THURSDAY, JUNE 29, 1876

GOVERNMENT AID TO SCIENTIFIC RESEARCH

WE publish below a correspondence which we cannot but regard with the greatest satisfaction; Government has at last seen it to be its duty to act upon the recommendation of the Duke of Devonshire's Commission, and make a substantial contribution towards the endowment of pure scientific research. We need scarcely remind our readers that from the first we have maintained that such endowment is the duty and interest of civilised states. But indeed it is long since the British Government practically acknowledged this to be the case; the grant of 1,000*l.* yearly to the Royal Society for purposes of research was first made twenty-five years ago. We hope the additional 4,000*l.*, making up 5,000*l.*, will be put to such excellent use that Government will not only renew the grant at the end of the five years, but see the necessity of increasing it to at least the sum suggested when the 1,000*l.* was first granted. No doubt the first to bring the duty of the State in respect to science prominently before the public in this country was the late Colonel Strange. He broached his scheme many years ago at the Norwich meeting of the British Association, and by his earnest and untiring advocacy he soon gained to his views most of the scientific men of the country, and Government became so impressed with the importance of the subject that the Science Commission was appointed in 1870. The substance of the various Reports of this Commission is familiar to our readers; the mass of evidence it has elicited has probably done more than anything else to enlighten the country and our Government as to the high importance and wide extent of scientific research. We can hardly expect Government to carry out all at once the recommendations of the Commissioners as to the extent to which unremunerative research should be assisted; but no doubt the 4,000*l.* which is to be annually entrusted to the administration of the Royal Society for the next five years, is the first, partly tentative step towards this. Then there were the strong words of Lord Derby, at Edinburgh, last December (see NATURE, vol. xiii. p. 141): "I think," he said, "that more liberal assistance in the prosecution of original scientific research is one of the recognised wants of our time." As the natural outcome of all this, and no doubt mainly as the result of the recommendations of the Science Commission, the Government has resolved to try what good results are likely to follow from a first and moderate endowment. We think we may safely prophecy that the result is likely in time to lead to the increase of the grant to at least the sum proposed to be entrusted to the Royal Society twenty-five years ago.

The difference between this new grant of 4,000*l.* and that of the old 1,000*l.*, should be noted. In the case of the latter the money had to be expended on instruments, &c., by the recipients, whereas in the case of the new grant the endowment may be personal; the grants may be made to individuals not merely to provide themselves with apparatus, but as a means of sustenance while prosecuting scientific researches incompatible with the pursuit of an ordinary means of livelihood. And here we should remark that we never advocated in these columns the wholesale selection of untried youths for the receipt of such endowments, nor is it meant thus to allocate the grant which has been made. The selection will be made from among those who by the work which they have already done have proved themselves capable of making a profitable use of the endowment.

Another point in the conditions attending the new grant we notice with pleasure, namely, the reconstruction of the Government Grant Committee of the Royal Society, so as to include the Presidents of the principal Scotch and Irish Societies, as well as those of the chief London scientific bodies. Thus the interests of Scotland and Ireland are as well cared for in this matter as those of England.

The Royal Society has now a great responsibility resting upon it. What with the annual 4,000*l.* from Government, in addition to the previous 1,000*l.*, and the 6,000*l.* which Mr. Jodrell has entrusted to its administration, it will have critical and important duties to science and to the country to perform. We are sure it will take every care so to allot these funds as to prove that it has only the interests of pure science at heart, and is quite competent to carry out the intentions of Government as well as of private donors.

The following is the correspondence in relation to the grant which has passed between Government and the Royal Society:—

LETTER TO THE PRESIDENT OF THE ROYAL SOCIETY.

Science and Art Department, South Kensington, S.W., April 29, 1876

SIR,—Her Majesty's Government have had under their consideration the question of giving some further aid to scientific research.

As you are aware a sum of 1,000*l.* is voted annually by Parliament "to enable the Royal Society to defray the expenses of scientific investigations considered by a Committee of the Society to be worthy of such aid." This Committee, called the Government Grant Committee, consists of the President and Council of the Royal Society and twenty-one other gentlemen of scientific eminence not members of the Council; and the Grant is expended in aiding investigators to provide themselves with apparatus and assistants, but never in personal payments to the investigators themselves.

It is proposed that this action of the State should be extended, and that further aid should be given to research by according permission to the Government Grant Committee to recommend in certain cases the payment of personal allowances to gentlemen during the time they are engaged in their investigations; that a sum of 5,000*l.*, including the above-mentioned 1,000*l.*, should be taken annually; that the Royal Society should be invited to aid Her Majesty's Government with their advice and assistance in its appropriation and expenditure, and as to the sums to be granted in each case, reporting annually to the Lords of the Committee of Council on Education on the progress made and the desirability or non-desirability of renewing the grant; and that this experiment should be tried for five years.

The Administration and expenditure of the grant, and accountability for it, should we consider be vested in the Science and Art Department of the Committee of Council on Education by which the vote will be taken; and all instruments purchased for investigations should be left in its charge when no longer required.

It would be advisable that the Presidents of the following Societies should be ex-officio members of the Government Grant Committee, viz. :—

The Royal Society of Edinburgh,
Royal Irish Academy,
Royal Astronomical Society,
Mathematical Society,
Chemical Society,
Linnean Society,
Zoological Society,
Geological Society,
Physical Society,
Institution of Civil Engineers,
Institute of Mechanical Engineers,
General Council of Medical Education and Registration of the United Kingdom.
Royal College of Physicians,
Royal College of Surgeons, and
British Association.

No definite rule can be laid down as to the amounts to be awarded in individual cases. These must depend upon various circumstances, especially on the amount of time which the investigator devotes to the inquiry.

There would be no objection to the application of some portion of this fund to the payment of such clerical assistance as may be found necessary.

I should feel obliged if you will consult the Council of the Royal Society on this scheme and inform me what is their opinion of it, and also give me the benefit of any suggestions as to modifications that may occur to them or to you.

I have the honour to remain, Sir,
Your obedient servant,
(Signed) RICHMOND AND GORDON

LETTER TO THE PRESIDENT OF THE ROYAL SOCIETY.

Science and Art Department, South Kensington, S.W., May 29, 1876

SIR,—In reference to our conversation on Monday last on the subject of the Duke of Richmond and Gordon's letter of April 29, I should feel obliged by your informing the Council of the Royal Society that the Lords of the Committee of Council on Education agree with you in thinking that, under the circumstances, it would perhaps be more advisable to leave the grant of 1,000*l.* exactly as at present. The conditions of the Lord President's letter would then apply only to the vote of 4,000*l.* Should the Council of the Royal Society concur in this view, we will communicate with the Treasury on the subject. The recommendations of the Royal Society with respect to the appropriation of the 4,000*l.* must, no doubt, be liable to revision by the Minister responsible to Parliament for its due administration, and of this responsibility he cannot divest himself. But the power is one, we believe, for the exercise of which there is never likely to be occasion. Should it, however, happen that the Committee of Council on Education found it inadvisable to act on all of the recommendations of the Royal Society, the best course would probably be to give the Council an opportunity of revising them ; so that, if thought desirable, the items of the grant, to which exception had been taken, might be allocated in some other way. If the Royal Society are still desirous that the grant should be accepted or rejected as a whole, the Lords of the Committee of Council on Education will of course undertake that this shall be done. But they believe on consideration that the Council will agree that such a course would be likely to have a mischievous effect, and entail great hardship on those recipients of grants who, from the success that had attended their investigations, might naturally have expected the continuance of their grants.

As respects the reports of progress, My Lords believe that the Council of the Royal Society will see that Parliament will naturally desire to have laid before them such a report from those capable of giving an opinion, as will enable them to judge of the nature and amount of work being done, and the desirability, or otherwise, of continuing the grants. It is not asked that the report should be in any great detail ; as a rule it would be sufficient if it were of a general character, unless some of the subjects should from their special nature seem to require more precise information. The Lords of the Committee of Council on Education are fully aware of the great difficulties which surround the question of the direct encouragement of research and of the labour and responsibility that must necessarily be entailed on those who undertake to organise the experiment in this country. They therefore are glad to find that they may reckon on the cordial co-operation of the Royal Society, to whom they naturally first appealed to aid them in this matter.

I have the honour to be, Sir,
Your obedient Servant,
(Signed) SANDON

J. D. Hooker, Esq., C.B., M.D., &c.,
President of the Royal Society

LETTER TO LORD SANDON.

The Royal Society, Burlington House, W., June 2, 1876

My Lord,—With reference to your Lordship's letter to the President of the Royal Society dated May 29, I am to inform you that the President and Council of the Royal Society concur in the proposal therein contained, namely that, while the grant of 1,000*l.* should remain exactly as at present, a vote of 4,000*l.* should be taken on the conditions expressed in the Lord President's letter ; and that, in case it should happen that the Committee of Council on Education found it inadvisible to act on all the recommendations of the Royal Society, the Council of the Royal Society should have an opportunity of revising them, so that, if thought desirable, the items of the grant to which exception had been taken might be allocated in some other way.

I have the honour to be, my Lord,
Your obedient Servant,
(Signed) G. G. STOKES, Secretary, R.S.

The Lord Sandon, &c., &c., &c.

WALLACE'S GEOGRAPHICAL DISTRIBUTION OF ANIMALS [1]

The Geographical Distribution of Animals, with a Study of the Living and Extinct Faunas, as Elucidating the Past Changes of the Earth's Surface. By Alfred Russel Wallace. Two Vols. 8vo. (London : Macmillan and Co., 1876.)

II.

THE second part of his great work on Geographical Distribution Mr. Wallace devotes to the discussion of fossil animals. It might seem at first sight, as our author observes, rather out of place to begin the systematic treatment of this subject with extinct animals rather than with recent ones. But those who take the trouble to read these most interesting chapters will be speedily convinced to the contrary. Imperfect as is our knowledge of the geological past, enough has been already ascertained to enable some enchanting theories to be started which account to a greater or less extent for some of the most difficult problems of the present. As regards the comparatively recent extirpation of large and important forms which has taken place in Europe, in North America, and in South America alike since Post-Pliocene times, "it is clear," our author tells

[1] Continued from p. 168.

us, "that we are now in an altogether exceptional period of the earth's history," some idea of which it is very necessary to realise. "We live in an impoverished world, from which all the hugest and fiercest and strangest forms have recently disappeared." The cause of this great change over such a large part of the world's surface was, in Mr. Wallace's opinion, the "glacial epoch," which, according to Mr. Belt's theory, heaped up most of the water in the earth in mountains of ice round the two poles and left the great ocean-beds comparatively dry. This, we are told, "must have acted in various ways to have produced alterations of the levels of the ocean as well as vast local flows, which would have combined with the excessive cold to destroy animal life." We are not sure that this is a *very* satisfactory explanation of the simultaneous disappearance of the great Irish Elk from Europe and the *Megatherium* from South America, but it is at all events *some* explanation of an obscure point, and deserves careful consideration. So also do those few cases in which geological evidence is already sufficient to give us indications of the original birth-place of some of the mammalian types, and of the mode in which has come about their present state of distribution.

The third section of Mr. Wallace's great work, which we now enter upon, is, in fact, the most important of the whole, and that to which the previous chapters may be regarded purely as introductory. Having shown us what the six great divisions of the earth's land-surface, zoologically considered, are, and how it may have come to pass that they are what they are, Mr. Wallace takes them one after the other in order, and gives us a separate memoir upon each of them, and their special zoological

Fig. 3.—A Brazilian Forest, with characteristic Mammalia.

characteristics. After a description of their territorial outlines, illustrated by hypsometrical maps in which the boundaries of the sub-regions are likewise indicated, general remarks are given upon their leading zoological features. The chief forms of mammals, birds, reptiles, batrachians, fishes, butterflies, beetles, and land-shells, which characterise them are pointed out. The Sub-regions into which they are divisible are then taken up and treated in greater detail, and the leading authorities from whose labours the necessary facts have become known to us are cited. At the end of each memoir "tables of distribution" are added, in which are given—first, a list of the families of the selected groups of animals represented within the Region, with an indication of their range, if any, beyond the Region, and secondly, a similar list of the genera of the terrestrial mammals and birds, with an indication of their ranges both within and beyond the Region. Three or four plates, drawn by the late Mr. J. B. Zwecker, accompany each memoir.

