of coral and other skeletons, with formation of carbonate of magnesium, which is subsequently, perhaps after solidification of the rock, with the remaining carbonate of calcium, converted into massive dolomite.

The last number of *Modern Medicine and Bacteriological Review* is of exceptional interest, inasmuch as it contains an original article by Prof. Metchnikoff, of the Pasteur Institute, on "the extra-cellular destruction of bacteria in the organism." This article is really a critical comment upon some of the conclusions deduced by Dr. Pfeiffer from his experiments on the destruction of cholera vibrios in the peritoneal cavity of guinea-pigs. Dr. Pfeiffer observed this destruction of cholera vibrios when the latter were introduced into animals previously vaccinated against this germ, and also in the case of unprotected animals when the vibrios were injected together with a small quantity of serum from vaccinated animals. In both cases Dr. Pfeiffer found that they were destroyed *outside* the cells in the peritoneal fluid, and he believes that this bacteria-killing fluid is secreted by the cellular elements in consequence of a special excitation produced by the injection of cholera vibrios, and that the immunity acquired by guinea-pigs is independent of phagocytosis. Prof. Metchnikoff, however, regards this as an episode in the battle between bacteria and phagocytes, and maintains, on evidence supported by experiments, that the leucocytes secrete this bacteria-killing fluid whilst undergoing a process of degeneration due to the injection of Pfeiffer's mixture of vibrios, serum, and broth. That although unable to engulf the vibrios, they are able still to destroy them by their secretions. Metchnikoff points out that if before introducing the vibrio-mixture, a few cubic centimetres of broth be injected into the peritoneal cavity, the leucocytes will gather together in great force after a few hours, and if the vibrio-mixture be then introduced, phagocytosis does take place, and the cholera bacteria are more rapidly destroyed by this process of intra-phagocytosis than by the extra-cellular destruction produced by the conditions of Pfeiffer's experiments. The mechanism of immunity is surrounded with so many complicated problems that the search for its solution, whilst one of the most interesting tasks afforded by the developments of bacteriology, must still remain one of the most puzzling and difficult.

The current number of the *Journal de Physique* contains an important paper by M. P. Curie on the magnetic properties of bodies at different temperatures. The author has examined the magnetic properties of a number of substances in fields of from 25 to 1350 C.G.S. units, and in some cases for temperatures from 15° to 1370° C. The body under observation was generally in the form of a coarse powder, and was enclosed in a glass bulb, which was placed in a non-uniform magnetic field produced by two electro-magnets. The force acting on the body was measured by means of the torsion of a wire. For the purposes of heating the glass bulb was surrounded by a fine clay jacket, and this latter was heated by a wire in which an electric current was passed, the temperature being measured by means of a thermo-electric junction. In the case of diamagnetic bodies, with the exception of bismuth and antimony, the author finds that temperature has practically no effect on their magnetic properties. Fusion and allotropic modification also seem to produce no effect, so that the magnetic properties of a body seem to depend not on the arrangement, but rather on the nature of the molecules of the body. Selenium, however, is an exception, for in this case the susceptibility is about 3 or 4 per cent. smaller in absolute value in the liquid than in the solid state. Phosphorus is another exception, for the susceptibility of the different allotropic modifications are slightly different. The susceptibility of bismuth increases with rise of temperature, according to a straight line law, up to the melting-point, where there is a sudden rise. The susceptibility of melted bismuth is independent of temperature, and is very nearly 0. Observations made on oxygen show that the coefficient (K), which, when multiplied into the strength of the magnetic field, gives the magnetic moment of the body per unit mass (the author calls this the coefficient of specific magnetisation), is independent of the pressure, and is between 20° and 450° inversely proportional to the absolute temperature. In the case of solutions of para-magnetic salts, K is also found to vary inversely as the absolute temperature; thus supporting the observations of Wiedemann and Plessner on this subject. Glass when cold is generally feebly diamagnetic; when heated, however, it becomes much more strongly diamagnetic. The rate of increase of the diamagnetism decreases as the temperature rises; above 300° C. no further change takes place. The author considers these changes to be due to the fact that glass consists chiefly of a diamagnetic substance, the properties of which remain unaltered when the temperature rises, and of a small quantity of a relatively strongly para-magnetic substance, the para-magnetism of which decreases as the temperature rises.

Messrs. George Philip and Son will shortly publish "The Exploration of Australia," by Mr. Albert F. Calvert. This book is designed to form a companion volume to Mr. Calvert's work, "The Discovery of Australia," and will trace the progress of maritime and land exploration from the period of Captain Cook, up to recent times.

A translation, by Mr. W. E. Baxter, is announced of Van Heurck's important treatise on the Diatomaceæ. It will contain about 2000 figures, illustrating every known genus of diatoms, and every species found in the North Sea and countries bordering it, including Great Britain.

THE second edition of "Elements of Marine Surveying," by the Rev. J. L. Robinson, lately published by Messrs. Macmillan and Co., contains several very useful additions and improvements. Young marine surveyors will find the volume an excellent aid to the study of the theoretical side of their profession, and would do well to include it in their outfit.

PARTICULARS of editions of Gilbert White's "Natural History and Antiquities of Selborne" have been compiled by Mr. Edward A. Martin, for the Selborne Society. Since the original edition was published in 1789, twenty-three other editions have appeared. The list compiled by Mr. Martin, gives the dates of the various editions, publishers, printers, editors, number of pages, and general description.

THE annual report of the Royal Botanic Gardens, Trinidad, for the year 1894, compiled by the Superintendent, Mr. J. H. Hart, furnishes evidence of the practical value of these colonial botanic gardens, and of their relation with the central institution at Kew. Under the Economic Section, information is given of the growth in the island of the sugar-cane, cacao, coffee, yam, gambier, vanilla, the Brazil nut, and cola, and of the principal enemies of these crops, and the best mode of combating them.

WE have received Part i. of "The Flowering Plants and Ferns of New South Wales," with especial reference to their economic value, by Mr. J. H. Maiden, assisted by Mr. W. S. Campbell, and issued under the authority of the Department of Mines and Agriculture for New South Wales. The present part contains descriptions and coloured drawings of four species—*Telopea speciosissima*, *Eucalyptus corymbosa*, *Actinatus helianthi*, and *Acacia glaucescens*. It is intended in this way to illustrate the principal flowering plants and ferns of the colony.

THE additions to the Zoological Society's Gardens during the past week include a Panolia Deer (*Cervus eldi*, ♂) from Hainan, presented by Mr. Julius Neumann; a Ruddy Ichneumon; (*Herpestes smithii*) from India, presented by the Earl of Hopetoun; a Spotted Ichneumon (*Herpestes nepalensis*) from India, presented by Mrs. Thompson; a Rosy-faced Love-Bird (*Agapornis pullaria* from West Africa, presented by Mr. Cecil M. Bevan; a Rufescent Snake (*Leptodira rufescens*) from South Africa, presented by Mr. J. E. Matcham; a Spiny Tree Porcupine (*Sphingurus spinosus*) from Peru, a Blossom-headed Parrakeet (*Palæornis cyanocephala*) from India, two Tuberculated Iguanas (*Iguana tuberculata*) from South America, deposited; two Guira Cuckoos (*Guira pirirgua*) from Para, purchased; a Japanese Deer (*Cervus sika*, ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MOTION OF THE SOLAR SYSTEM.—The methods elaborated by Argelander and Airy for the numerical solution of this problem have been followed with more or less variation by a host of investigators. As a rule the deviations in method have involved matters of detail rather than any fresh departure. Various suppositions have been made as to the motions of the stars themselves (*motus peculiares*): that the magnitude and direction of these motions have no connection with position, or that, in general, all these motions take place with the same angular velocity parallel to the galactic circle. Stars may be grouped according to their brilliancy, or the amount of their proper motion, or they may be arranged with more or less ingenuity according to their apparent position; but when the final equations are solved, the results are found to be fairly accordant. This fact has been recently demonstrated by M. Pannekoek, who, to vary the problem as much as possible, has based his investigations on the type of spectrum presented by the star. The zone from which the stars are selected is somewhat limited, being restricted to 0°—20° of declination, the spectra of which have been observed at Potsdam. The stars have been divided into four groups, according to the amount of the proper motion, with the following results:—

Stars of the First Type.

	No. of stars.	Centennial proper motion.	Position of apex. R.A. (α).	Declination (δ).
I.	203	2″·11	322°·8±19°·2	+14°·7±7°·0
II.	93	5·58	304·7± 4·6	+12·1±3·4
III.	58	9·84	275·8± 6·1	+18·3±3·6
IV	48	34·36	251·6±12·1	+33·0±7·3

Stars of the Second Type.

	No. of stars.	Centennial proper motion.	R.A. (α).	Declination (δ).
I.	77	2·07	274·6± 9·6	− 2·6±6·3
II.	52	5·93	280·1± 9·9	+35·8±6·5
III.	65	20·85	268·6± 7·1	+31·4±4·6

The result derived from stars of small proper motion of either the first or second type of spectra is scarcely accordant with previous investigations. The Right Ascension of the one and the Declination of the other are sensibly different from results involving larger numbers of stars. The author remarks, however, that all the values in R.A. can be rendered less discordant by an increase in the constant of precession of +0″·01, and in Declination by assuming a constant negative error in the proper motions themselves. Here we have again evidence that no rearrangement of groups materially alters the position assigned for the apex of the sun's way; but when processes sensibly different in their conception are employed, the accordance in the results is not so gratifying. For instance, the attempt to determine the position of the apex from Vogel's measurements of the motion of stars in the line of sight led to either of the two results, according to the method of "weighting" employed.

	I.	II.
α	206°·1±12°·0	159°·7±20°·2
δ	+ 45·9± 9·2	+ 50·0±14·3

Here, if the Declination be fairly satisfactory, the Right Ascension is hopelessly discordant. On the other hand, Dr. Kobold's treatment of the problem according to the graphical method suggested by Bessel, a method which does not easily lend itself to numerical treatment, gives a fairly satisfactory result in R.A., but the Declination will scarcely be accepted. The position assigned to the apex by this method is α = 266°·5; δ − 3°·1. This result is based on 1425 stars, and ought to be entitled to considerable weight if it could be satisfactorily demonstrated that all ambiguity, which arises from the definition of the poles of the great circles in which the proper motions take place, had been satisfactorily removed. This question is still *sub judice*, and while distinct methods give conflicting results, it is not wise to insist too strictly on the direction of the motion of the solar system.

THE ROTATION OF MARS.—Among numerous observations of the planet Mars during the last opposition, Mr. Percival Lowell gave his attention to the measurement of the longitudes of some of the more conspicuous markings. The observations covered 36 points in all, and were made with a power of 440 on the 18-inch refractor of the Lowell Observatory. The first fact that emerged from the observations was that all the longitudes as given in Marth's ephemeris were affected by a systematic error of about 5°; or, in other words, the Martian features were retarded by about twenty minutes as compared with the computed times. The cause suggested for the discrepancy between the calculated and observed positions is that the received time of rotation of the planet is a trifle too small, and that the longitudes are consequently falling slowly behind their predicted times of meridian passage.

A somewhat similar discrepancy appears to have been noted by Prof. Keeler in 1892, who ascribed it partially to the constant error in estimating the position of the diameter of a large disc (*Astrophysical Journal*, May).

THE SUN'S STELLAR MAGNITUDE.—A new method of computing this important constant, being the number representing the sun's brightness on the scale in which the magnitudes of stars are represented, has been employed by Mr. Gore (*Knowledge*, June). Taking one of the outer planets, the known size and distance enable us to determine the fraction of the sun's light which it receives, and correcting for the albedo, it is easy to calculate the brightness of the sun in terms of that of the planet, the exact stellar magnitude of which can be found by direct measurement. Thus, Mr. Gore finds that the apparent diameter of Mars in opposition, as seen from the sun, is 6"·17, so that the area of the disc is 29·9 square seconds. Dividing the number of square seconds in a hemisphere by the latter, it is found that if the surface of Mars were a perfect reflector, the sun as seen from Mars would be 8,940,450,000 times brighter than Mars appears to us when in opposition.

According to Zöllner, the reflecting power of Mars is only 0·2672, so that the previous number must be raised to 33,459,768,000. This, however, is for mean distance 1·5237, so that when reduced to the earth's distance (by multiplying by the square of 0·5237), we get the light of the sun as seen from the earth to be 9,174,668,385 times the light of Mars when in opposition; this number, on the basis of a light ratio of 2·512 corresponding to a difference of ·1 magnitude, represents 24·9 magnitudes. Prof. Pickering's photometric measurements show that the stellar magnitude of Mars at mean opposition is 2·25, so that the deduced stellar magnitude of the sun is −27·15. Similar calculations from the data relating to Jupiter give a value of −27·17, and from Saturn −27·11. Though agreeing so remarkably among themselves, these new values differ very considerably from the value hitherto adopted, namely −25·5. The new value, however, receives confirmation in the fact that it is very nearly equal to the magnitude which α Centauri would assume if it were brought to the sun's distance from the earth, assuming the parallax to be 0"·76, the spectrum of this star resembling the spectrum of the sun.

THE GREENWICH OBSERVATORY.

THE Report of the Astronomer Royal to the Board of Visitors of the Royal Observatory, Greenwich, was read at the annual visitation on Saturday. A few of the developments made during the year covered by the report, and some observations of interest, are referred to in the subjoined extracts.

Provision has been made in the Navy Estimates for the erection in Greenwich Park of a magnetic pavilion for absolute determinations of the magnetic elements, and the plans are now being prepared in the Director of Works' Department. It is proposed to establish this station in the immediate neighbourhood of the Observatory, and at such a distance that there would be no suspicion of disturbance from the iron in the buildings.

WORK WITH EQUATORIALS.

The flint and crown discs for the new photographic telescope of 26 inches aperture, the gift of Sir Henry Thompson, have been received at the Observatory. The details of the design for the mounting have been carefully worked out, and good progress has been made with the mechanical work.

The 28-inch refractor has been in use throughout the year, and is quite satisfactory. It moves easily in R.A. and Declination, the new slow motion screws work successfully, the water clock in general drives it with great precision, and the performance of the object glass under good atmospheric conditions is admirable. Various improvements in the accessories of the instrument have been carried out in the past year. A spectroscope specially adapted to photography, for use with this refractor, is being made.

Micrometer measures of sixty-three double stars have been made; in 27 of these the distance of the components was under 1", and in 13 it was 0"·5 or under. The most remarkable of these measures are those of κ Pegasi (β 989). The components of this star, though only 0"·14 apart, were distinctly separated with a power of 1030.

Measures of the positions of satellites of Mars near elongation were made on two nights. Several attempts were also made to measure Jupiter's fifth satellite, but the results obtained were discordant. A series of measures of the polar and equatorial diameters of Jupiter and his satellites was made. Measures of the dimensions of Saturn and his rings and the positions of the satellites have also been made, and are being continued.

With the astrographic equatorial 595 plates, with a total of 1450 exposures, have been taken. Of these, 162 have been rejected for various reasons, such as:—partially fogged plates; because the reticules were not clearly printed; because the images were too faint to show 9th magnitude stars with a twenty-seconds exposure; for faults in development; for mistakes of setting; and for miscellaneous defects. It is hoped that a much smaller number of plates will need to be rejected in future for these causes.

The total number of celestial fields photographed since the commencement of work for the chart is 422, and the total number of fields photographed for the catalogue is 617. Only half as many fields for the chart and catalogue have been photographed this year as during last year. This is due partly to the unfavourable weather, and partly to the telescope being out of use for two months while the shutter of the dome was being repaired.

SPECTROSCOPIC AND HELIOGRAPHIC OBSERVATIONS.

Since 1894 December 19, when the spectroscope was brought into adjustment, 98 measures have been made of the displacement of the F line in the spectra of 13 stars, and 16 of the *b* line in the spectra of four stars. Some experiments have also been made in photographing stellar spectra, to give data as to the work to be done with the new photographic spectroscope.

Photographs of the sun were taken with the Dallmeyer photoheliograph on 199 days, and of these 375 have been selected for preservation, besides 18 photographs with double images of the sun for determination of zero of position-angle.

The 9-inch photographic telescope presented by Sir Henry Thompson, which has been mounted on the Lassell equatorial, was also in regular use as a photoheliograph up to October 15, when the progress of the building operations prevented its further use. Photographs of the sun had been obtained with it by that time on 80 days, of which 121 have been selected for preservation. In all, with one photoheliograph or the other, a record of the state of the solar surface has been secured on 213 days during the year.

The mean daily spotted area of the sun was only slightly smaller in 1894 than in 1893, the marked falling off in the spring of 1894 noted in the last report being followed by an increase during the summer months. The number of sun-spots was greater than in 1893. The spring of this year has shown a decline both in the number and area of spots.

MAGNETIC OBSERVATIONS.

The variations of magnetic declination, horizontal force and vertical force, and of earth currents have been registered photographically, and accompanying eye observations of absolute declination, horizontal force, and dip, have been made as in former years. Increased magnetic activity was shown in the year 1894, and great disturbances occurred on July 20 and August 20; the spot of light of the vertical force magnet, on the former date, and the spots of light of the horizontal force and vertical force magnets, on the latter, having passed beyond the range of the registering sheets for some hours. In July and August the disturbances in the earth-current registers caused by the South London Electric Railway showed a great increase, which is presumably due to the experiments then being made in the use of motors on the carriages of the railway instead of separate locomotives.

The following are the principal results for the magnetic elements for 1894:—

Mean declination	17° 4'·6 West.
Mean horizontal force	3·9661 (in British units). 1·8287 (in metric units).
Mean dip	67° 16' 5" (by 9-inch needles). 67° 17' 8" (by 6-inch needles). 67° 18' 43" (by 3-inch needles).

In the year 1894 there were ten days of great magnetic disturbance and thirteen other days of lesser disturbance. Tracings of the photographic curves for all of these days are being made, and will be published in the annual volume according to the arrangements made with M. Mascart. The calculation of diurnal inequalities from five typical quiet days in each month has been continued.

METEOROLOGICAL OBSERVATIONS.

The registration of atmospheric pressure, temperature of the air and of evaporation, pressure and velocity of the wind, rainfall, sunshine, and atmospheric electricity has been continuously maintained, except that during the winter the register of atmospheric electricity was interrupted during the greater part of February by freezing of the water in the exit pipe.

The mean temperature of the year 1894 was 49°·9, being 0°·5 above the average for the fifty years 1841–1890. The severe frost which set in on December 30, and continued with slight intermission until March 9, was the most remarkable meteorological feature of the year. The cold wave, defined as the period during which the mean daily temperature was below the average, extended from 1894 December 30 to 1895 March 9, with a break from January 14 to 20, and on March 1, a period extending over seventy days in all. The total defect of mean daily temperature below the fifty years' average during this period was 489°, or 7°·0 per day.