These are intended to illustrate the physical aspect and zoological character of some well-marked division of the region, and as only such species are figured as "do actually occur together in a state of nature," the scenes represented are "at all events not altogether impossible ones," which is more than many of our artistic friends can say of *their* productions! While we could have wished that Mr. Zwecker had resorted in some cases to the Zoological Society's Gardens rather than to previously published figures for the models of some of his animals, we must acknowledge generally the truthfulness of these illustrations and the faithful manner in which they have been executed. At home alike in the tropics of the Oriental

and of the Neotropical regions, no one surely could have been more competent than Mr. Wallace to select the most characteristic forms for these plates, and we have great pleasure in reproducing some of them in these columns.

To those who know anything of Natural History the enormous labour involved in the compilation of these six memoirs will be at once apparent. The mass of details to be gone through in bringing together the most prominent known facts connected with the mammals, birds, reptiles, amphibians, fishes, butterflies, beetles, and land-shells of every different part of the world's surface, is a task that the boldest naturalist might well stand aghast at, especially when it is recollected that these details have to be picked out from several hundred different works and periodicals published in every quarter of the globe. That errors can be escaped in such a compilation even by a writer so cautious and so competent as Mr. Wallace is manifestly impossible. No intellect could expect to obtain personal acquaintance with more than a few selected branches of such a multifarious subject, and for the rest an author must trust to second-hand information. The selection of such second-hand information and its reduction into a uniform shape, is of itself a task of appalling magnitude, and we can only congratulate Mr. Wallace on having had strength and leisure to accomplish such a Herculean labour.

The fourth and last part of Mr. Wallace's work contains, as we have already explained, a review of the distribution of the different groups of animals which he has selected for the illustration of geographical distribution arranged in systematic order. The families are taken up

FIG. 4.—A Forest Scene on the Upper Amazon, with some Characteristic Birds.

one after another, the principal genera are mentioned, and notes are given on the more remarkable species. At the end of each order is appended a series of remarks on the general distribution of the whole group. This is in fact the storehouse of information from which the essays on the six zoological regions have been compiled, and should in strictness have preceded the third section of the work instead of following it. The author wisely recommends persons not well versed in zoology to read the more important parts of it—especially the observations at the close of each order—before they begin Part III. As regards this systematic treatise the observations which we have already made on the difficulties to be mastered in the compilation of the memoirs relating to the six geographical regions are still more applicable. It would be easy to point out many passages in which Mr. Wallace has not in our opinion made the most judicious choice of authorities. Errors of detail are, however, as has been already stated, unavoidable in a work of this extent—happy is he who makes fewest of them! Even in the case of some of the largest and most prominent families of the great class of mammals, naturalists are by no means yet agreed as to the number of species and genera that should be admitted. For example, Mr. Wallace, we observe, assigns "four, or perhaps five" rhinoceroses to Africa, but Prof. Flower—one of the highest living authorities on this class of animals, in a recent paper read before the Zoological Society of London—could only recognise *two*. Mr. Wallace admits the validity of *Elasmognathus* of

Gill as a genus of Tapirs, and adopts Dr. Gray's multitudinous division of the well-defined and eminently natural group of Eared Seals (*Otaria*). Many naturalists would hesitate before following Mr. Gill and Dr. Gray as authorities on these (or perhaps we may add on many other) subjects. But such and similar errors on questions of detail do not, we believe, affect the validity of Mr. Wallace's general conclusions. After the miserable stuff usually thrust before us in even the best and most recent treatises on geography, when the question of distribution comes to be touched upon, it is truly refreshing to turn to Mr. Wallace's broad and enlightened views on this subject. Future compilers of geographical manuals will have an easy task when they come to this most important but hitherto most ill-used part of their work, if they will only cast aside all that they have previously written, and borrow freely from the volume now before us.

Mr. Wallace has already registered many claims on the gratitude of naturalists present and future. In their interest he has explored the tropics of the east and the wildernesses of the west, and has brought home numberless novelties. He has written one of the best and most instructive books of naturalists' travels ever yet issued. He was, as is well known, the joint inventor with Mr. Darwin of the theory of "Natural Selection." But beyond all these scientific feats—and they are no mean ones—he has accomplished a task that will extend his fame even more widely amongst those who love science, as the author of the first sound treatise on zoological geography.

TWINING'S "SCIENCE MADE EASY"

Science Made Easy: a Series of Familiar Lectures on the Elements of Scientific Knowledge most Required in Daily Life. By Thomas Twining. (London: Chapman and Hall, 1876.)

THESE thin clearly printed quartos represent a remarkable experiment; an attempt to diffuse good teaching without good teachers, and to reproduce first-rate popular lectures without the need of multiplying skilled lecturers to deliver them. The author, Mr. Twining, constructed in 1856 an Economic Museum at Twickenham, which exhibited illustrations of scientific knowledge as applicable to the concerns of daily life. After fifteen years of continuous improvement this collection was destroyed by fire; but the experience gained in working it strongly impressed upon its author the conviction that the level of popular culture in this country is below the point at which intelligent appreciation of the simplest scientific object becomes possible; since his fine museum, with its methodical classification, its careful explanatory labelling, and the oral instruction of its active curator, failed to convey knowledge to the mass of visitors, to whom the very alphabet of science was unknown, and whose minds were untrained to the reception of the simplest truths. It is a bold thing for one man to enter on the task of educating a people; but Mr. Twining's enthusiasm was equal to the attempt. Precluded himself from lecturing, he prepared carefully-written lectures, founded on his Twickenham experience, and entrusted them to others to deliver. The swimming bath of East Lambeth, dry and unused in the winter, was fitted up as a lecture-room, and a course of five lectures was there delivered to attentive audiences of more than a thousand persons. Demands for their repetition arose from all parts of London; and during the last nine seasons they have been delivered in various mission-rooms, institutes, and clubs of the working-classes to crowded and eager hearers. Uneducated learners, however respectfully attentive, yet carrying away from a lecture ideas crude and disjointed, may lapse within a few days into their original ignorance; Mr. Twining therefore began early to test his audiences by a system of examinations, so modified as to meet the inexperience of candidates and the elementary character of the teaching. Examination programmes were issued, containing a full set of possible questions on the course, from ten to fourteen being allotted to each lecture, with the understanding that from every one of these groups two questions would be selected by the examiner; while a preliminary examination "of a friendly kind" struck off all who were clearly incapable of presenting themselves with any prospect of success. Under these limitations we are told that a large number of candidates have obtained prizes and certificates at successive examinations, their papers showing that they had grasped and could reproduce intelligently a fair amount of the teaching which they had received.

Mr. Twining thinks that what has been done in London may easily be done elsewhere; he therefore prints his lectures, and prefaces them with minute instructions for the guidance of such amateurs as may wish to organise and carry out the course. In its delivery two persons are necessary, a "reader" and a "demonstrator." The reader must be a good elocutionist, and need be nothing more; need know nothing of science in general, nothing of the particular science on which he is discoursing. If he is clever enough to introduce here and there a happy local *à propos*, so much the better; but he is a mere vehicle for the transmission of the matter contained in his text, and is not required to do more than utter it. The demonstrator must know something of science, and have some practice in manipulation; but the simplicity of the experiments and the fulness of the printed directions reduce this necessity to a minimum, so that the author proposes to himself as suitable interpreters in a country town the national schoolmaster as reader, and the doctor or dispensing chemist as demonstrator. Reference numbers, dotted lines, and other devices, indicate the relative duties of the two performers, who cannot of course expect to work smoothly and in concert without repeated and laborious rehearsals.

The ordinary science teacher, luxuriating in abundant time, in ample apparatus, and in educated hearers, might be tempted to speak unfavourably of the lectures themselves, as too condensed for practical usefulness. He might say, and say truly, that the matter contained in the three lectures on Mechanical Physics could scarcely, by a master teaching boys, be included in the five-and-twenty lectures of an ordinary school term; that the two lectures on Chemistry are overgrown object lessons; that no one of the seventeen topics treated in the single lecture on Chemical Physics would demand less than an hour's careful teaching in a class-room; and that the "questionary," or examination programme, represents pure and simple cram. But such criticism would be wholly unfair applied to Mr. Twining's enterprise, as overlooking its

condition and its aim: the condition, an audience of weary working-men, with little time to give, and who reject all instruction which is not easily grasped and enlivened by amusing spectacles: the aim, to communicate entertaining knowledge in a utilitarian spirit, to open a glimpse of intellectual enjoyment such as may at the same time bear practically on the comfort and happiness of daily life. In the experience necessary for such a taste Mr. Twining probably stands alone, and in reviewing the forms his efforts have taken we may fairly bow to the judgment which shaped them.

But the main objection to this curious and novel system will occur to everyone. Is it possible that any man uttering the knowledge and the thoughts of others on a subject with which he is quite unfamiliar can import into his task the enthusiasm necessary to kindle and inform an audience? A purchased sermon read from a pulpit never yet edified anyone; will it be more inspiring to receive scientific truth from the lips of a man who articulates by rote instead of teaching from that lofty standpoint of superior knowledge which converts hearers into disciples? Mr. Twining speaks gratefully of the admirable readers he has been fortunate enough to find in London. They were probably not mere elocutionists, but possessed of dramatic minds, and able to generate at will enthusiasm in a noble though unfamiliar subject, and their like will not be met with every day. Mr. Twining shows his uneasiness on this point by his strong injunctions to careful practice on the part both of reader and demonstrator, and whoever attempts to carry out the scheme will have to lay special stress on this. Nor can we omit to mention the subject of expense. The apparatus necessary only for the six lectures before us costs, exclusive of plans and diagrams, from 44*l.* to 48*l.* 10*s.* A club, society, or institute, including dexterous workmen amongst its members, could probably obtain all that is wanted at half this price, but in many places the difficulty of meeting the expense might turn the scale against the introduction of the lectures.

These difficulties have, no doubt, been well considered by the author of the scheme, and are thought by him to be not insurmountable. We most sincerely hope that it may be found so. His enterprise will be watched with no slight interest by all who feel that the spread of scientific knowledge among the operative classes is a pressing national necessity, and that one who devotes to it, as Mr. Twining has done, experience, thought, and toil, deserves the gratitude and the help of his countrymen.

W. T.

OUR BOOK SHELF

Life with the Hamran Arabs. An account of a Sporting Tour of some Officers of the Guards in the Soudan during the winter of 1874-5. By Arthur R. Myers, Surgeon, Coldstream Guards. With Photographs. (London: Smith, Elder, and Co., 1876.)

THE sporting tour of which Mr. Myers gives the narrative in this volume was made at the same time as that described by the Earl of Mayo in the work which we recently noticed. Indeed the two parties started together, and their work lay in regions not far distant from each other. Mr. Myers and his party were much more fortunate than the Earl's party. They did not meet with so many hindrances, and were much more fortunate in the number and variety of animals that came in the way of their rifles. The region to which Mr. Myers's work refers is on the borders of Abyssinia and Egypt, and has been already made familiar to English readers by Sir Samuel Baker in his "Nile Tributaries." Mr. Myers simply pretends to tell of his sporting adventures, and therefore we have no reason to complain if he adds little to our knowledge of the country of the Hamran Arabs. He writes in an unpretentious style, and his work will be found interesting by the general reader, and specially so by those who love sport. It contains photographs of some of the trophies brought home, arranged by Ward and Co.; they give a good idea of the variety of animal life to be met with in this part of the Soudan.

LETTERS TO THE EDITOR

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

The Decrease of the Polynesians [1]

I BELIEVE there are some errors popularly received respecting the rapidity with which the inhabitants of Polynesia, as a whole, are disappearing before an advancing civilisation. I wish to make a few statements on this subject in connection with a review of Miss Bird's book on "The Hawaiian Archipelago," which appeared in NATURE, vol. xi. p. 322.

The primary source of error is the excessively high estimates as to the population of different islands in Polynesia made by early visitors and residents. In most of the islands the people live chiefly, or entirely on the coasts; whereas, in the estimates, allowance is made for a proportionate population in the interior.

Another error, I believe, is the supposition that the decrease of the people is entirely (or almost entirely) owing to their contact with foreigners. From personal knowledge of Polynesia I feel convinced that the people were rapidly decreasing before their intercourse with civilised races commenced.

It is also a mistake to suppose that decrease is by any means universal at the present time. While in some islands the decrease of the natives has been accelerated since they have come into contact with modern civilisation and its attendant evils, in other islands the previous decrease has been greatly retarded, or even changed into an increase, by the beneficial influences of a Christian civilisation. This change has been brought about by such causes as the following:—The partial or complete cessation of wars; the discontinuance of human sacrifices (in some islands the cessation of cannibalism may be added); the cessation of infanticide; the greater respect paid to women, which leads to their release from some of the hard work which, in heathen times (in some portions of the Pacific) fell almost entirely to their share, and the consequent increase of living and healthy progeny; the increased care taken of infants and aged people, and the general progress of industry resulting from more settled habits, which leads to a more regular supply of food.