A comparison with some of the coldest winters since 1841 is given in the following table:—

Period of cold wave.	Number of days.	Total defect of mean daily temperature.
1845 Jan. 27—March 21	54	443°
1855 Jan. 10—Feb. 24	46	467°
1870 Dec. 21—1871 Feb. 3	45	320°
1886 Jan. 5—March 18	73	408°
1890 Nov. 25—1891 Jan. 22	59	560°
1894 Dec. 30—1895 March 9	70	489°

The cold on the four days February 6, 7, 8 and 9 was particularly severe, the mean temperature being 18°·6 or 20°·5 below the average of the 50 years from 1841–1890, and there is no other instance of four consecutive days since 1841 with so low a temperature.

The lowest temperature recorded during the winter was 6°·9 on February 8, the lowest temperature in February since 1841, the next lowest being 7°·7 on 1845 February 12. Lower temperatures have been registered twice since 1841, viz. 4° on 1841 January 9 and 6°·6 on 1867 January 5. The mean temperature throughout the whole of February was 28°·9, or 10°·5 below the 50 years' average. The mean in February 1855 was 29°·2.

The mean daily horizontal movement of the air in the twelve months ending 1895 April 30 was 283 miles, which is slightly above the average. The greatest movement was 867 miles on December 22, and the least 50 miles on August 30. The greatest pressure of the wind was 36 lbs. on the square foot on March 24, with a velocity of 56 miles in the hour. During the gale of December 22, the greatest pressure recorded was 30 lbs., with a velocity of 50 miles in each of two hours.

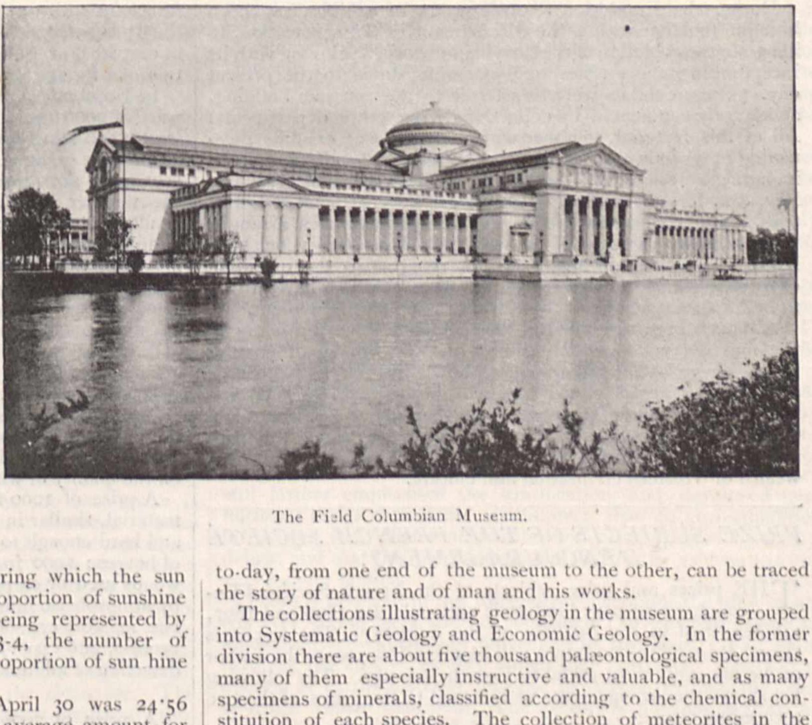
The number of hours of bright sunshine recorded during the twelve months ending 1895 April 30 by the Campbell-Stokes instrument was 928 out of the 4454 hours during which the sun was above the horizon, so that the mean proportion of sunshine for the year was 0·208, constant sunshine being represented by 1. In the corresponding period for 1893-4, the number of hours of sunshine was 1364, and the mean proportion of sun hine was 0·306.

The rainfall in the year ending 1895 April 30 was 24·56 inches, which is very nearly the same as the average amount for the 50 years 1841–1890. The number of days on which rain fell was 187.

THE FIELD COLUMBIAN MUSEUM.

THE museum founded to commemorate the World's Columbian Exposition at Chicago has reached a stage which enables it to commence a series of publications designed to present to the world the results of research conducted under its auspices. The first of this series is before us, and is devoted to an historical account of the movement that resulted in the establishment of the museum. From this description we extract the following sketch of the early history of the museum, and of the general character of the contents.

The formation of a museum at Chicago, after the Columbian Exposition, was suggested by Prof. Putnam in 1890, and received the support of Prof. Goode, Director of the U.S. National Museum, Prof. Wilson, of the Smithsonian Institution, and other representative men. In the summer of 1893, a number of the prominent citizens of Chicago resolved "to establish in Chicago a great museum that shall be a fitting memorial of the World's Columbian Exposition, and a permanent advantage and honour to the city." The delicate and important task of securing the funds necessary to carry the resolution into effect was at once begun, but the appeal at first met with little response. A munificent gift from Mr. Field gave confidence in the assured prominence and success of the museum. Mr. G. M. Pullman followed with a subscription of 100,000 dollars, and a like sum was contributed by Mr. H. N. Higinbotham. Mrs. M. D. Sturgis gave 50,000 dollars, and a number of other donations for various amounts were made, as well as Exposition stock having the approximate par value of 1,500,000 dollars. With these funds in hand, the museum committee felt justified in making extensive purchases, including the exhibits from Paraguay, Peru, Java, Samoa, the Hagenbeck collection, and the Ward collection of natural history, for which a sum of 95,000 dollars was paid. The new President of the museum, Mr. E. E. Ayer, presented the Ayer anthropological collection, valued at 100,000 dollars, to the museum, and other donations of material followed. Many exhibits were purchased at the close of the Exposition, and these furnished the broad foundation upon which the present collections have been built. Great gaps in the continuity of separate subjects have thus been, to a large degree, obviated, until to-day, from one end of the museum to the other, can be traced the story of nature and of man and his works.



The Field Columbian Museum.

The collections illustrating geology in the museum are grouped into Systematic Geology and Economic Geology. In the former division there are about five thousand palæontological specimens, many of them especially instructive and valuable, and as many specimens of minerals, classified according to the chemical constitution of each species. The collection of meteorites in the same division includes several very large specimens, notably the meteoric stone from Phillips County, Kansas, weighing 1184 lbs.; two masses weighing respectively 465 and 344 lbs., with several smaller ones from the meteorite of the Kiowa County, Kansas; two masses weighing 1013 and 265 lbs. respectively, and several smaller ones of the Cañon Diablo, Arizona, meteorite; about 650 individual aerolites of the Winnebago County, Iowa, fall, and many other specimens. Physical geography, structural and dynamical geology, and lithology are also well represented in the division of systematic geology.

The collections of the division of Economic Geology were obtained through the Chief of the Department of Mines, Mining and Metallurgy of the World's Columbian Exposition, from exhibits made in that exposition. Being designed to illustrate the practical bearings of the science of geology, they consist chiefly of specimens which show modes of occurrence in nature of minerals having economic importance, and the localities where they may be obtained. In addition to these, however, are many illustrations of the processes employed in the extraction and treatment of minerals or ores, and of the application of resulting products to human arts and industries.

An immense amount of material, illustrative of the botany and forestry of all parts of the world, came into the possession of the museum at the close of the Exposition. These exhibits are gradually being arranged in geographical sequence, but some time must elapse before all the specimens can be fully identified and labelled.

The Department of Zoology includes all the classes of animals except birds, and six large halls of the museum building are set apart for the specimens belonging to it. The mounted collection of birds in the Department of Ornithology is essentially one of comparative ornithology, in which the bird fauna of the world is represented by some 650 species. North American bird-life is at present only represented by some 150 species out of a possible 825. Among the treasures of which the museum can boast, however, is a pair of the now (probably) extinct Labrador Duck (*Camptolaimus Labradorius*).

The extensive exhibits illustrating the archæology and ethnology of America, brought together by Prof. F. W. Putnam, were transferred to the museum at the close of the Exposition. A number of other very important collections, representing primitive culture in many widely separated regions of the world, were also obtained. Belonging to the Department of Anthropology are psychological and physical laboratories, and collections of cranic casts, &c., illustrating the physical characteristics of man.

During the Exposition a great group of exhibits had been brought together within the Department of Transportation, to illustrate the evolution of the carrying industry, beginning with its inception in remote times, and extending down to the present day. These exhibits were transferred to the museum building, and largely augmented by collections from other departments. All of this material, together with a number of exhibits illustrating other industries of especial importance to civilised man, including ceramics, the textile art, the leather industry, jewellery, &c., have been brought together in a Department of Industries. The collections in this department have been arranged to show, as far as possible, the more important steps which have led to improvement in handiwork, or progress in the invention of those implements, machines, and processes which have proved to be important factors in the world's material development.

Although but a few months have elapsed since the doors of the museum were publicly thrown open, a course of popular lectures have been inaugurated, a publication series established, and several scientific expeditions sent into the field for augmenting its collections. In these and other directions, the Field Columbian Museum appears to be advancing along the path marked out for it, and performing its part in adding to the wealth of Western civilisation and culture.

PRIZE SUBJECTS OF THE FRENCH SOCIÉTÉ D'ENCOURAGEMENT.

THE prizes and prize subjects of the French Société d'Encouragement pour l'industrie nationale, for 1896 and 1897, are described in the *Bulletin* of the Society. The Society's Grand Prize of 12,000 francs will be given this year to the author of the discovery most useful to French industry. The following list shows the arrangements with regard to the prizes of the two succeeding years :—

1896.

Grand medal to the author, of any nationality, of works that have exercised the greatest influence on the progress of French mechanical arts during the preceding six years.

The Henri Giffard prize of 6000 francs for signal services to French industry. The Parmentier prize of 1000 francs for researches tending to improve the material or processes of agriculture and alimentary industries. The Meslens prize of 500 francs for the author of an application of physics or chemistry to electricity, ballistics, or hygiene.

In the section of Mechanical Arts, a prize of 3000 francs is offered for the best motor fed with some commercial oil. Other prizes are :—3000 francs for an engine of from 25 to 100 horse-power, using as a maximum, when working, $7\frac{1}{2}$ kilogrammes of steam per hour and per indicated horse-power ; 2000 francs to the manufacturer who first produces, mechanically, linen threads of which at least 100,000 metres go to one kilogramme, or, in the case of hemp, 15,000 metres per kilogramme ; 2000 francs for an investigation, or a method tending to prevent, or at least reduce in amount, the leakages, known as "fuites aux tubes," in marine boilers ; 1000 francs for the best memoir on the cost price of the motive power of steam ; 2000 francs for a small motor suitable for a home workshop, and which will work by the use of some simple power available in the house, or by energy transmitted from a central station ; 3000 francs for improvements in the processes of retting linen and hemp in industrial use.

The prizes offered in the section of Chemical Arts are : 1000 francs for the utilisation of waste products ; 2000 francs for a work or memoir of use to chemical or metallurgical industry ; 2000 francs for an experimental study of the physical or mechanical properties of one or more metals or alloys, selected from those which are in current use ; 2000 francs for a new process for the production of fuming sulphuric acid, or sulphuric anhydride ; 2000 francs for an improvement in the manufacture of chlorine ; 1000 francs for the discovery of a new alloy useful to the arts ; 2000 francs for a scientific study of combustion in the furnaces used for the production of gas ; 2000 francs for an investigation of the expansion, elasticity, and tenacity of ceramic clays and coverings ; 1000 francs for the substitution of sulphuric acid in dyeing, and especially in silk dyeing, by another compound which will give to the fibres the desired stiffness, without exercising any destructive action ; 2000 francs for an investigation of the physical and mechanical properties of glass ; 2000 francs for the discovery of processes capable of yielding, by certain chemical changes, useful organic products, such as quinine, cane-sugar, &c. ; 2000 francs for an investigation on an industrial process of which the theory is but imperfectly known ; 2000 francs for the production of cast steel or iron having useful properties, by the incorporation of a foreign substance.

In Economic Arts the following are the prizes and subjects. A prize of 2000 francs for the invention of a new process in which at least 0·800 kilogrammes of petroleum can be used without danger, as a source of light or heat, either in industry or in domestic economy ; 2000 francs for the discovery of methods to diminish the number of chimney fires, and reduce the damage which results from them ; 2000 francs for an incandescent electric lamp of one-tenth candle power when a current of 0·05 ampere is passing through it at a potential of 100 volts.

In Agriculture the prizes and subjects are as follows :—2000 francs for the best investigation of the comparative physical and chemical constitution of the soils of one of the natural or agricultural regions of France ; 1500 francs for the best varieties of barley for brewing ; 3000 francs for the re-establishment of vineyards on chalk soils ; 1500 francs for the introduction and culture, on a large scale, of a new forage plant ; 2000 francs for the best study of the culture of the vine in various regions o France, and of the influence of various processes of vinification on the quality of wine.

A prize of 1000 francs is offered for the discovery of a plastic material, similar in appearance to some stone, marble, or brick, and hard enough to be used either for the insides or the outsides of houses ; 1000 francs for the discovery of a process to prevent woods used by carpenters and cabinet-makers from deformations by atmospheric influences ; 1000 francs for the author of the best memoir on some practical process other than a chemical process, and capable of being applied in the workshop, for the detection of adulterated Portland cement.

1897.

A prize of 2000 francs is offered for improvements in the methods of grinding grain ; and a prize of 2000 francs for a motor weighing less than fifty kilogrammes per horse-power. This prize is offered with the idea of furthering the problem of aerial navigation. A second prize, having the same object, is for a study of the coefficients necessary to the mechanical calculation of an aerial machine. There is also a prize of 3000 francs for improvements in the manufacture of permanent magnets ; and prizes of 3000 francs for an investigation of alcoholic ferments, and 2000 francs for the best investigation of the deterioration of cider, and the means to prevent the changes to which the loss of vivacity is due.

The prizes are open to investigators of any nationality, but the memoirs, and descriptions of inventions, should be written in French. Models, memoirs, descriptions, and specimens intended to compete for prizes must be sent to the Secrétariat de la Société d'Encouragement pour l'industrie nationale, 44 rue de Rennes, Paris. Competitors for the prizes of 1896 must send in before the end of the present year ; the latest time for entering memoirs, &c., for the 1897 competition is the end of 1896.

RECENT GLACIAL STUDIES IN GREENLAND.[1]

DURING the summer of 1894, Mr. Chamberlin was enabled to devote some time to a personal study of the glaciation of Greenland, and the results of his observations are so interesting, that all geologists who seek to interpret the records of the "Great Ice Age," will gladly make acquaintance with them. Seldom has a geologist so experienced in the study of glacial drifts and of the problems connected with them, had the advantage of examining the behaviour of ice in the Arctic regions.

His observations were specially directed to the way in which a glacier gathers up detritus along its course, to the way in which it carries it forward and finally puts it down. The main problem he sought to solve, was connected with the basal material of glaciers, débris which, of course, is largely concealed.

In comparing the glaciation of Greenland with that of the mainland of North America, he had to bear in mind, that for the most part the continental drift is spread over a vast plain. In Greenland the ice-fields rest mainly on plateaus fringed by rugged mountains, and he sought for a tract free from such bordering elevations. This was found at Inglefield Gulf, where the borderland is a plateau about 2000 feet above sea-level, and where the margin of the great ice-sheet may be studied on relatively smooth ground, on undulating ground, and in lobes or tongues that descend the valleys. Of the thirty or forty glacial tongues which descend towards Inglefield Gulf, less than one-third reach the shore, and scarcely one-half of these discharge notable icebergs. The majority terminate in valleys whose bottoms are formed of glacial débris, and whose lower gradients are moderate.

The fact that great part of Greenland appears to consist of ancient gneissic rocks, renders the débris more or less stony and arenaceous; clayey material is rare. About Inglefield Gulf, however, the older rocks are covered by thick layers of sandstone and shale, traversed by basic igneous dykes. Hence it is possible there to tell how late the erratics from this sedimentary series were introduced into the ice, to ascertain what courses they pursued, and the actions they suffered.

The margins of the Inglefield glaciers rise abruptly like escarpments of rock, 100 or 150 feet or more. The layers of ice are cut sharply across, exposing their edges; and the formation of these scarps is attributed to the lower inclination of the sun's rays, which strike vertically and effectively against the edges of the glacier, whereas its back is affected only by rays of low slant.

The stratification of the glaciers attracted particular attention. The ice was found to be almost as distinctly bedded and laminated as a sedimentary rock. The vertical face was seen usually to present two great divisions—an upper tract of thick, obscurely laminated layers of nearly white ice, and a lower laminated tract discoloured by débris. At the base there is usually a talus-slope, and sometimes there is a moraine. In the lower portion of the ice there are, here and there, interstratified layers of sand and silt, rubble and boulders. These vary from a mere film of silt to a heterogeneous mixture of débris and ice several feet thick. The detritus is usually arranged in definite and limited horizons, the ice above and below being firm, clean, and pure. Often a fragment of rock, or a boulder of considerable dimensions, will be several times thicker than the silt layer, and it projects above and below into the clean ice. The débris-layers, though often regular and persistent, frequently thin out and disappear. Lenses of débris also appear, and the layers are sometimes doubled back upon themselves.

The laminæ of the ice are sometimes very symmetric, straight, and parallel, but often wavy and undulating. In many instances they are greatly curved or contorted. Thus, as Dr. E. von Drygalski has remarked, they closely simulate the foliation and contortion of gneiss.

The débris-belts, which are essentially parallel to the base of the glacier, are confined chiefly to the lower 50 or 75 feet, but they occur up to 100 feet and, perhaps, to 150 feet. They are more abundant at the sides of the lobes than in the centre; a notable portion of the débris having evidently been introduced after the lobes were formed. Thus the detritus appears most abundant in glacier-lobes which descend as cataracts, or crowd between closely hugging cliffs.

In meeting obstacles the basal beds of the glacier sometimes simply curve upwards, carrying their débris with them over the obstacle; at other times, the laminæ of ice are much crumpled. Not only are the foliations of the ice twisted, but they are at times fractured and faulted, and along the fault-plane the laminæ are affected by "drag," as in faulted rocks.

The general stratification of the ice had its initial stages in the original snow-falls; and the seasons doubtless developed annual subdivisions. The more definite partings and the introduction of the layers of débris, arose through a shearing movement between the layers of ice.

The actual process of intrusion of detritus was observed in proximity to a large boss of rock which, protruding through the margin of the ice, had been partially cut away. Trains of débris, apparently rubbed from the surface of the rocky dome, were carried out almost horizontally into the ice in its lee. Some of these were short, while others extended several rods into the ice, passing into the body of it instead of following its base. At one point the overthrust of the ice reached such a degree as to carry the earthy layers obliquely across the thickness of the glacier, producing a marked unconformity.

In another instance similar features were observed below an ice-cataract. Tongues of débris, having their origin in the boulder-clay below the glacier, were seen to reach out into the basal portion of the ice as though they were being introduced into it by the differential movement of the layers upon each other. Thus when the ice is forced over a prominence it settles down a little in its lee, and is then protected somewhat from the thrust of the ice behind. The next ice that passes over, being prevented by the former portion from settling down at once, is thrust forward over it. This is accomplished by the bending and doubling of the layers, and also by distinct shearing. At length, however, the first layer is compelled by the general friction to move somewhat forward, and in time to join the common moving mass, carrying the overthrust layer of débris between it and the ice-layer above.