As an example, in proof of the correctness of my statements I will cite the Samoan Islands. In the "Encyclopædia Britannica" (eighth edition) we read:—"The population of Samoa

[1] I wrote this paper some months ago, intending to send it for publication in NATURE, but I afterwards determined on withholding it for the present, hoping at some future time to discuss in a more systematic and thorough manner this subject, together with some other questions bearing on the ethnology and anthropology of Polynesia. I am now, however, induced, by the reference in Prof. Rolleston's address before the British Association at Bristol, to publish it as it was first written, hoping it may prove a small contribution towards a correct understanding of this subject.

I take this opportunity to thank Dr. Rolleston for putting in its true light the relation which the work of missionaries bears to the decrease of Aboriginal populations. It is high time that the ignorance, prejudice, and narrowness manifested by many literary and scientific men gave place to a broad, common-sense, and enlightened view of the matter. Missionaries are sometimes represented as if they were the actual *destroyers* of the weaker races; a view somewhat *smartly* set forth in one of Mr. Bernard Quaritch's scientific book catalogues (No. 294, Jan. 1875) in the following words:—"The missionary is a grand and striking figure in the history of the world. Robed in black, and bearing the *Word of Life*, he moves among the weaker races of mankind; around his path they sicken and perish, and countless nations of men are swept away." In Polynesia, the agents of the London Missionary Society, at least, usually dress in *white*, and not in *black*, and I imagine most sensible missionaries who live in the tropics, do as we do in this respect. But whether we wear the ominous black, or adopt the more hopeful (or comfortable) white, I fancy Mr. Quaritch is guilty of what the logicians call an *ignoratio elenchi*.

S. J. W.

Samoa, Dec. 30, 1875

has been variously estimated from as many as 160,000 to as few as 38,000. The Rev. J. Williams estimated them at the former number in 1830, and Capt. Erskine, in 1854, at the latter; but by the missionaries (*Samoan Reporter*, 1845), the population was reckoned at from 50,000 to 60,000. At present the Samoan nation does not probably exceed 40,000 souls" (vol. xviii. p. 278. See also vol. xvi. p. 88).

In 1853 the first census of the population of these islands was taken, and it was then found that the natives numbered 33,901. Thus, according to the Rev. J. Williams's estimate, there was a decrease of 126,099 in twenty-three years, or 5,482 per annum! According to the lowest estimate of the missionaries in 1845, there was a decrease of 16,099 in eight years, or 2,012 per annum! Capt. Erskine's more moderate estimate was 4,099 above the actual number ascertained by census the year before he made it.

In 1863 a second census of the population was taken, and the natives then numbered 35,097, showing an increase of 1,196 in ten years, or $119\frac{6}{10}$ per annum.

In 1874 another general census of Samoa was taken, and the entire native population was found to be 34,265, showing a decrease of 832 in the eleven years since 1863, but still giving an increase, in the twenty-one years since 1853, of 364: the decrease during the shorter period averaging $75\frac{7}{11}$ per annum, while the increase during the longer period averages $17\frac{4}{7}$ per annum. This difference is easily accounted for. During the eleven years which intervened between the second census and that recently taken, there was a civil war in the principal islands which lasted more than four years, in consequence of which the usual death-rate was largely increased. This was not merely owing to the actual number of people killed in fighting, but chiefly to privation and suffering in those districts where the fighting took place. The census shows a decrease on the different islands in proportion to the amount of damage done, and the consequent privations suffered by the people. Thus Upolu, which was the principal seat of the war, suffered very severely; and here there was a decrease of 988 in a population of 17,556, or more than 5 per cent. Savaii, which suffered comparatively little, shows a decrease of 140 in a population of 12,670—slightly over 1 per cent. On the other hand, the island of Tutuila, which was not involved in the war, shows an *increase* of 296, or more than 8 per cent. in the eleven years: the population in 1863 being 3,450, while in 1874 it amounted to 3,746.

In one part of Upolu, where a register of births and deaths was kept for several years previous to the above-mentioned war, there was an annual excess in the number of births over the deaths, averaging from 1 to 2 per cent. I believe the decrease in the aggregate population during the eleven years is entirely owing to the war.

The population of the small island of Niuē (Savage Island) was counted in 1859 and found to number 4,300. It was counted again in 1864, and found to number 5,010, showing an increase of 710 in five years, or more than 3 per cent. per annum. In 1868 the population was again numbered, and found to amount to 5,060, showing an increase of fifty only in four years. But at the latter date many of the natives were away as voluntary immigrant labourers in other islands—seventy-five being in Samoa—and many others were sailors on board vessels: hence that does not represent the actual increase. I know many other islands in Polynesia where there is a steady increase in the population year by year, since the abolition of paganism.

But notwithstanding these facts, which give some hope for the Polynesians, I fear the balance is against them in the aggregate, and that the general tendency is towards a more or less rapid decrease which—unless some measures for their conservation are found—will greatly diminish, if not destroy them. The causes which produce this tendency are, *first*, those epidemic diseases which commit such fearful havoc in localities to which they are newly introduced. Some of them, such as influenza and measles, are comparatively harmless in countries where they have long been prevalent. But they are terribly fatal in a new country, as has lately been seen in Fiji. This excessive mortality is not, I believe, owing to the want of stamina in the constitutions of the natives; but may be accounted for by their mode of life, and by the fact that the inhabitants of entire villages are stricken together, leaving none in health to procure food for, and attend to, the sick.

But fearful as the effects of these epidemic diseases are, they do not recur, and, in my opinion, there are other causes which, in the end, prove themselves far more destructive. These are constantly working, and are every day working with augmented power; and these the Polynesians owe entirely to their intercourse with foreigners. They are *ardent spirits* and *syphilis*. In the case of the Hawaiian Islands, *leprosy* may be added; for in that archipelago these three scourges are working with fearful effect, and they bid fair to sweep off the greater part of the natives. But those islands must not be taken as fairly representative of the state of Polynesia as a whole. In many islands the drinking of foreign spirits is almost unknown, and in many more syphilis is rarely if ever met with.

The question may be asked, What possible remedies can be suggested which may, by moderating, or removing, the causes of decrease, help in the conservation of the Polynesians? The only possible remedies which at present occur to me are: (1) Strict quarantine regulations wherever there is a government by which they can be enforced. (2) A heavy duty (which would be, practically, prohibitory) on the deleterious kinds of spirits commonly imported into the islands and vended to the natives.

The introduction of measles into Fiji since the establishment of British rule there does not speak very strongly in favour of the efficacy of the first remedy. But there surely must have been some serious oversight or neglect on the part of medical officers, when infected persons were permitted to land on those islands from a British man-of-war, and such oversight or neglect ought not to be repeated elsewhere.

It would be a blessing if some measures could be taken to protect the Polynesians against one of their worst enemies—ardent spirits. It is notorious that an immense quantity of a noxious kind of spirit is constantly imported into some of the islands and sold to the natives. The taste for this deleterious drink is increasing, and likely still to increase. If low traders will continue to vend such a vile compound, without regard to the amount of human misery, or even loss of life, which may result therefrom, it appears to me that all respectable merchants who do business in Polynesia should set themselves against it and keep their hands clean from the traffic.

Samoa, South Pacific S. J. WHITMEE

Wind Driftage

IN the interesting narrative of the cruise of the *Challenger* that appeared in NATURE (vol. xiv. p. 93), the wind-formed rocks and drift of the Bermudas are referred to. This probably will call attention to the much-neglected subject of wind driftage; but I sincerely trust Prof. Thomson and his Colleague will discard such an ill-advised name as "sand-glaciers" for the inundations of "Æolian" or "blowing sands." The term glacier belongs to ice; beside, these sand-streams do not act like glaciers, their advance being more similar to that of a lava flow.

Somewhat similar sands occur in Australia, and were described years ago under the name of "Æolian drift," by an officer of the Royal Engineers (whose name I now forget), those in the vicinity of Melbourne Bay being remarkable for containing regular strata of empty bottles. In Kutch there are extensive wind-formed rocks, in some, such as Meeta and Kara, the cement being principally salt. On the coast of Ireland travelling sands can be studied on a small scale. At Bundoran, the late Lord Palmerston stopped the Æolian drift from travelling by planting it with the Austrian pine; on the west coast in places considerable encroachments take place, one of the most conspicuous now in progress occurring to the east of Broad Haven, co. Mayo. Here, a few years ago, the "bent," or grass on a large accumulation of sand was cut by the natives, and the sand began to travel eastward. Now it has destroyed several hundred acres of tillage land and driven the inhabitants before it over the brow of the hill into a boggy valley. G. HENRY KINAHAN

Wexford, June 10

Freezing Phenomenon

PROBABLY the following statement may be of use, probably also it is nothing new, but in the faint hope of its being a mite of value, I send it.

In a wash-hand basin, placed in an out-house where fine dust fell on the surface of the water, I noticed this last winter, that there was a thin sheet of ice atop, and that the dust had fallen to the bottom of the basin, and there was arranged in precisely the same patterns as were to be seen in the hoar-frost on the panes of glass of some neighbouring hot-beds.

This would seem to show that, in freezing, water goes through a series of fantastic movements.

Could such motions be at all comparable with the changes that the particles of iron go through from cold, and occasionally

rendering a supposed good piece of metal quite brittle and dangerous if trusted to do its ordinary duty, such accidents happening as a rule in cold weather?

June 24 WILMOT H. T. POWER

Sagacity in Cats

THE following facts are curious: I should be glad if any of your readers can inform me whether anything of the kind has been already noticed. I have a cat of half-Persian breed; she is about eight years old, and has always been remarkable for her aversion to strangers, more especially to children. If children have at any time come into the house where she was, she has invariably decamped and secreted herself. She never could bear to be handled or pulled about (which so many cats seem to enjoy) by anyone but by her master.

During the present year this cat has remained in Scotland; a few weeks ago my little boy went to reside in the house where the cat is at present. This boy is just at that age when children delight in pulling about everything they can get hold of: naturally a cat was a perfect godsend to him. After a few days the cat was seen to smell the child repeatedly; she seemed to be satisfied of his relationship, and since that time she follows him about the house (a thing which she never did to anyone but her master), rubs herself against him, and allows him to pull her tail and ears and draw her about by the legs.

Owens College, June 22 M. M. PATTISON MUIR

OUR ASTRONOMICAL COLUMN

LE VERRIER'S TABLES OF SATURN.—Vol. xii. of the *Annales* of the Observatory of Paris, containing, in addition to his Tables of Jupiter, the more extensive Tables of Saturn, was presented by M. Le Verrier to the Academy of Sciences on the 5th of the present month. To insure, as M. Le Verrier has explained, their accurate and convenient application, the Tables of Saturn occupy two-thirds more space than those of Jupiter, or 278 and 170 pages respectively, though their general form and arrangement appears to be the same, and as those who have seen the Tables of Jupiter will be aware, is materially different from the arrangement of the Tables of Mercury to Mars inclusive. The tables of Saturn represent exactly the observations from Bradley to the present day.

M. Le Verrier again mentioned that his theories of Uranus and Neptune were complete, and susceptible of being extended to an indefinite number of years. The comparison of the theories with observation is already sufficiently advanced to enable him to pronounce upon their satisfactory agreement.

36 OPHIUCHI.—It appears to have been somewhat hastily concluded that this star, so remarkable for its identity of proper motion, both as regards amount and direction with the distant 30 Scorpii, is also a binary system. So far the measures by no means bear out this inference, and unless they are affected with unusual errors it is not easy to explain them. For comparison the following may be selected:—

Herschel and South	1822·52	Pos.	227°·4	Dis.	5″·55
Herschel	1835·19	,,	223°·5	,,	4″·88
Dawes	1841·59	,,	219°·3	,,	4″·78
Jacob	1846·21	,,	216°·2	,,	4″·66
,, ...	1850·62	,,	214°·9	,,	4″·49
,, ...	1854·07	,,	214°·4	,,	4″·13
Barclay	1871·51	,,	210°·6	,,	5″·01

It might be supposed from these measures, that while the angle has been slowly retrograding between the years 1822 and 1871, the distance had diminished until 1854, and is now on the increase, but on projecting the measures it will be seen that this would indicate a motion of one star in a curve convex towards the other. If there are material errors of observation, the real motion of the companion may be rectilinear, or the change in angle and distance may be caused by a slight difference in the proper motions of the stars. Further careful measures, however, are yet required before any safe inference can be made, and more especially in latitudes where the star rises to a greater altitude than in this country. Capt. Jacob's comparatively small distance in 1854 is not supported by the meridian observations at Greenwich, Oxford, and Washington, from which we might conclude, it was nearly one second greater than his result, but meridian observations are not always reliable for such delicate comparisons, and besides it seems hardly probable that so practised and excellent an astrometer as Capt. Jacob would be in error 1″ in the distance of so easy a star, favoured as he was by his positions at Poona and Madras; Secchi also is confirmatory. The statement of Chr. Mayer that the companion was 13″·2 due S. of the principal star, or on an angle of 180° (not 360° as given by Smyth in the "Cycle"), does not assist an explanation.

NOVA OPHIUCHI, 1848.—This star has been a difficult object for the generality of telescopes during the last few years. In 1856 it had descended to the eleventh magnitude; ten years later it was a faint twelfth, and in 1874-75 not higher than thirteen in the scale in ordinary use. Herr Julius Schmidt carefully examined the vicinity at Athens in August 1867, fixing the positions of the small stars near the variable, which were discernible in the 6-feet refractor of that observatory. These places are here brought up to the beginning of the present year, with the view to facilitate the recognition of the object which became so suddenly conspicuous to the naked eye at the end of April 1848. The magnitudes are Schmidt's.

No. of Star.	Magnitude.	R.A. h. m. s.	N.P.D. ° ′ ″	
1	... 11 ...	16 52 3·9 ...	102 46 1	
2	... 13 ...	16 52 28·7 ...	102 44 4	
Nova	... Var. ...	16 52 33·1 ...	102 42 3	From the first Radcliffe Catalogue.
3	... 11 ...	16 52 55·4 ...	102 49 12	
4	... 11 ...	16 53 2·5 ...	102 34 1	
5	... 10·11 ...	16 54 19·8 ...	102 49 6	

The ninth magnitude, Lalande 30853, R.A. 1876·0, 16h. 52m. 18 5s. N.P.D. 103° 0′ 24″, may be used to identify the variable which it precedes 14·6 seconds, and is 18′ 21″ more southerly. Schmidt thought there might be a star 13·14, following the variable 5s. or 6s. His stars (1) and (3) had these position-angles and distances, while *Nova* was still visible without the telescope in 1848.

(1) Position ...	249°·4	Distance ...	475″·4
(3) ,, ...	144°·5	,, ...	530″·8

The star (2) was repeatedly measured in position and distance in 1848, with the 15-inch refractor at Harvard College by the Bonds, with the view to discover if there were appreciable parallax in the variable; it is called a fifteenth magnitude in the Harvard scale, and by a mean of ten nights' measures its position was 212°·11, and distance 115″·65 for 1848·52.

STEPHAN'S COMET, 1867 (I).—This comet, for which Mr. Searle found an elliptic orbit, period 33·62 years, or almost precisely that of the comet of the November meteors, and which was shown some years since to make a very near approach to the orbit of the planet Uranus, appears also to pass at a short distance from that of Mars. In heliocentric ecliptical longitude 81° 53′, and latitude + 1° 5′, with true anomaly, 6° 10′, the distance between the two orbits is only 0·0207, thus affording a similar instance of close approximation to this planet which Dr. Brünnow found to take place in the case of De Vico's comet of short-period. It is singular that a comet's orbit should lie so very near to the orbits of two of the planets, in one instance near its perihelion, and in the other not far from aphelion.

THE COMET OF 1698.—In the first orbit of this comet in the last *Astronomical Column*, the perihelion passage should be dated *October*,

THE SATELLITE OF VENUS[1]

AN indirect result of the recent Transit of Venus has been the revival of a nearly forgotten but not uninteresting speculation as to the possible existence of a satellite accompanying her. Nothing of the kind was observed on the late occasion, but the planet's path was so far from central that an attendant might readily have remained outside the solar disc; and therefore, though the negative evidence, if it had required additional strength, would have received it from this non-appearance, it would not have been rendered absolutely conclusive on that ground alone; and, so far as the Transit is concerned, there is still room for an essay like that before us, which, previous in composition though subsequently published, advocates the affirmative opinion. That opinion, after so many years of additional observation since Lambert's memoir in 1777, is not likely to find favour with astronomers now, and certainly will not be established by the present treatise. It is an unpleasant task to express any other than a favourable estimate of any work undertaken with a view to enlarge the boundary of knowledge; but in the present instance it is unavoidable. There is, indeed, a very considerable accumulation of historical matter, and there are some pleasant anecdotes, and a few valuable and little known facts; but the materials of some portions at least are neither complete nor accurate; the pretermission of recent discoveries—especially spectroscopic—is sometimes simply unaccountable; and the hypotheses occasionally partake of an extravagance that outstrips all probability. The subject is, however, as has been remarked, not devoid of some interest, nor, to say the truth, disentangled from some perplexity of an obstinate character; and it is worthy of a more satisfactory elucidation, which might be comprised in a narrow compass, as its literature is not extensive. A few remarks only can be attempted here.

That something strongly resembling a satellite has been occasionally seen near Venus, especially about the middle of the last century, is beyond a doubt. It is equally certain, and familiar to all experienced observers, that reflected images, or technically "ghosts," may, under certain circumstances, be formed in the eye-piece of the telescope, and might be the means of causing deception: and the whole matter is reduced to the simple inquiry, whether all the recorded instances admit of this easy explanation; though, if they do not, it must be remembered that the existence of a satellite would not necessarily follow.

The Abbot Hell, who published an elaborate dissertation on the subject in the appendix to the Vienna Astronomical Ephemerides for 1766, seems to have been the first to study systematically the formation of telescopic ghosts. The Vienna Observatory was possessed in those days of two good English telescopes, left to it in 1757 by Cardinal de Trautson; a 2 ft. Gregorian, and a 4½ ft. Newtonian. About December in that year, the Abbot, examining Venus with the former instrument and a power of 70 or 80, perceived a star of an ill-defined aspect near it like a little comet, but as it was invisible both in that Newtonian and in another of the same construction of 4 ft., he referred it to a reflection from the interior of the tube. In March 1758, Venus being at her greatest elongation, the illusion returned, on which he blackened the tube, and for some days did not see it again; but when at length it reappeared, on moving his eye very gently towards the eye-piece he found it change into a perfect image of a satellite with the phase of the primary. Beyond the limit of barely half a line either way from this position, it was invisible. When Venus occupied the centre of the field, this "spectrum," as he calls it, was near the edge; as he moved his eye round, or up and down, the image moved the same way, generally disappearing in the neighbourhood of the planet. A set of experiments instituted in consequence satisfied him that this image was formed by rays reflected first from the convexity of the "pupil" (cornea), and a second time from the concave face of the meniscus lens which in this case formed the eye-glass, though it would be shown by any eye-piece possessing a surface concave towards the retina. Cases were even possible, but difficult in management, when an image might be seen, though the object was not in the field; but this was formed by rays passing outside the telescope, and the ghost would be inverted and of much smaller dimensions. The magnitude of the image would depend on the proportion of curvature of the reflecting surfaces. This being once understood, the Abbot found that he could always produce, for himself or others, a spurious satellite of Venus, or Mars, or Jupiter, under the following essential conditions:—That the power should not be less than 50 or 80, or the image would be too minute to be visible, or would only resemble a small star;—that the eye must be placed at a definite distance from the eye-glass, and be moved most deliberately and cautiously backwards and forwards to find that point, the limit of visibility being sometimes only a quarter of a line either way;—and that the eye must be a little on one side of the optical axis, or the image will coincide with its primary. And it becomes readily intelligible why an observer, ignorant of these conditions, may never be able to recover an image which he had once accidentally seen. Thus far, in substance, the astronomer of Vienna, who certainly deserves credit for his ingenious and careful investigation. His reasoning is, nevertheless, a curious and instructive exemplification of the way in which a preconceived opinion may block up the mental view, and prevent a sound argument from being carried out to its legitimate consequences.

We are now in a position to examine how far this criterion is applicable to the recorded phenomena. Of these, Dr. Schorr has enumerated sixteen, in a table taken apparently from Lambert, but with the addition of an observation by Andreas Meier (Mayer). Hell had given three from Fontana, but Lambert seems to have thought one only of any consequence, and even this may well be omitted, leaving the following for our consideration.

The name of Cassini at the head of them at once commands attention, but there is nothing in his two observations in 1672 and 1686 that does not lend itself to Father Hell's hypothesis, excepting the care and experience of such an observer, who must have been familiar with every telescopic defect. The observation of Meier, which seems to have lain unnoticed in the *Astron. Jahrbuch*, 1788, till brought forward by Schorr, is on that account worthy of being cited in full. "1759, May 20, about 8h. 45m. 50s., I saw above Venus a little globe of far inferior brightness, about 1/12 diam. of Venus from herself. Future observations will show whether this little globe was an optical appearance or the satellite of Venus. The observation was made with a Gregorian telescope of thirty inches focus. It continued for half an hour, and the position of the little globe with regard to Venus remained the same, although the direction of the telescope had been changed." During so lengthened an observation it seems natural to suppose that the eye must have been repeatedly removed and replaced, which could not have occurred without the detection of an optical illusion.

In 1761, when the expected transit drew attention to Venus, Montaigne, at Limoges, was persuaded to undertake the inquiry, though he had little faith in the existence of the satellite, and was not greatly disposed to enter upon an examination in which so many great men had failed. However, on May 3 he saw a small crescent

[1] "Der Venusmond," &c., von Dr. F. Schorr. Braunschweig, 1875, pp. 186.

20′ from Venus; it is expressly stated that the observation was repeated several times, and that after all he was not certain if it was not a small star; which, with a power of between forty and fifty, was not surprising. The next evening and on the 7th and 11th it was again seen, rather more distant, and each time in an altered position, but with the same phase as its primary; and on the 7th it was seen, and even much more distinctly, when Venus was not in the field. The improbability is obvious of such persistency in an illusion so readily detected. The cause may indeed have lain in the object-glass; such telescopes have been known. Wargentin, at Stockholm in the same year, found that his instrument produced a deception from this cause; and the 6-inch Cauchoix achromatic at Rome showed minute comites to bright stars a little too frequently for the credit of those who trusted it. Montaigne's changed position-angles may be thought to indicate this cause of error, as his 9-ft. refractor probably admitted of rotation in its bearings, but it is a singular coincidence that these changes should all have been in the direction of orbital revolution, and still more, in such proportions as to be reconcilable with Lambert's calculated period of about eleven days; and it is quite unintelligible that he should not have subsequently detected the fault in his telescope, as from his estimation of angles and distances he was evidently not a novice in observation. Three years later, in 1764, Rödkier, in Copenhagen, saw such an appearance on two evenings with a power of thirty-eight on a 9½ ft. refractor; on the latter occasion with a second telescope also. There is little in this to contravene the Vienna theory, especially as this second telescope had a coloured meniscus eye-glass, and he failed in finding it with two other instruments: but it is more remarkable that on two evenings a week later the same telescope told the same tale to four different observers, one of whom was Horrebow, the Professor of Astronomy, and who, we are assured, satisfied themselves by several experiments before the second observation that it was not a deception. That the necessary conditions for its being such could have been maintained before so many eyes, is, notwithstanding its admitted pale and uncertain aspect, what could not possibly have been anticipated. But we have not yet done with this temporary outbreak, so to speak, of visibility. Before this month of March was ended, Montbarron at Auxerre, far removed from all possibility of communication, and with a very different kind of telescope, a Gregorian reflector of thirty-two inches, which of course was fixed as to its optical axis, perceived on three separate evenings, at different position-angles, something which, though it had no distinguishable phasis, was evidently not a star, and which he never could find again.

There remains still the observation of the celebrated optician Short. It is indeed chronologically misplaced here, but has been intentionally deferred as affording the strongest point in the whole affirmative evidence. As his own account is an interesting one, and has seldom, if ever, been reprinted, our readers may not be displeased to see it here as it stands in Phil. Trans. vol. xli.:—

"An Observation on the Planet Venus (with regard to her having a satellite), made by Mr. James Short, F.R.S., at sunrise, October 23, 1740.—Directing a reflecting telescope of 16·5 inches focus (with an apparatus to follow the diurnal motion) towards Venus, I perceived a small star pretty nigh her; upon which I took another telescope of the same focal distance, which magnified about fifty or sixty times, and which was fitted with a micrometer in order to measure its distance from Venus, and found its distance to be about 10° 2′ 0″ (*sic*). Finding Venus very distinct, and consequently the air very clear, I put on a magnifying power of 240 times, and to my great surprise found this star put on the same phasis with Venus. I tried another magnifying power of 140 times, and even then found the star under the same phasis. Its diameter seemed about a third, or somewhat less, of the diameter of Venus; its light was not so bright or vivid, but exceeding sharp and well defined. A line, passing through the centre of Venus and it, made an angle with the equator of about eighteen or twenty degrees. I saw it for the space of an hour several times that morning; but the light of the sun increasing, I lost it altogether about a quarter of an hour after eight. I have looked for it every clear morning since, but never had the good fortune to see it again. Cassini, in his Astronomy, mentions much such another observation. I likewise observed two darkish spots upon the body of Venus, for the air was exceeding clear and serene."