It appears obvious that the ice in the lee of a rocky prominence moves more slowly than that above; hence the doubling of the laminæ upon themselves. Moreover, there is a gradation from laminæ that simply suffered doubling up, to layers that obviously sheared upon each other and produced manifest unconformity by overthrust.

Evidence showed that the more solid (blue) bands in the ice are produced by exceptional pressure in moving over rugosities, and that their position in the ice is parallel to the ice-movement; while at the same time blue bands may be developed nearly at right-angles, after the manner of slaty cleavage.

Summarising the above conclusions, it appears that stratification originated in the inequalities of deposition, emphasised by intercurrent winds, rains, and surface meltings; that the incipient stratification may have been intensified by the ordinary processes of consolidation; that the shearing of the strata upon each other still further emphasised the stratification, and developed new horizons under favourable conditions; that basal inequalities introduced new planes of stratification, accompanied by earthy débris, and that this process extended itself so far as even to form very minute laminæ.

There is involved in the foregoing conceptions the idea of an ice-layer acting as a unit of movement; at any rate, there is recognised individuality of movement in the layer. This view involves the idea of rigidity rather than viscosity. The introduction of earthy material into the ice-layers involves the idea of thrust rather than pull. The picture is not that of gravitation pulling a thick, stiff liquid down the lee side of a rocky prominence, but of a rigid body thrusting itself over the crest by means of a force in the rear.

The extreme fragility of the ice is difficult to harmonise with the idea of viscosity. Wherever the ice passed over an undulation of even moderate dimensions, it was abundantly crevassed. There was no indication that boulders descend through the ice as heavy substances descend through viscous bodies. The rigidity did not prevent contortions and foldings of the laminations, such as take place in crystalline rocks, but faulting and vein-structures also occur; and there seems no more occasion to assume viscosity in the one case than in the other. Even if a certain measure of viscosity be admitted, it does not follow that viscosity was an essential agency of motion. The crystalline body may readily be made to change its form by the removal of particles from one portion by melting, and their attachment at other points by congelation; but not, apparently, by the flowing of crystallised particles over each other in their crystalline condition.

It has been already pointed out that much basal material is

[1] Abridged from a paper by T. C. Chamberlin, in the *Bulletin* of the Geological Society of America, February 1895.

carried in the lower layers of ice. It was also a matter of frequent observation that débris lies under the ice. Apparently the ice sometimes pushes this along, and sometimes slides over it. At the end of the glacier the débris within the ice is freed by melting, and accumulates as a talus-slope. This sometimes protects the basal layers from melting, and they become at length incorporated in the growing accumulation.

It appeared, from the stages presented by the several glaciers, that where the ice is slowly advancing, the talus-slope gradually grows forward and constitutes an embankment, upon which the glacier advances. It thereby grades up its own pathway in advance. On seeing this process, one is at no loss to understand how ice can advance over fields of sand or soil without in any way disrupting them. It buries them before it advances upon them.

Where the frontal material accumulates in a large mass, it opposes such a degree of resistance to the ice that its layers are curved upward on the inner slope; and if the glacier subsequently advances, the ice rides up over the moraine. Several such instances were observed, but none was seen where the ice showed any competency to push even its own débris, in notable quantity, in front of it. The ice is weaker than the moraine as a whole.

Great quantities of snow are carried by winds from the region of the great ice-cap, and this snow may be lodged in immense heaps in the lee of the terminal moraines. Such a border-drift may have a breadth of from 1000 to 3000 feet. It becomes solidified after the fashion of a glacier, and may serve to arrest or deflect the main ice; for it was observed that the basal layers of the ice in places curved upwards on encountering the resistance of this wind-drifted accumulation.

The rate of movement of the majority of the glaciers was found to be exceedingly slow, though a few which produce large icebergs are notable exceptions.

The amount of drift on the territory once occupied, but now free from ice, was scanty. At some points there are considerable accumulations of drift within a mile or two of the present ice-front, but over much of the area no great moraines, nor any thick mantles of drift, were to be seen. There was but moderate evidence of glacial action; the land was gently rounded, but not greatly moulded. In this area of Southern Greenland tracts of angular, unsubdued topography alternate with rounded, flowing contours. The inference was drawn that the ice formerly so extended itself as to reach the present coast for about half its extent, while in the remaining portion the ice fell short. Thus the conclusion seems unavoidable that the ice of Greenland, on its western side, at least, has never advanced very greatly beyond its present border in recent geologic times. This carries with it the dismissal of the hypothesis that the glaciation of the mainland of North America had its source in Greenland.

There is no ground to question the former elevation of Greenland, but it would appear that this was not coincident with conditions favouring glaciation. H. B. W.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Among the distinguished men upon whom it is proposed to confer the honorary degree of D.C.L., on June 26, are Sir W. H. Flower and Prof. Michael Foster.

IN a Convocation held on Tuesday, the statute appointing Dr. E. B. Tylor professor of Anthropology during the tenure of his office as Reader in Anthropology was finally approved. In a Congregation, held on the same date, the Statute on Research Degrees received the final approval of the house, and it only remains for it to be passed by Convocation The proposed Final Honour School of Anthropology was again brought before Congregation, and excited some opposition. On a division the proposed statute was carried by a considerable majority, the numbers being: Placet, 47; Non-placet, 28. The statute has yet to run the gauntlet of Convocation before it finally passes into law. In the same Congregation, the dates of the preliminary examinations in the Honour Schools of Natural Science were fixed for the Monday after the eighth week of Full Term in Hilary Term in each year, instead of in the last week or last week but one, as has hitherto been the custom; and the grant of £300 per annum to the chemical department of the University Museum was renewed for a period of five years. The published lists of the candidates for the final and preliminary examinations in Natural Science show that there are 44 candidates in the final school and 64 candidates in the preliminary school. These figures do not include women students.

CAMBRIDGE.—The following is the speech delivered by the Public Orator, Dr. Sandys, on May 30, in presenting for the honorary degree of Doctor in Science, Dr. John Murray, editor of the *Challenger* publications.

Meministis omnes poetae nostri maximi locum insignem, ubi Northumbriae Ducis filius acerrimus non recusavit gloriam aut ex ipsa luna audacter deducere, aut maris in profundo demersam extrahere, modo solus sine rivali laudem omnem sibi vindicaret. Quanto pulchrius autem rerum naturae penetralia intima assidue perscrutari, eque oceani altitudine immensa laudem cum sociis optimis participatam reportare. Adest unus ex illis qui, plusquam tribus annis in oceano explorando fortiter toleratis, ut poetae antiqui verbis sensu novo utar,

"referebant navibus altis
occulta spolia, et plures de pace triumphos."

Una saltem nominis bene ominati navis velut ipsam rerum naturam ad certamen provocavit, ipsamque veritatem in profundo abstrusam orbi terrarum patefecit. Tanti autem itineris monumenta, quinquaginta voluminum in serie ingenti a collegis plurimis parata, viri huiusce praesertim industria infinita non modo adaucta et summatim descripta sed etiam ad terminum felicem perducta et diei in lucem prolata sunt. Quid non potuit rerum naturae, quid non potuit veritatis amor?

"Merses profundo; pulchrior evenit."

Duco ad vos Universitatis Edinensis alumnum, oceani indagatorem indefessum, virum etiam in posterum sine dubio laudem indies maiorem meriturum, IOANNEM MURRAY.

The Master of Downing (Dr. Hill) and Dr. Barclay-Smith will give a course of instruction in Practical Histology during the Long Vacation, beginning on July 6.

The State Medicine Syndicate propose to make a grant of £50 to the Department of Pathology, in aid of the course of laboratory instruction in Bacteriology therein provided for candidates for the diploma in Public Health.

Prof. Ewing's serious illness has made it necessary to appoint Mr. Dalby, Demonstrator in the Engineering Laboratory, to act as Examiner for him in the Mechanical Sciences Tripos.

The Smith's Prizes in Natural Philosophy have been awarded (1) to G. T. Manley, of Christ's College, for his essay on "The Conformal Representation of a Quadrilateral on a Half Plane," and (2) to G. H. J. Hurst, of King's College, for his essay on "Electro-magnetism and Magneto-optic Rotation." Mr. Manley and Mr. Hurst were respectively Senior and Second Wrangler in 1893. The essays of H. E. Atkins, of Peterhouse, and P. E. Bateman, of Jesus College, are declared worthy of honourable mention. Mr. Atkins was bracketed Tenth Wrangler, and Mr. Bateman bracketed Fifteenth Wrangler in the same Tripos.

Mr. S. S. Hough, of St. John's College, has been elected Isaac Newton Student in Astronomy for the three years ending June 15, 1898.

Mr. Charles Chree, Director of the Kew Observatory, has been approved for the degree of Doctor of Science.

Mr. W. N. Shaw has been appointed Chairman of the Examiners for the Mechanical Sciences Tripos, in the room of Prof. Ewing, who has resigned on the ground of illness.

Mr. Charles Smith, Master of Sidney Sussex College, has been elected Vice-Chancellor for the ensuing academical year.

Classes in Osteology, in General Chemistry, in Geology, and in Experimental Physics, are announced to be held in the Long Vacation.

Mr. A. E. Shipley, University Lecturer in Invertebrate Morphology, has been appointed a member of the University Press Syndicate.

PROF. W. T. A. EMTAGE, of University College, Nottingham, has been elected Principal of the Technical Institute, Wandsworth.