It has been justly asked by Schorr whether this observer, who was the greatest optician of his time, must not have known his telescopes better than to mistake the reflection of Venus on the eyeglass for a satellite? And Lambert puts the case very strongly, remarking that Short had the object before him for a whole hour with greatly varied powers, and it is not probable that he kept his eye immovable all the time, and after every change in the telescope replaced it at the precise point where the apparent position and distance from Venus would continue unaltered, especially as he used so high a power, with which the slightest change would have been remarked, and a micrometer, the employment of which would have necessarily implied movement in the eye. Lambert might have further strengthened his argument had he had an opportunity of consulting the original record, which shows that another telescope was employed, making in all four eye-pieces, and that Short viewed it not continuously, but at intervals during an hour, increasing every time the chance of detection; nor should the important consideration be overlooked that, with the higher powers, the apparent motion of the planet through the field would be rapid enough to give the illusion a movement in the reverse direction, which would unmask it at once. An examination of one of Short's reflectors might be necessary to decide whether with *his* power of 240 (he was said to have considerably over-rated his magnifiers) the field would have included the attendant with the primary.

The evidence against Father Hell's explanation had even previously become very formidable. The conditions under which his "ghost" is visible are so restrained, the limits so narrow, that there is considerable presumption in any individual case against such an illusion having been formed, or at least against its having passed unchallenged, when a trifling change in the supposed obliquity of indirect vision would at once shift the position of the false image with respect to its origin, and an equally minute alteration in the distance of the eye would deface or obliterate it. But if this is so in each separate instance, the enumeration of so many, with instruments and observers so varied, increases the improbability afresh at every remove, and the careful observation of a man like Short is peculiarly conclusive against the possibility of deception, at least from the assigned cause.

Thus far the advocates of a satellite have it their own way; and to what has been said they would add some curious facts as corroborative evidence. The object, when its size has been remarked, has always been recorded of the same magnitude, one-fourth, or less than one-third, of its primary. It showed itself seven times in one month (March 1764), at a period when telescopes were no longer in their infancy, and in two places at a great distance from each other. And its position-angles, which chance would have placed anywhere, agree sufficiently well with orbital revolution to admit of the calculation of a period, which Lambert has given at 11d. 5h., to which, however, Schorr prefers his own of 12·17d. Many astronomical details are probably accepted among us for which there are no stronger grounds of belief.

But it is one thing to invalidate an opponent's conclu-

sion—another, to establish one's own. As we have already remarked, the abandonment of Hell's solution is not the demonstration of a satellite; and we have yet to hear the opposite side. Some adverse points we have noted as we have passed along; and we might have added the fact that at the epoch of Rödkier's second observation Uranus and Venus were not far apart; perhaps "within blundering distance." But of course the main strength of the denial lies in the fact that, though the alleged appearance can require but little optical advantages, it has been so frequently sought in vain through a long series of years. During that very spring of 1764, when the primary occupied an especially favourable position, it was very carefully looked for by many observers—among others, the acute and experienced Messier, but nowhere seen except at Copenhagen and Auxerre. Cassini and Short, with interest awakened by their own apparent success, could never with all their diligence recover it; and the latter, twenty-three years after his own striking observation, was thought by Lalande, then in London, to disbelieve the satellite's existence. Not to mention Bianchini and others, the elder Herschel never saw a trace of it; nor Schröter, the close observer of Venus during fifteen years; nor Harding, nor Struve, nor Lamont, Smyth, De Vico, Secchi, or any other of the first observers armed with the first telescopes of modern times. And though the subject has now ceased to attract attention, yet, in the unprecedented multiplication of observers and instruments, it would hardly have had a chance of escape. On the whole, therefore, though the evidence may exclude the intrusion of an ordinary "ghost," it seems irresistible against the reality of a satellite.

What, then, was that which was seen? for that something really has been seen, the character of some at least of the witnesses renders a certainty. A reflection in the telescope independent of the position of the eye would have been always visible as a permanent defect; and the fact of its never recurring is equally adverse to the idea of a satellite, and that of an instrumental deception. The only alternative which remains would seem to be that of atmospheric reflection, or "mirage." There would certainly be some difficulty in finding a parallel among recorded facts, though Brewster, if I recollect aright, speaks of having once seen two images of the crescent moon; but the known instances of atmospheric illusion are some of them so very strange and inexplicable, and yet so abundantly attested, that we may possibly, though with little confidence, seek in this direction a solution of the ancient mystery.

Before concluding these remarks, I may be permitted to relate something which fell under my own notice many years ago, and which may perhaps have some connection with the present subject. The observation which I am about to describe took place in the year 1823; it was not reduced to writing till nine years afterwards, but the recollection of it was then very vivid and fully to be trusted; and a small diagram of the relative position of the objects made at the time in the margin of a pocket-book of that year fixes the date to May 22. Until that evening I had never seen the planet Mercury, but finding that he was then in a favourable position I looked out for him with a little common hand-telescope (my near sightedness and the want of an eye-glass preventing me from detecting him otherwise), and soon found him low in the sunset horizon. The telescope in question had a good achromatic object-glass of 1·3 inch aperture and 14 inches focus, and was fitted with a terrestrial eye-piece, magnifying perhaps thirteen or fourteen times; it was a favourite instrument in those early days, and I had succeeded in detecting with it several of the brighter nebulæ and clusters, especially, at the extreme limit of visibility, the large nebula in Triangulum (M. 33). When I had looked at Mercury, I turned to Venus, then high in the S.W., and saw a star, exactly resembling Mercury, or a miniature Venus, *p* or *s p* the planet, at a short distance, perhaps 20′ or 30′, and ⅓ or ¼ of its diameter, or rather its impression on the eye, as of course with so low a power the disc of the planet could not be well made out. I had, when I wrote, a very distinct recollection of its great resemblance to Mercury. My mother, who had an excellent sight, coming into the garden, I showed her Mercury and this appearance with the glass, and she not only saw it readily, but we both believed afterwards that she perceived it without that aid. On the next evening, or more probably on the next but one, I could not find it again. As far as I can ascertain, I had in those early days no knowledge of the suspicion that had been entertained of a satellite: and I did not enter it, as in that case I should have done, in a little note-book of remarkable phenomena that I kept. Through the kindness of Mr. Lynn I have been enabled to ascertain that the star ε Geminorum was not far from the planet on that day, only about 30½′ further S., which would agree very fairly in that direction, but lying 6½ m. more to the E. Independently of this discrepancy—a serious one, for I have no doubt of the *p* or *s p* position of the satellite, not only clearly remembered but shown in the little diagram—it does not seem probable that a star of 3–4 mag. should have been so conspicuous in such an instrument in the twilight. I have no note of the hour, but as Mercury had not sunk into the smoke of the town (Gloucester) in the W. horizon, it must have been comparatively early, and at that time of year the twilight is strong. It may be too hazardous under all the circumstances to include this with the other observations of the pseudo-satellite, but there seems no reason why it should pass into entire oblivion.

T. W. WEBB

THE MISSING LINK BETWEEN THE VERTEBRATES AND INVERTEBRATES[1]

THE views which Dr. Dohrn has recently put forth as to the details of the steps by which the vertebrate stock arose out of an ancestry not very much unlike the existing Annelids, are of such interest that, notwithstanding previous reference to the subject, no apology is needed for presenting the readers of NATURE with a condensation of the main argument contained in "The Origin of Vertebrata."

Dr. Dohrn first draws attention to the correspondences between vertebrate and insect embryos, which have been too little regarded in consequence of our designating the nervous side in the one as dorsal, in the other as ventral. Yet the facts that, in both, the nervous system is developed on the convex side of the embryo and acquires a strong convex flexure anteriorly, and that the body-cavity is finally closed up on the side of the body opposite to the nervous system, point to a common origin at a comparatively high level. The surface of the animal which is called ventral is determined by the presence of the mouth on that surface; and if any Vertebrates had a mouth-opening between the brain and the spinal cord on the dorsal surface, that dorsal surface would necessarily become ventral. Since, moreover, the ancestors of the Vertebrata must have had a nervous ring surrounding their gullet, it would appear more reasonable to suppose that the mouth-opening had been changed in the course of development than that the situation of the nervous centres had been altered. We are thus led to look for traces of an old mouth-opening on that surface of the early Vertebrates which corresponded to our dorsal surface, and to seek reasons for regarding our present mouth as a comparatively modern development.

Dr. Dohrn believes that the old mouth passed through the nervous centres between the crura cerebelli, or more

[1] Der Ursprung der Wirbelthiere und das Princip des Functionwechsels: Genealogische Skizzen von Anton Dohrn. (Leipzig: Engelmann).

accurately, in the fossa rhomboidea, or fourth ventricle, which is remarkable for being of greater proportionate size early in development, and afterwards undergoing retrogression. At an early stage we only need to conceive a slit to be made in the nerve tube at the bottom of the fossa rhomboidea, in order to furnish a suitable passage into the alimentary canal. His first reason for regarding the vertebrate mouth as a modern structure is that it arises so extraordinarily late in development. The embryonic body is almost completely framed, all the great systems are established, the circulation is in active operation, while as yet there is no mouth. Again, the mouth does not arise in the position in which it permanently remains in the great majority. It undergoes considerable shifting forwards. Only in the Selachians and Ganoids does it retain its primitive situation. Moreover, the study of development is steadily tending to establish the idea that the mouth of Vertebrates is homodynamous with the gill-clefts. It is limited, like them, by a pair of arches, lies just in front of the first pair of gill-clefts, arises simultaneously with them in the embryo, and opens into the alimentary canal. A glance at the ventral surface of a Ray shows the likeness of the mouth to a pair of coalesced gill-clefts. Consequently, it becomes probable that the present mouth-opening once existed and functioned as a gill-cleft; that at a certain period in the ascending development, both the old and the new mouths supplied nourishment, that the latter gained the predominance, and that finally the old mouth became aborted.

The next problem attacked is the origin of the gill-clefts. A very elaborate account is given of the supposed process by which the external gills and segmental organs of Annelids were metamorphosed into the gills and gill-clefts of Vertebrates and the skeletal elements connected with them. The great difficulty which Dr. Dohrn confesses in this matter is the connection of the inner extremities of the segmental organs with the wall of the alimentary canal. But if this be granted it is comparatively easy to understand how the shortening and widening of the segmental organs might give rise to gill-cavities such as those of the Selachians. The process by which Dr. Dohrn conceives that the limbs of Vertebrata might have been developed from two pairs of gills in Annelids is a great evidence of ingenuity, though it is to be expected that it will be viewed rather incredulously.

It follows from the view of the origin of Vertebrates thus expounded that Amphioxus loses much of its interest, for there is no place for Amphioxus among Annelids, nor among the primordial Vertebrates; it lacks almost all that they possess. Yet nothing can be gained by excluding Amphioxus from the Vertebrates; for it is so connected with the Cyclostome fishes that it cannot be placed at any great distance from them; while on the other hand it is so related to Ascidians, that the latter must be included among the Vertebrata.

Dr. Dohrn then proceeds with a long argument to show that the Cyclostome fishes are degenerate from a higher type of fishes, and that Amphioxus is a result of still further degeneration. He shows how their mode of life necessitates many of the modifications they have undergone; and that the diversities of the details of structure in Cyclostomes are inconsistent with their being viewed as representing stages in upward development. Finally, the larva of Ascidians is represented as a degenerate fish—a degenerate Cyclostome possibly—which carries to the extreme all the departures of the latter from the fish-type. The most important element in this degeneration results from the fact that Ascidians, instead of being attached to fishes or to any objects from which they can derive nutriment, are fixed to stones, plants, &c., or to such parts of animals (cephalothorax of crabs, tubes of tubicolous annelids) as do not afford them nourishment. Consequently they have lost the old mouth in the organ of attachment, homologous with that of all Vertebrates, and have developed a new one, homologous with the nasal passage of *Myxine*. Thus we can explain the astonishing fact that the mouth-opening of the Ascidian-larva has a communication with the fore-wall of the so-called cerebral vesicle. It is the last vestige of the openings in the nasal sacs by which the olfactory nerves entered.

The most patent objection to Dr. Dohrn's view about Amphioxus is that it fails to account for the development of a many-segmented respiratory apparatus as a degeneration from a higher animal with a small number of gill-arches. It would appear far more reasonable to suppose Amphioxus to be a degeneration from a much lower elevation than the Cyclostome type, viz., from some stage where the respiratory apparatus retained the multiserial character derived from its Annelid forefathers.

The keynote of the author's reasonings is to be found in the principle of Transformation of Function (*Functionswechsel*), on which he lays great stress. He states it as follows:—The transformation of an organ happens through a succession of functions being discharged by one and the same organ. Each function is a resultant of several components, of which one constitutes the chief or primary function, while the others are lower or secondary functions. Diminution of the importance of the chief function with increase of the importance of a secondary function, alters the entire resultant function; the secondary gradually rises to be the chief function, the resultant function becomes different, and the consequence of the whole process is the transformation of the organ. This principle is considered to be a complete answer to the difficulty so strongly insisted on by Mr. Mivart, the incompetency of natural selection to account for the incipient stages of subsequently useful structures. Dr. Dohrn's statement of his principle does not strike us as very different from Mr. Darwin's ("Origin of Species," 5th edition, p. 251), though a little more definitely stated. Mr. Darwin says: "The same organ having performed simultaneously very different functions, and then having been in part or in whole specialised for one function; and two distinct organs having performed at the same time the same function, the one having been perfected whilst aided by the other, must often have largely facilitated transitions." The illustrations given by Dr. Dohrn of the steps by which the anterior extremities of Crustacea became applied to mastication, how the mouth of Vertebrates originated from a pair of gill-clefts, how the respiratory apparatus of Tunicates originated from that of Vertebrates, &c., are, however, exceedingly interesting.

An English translation of Dr. Dohrn's pamphlet could not fail to be serviceable to the large number of students who take an interest in the genealogical problems of morphology.

G. T. BETTANY

MAGNETIC OBSERVATIONS IN CHINA[1]

THE first annual report of the magnetic observations at this new observatory has just reached Europe, and it contains results of considerable interest to those engaged in the study of terrestrial magnetism.

The position of Zi-ka-wei is 31° 12′ 30″ N., and 8h. 5m. 45s. E. of Greenwich, being rather less than four miles to the S.W. of Shang-Hai. The observatory is in possession of an excellent set of instruments for determining the absolute values of the magnetic elements, procured by the kind assistance of the Director of Kew Observatory, and a set of self-recording magnetographs by Adie, verified at Kew, have just been erected in a suitable building. The observer, the Rev. M. Dechevrens, S.J., spent a considerable time at Stonyhurst Observatory previous to his departure for China, in order to make himself thoroughly acquainted with the methods of observation, and with the use of the instruments.

The observations in the report extend from April 1874 to March 1875, and furnish the following data for the epoch Oct. 1, 1874:—

[1] "Observatoire Météorologique et Magnétique de Zi-ka-wei." Chine, Magnétisme Terrestre, 1874-5.

Declination	1° 54′·72 W.
Dip	46° 15′
Total Force	10·04850

The value of the declination is very reliable, as it depends on observations taken every half hour from 6 A.M. to 6 P.M. on four days each month in 1874, and on eight days a month in 1875. The dip results from six complete observations, and the horizontal component of the intensity was determined twice a month in 1874, and every week in 1875.

Previous dip observations at Shang-Hai, by Sir E. Home in 1843, and by Capt. Shadwell in 1858, give − 2′·2 and − 3′·4 as the secular variation for 1851 and 1862, the latter differing but slightly from the present variation in England.

Comparing the monthly means of the horizontal force for the winter and summer of 1874–75, we find an excess of 0·00074 in favour of the winter, when the sun is nearest the earth. The extreme variation is only 0·00577, and both maximum and minimum occur in the summer months.

From a limited number of night observations it appears that the range of the declination needle is much more confined, whilst the sun is below the horizon than during the day hours. The diurnal variation is regular throughout the year, but the daily changes in winter are less simple than those of summer. The following are the mean results for the separate seasons :—

	Mean.	Min. at	Max. at
Spring	1° 50′ 49″	9 A.M.	1° 47′ 33″ ... 2 P.M. 1° 54′ 3″
Summer	1 49 39	8 „	1 45 45 ... 2 „ 1 53 3
Autumn	1 59 35	9 „	1 58 9 ... 1 „ 2 1 10
Winter	1 58 51	9 „	1 57 32 ... 1 „ 2 0 5

The time of the principal minimum is more constant than that of the maximum, the latter being anticipated by one hour in winter.

A sudden change from 1° 50′ 13″ on Sept. 21 to 1° 56′ 51″ on Sept. 26, 1874, seems to require further confirmation (which it did not receive in 1875) before it can be considered as more than accidentally connected with the passage of the sun through the autumnal equinox.

The monthly mean value of the declination is greatest in November and least in June, and the absolute maximum and minimum were :—

2° 3′ 49″ at 11h. 15m. A.M. on November 8,

and

1° 41′ 58″ at 9 A.M. on June 29.

giving a yearly range of only 21′ 51″, whilst the secular variation amounts to + 5′·85. The value on Nov. 8 was also evidently increased by some irregular disturbance.

The comparison of the yearly means for the different hours with the hourly means for each season, shows that the sun's position with regard to the equator has a decided effect on the magnetic declination, as increase and diminution in summer invariably correspond with diminution and increase in winter.

In discussing the hourly velocity of the needle, it is found that the acceleration is greatest between 10 and 11 A.M., when the magnet is near its mean position, and that the A.M. maximum velocity is an hour earlier, and the P.M. maximum an hour later in summer than in winter, the greatest velocity being about 1″·5 per minute.

The mean amplitude of the daily excursions of the declination magnet is 7′·88 in summer against 3′·68 in winter, June giving the maximum mean amplitude of 9′·06, and December the minimum of 2′·95. The value of 1′·92 in February appears to be exceptional. The greatest extent of a daily oscillation in the course of the twelve months was 11′·05 on June 1, and the least 1′·13 on Feb. 20, giving a maximum yearly variation of 9′·92.

The changes of the magnetic elements appear to be remarkably small throughout, and very free from irregular disturbances. The care with which the observations are taken, and the efficient way in which they are discussed, are an earnest of the plentiful harvest we have every reason to expect from this land once so famous, but hitherto so neglected by modern science.

Stonyhurst Observatory, April 13 S. J. PERRY

THE CHALLENGER EXPEDITION

WE have great pleasure in availing ourselves of the permission to publish the following correspondence which has passed through our hands, and in congratulating the staff of the *Challenger*, on having deserved so weighty a testimonial of success. It is an additional assurance that their three years' labour has not been in vain, that so many distinguished men of science have been impelled to speak of it in such terms, as well as a guarantee to the British Government that they did a wise thing in equipping the expedition ; we hope it will be an encouragement to the latter to continue to deserve such golden opinions.

To the Editor of "Nature"

Vienna, June 12, 1876

SIR,—After having followed the reports of the naturalists of H.M.S. *Challenger* with the utmost interest, we beg leave to ask you kindly to transmit this simple but sincere expression of a hearty welcome and of thankful admiration to these distinguished gentlemen, as well as to the officers and the crew of this gallant ship, which has been called to render such prominent services to science. Yours most respectfully,

EDW. SUESS, M. P. Prof. University, Vienna,
C. CLAUS,
G. TSCHERMAK,
F. STEINDACHNER, Director of the Imper. Zoolog. Museum,
Dr. FR. BRAUER, Custos of the Imper. Zoolog. Museum,
E. v. MARENZELLER,
Prof. Dr. J. HANN,
F. KARRER,
TH. FUCHS, Custos am k.k. Hof. Min. Cab.,
PELZELN, Custos am k.k. Zoolog. Cabinete.

To this the following reply has been made by Sir C. Thomson :—

To the Editor of "Nature"

20, Palmerston Place, Edinburgh, June 23, 1876

MY dear Sir,—I received your note and enclosure last evening. Will you allow me through you to express on my own part and on that of my colleagues Civilian and Naval on board the *Challenger*, our deep gratification at the kind way in which the leaders of Natural Science in Vienna have expressed their approval of our efforts to extend the limits of knowledge in Physical Geography ?

We hope that the Empire, which by the most instructive voyage of the *Novara* immediately preceded us in a similar line of research, may be among the first to aid in filling up the rich details of the new zoological region of which we have been able hitherto to supply only an outline.

I am, my dear Sir, yours very faithfully,

C. WYVILLE THOMSON,
Director of the Civilian Scientific Staff of the *Challenger* Expedition.

ABSTRACT REPORT TO "NATURE" ON EXPERIMENTATION ON ANIMALS FOR THE ADVANCE OF PRACTICAL MEDICINE [1]

III.

Experimental Researches on Anæsthesia Local and General.

THE revival of methods for rendering surgical operations on men and animals perfectly painless, while it has been one of the greatest of the advances of modern medical art, has not been without its alloy. The present generation can scarcely appreciate what were the scenes of the operating theatre before the introduction of anæsthesia. The present generation that is not medical cannot appreciate now what is the scene at an operation when the agent employed to prevent pain proves an agent of death. One surgeon I know has been present at six of these fatal catastrophes under and from anæsthetics. Such an experience shakes the strongest heart. Here is a human being talking cheerfully and resigning himself with full confidence to his medical friends. The operation to be performed may be the act of seconds

[1] Continued from p. 152.

only, but the dread of the pain enforces on the operator the necessity of administering the anæsthetic. A few inhalations of the narcotic vapour are made, and in an instant the body, a moment or two ago animated and full of life and energy, is lifeless in the hands of the administrator of the narcotic.

There is no more painful agony to a practitioner of medicine than a catastrophe of this character. He feels as if the whole beneficent art of anæsthesia were, after all, a mockery; as if it were better that tens of thousands should suffer pain than that one should die under his directing hand merely to save a brief period of pain.

From the first of the reintroduction of anæsthetics these unhappy fatal failures from them have occurred to darken with the shadow of death the retreat of pain from the earth. What more natural, what more humane a labour than that which is devoted to the discovery of a means by which this shadow of death may also be made to fade from the picture? To me this labour has been a life's work. I have pursued it in two directions.

(*a*) By endeavouring to discover anæsthetic methods which shall carry with them no danger to life.

(*b*) By endeavouring to discover means that shall restore safety when danger is incurred from the use of the present imperfect anæsthetics.

In conducting both these lines of research it has been necessary to experiment on the inferior animals. There is no other method. If the most promising new chemical agent for anæsthesia were put into my hands to-day by the scientific chemist, I could not administer the agent direct to the human subject on mere speculation. It is true I have, from long experience, been able so to understand the characters of anæsthetics that I can formulate them theoretically. If the chemist gives to me a substance and tells me its atomic composition, its physical properties of solubility, of weight, vapour density, and boiling point, I know at once whether it is or is not an anæsthetic, and I can reject on the spot some substances from and by reason of this knowledge, all of which, by the way, has been acquired by experimental research. But if the chemist gives to me the very thing I want it is still impossible to proceed to apply it to practice on man before testing its action on animals inferior to man, for I have found that some of the very simplest and seemingly most innocuous of substances are most fatal.

One of the pioneers of anæsthesia with whom I had the privilege to live and work, did once introduce into practice a new, effective, and, in atomic construction, very simple anæsthetic. In the course of a comparatively few administrations of this agent to man, two deaths resulted. To the end of the useful life of this, my friend, he never ceased to regret that he had not first subjected the agent to more vigorous tests of action on animals inferior to man. Once in my researches I got under observation another anæsthetic which seemed perfect. I should have introduced it into practice, had not the lesson I had learned above corrected the error. For on submitting the new agent to the required strain of experiment, I found it so fatal to animals that had I put it forward I should certainly have deepened the shadow of death on the picture of retreat of pain. Twice in the same manner I have prevented other men from introducing anæsthetics which did not bear the full test of proof of experiment on the inferior animal. The reasonable mind will take in all these practical points, and, I think, will come to the conclusion that for no application to the necessities of man and of all other animals could the lives of inferior animals be more justly applied. To kill animals for food, to apply them to works of useful labour, is not more just.

Method of Experimentation.

The method of experimentation I have pursued has taken two courses :—

(*a*) The subjection of animals to narcotising gases or vapours for the purpose of inducing in them anæsthetic sleep, observing the action of the narcotic through all its degrees of action, and the mode in which it destroys life when it is pushed to the point of destruction of life.

(*b*) The subjection of animals to local methods of abolishing pain, or, more correctly, of destroying pain in parts of the body locally, so that operations may be performed painlessly while the general consciousness remains, and without any danger at all to life.