HONORARY degrees were conferred, by the Chancellor of Victoria University, last week, upon Lord Kelvin and Sir Henry Roscoe, among others, for distinguished services rendered to the University.

THE twelfth annual report of the Mitchell Library, Glasgow, is before us. The library is open to the public, and is administered by a committee of the Glasgow Town Council, from which it obtains a grant of £2000 a year, from the moneys received under the Local Taxation (Customs and Excise) Act; it is also fortunate in being the recipient of several bequests from persons interested in its work. A noteworthy point is that, out of a total of 112,447 volumes contained in the library, no less than 20,812 are classified under "Arts, Sciences, Natural History." This is two thousand volumes more than are included under any other head. The most important accession to the library during the three years covered by the report (1892-94) consists of a complete set of the *Transactions* of the Royal Society, in 183 volumes. A very valuable addition to the scientific resources of the library has resulted from agreements entered into with the Glasgow Natural History Society, and with the Glasgow Geological Society. These societies have transferred to the library their sets of the *Transactions* and *Memoirs* of foreign scientific societies, the Library Committee undertaking on their part to continue to the members their privilege of borrowing the books, to bind such as required it, and to bear the expenses attending the printing extra copies of the *Transactions* of the Glasgow societies, and forwarding the same to the foreign societies as an exchange. During last year, 115,788 scientific works were issued, the daily average being 386. It would be well if there were more public libraries conducted on the enlightened plan of the Mitchell Library.

ANOTHER library of which we have received the report (in this case the first report) is that of St. George, Hanover Square. Though on a much smaller scale than the Mitchell Library, the Commissioners appear to aim at making the library a means of education as well as of recreation. There are 11,860 volumes in the lending library, of which twenty per cent. are fiction, and 6206 in the reference library, none of which are novels. To obtain a satisfactory conclusion as to the work of a library, the use made of the library as a whole, and not of any particular department, ought to be taken into account. The records of the institution show that out of 416,760 visitors during the year, only thirteen per cent. of the readers went for the purpose of borrowing works of fiction from the lending library. A noteworthy feature in connection with the library is a museum of objects arranged as an elementary and self-explanatory collection, as an introduction to larger museums of natural history.

IT is proposed to hold a Technical Education Conference at the Society of Arts on June 20. The Society has addressed a letter to Technical Education Committees, asking them to send delegates to the Conference. Among the subjects to be considered is the "lack of a central organisation which might deal especially with such questions as the examination and inspection of classes. In spite of the valuable work which has been done by the City and Guilds of London Institute, and by other bodies, it is only in a portion of the subjects sanctioned as subjects of technical instruction that examinations are held. The wide field of agriculture and home industries is untouched; while no means are provided for anything like a general system of inspection which local authorities may call to their aid should they desire to do so." There are also other points with regard to which common action would be desirable, and it is hoped that by bringing together those who are interested in technical education the best way in which the Society can enlarge the scope of its present action in connection with the subject will be found.

THE Technical Instruction Committee of the Essex County Council have arranged for a short course of elementary instruction in horticulture, to be given at the County Technical Laboratories, Chelmsford, during the first three weeks in July. The course of study is intended to give sound elementary instruction in the cultivation of plants, based upon a knowledge of plant physiology. The teaching throughout will be practical; every lecture will be abundantly illustrated and immediately followed by demonstrations and individual practical work by the students themselves.

SCIENTIFIC SERIALS.

Internationales Archiv für Ethnographie, Band viii. Heft ii.—On the ethnography of Matty Island, by Dr. F. von Luschan. Although Matty is a small island, about ninety-three miles north of German New Guinea, between 142° and 143° E. long., Dr. von Luschan comes to the conclusion that the natives are not Melanesians; they are much lighter than almost any Melanesians, some being of a deep red flesh colour, eyes slit-like, nose narrow, hair black and in long locks. Of the thirty-eight weapons and utensils in the Berlin Museum not one can with certainty be allocated to any known culture-mixture; any Micronesian resemblance is purely superficial. It seems probable that the people have remained isolated for at least 300 years. Three plates of utensils, &c., illustrate the paper.—Dr. O. Schellong's note on some Melanesian drawings is illustrated by two coloured plates, and is supplemented by some notes by J. D. E. Schmeltz. The drawings are interesting as showing how unlike the objects intended native delineations may be. It is to be hoped that more illustrations of this aspect of the art of savages will be forthcoming. Of the notices of recent publications, those on "Arrow-poison" and "Ethnological Botany" are especially interesting.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 2.—"Alternate Current Dynamo Electric Machines." By J. Hopkinson, F.R.S., and E. Wilson.

The paper deals experimentally with the currents induced in the coils and in the cores of the magnets of alternate current machines by the varying currents in and the varying positions of the armature. It is shown that such currents exist, and that they have the effect of diminishing to a certain extent the electromotive force of the machine when working on resistances as a generator without a corresponding effect upon the phase of the armature current. It is also shown that preventing variations in the coils of the electromagnet does not, in the machine experimented upon, greatly affect the result, and that the effect of introducing copper plates between the magnets and the armature has not a very great effect upon the electromotive force of the armature, the conclusion being that the conductivity of the iron cores is sufficient to produce the main part of the effect. A method of determining the efficiency of alternate current machines is illustrated, and the results of the experiments for this determination are utilised to show that in certain cases of relation of phase of current to phase of electromotive force, the effect of the local currents in the iron cores is to increase, instead of to diminish, the electromotive force of the machine.

May 9.—Bakerian Lecture: "On the Laws of Connexion between the Conditions of Chemical Change and its Amount." By A. Vernon Harcourt, F.R.S., and William Esson, F.R.S. "III. Further Researches on the Reaction of Hydrogen and Dioxide and Hydrogen Iodide."

In this paper are considered the effect upon the reaction of (1) substances not directly participating in reaction, (2) temperature.

The general conclusion as to the effect of the medium upon the reaction is expressed as follows:—

Each constituent of the medium produces an effect on the rate of change of unit peroxide and unit iodide, proportioned to the mass, and varying with the nature of the constituent. The increment of this rate per unit mass of each constituent is constant so long as the quantity of the predominant constituent present in the medium is sufficiently large, in comparison with the other constituents of the medium, to render the media in successive experiments practically homogeneous. For example, when the ratio of the numbers of H^2SO^4 and HI in the medium exceeds 20, the formula for the rate at a given temperature is

$$\alpha = i\{a + b(i-1) + ds\},$$

a being the theoretical rate with unit of HI, b the increment per unit of hydrogen iodide per unit of iodide, and d the increment per unit of hydrogen sulphate per unit of iodide. If the ratio falls below 20 the formula is

$$\alpha = i\{a + b'(i-1) + d's\},$$

in which b' and d' depend upon the relative masses of sulphate and iodide present in the medium.

Variation of Temperature.

The discussion of the numerous experiments made at temperatures ranging from 0° to 50°, in media in which the quantities of iodide range from 3·64 *HI*, to 23 *HI*, the quantities of hydrogen sulphate from 45 H^2SO^4 to 468 H^2SO^4, and the quantities of hydrogen chloride from 70 *HCl* to 547 *HCl*, leads to the following law of connexion between chemical change and temperature.

If α_1 is the rate of chemical change at a temperature $t_1°$ in a homogeneous medium consisting of given constituents per unit volume, and α_2 is the rate at a temperature $t_2°$ in the same medium, the ratio of α_1 to α_2 is $\{(273 + t_1)/(273 + t_2)\}^m$. *m* being a constant depending upon the character of the constituents of the medium. When the temperatures are measured from the absolute zero − 273°, and are denoted by T_1, T_2, the formula assumes the simpler form,

$$\alpha_1/\alpha_2 = (T_1/T_2)^m.$$

The constancy of the value of *m* for a particular medium is secured when the quantity of the predominant constituent of the medium is sufficiently large in comparison with the quantities of the other constituents to make the medium practically homogeneous. When this is not the case the value of *m* has some value intermediate to the values which it has when one or other of the constituents is sufficiently predominant to secure a constant value.

In media in which hydrogen sulphate is sufficiently predominant, the value of *m* is 20·38; similarly for hydrogen chloride the value of *m* is 21·17. When the medium consists of water and hydrogen iodide, the value of *m* is 24·1. The introduction of sodium sulphate in large quantity into a medium otherwise consisting mainly of hydrogen sulphate reduces the value of *m* from 20·38 to 18·1. In a medium in which the main ingredient is sodium hydrogen carbonate, the value of *m* is approximately 10.

A further confirmation of the law of connexion between chemical change and temperature is obtained from the discussion of experiments on the rate of change of hydrogen chlorate and potassium iodide made by W. H. Pendlebury and M. Seward. The value of *m* is in the case of this chemical change 40·5.

It follows from the law enunciated above that at the temperature of absolute zero no chemical change can take place.

If the smallest value of *m*, viz. 10, is taken, a chemical change, which is completed in one minute at a temperature zero, would require for its completion, at a temperature of − 200°, a little more than a year. If 20 is taken as the value of *m*, the minute would be increased to more than half a million of years by the same reduction of temperature.

The law enunciated above may also be stated in the following form.

The increment of each unit of chemical change due to a rise of temperature varies as the increment of each unit of absolute temperature.

This law is expressed by the formula

$$D\alpha/\alpha = mDT/T.$$

Chemical Equilibrium.

A case of equilibrium between the reactions

$$H^2O^2 + 2HI = 2H^2O + I^2,$$
$$2H^2O + I^2 = H^2O^2 + 2HI,$$

leads to a discussion of the general equations of chemical equilibrium, which is given in an appendix to the paper. These equations are employed to interpret the results of experiments published by Dr. Gladstone in the *Transactions* of the Royal Society (*Phil. Trans.*, vol. cxlv.). They had been previously applied to the case of chemical equilibrium investigated by Prof. Dixon, in a paper published in vol. clxxv. of the *Transactions* of the Royal Society, the reactions in that case being

$$H^2O + CO = H^2 + CO^2,$$
$$H^2 + CO^2 = H^2O + CO.$$

Physical Society, May 24.—Captain W. de W. Abney, President, in the chair.—Dr. Kuenen read a paper entitled "On the condensation and the critical phenomena of mixtures of ethane and nitrous oxide." If the vapour of a pure substance is compressed at constant temperature, then when a certain pressure is reached the vapour commences to condense, and the pressure remains constant until all the vapour is liquefied. Taking the pressure and temperature as coordinates the corresponding temperatures and pressures at which liquefaction takes place are plotted, the curve obtained is called the vapour pressure curve, and this curve ends at the critical temperature and pressure of the given substance. On the other hand, if a mixture of two vapours is compressed at constant temperature the pressure no longer remains constant while condensation is taking place, but gradually rises. The points at which condensation commences and ends lie on a U-shaped curve having its vertex turned towards the direction of increasing temperatures. Such a curve the author calls a "border curve." The point at which a line parallel to the axis of *p* touches a border curve corresponds to the critical point (R) of the given mixture. For all temperatures higher than that corresponding to R there is no condensation into liquid possible, while for any temperature below the critical temperature there are two vapour pressures, one corresponding to the commencement, and the other to the conclusion of liquefaction. The envelope of all the border curves for mixtures containing different proportions of the two bodies is a curve, called the plait-point curve, joining the critical points of the two constituents. The point of contact (P) of a border curve with the plait-point curve corresponds to the plait-point on van der Waal's, thermodynamic surface. If when we go along the border curve, starting from its lower branch, we first reach R and then P, and if we indicate the temperatures corresponding to P and R by T_P and T_R, then for temperatures between T_P and T_R as the pressure is increased the quantity of liquid firts increases, reaches a maximum, and after that decreases till it disappears. This is called retrograde condensation of the first kind, and has been observed by the author in the case of mixtures of methyl chloride and carbon dioxide. If P, however, lies beyond R the process of condensation for temperatures between T_P and T_R is different. In this case the volume of vapour increases, reaches a maximum, and then decreases. This constitutes retrograde condensation of the second kind. It was with a view to the experimental observation of this second kind of retrograde condensation that the author undertook his observations. A series of observations were made with each of the pure gases, and gave the following values for the critical temperature :—

Ethane	32°·3 C.
Nitrous oxide	36°·1 C.

In the case of the mixtures, the very interesting result is obtained that the critical temperature is in some cases less than that of either of the constituent gases. Thus a mixture containing 10 per cent. of C_2H_6 has a critical temperature of 32°, the same critlcal temperature as for pure ethane. All mixtures containing more than 10 per cent, of ethane have a lower critical temperature than 32°; the lowest critical temperature obtained is 25°·8, and belongs to a mixture containing equal volumes of ethane and nitrous oxide. Another important point is that the border curves do not all lie between the vapour pressure curves of ethane and nitrous oxide. Hence for any temperature there is some mixture which gives a maximum vapour pressure. It also appears from the curves, given in the paper that the maximum vapour pressure is obtained with almost the same mixture at all temperatures, and that this maximum vapour pressure doe not disappear with increase of temperature, but remains even up to the critical region. For mixtures containing between 20 and 50 per cent. of C_2H_6 retrograde condensation of the second kind takes place, but the author has no been able to observe it, since the difference between T_P nd T_R for the two substances experimented on cannot be more than 0°·1, and the temperature could not be maintained sufficiently constant to hope to be able to detect any phenomenon taking place over such a small temperature range. The author showed his arrangement for stirring the liquid and vapour in the experimental tube so as to prevent any retardation of the different phases due to slow diffusion in the long narrow tubes employed. A small piece of iron with enamel beads on the ends is enclosed in the experimental tube, and by means of a small magnetising coil which surrounds the jacket used to keep the temperature of the tube constant, this piece of iron can be moved up and down the tube so as to keep the liquid and vapour thoroughly stirred. Prof. Carey Foster and Prof. Ramsay complimented the author on the very lucid way he had expounded a by no means easy subject. Dr. Sidney Young congratulated the author on the able use he had made of his lucky discovery of two bodies such that their mixture should have a lower critical temperature than that of either of the pure substances. Prof. Ramsay and he (Dr. Young) had made experiments on the vapour pressure of mixtures of alcohol and ether, and had found great difficulty in preventing the separation of the components when the volume was altered, and he could, therefore, thoroughly appreciate the utility of the author's device for overcoming this difficulty. They had also experienced considerable difficulty in filling the tube with a mixture of known composition and free from air, and he considered that when dealing with mixtures it was better to employ gases, although they could not be obtained in so perfect a state of purity as liquids, on account of the greater ease with which a mixture of known composition can be obtained. The plan of making separate observations on the pure substances was a good one, and considering that the author measures the increase of pressure during the process of condensation, so that any air which happened to be present produced the maximum effect, the small rise in pressure obtained indicated a high degree of purity in the gases employed. He would like to ask the author if in the case of mixtures he found it possible to determine accurately the point where condensation commenced and ended, for with the alcohol and ether mixtures they had found it very difficult to determine these points. He also hoped the author would continue his observations in the direction indicated in the paper. Mr. Inwards suggested that in the case of liquids which act on iron, the iron stirrer could be enclosed in glass or india-rubber. It might also be possible to obtain more efficient stirring by means of a small fan or propeller worked by an electro-magnet rotating outside the tube. The author, in his reply, said that when the mixtures were well stirred, the pressures at which condensation commenced and ended were well marked.—Mr. Burstall commenced the reading of a paper on the measurement of a cyclically varying temperature. The experiments were undertaken with a view of measuring the temperature inside the cylinder of a gas engine at different points of the stroke of the piston. A modified form of platinum thermometer is employed to measure the temperature, and since the variations in temperature are extremely rapid, the wire had to be very thin and unprotected by any covering such as is ordinarily employed. The leads of the thermometer pass through a slate plug fixed in a seamless steel tube, asbestos being used as a packing to prevent leakage. The resistance of the thermometer is measured by means of a Wheatstone's bridge. Since the temperature at a certain part only of the *working* stroke had to be measured, the galvanometer circuit was broken in two places; one of these breaks was closed by means of a cam on the shaft of the engine at a given point of each revolution, while the other was closed when an explosion took place by means of a relay worked by the pointer of a steam engine indicator attached to the cylinder of the engine. The remainder of the paper was postponed till the next meeting.

Linnean Society, May 24.—Anniversary Meeting.—Mr. C. B. Clarke, President, in the chair.—The Treasurer presented his annual report, duly audited, and the Secretary having announced the elections and deaths during the past twelve months, the usual ballot took place for new members of Council. The following were elected:—Prof. J. B. Farmer, Mr. A. Gepp, Prof. Howes, Dr. St. G. Mivart, and Mr. A. S. Woodward. On a ballot taking place for the election of President and officers, Mr. Charles Baron Clarke was re-elected President, Mr. Frank Crisp Treasurer, Mr. B. D. Jackson Botanical Secretary, and Prof. G. B. Howes Zoological Secretary. The Librarian's report having been read, and certain formal business disposed of, the President delivered his annual address, prefaced by some remarks on the present position of the Society. On the motion of Sir Joseph Hooker, seconded by Dr. John Anderson, a vote of thanks was accorded to the President, with a request that he would allow his address to be printed. The Society's gold medal was then formally awarded to Prof. Ferdinand Cohn, of Breslau, and was received on his behalf by Mr. B. D. Jackson for transmission through the German embassy. The President having called attention to the retirement of the Zoological Secretary, Mr. W. Percy Sladen, after holding office for ten years, an announcement which he felt sure would be received with universal regret, it was proposed by Mr. Carruthers, seconded by Mr. Crisp, and supported by Mr. Charles Breese—"That the Fellows of this Society, regretting the retirement of Mr. Walter Percy Sladen from the post of Zoological Secretary, which he has occupied for the past ten years, desire to record upon the Minutes of the Society an expression of their high appreciation of the services which he has rendered to the Society, and of the very able manner in which he has at all times discharged the duties of his office." This resolution having been put, was carried unanimously, and after a sympathetic reply from Mr. Sladen, the Society adjourned to June 6. In the evening a number of Fellows of the Society dined together at the Grand Hotel, Charing Cross, the President occupying the chair, and being supported by several distinguished visitors.

Royal Meteorological Society, May 15.—Mr. R. Inwards, President, in the chair.—Mr. G. J. Symons, F.R.S., and Mr. G. Chatterton read a paper on the November floods of 1894 in the Thames Valley, which they had prepared at the request of the Council of the Royal Meteorological Society. This consisted of a systematic description of the causes which led to the great floods of November last, and an analysis of the records obtained from the Thames Conservancy Board, from the engineers of several of the towns along the river, and also from rainfall observers throughout the Thames watershed. The information was given chiefly in the form of tables, one of the first being a chronological history of floods in the Thames Valley from the year 9 A.D. down to the present time. This was followed by a short description of the damage wrought in November 1894, which was illustrated by a number of interesting lantern slides. Details were then given of the levels reached at various places in all the principal floods from 1750 to the present time. The authors exhibited a map showing the relative elevation of all the parts of the Thames basin, and then gave details of the rainfall for each day from October 23 to November 18, 1894. The results obtained by the Thames Conservancy Board, showing the flood levels at each lock, were exhibited on a longitudinal section from Lechlade to Teddington, and the hydraulic inclinations from lock to lock were shown in a tabular form. The volume of flood water, as gauged by the Thames Conservancy at Teddington, rose rapidly from 4000 million gallons per diem on November 12, to 10,250 million gallons on the 16th, 12,800 million gallons on the 17th, and to over 20,000 million gallons on the 18th, when the discharge reached its maximum. The last-named discharge is equivalent to 0·37 inch over the whole watershed of the Thames above Teddington Lock.—Mr. F. J. Brodie read a short paper on the barometrical changes preceding and accompanying the heavy rainfall of November 1894, from which it appeared that the latter half of October was characterised by unusually bad weather, especially in the more western and southern parts of the British Isles. The torrential rains from November 11 to 14, which actually caused the floods, were due to two secondary depressions which developed a certain amount of intensity as they passed over the southern part of England.