In carrying out the first of these inquiries, the plan pursued was as follows :—A narcotising chamber was used, the precise capacity of which was determined. The chamber, made of glass and iron, was, when closed, air-tight, but it was furnished with openings through which it could be charged with the precise measures of the narcotic vapour or gas required. It was also so arranged that the temperature and dryness, and when necessary, pressure of the atmosphere within it could be moderated. Briefly, the chamber was so constructed that the action of every volatile narcotic substance could be tested in it under all known external conditions.

The animals subjected to experiment have as a rule been of two kinds—rabbits and pigeons. Rabbits have been used because when they are allowed to sleep to death in the vapour, or when they accidentally sleep to death, they are good subjects for examination after death, and tell clearly the reason of death. Pigeons have been used for two reasons : first because they succumb more easily to anæsthetics than any other animals, easier even than man; secondly because during sleep they give indications of dangerous or troublesome effects, such as rigidity and vomiting, quite as easily as man. If, therefore, a pigeon will go safely and easily through an anæsthetic sleep, the inference is fair that a man will do so; and in all cases where I have found the anæsthesia so safe and satisfactory on these animals—rabbits and pigeons—as to commend the anæsthetic which produced it, I have always proceeded to try the effect on the human subject by inhaling the anæsthetic myself until it produced the insensible sleep.

In experimenting on the animals, they have been gently introduced into the narcotic chamber from above, and as they have passed into insensibility, each of the stages of narcotism—usually four in number—have been carefully recorded by their phenomena. The facts have been tabulated in set form so as to show, per-centage of vapour diffused, time required to produce insensibility, period of each stage, muscular disturbance, state of the respiration, state of the heart pulse, change of animal temperature, and condition of the pupil. In cases of recovery from the anæsthetic, the signs and period of recovery have been recorded; in cases of death in the anæsthetic sleep, the time and mode of death whether by the heart or by the respiration, have been recorded.

I should remark that these researches have not been made at any regular times. They have been suggested by the study of some chemical substance which presented some promising qualities for the object in view. I believe no new substance of this kind has for the last twenty-five years escaped my observation.

On the animals themselves no pain can be said to have been inflicted. The worst that has happened to them has been that they have passed into deep sleep and have waked again just as a human being who has taken chloroform successfully for an operation, sleeps and wakes. Or else they have passed into sleep and from sleep into death, a mode of dissolution so serene, so painless, as to be an enviable imitation of natural euthanasia.

In the researches on local means of relieving pain, the part to be anæsthetised has been simply subjected to the action of the anæsthetic. At first I used lower animals for this method of inquiry, but owing to their comparative low sensibility they proved unsatisfactory. A mode of local anæsthesia which on a dog or rabbit seems abso-

lutely perfect may, I found, be most imperfect on a man or woman. I once thought I had established a perfect local anæsthesia by applying to animals narcotic solutions locally, in combination with a gentle continuous electric current. It seemed to me that the current caused a rapid absorption of the narcotic, or so acted with it on the minute blood-vessels as to produce contraction of them and destroy local insensibility. Under this plan I performed a number of operations on the lower animals without exciting the slightest evidence of pain. When I came to man the process broke down; some insensibility was, without doubt, produced, and seventeen operations were performed by the local plan. But the more exalted sensibility of the higher animal was not satisfied, and I learned that what would do perfectly for a dog was quite inefficient for a human being.

It is a curious episode in this research and worthy of record, that one of my scientific critics, the late Dr. Waller, a man of great genius, actually showed that he could perform on dogs without any anæsthesia at all, the same operations that I performed with this local anæsthesia, and with similar apparent freedom from pain. The result was that I continued all my after experiments on local anæsthesia, first on my own body, and then on other human subjects who required such anæsthesia for operation. All my experiments with sprays to produce insensibility by intense cold, on Dr. James Arnott's most original design were first performed in this manner, and the process was only applied to the inferior animals after it had been made perfect for the surgical purposes for which they required it. In this instance therefore man became the subject of physiological experiment for the benefit of the inferior animals as well as for his own.

Primary Results of the Experimentation with Anæsthetics.

The primary results of these experiments on different modes and processes for inducing anæsthesia may be put forward in a few sentences. They were all of them results which could not have been reached by any other line of research.

1. The experimentation has enabled me, as a physician, to keep on a level with the chemist in applying to the services of man all those agents for the relief of pain which the chemist produces. The chemical bodies of the methyl, ethyl, bulyl, and amyl series with several others which have promised to be of any service have been tested, and their respective values carefully chronicled.

2. For general anæsthesia I have been enabled, by the research to add many new and useful anæsthetics. Bichloride of methylene, which has been very largely used, and which Mr. Spencer Wells invariably uses with signal success for ovariotomy, came from this research. Methylic ether, the safest anæsthetic I have yet known, was proved by this research. Methylal, another very valuable agent of the same kind, and which has to be practically applied, is another good anæsthetic added by these inquiries; while several agents tried for anæsthesia which have not answered, have been accidentally discovered to possess other and valuable curative properties. The introduction of the etherial solution of peroxide of hydrogen, an exceedingly useful remedy, and the local use of butylic alcohol for toothache, are two instances amongst many more of this kind.

3. The researches have enabled me to formulate the physiological properties of the organic bodies that produce anæsthesia, so that the value of the anæsthetic compounds may be calculated from their physical characters and composition. I have been able to show that some elements—such as chlorine—are objectionable parts of an anæsthetic agent, others favourable; that certain degrees of solubility are objectionable, others favourable; that certain vapour-densities are objectionable, others favourable. I have been able to point out a distinct theoretical standard of qualities which, being found, will yield a safe, manageable, and agreeable anæsthesia. Lastly, I may add, from an experience in the study of anæsthetics extended from the time when they were first introduced until this hour, the positive assurance that careful and steadily pursued experimental research must result in the discovery of all the laws relating to anæsthesia, and to the further discovery of an absolutely safe mode of producing it. For I have learned that no man, no animal, ever yet has died because it was rendered insensible to pain, and the deaths which have occurred have invariably been due to some property of the substance used that had no relation to the anæsthetic property—some independent bad quality which we may fairly expect science to eliminate for the benefit of man.

4. While striving to apply the results of experimentation to the advantage of the human family, I have not forgotten the inferior creation, and in nothing have I been more successful than in their behalf. For operations on animals I have been able to make the application of local anæsthesia so perfect that there is no necessity whatever that any lower animal should ever feel a pang from the knife of the operator for any external cutting operation it may have to undergo. The Society for the Prevention of Cruelty to Animals has itself published the facts of an operation, for removal of a large tumour from a horse belonging to Sir Wm. Erle, that was performed by my method of operating under ether spray while the animal stood in the stable without halter or bridle, oblivious of all pain. That fact,—one of a hundred similar,—I put forward, not as in itself peculiar, but because of the record from which it is taken. It could not have been recorded even there but for the experimentation that gave it birth.

BENJAMIN W. RICHARDSON

NOTES

HER MAJESTY has been pleased to confer on Prof. Wyville Thomson the honour of knighthood.

IT is stated that Sir C. Wyville Thomson and the members of the scientific staff of the *Challenger* will be entertained at dinner in Edinburgh on July 7. The Lord Provost has consented to take the chair.

WE notice from the official announcement in connection with the Loan Collection, that during the present week, fourteen demonstrations of apparatus were given on Monday, eleven on Tuesday, four on Wednesday and Thursday, while seven will be given on Saturday. With regard to the complaint in the *Times* as to the occasional non-attendance of the lecturers, it should be remembered that these demonstrations are given out of pure good-will by some of the most eminent and busy of the scientific men of the day, who are not always masters of their own time. The Department's arrangements are entirely dependent on the convenience of these men, and it should not therefore be blamed if its proposed programmes are not always rigidly carried out. The following arrangements have been made for future free evening Lectures on the Instruments in the Collection:—Saturday, July 1, Prof. Tyndall, F.R.S., on "Faraday's Apparatus," in the Lecture Theatre, South Kensington Museum; Monday, July 3, the Right Hon. Lyon Playfair, C.B., M.P., F.R.S., on "Air and Airs," as illustrated by the Magdeburg Hemispheres and Black's and Cavendish's Balances; Saturday, July 8, Dr. Gladstone, F.R.S., "The Work of Davy and Faraday," as illustrated by the Apparatus lent by the Royal Institution; Monday, July 10, Rev. R. Main, M.A., F.R.S., on "The Instrumental Foundations of Practical Astronomy;" Saturday, July 15, Dr. W. H. Stone on "Modes of Eliciting and Reinforcing Sound;" Monday, July 17, Mr. C. V. Walker, F.R.S., on "Galvanic Time Signals;" Saturday, July 22, Mr. W. Chandler Roberts, F.R.S.,

on "Graham's Apparatus;" Monday, July 24, Mr. J. N. Douglass, "The Lighthouses on the Great and Little Basses Rocks, Ceylon."

WE would draw the attention of our readers to a leader in yesterday's *Daily News*, in which a proposal is referred to for obtaining a charter to incorporate Owens College, Manchester, into an University. The subject is one of the greatest importance, and now that France is following the lead of Germany in the matter of University reform, we are glad to see some signs that this country is also beginning to feel the necessity of extension and reformation in this direction.

WE read in the *Scotsman* of June 26 that "the prediction of Capt. Saxby that a great storm might be expected last week had a very prejudicial effect on the fishing of Anstruther, and the fishermen suffered a loss of at least 500*l.* from their too ready acceptance of the prophecy." The point to be wondered at is, not that the fishermen of Anstruther, where a terrible loss of life took place in November last, accepted the prediction and acted upon it, but that such a prediction, when made, should be gravely and generally circulated broadcast over the country by the newspaper press, even though, in the present state of our knowledge, two, or at the very utmost three days' forecast of a storm is all that can be attempted, any more distant prediction being the merest guess-work.

AT Monday's meeting of the Royal Geographical Society, a letter from General Stone (Cairo), on "The Circumnavigation of the Lake Albert Nyanza," by M. Gessi, was read. The points of importance in M. Gessi's paper were that the Lake Albert Nyanza is one hundred and forty miles long and fifty broad, and that in the east there is a river flowing into the lake which is now confidently believed to be one of the sources of the Nile. This, Sir R. Alcock said, was a most important result of M. Gessi's expedition, as it made it quite clear that the White Nile issued from the Lake Albert Nyanza. Sir Samuel Baker had written to him (Sir R. Alcock) endorsing the importance of M. Gessi's discoveries, which had established a fact that for eighteen centuries had baffled all the geographers of the world. The secretary read a letter which had been forwarded to the Society by the Earl of Derby, giving a summary of information which had reached her Majesty's government in regard to the movements of Col. Gordon, who expects that within a very short time the interior of Africa will be sufficiently secure to allow both merchants and travellers to traverse the country in perfect safety. A paper was read by Capt. Hay describing the district of Akem in West Africa. He had found the country rich in minerals and studded with well-built towns. The men had a peculiar formation of the cheek-bones which closely resembled horns, the chief executioner having this peculiarity so largely developed as seriously to interfere with the performance of his official duties. The women of the country were free from this deformity.

DURING last week a young living male gorilla was seen at Liverpool for a few days on its way to Hull, and thence to Germany. It had been brought from the West Coast of Africa by the German African Society's Expedition, and measured three feet in height. This is the second specimen of a gorilla which has, with certainty, been seen living in this country. The first during its lifetime, twenty years ago, was mistaken for a chimpanzee.

THE Dublin Corporation have resolved to co-operate with the Royal Dublin Society to invite the British Association to that city in 1878.

THE Italian naturalist Signor Odoardo Beccari has arrived at Genoa, from his fourth journey into New Guinea, and brings with him a valuable collection of objects illustrating the natural history of the country.

LAST week a deputation from Scotland waited upon the Prime Minister to urge that grants should be made out of the Imperial Exchequer to extend and improve the buildings of the University of Edinburgh. They presented a memorial showing that this was very much wanted; that Scotland had already subscribed 81,000*l.* out of a total of 261,000*l.* required; that the University conferred benefits upon the whole country, and on that ground they asked for Imperial funds. Mr. Disraeli said that the subject should occupy the thorough attention of her Majesty's government, with, he was sure, a desire on their part to meet any reasonable expectations.

THE REV. A. H. SAYCE has been appointed Deputy-Professor of Comparative Philology in the University of Oxford. Mr. Max Müller still holds the professorship although absent from Oxford.

LIEUT. WEYPRECHT and Count Wilczek have proposed to the Geographical Society of Paris to co-operate in the establishment of meteorological stations under the polar circle. Nine stations are to be located at Point Barrow, Uperniavik, mouth of Lena, Novaya Zemlya lat. 76°, Spitzbergen lat. 80°, Eastern Greenland, and Finmark. The French Geographical Society is willing to lend its assistance, but very likely will insist upon postponing the establishment of these observatories till 1878, when it is expected news from the English Arctic Expedition will have been received, and advantage may be taken of any facts thus elicited.