CAMBRIDGE.

Philosophical Society, May 13.—Prof. J. J. Thomson, President, in the chair.—Exhibition of some recent photographs of the moon, by Mr. Newall.—On the "volume heat" of aniline, by Mr. E. H. Griffiths. The results of an inquiry (by what may be termed an absolute method) into the influence of temperature on the capacity for heat of aniline were published in the *Philosophical Magazine*, January 1895. During last autumn, Mr. C. Green, of Sydney College, made a series of observations on the *density* of the same compound, over the temperature range 15° to 52° C. Three separate sets of determinations of the density gave very concordant results. If the capacity for heat of equal volumes at different temperatures be denoted by the phrase "*volume heat*," then the "volume heat" at any temperature is the product of the capacity for heat and the density. In the case of aniline, the "volume heat" appears to be constant. Our knowledge of the changes in the capacity for heat of water due to changes of temperature is so uncertain that the *relative* values of the changes in the specific heat of other substances are of little absolute value. The author, therefore, has been unable to extend the inquiry into the "volume heat" of other bodies than aniline, for he has not succeeded in finding any other determinations which do not rest on some assumption as to the behaviour of water.—Exhibition of Goldstein's experiments on kathode rays, by Mr. J. W. Capstick. Mr. Capstick showed Goldstein's experiments on the effect of a stream of kathode rays on salts of the alkalies. When the rays are directed on potassium chloride, for instance, the salt becomes of a heliotrope colour, and retains the colour for several days if kept out of contact with moisture. The effect appears to be due to a chemical change in the substance—probably the formation of a sub-chloride—but the layer of altered salt is so exceedingly thin that it is difficult to get unequivocal chemical evidence as to its nature.—On a curious dynamical property of celts, by Mr. G. T. Walker. Mr. G. T. Walker exhibited celts which possessed the property of spinning in only one direction upon a horizontal surface.—On the formation of cloud in the absence of dust, by Mr. C. T. R. Wilson. The cloud-formation is brought about, as in the experiments of Aitken and others, by the sudden expansion of saturated air. A form of apparatus is used in which a very sudden and definite increase in volume is produced, and in which the possibility of dust entering from the outside seems to be excluded. If ordinary air is started with, it is found that after a comparatively small number of expansions, to remove dust particles by causing condensation to take place on them, there is no further condensation unless the expansion exceeds a certain definite amount. With expansion greater than this critical value condensation invariably takes place, and the critical expansion shows no tendency to rise, however many expansions be made. The latest result for the ratio of the final to the initial volume, when the critical expansion is just reached, is 1·258 (when initial temperature = 16·7). This corresponds to a fall of temperature of 26° C., and a vapour pressure 4·5 times the saturation pressure for a plane surface of water. The radius of a water drop just in equilibrium with this degree of supersaturation = $6·5 \times 10^{-6}$ cm., assuming the ordinary value of the surface tension to hold for drops of that size.

May 27.—Evaluation of an automorphic function, by Mr. H. F. Baker.—On a construction in geometrical optics, by Mr. J. Larmor.—Note on the steady motion of a viscous incompressible fluid, by Mr. J. Brill.

PARIS.

Academy of Sciences, May 27.—M. Cornu in the chair.—On an algebraical problem connected with Fermat's last theorem, by M. de Jonquières.—A contribution to the history of the cerium earths, by M. P. Schutzenberger.—On the accumulation in the soil of cupric compounds used in the treatment of parasitic diseases in plants, by M. Aimé Girard. The evidence furnished by the author, in addition to the facts made known by other writers, completely proves that continuous treatment with copper compounds for a long period has no influence either upon the quantity or the quality of the crop obtained from the vine or potato.—Dr. Frankland was elected Foreign Associate of the Academy.—Injection of ethyl alcohol into venous blood, by M. N. Gréhant. From experiments made on a dog, it is concluded that, after the injection into the blood of a considerable volume of alcohol, the proportion of this substance in the blood five minutes after the injection and for more than eight hours afterwards becomes absolutely constant.—Spectroscopic researches on Saturn's rings, by M. H. Deslandres. The rotation of the planet and of its inner and outer rings has been measured by the methods used first by the author with the planet Jupiter, and employed by Keeler in his recently published researches on the subject of this paper. The author differs from Keeler insomuch as he does not regard this kind of evidence as a proof of the meteoric nature of the rings.—On the reduction of nitric oxide by iron or zinc in presence of water, by MM. Paul Sabatier and J. B. Senderens. The reduction of gaseous nitric oxide or nitric oxide dissolved in ferrous sulphate solution results in the production of nitrous oxide and nitrogen, finally the nitrous oxide is completely reduced also. A small amount of ammonia is formed, and a considerable quantity of hydrogen liberated, when the reaction is permitted to go on for a considerable time.—On the reduction of silica by aluminium, by M. Vigouroux. Silicon obtained in the crystalline form by this process is described.—A study of some reactions of lead sulphide, by M. A. Lodin. Mr. James Hannay's conclusions concerning the hypothetical compound PbS_2O_2, and the part played by it in the metallurgy of lead, are controverted. It is found that lead sulphide fuses at 935°, but exerts a considerable vapour pressure at temperatures much lower; hence the explanation of the volatilisation of galena requires no new compound to be supposed to exist. The long-admitted equations expressing the reactions taking place in the reverberatory furnace are completely verified by the author.—On campholenic derivatives, by M. A. Béhal.—On crystallised cinchonicine, by M. Ferdinand Roques.—Transformation of an aniline salt into an anilido-acid. Pyruvic acid forms with aniline a condensation product, $CH_3 . C (NC_6H_5) . COOH$. Phenylglyoxylic acid, under the same conditions, forms the salt, $C_6H_5 . CO . CO_2H . NH_2 . C_6H_5$. On dissolving this in methyl alcohol, the condensation product, $C_6H_5 . C (NC_6H_5) . CO_2H$, separates out in the crystalline form in a few minutes in the cold.—On ozobenzene, by M. Adolphe Renard. By the action of ozone on benzene a white explosive substance is produced having the composition $C_6H_6O_6$.—On the fixation of iodine by potato-starch, by M. Gaston Rouvier.—On the elimination of magnesia in the urine of infants suffering from rickets, by M. Oechsner de Coninck.—On the employment of serum from animals immunised against tetanus, by M. L. Vaillard. The antitetanic serum is able to confer complete immunity for from two to six weeks, but if the tetanus has become established, inoculation is not able to prevent progress of the disease. The toxine in tetanus is perhaps the most active of the bacterial poisons, yet the antitoxine of the serum is even more active.—The relation between relief and the frequency and intensity of earthquakes of any region, by M. de Montessus.—Atmospheric and seismic perturbations of the month of May last and their connection with solar phenomena, by M. Ch. V. Zenger.

BOOKS AND SERIALS RECEIVED.

BOOKS.—On certain Phenomena belonging to the Close of the last Geological Period and on their Bearing upon the Tradition of the Flood: Dr. J. Prestwich (Macmillan).—Fallacies of Race Theories as applied to National Characteristics: W. D. Babington (Longmans).—A Junior Course of Practical Zoology: Prof. A. M. Marshall and Dr. C. H. Hurst, 4th edition (Smith, Elder).—Handbuch für Botanische Bestimmungsübungen: Dr. F. Niedenzu (Leipzig, Engelmann).—Cours Élémentaire d'Electricité: M. B. Brunhes (Paris, Gauthier-Villars).—Verlagskatalog von Wilhelm Engelmann in Leipzig bis ende des Jahres 1894 (Leipzig, Engelmann).—The Time Machine: H. G. Wells (Heinemann).—A Text-Book of Physiology: Dr. M. Foster, 6th edition, Part 2, comprising Book 2 (Macmillan).—The Lumleian Lectures on certain Points in the Ætiology of Disease, and the Harveian Oration: Dr. P. H. Pye-Smith (Churchill).—Meteorological Charts of the Red Sea (Eyre and Spottiswoode).

SERIALS.—Proceedings of the Royal Society of Edinburgh, Vol. xx. pp. 305-384 (Edinburgh).—National Review, June (Arnold).—Humanitarian, June (Hutchinson).—Natural Science, June (Rait).—Contemporary Review, June (Isbister).—Scribner's Magazine, June (Low).—Zeitschrift für Physikalische Chemie, xvii. Band, 1 Heft (Leipzig, Engelmann).—Fortnightly Review, June (Chapman).—North American Fauna, No. 8 (Washington).—Proceedings of the American Philosophical Society, May 1893 (Philadelphia).—Ditto, July to December, 1894 (Philadelphia).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1894, Part 3 (Philadelphia).—Bulletin from the Laboratories of Natural History of the State University of Iowa, Vol. 3, No. 3 (Iowa).—Geographical Journal, June (Stanford).

CONTENTS.