PROBABLY few of our readers are aware that at the rooms of the Horticultural Society, at South Kensington, exists a valuable botanical and horticultural library, free alike to Fellows and non-Fellows of the Society. This is known as the Lindley Library, having belonged to the late Dr. Lindley, and since it was purchased by the Society it has received valuable additions. From want of sufficient funds and proper accommodation it is not, however, so useful as it might be; and the Society will be glad to receive additions of books, pamphlets, periodicals, &c. Such gifts, we are sure, would be well bestowed. Communications should be addressed to Mr. W. B. Hemsley, librarian and secretary to the trustees.

THE French geographical journal, *L'Explorateur*, for June 22, has an article on the last cruise of the *Challenger*, in which several of the illustrations in our *Challenger* number for June 1, are reproduced, including a very good woodcut copy of the steel portrait of Sir C. Wyville Thomson.

M. LEVERRIER has appointed a Commission to report on the working of the great reflector, and to suggest improvements. The investigations have been first directed on the mechanical work, which is admirable, and a reward is to be proposed to be given to M. Eichens, the maker. But the optical part is said to admit of improvements in respect to the mirror, which does not appear quite so good as was supposed at first. No pains will be spared to approach perfection as far as possible, as the observatory is to be considered as an annexe of the International Exhibition, and foreign astronomers will be admitted to use the great reflector under certain regulations.

A PHILOLOGICAL novelty in American literature is furnished by the appearance in German of the annual report of the Natural History Society of Wisconsin (Jahresbericht des naturhistorischen Vereins von Wisconsin) for 1876, this being, so far as known to us, the only scientific serial published in that language in America. Canada has one or two French scientific journals, and Mexico several, of course published in Spanish.

IN the *Repertorium für Meteorologie*, vol. v., No. 4, St. Petersburg, Baron F. Wrangell has written a very suggestive paper on the causes of the bora at Noworossisk, a local wind characterised by peculiar violence and destructiveness to shipping in that part of the north Caucasus coast. The author

rests his explanation exclusively on the physical peculiarities of the district and recognised physical laws. To the north-east of the bay, where the bora is most severely felt, and at a distance of about two miles, lies the mountain range of the Waradáh, about eleven miles in length, which, as regards winds, cuts off all communication between the coast and the interior, except over the ridge of the chain; and, further, has several valleys on the landward side of the range looking to north-east. It follows that on particular occasions, notably when the wind is in the north-east and light, the air resting on the bay and shore adjoining will be widely different in temperature, humidity, and consequently density, from the air on the other side of the range. Observations render it highly probable that it is just on such occasions that the bora occurs. Baron Wrangell's hypothesis regarding the bora is that it is occasioned by the overflow, by way of the ridge, of the dry, cold, and dense air of the interior down upon the moist, warm, and light air which fills the basin of the bay—a supposition in accordance with all the known phenomena accompanying the bora, including the hour of the day and the general weather conditions under which it occurs. In the neighbouring bay of Gelendschik, on the other hand, which has a deep valley opening directly into it from the north, and therefore does not afford such facilities as Noworossisk does of bringing together, with only a ridge between them, two widely different masses of air, the bora is much less sudden and violent. From the practical and scientific importance of the inquiry, we hope Baron Wrangell's suggestion will be carried out, and several stations be established in addition to the present stations, at different heights on both sides of the Waradáh chain, for observations of pressure, temperature, humidity, and winds, so that the causes from which the bora and other violent local winds take their origin and attain their greatest intensity, may be determined.

THE Rev. R. Main, of the Radcliffe Observatory, has published a short paper on the rainfall at Oxford for the past twenty-five years, with tables of the monthly and annual amounts, the summer rainfall of each year, all the days on which an inch of rain or upwards fell, and the daily amounts during October and November, 1875. In every way in which the figures can be looked at, October is the month of greatest, and February that of least, rainfall, as holds generally over nearly all the south of England. June, which in the north-west of Great Britain is the month of least rainfall, has at Oxford a rainfall exceeded only by that of October, a result doubtless due to the much greater prevalence of thunderstorms at this season at Oxford, and of those weather conditions out of which thunderstorms originate. Another noteworthy feature of the Oxford rainfall is the small amount in December as compared with January. The average annual amount is 25·775 inches, the least 17·564 inches in 1870, the greatest 40·416 inches in 1852; the greatest monthly fall 7·531 inches in October 1875; the largest daily fall 2·050 inches on July 25, 1861; and 1·180 inch appears to have fallen in two hours on July 20, 1859.

A RUSSIAN scientific congress, *Iron* states, is to meet at Warsaw next September, at which the question of adopting the Gregorian calendar in Russia will be discussed.

THE Municipal Council of Paris has resolved to support a resolution of the Société Française de Navigation Aérienne, which has petitioned the French Government to be recognised as an Establishment of Public Utility. This step is necessary according to the French laws, to give to the Society a legal existence and enable it to hold property and receive legacies.

THE *Quarterly Bulletin of the Nuttall Ornithological Club* is the title of a new ornithological periodical published at Cambridge, Massachusetts. It forms twenty-eight pages and contains a plate. The size of the future parts will depend to a great extent upon the number of subscribers, and a plate cannot be promised in future unless the means assure it. Vol. i. No. 1, contains a description and figure of a new species of Helminthophaga, by Mr. Wm. Brewster; the account of a specimen of the Common Buzzard in North America, by Mr. Maynard; note on the nestling of the Golden-winged Warbler in Massachusetts, by Mr. J. Warren; notes on the Rough-winged Swallow in Pennsylvania, by Mr. W. van Fleet; and on the breeding of the Black-throated Blue Warbler in Connecticut, by Mr. C. M. Jones. Mr. Henshaw writes on *Empidomax traillii* and *E. acadicus*, Mr. R. Deane on Albinism and Melanism among North American birds, and Mr. H. B. Bailey ends the volume with notes of birds found breeding on Cobb's Island, Virginia.

TO judge from the Second Annual Report of the Hastings University School Naturalists' Field Club (1875–6), that Society is in a healthy condition. It consists of forty-eight members, and its object is to study and collect specimens to illustrate the Natural History of Hastings, and to compile a list of its flora and fauna, and to form a museum representing its zoology, geology, and botany. The Society is divided into five sections, and seems to be animated with a laudable enthusiasm for its objects, which we hope will be maintained. A large proportion of papers read at the meetings of last session were by members.

THE principal papers in part iii. of vol. xiv. of the *Transactions* of the Manchester Geological Society is on "Fires in Coal Mines," by Mr. J. Thompson, F.G.S.

WE are only able to note the receipt of the Sixth Annual Report of the Wellington College Natural Science Society. The work done by the Society, the Preface states, has been up to the average of former years, though evidently not what it might be with increased energy. We hope, with the Preface, that now that science has become an integral part of school work, a corresponding increase of interest will be manifested by the pupils in the Natural Science Society.

FROM the Annual Report of the Belfast Naturalists' Field Club (1874–5), we learn with pleasure that that society is materially in a prosperous condition. It was this Society, our readers may remember, who got up the admirable "Guide to Belfast and Adjoining Counties," in view of the meeting of the British Association in Belfast. There are a number of good papers in the present Report, of which we may mention the following:—"On the Origin of Eskers," by Mr. Harbison, and "Notes on the Rudely-worked Flints of Antrim and Down," by Mr. William Gray.

MESSRS. SAMPSON LOW & CO. have published in a separate form from the large work on South Australia, edited by Mr. Harcus, and recently noticed by us, Mr. J. Boothby's "Statistical Sketch of South Australia."

THE additions to the Royal Aquarium, Westminster, during the past week, include the following:—Smooth Serranus (*Serranus cabrilla*), Small-mouthed Wrass (*Acantholabrus exoletus*), Jago's Goldsinny (*Ctenolabrus rupestris*), Lesser Weever (*Trachinus vipera*), Gemmeous Dragonet (*Callionymus lyra*), Cornish Suckers (*Lepadogaster cornubiensis*), Chub (*Cyprinus cephalus*), Barbel (*Barbus fluviatilis*), Whitebait (*Clupea alba*, Yar.), Octopus (*Octopus vulgaris*), thirty specimens.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from East Africa, presented by Dr. Stirling; two Tigers (*Felis tigris*) from Amoy, China, presented by Dr. Marchant Jones; five Red-headed Weaver Birds (*Foudia madagascariensis*) from the Isle of France; a Pine Martin (*Martes abietum*), European; a Sclater's Muntjac (*Cervulus sclateri*) and two Darwin's Pucras Pheasants (*Pucrasia darwini*) from China, deposited; an Eland (*Oreas canna*), born in the gardens; a Central American Agouti (*Dasyprocta punctata*) from South America.

SCIENTIFIC SERIALS

THE current number of the *Ibis* commences with two papers on the ornithology of the Fiji Islands, by Mr. E. L. Layard, in which the following species are described:—*Platycercus taviunensis*, *Myiolestes macrorhynchus*, *M. compressirostris*, *Pachycephala torquata*. Additional notes on other birds are given, including *Lamprolia victoriæ*.—Mr. H. Durnford has ornithological notes from the neighbourhood of Buenos Ayres, in which the habits of the birds of the district are briefly described.—Mr. R. Ridgway writes on the genus *Helminthophaga*, precisely defining the distribution of the ten species and their specific characters.—Mr. H. E. Dresser continues his notes on Severtzoff's "Fauna of Turkestan," the species of birds most lengthily noticed being *Leptopoecile sophiæ*, *Anthus pratensis*, and *Lanius isabellinus*, together with *Caprimulgus pallens* and *C. arenicolor*.—Mr. F. Barratt gives ornithological notes made during trips between Bloemfontein and the Lydenburg gold-fields, figuring *Bradypterus barrati*.—Messrs. H. Seebohm and J. A. Harvie Brown continue their notes on the birds of the Lower Petchora, figuring the eggs of *Squatarola helvetica*.—Mr. J. H. Gurney continues his notes on Mr. Sharpe's "Catalogue of the Accipitres in the British Museum," devoting himself on this occasion to the American Buzzards.—Mr. P. L. Sclater gives an interesting account of the recent ornithological researches of Beccari, D'Albertis, and von Rosenberg in New Guinea, and Count Salvadori writes on two New Guinea species, *Sericulus xanthogaster* and *Xanthomelus aureus*.—Canon Tristram describes a collection of birds from New Hebrides, among which is a new species of *Porphyrio*, *P. aneiteumensis*.

Poggendorff's Annalen der Physik und Chemie.—Ergänzung, Band vii., Stück 4.—We have here a valuable second memoir by M. Chwolson on the mechanism of magnetic induction, which process he seeks to explain by the supposed existence of molecular magnets that are turned by the external force in one direction. In his former paper he dealt with the case of temporary induction in soft iron; he here treats of magnetic induction in steel. The paper is in five chapters: in the first are summarised the results obtained by previous observers, those of Jamin being given with special fulness. In the second the author describes his experiments, which require a modification of Jamin's theory. Of Jamin's two laws relating to the action of positive and negative currents on permanently magnetised bars, M. Chwolson finds the first absolutely correct; the second incorrect. Jamin's mistake he considers to be in the supposition that the negative current only acts on the surface layers, leaving those below untouched; it is shown, on the contrary, that the least negative current acts on all the layers and diminishes their intensity. Then he gives a mathematical theory of induction in steel; supposed the first attempt of the kind (if Maxwell's but partly successful one be excepted). In the fourth chapter he explains, on the basis of theory, the various experimental results got by different observers; and in the fifth, shows how certain results that might *à priori* be foreseen, from the theory, have been verified.—M. Holtz has a paper on some changes of form of the Leyden battery (with a view to extending the length of spark), and its use with influence-machines; and he describes some good phenomena of discharge. The remaining papers are extracts.

Der Naturforscher, February.—In this number we may note an account of observations by M. Mallard on the velocity of inflammation in a mixture of fire-damp and air. The various mixtures were set in motion with different velocities, and that velocity at which the zone of combustion remained stationary measured the velocity sought. The highest velocity of inflammation was 0·560 metres in a second, and it occurred in a mixture of 0·108 vol. of fire-damp in one volume of the mixture. On increasing or diminishing the proportion of fire-damp, the velocity in question diminished very rapidly, becoming *nil* with a proportion of 0·077 vol. on the one hand, and 0·145 vol. on the other, below which the mixtures are neither explosive nor inflammable. It is notable that a variation of even 0·01 in the proportion of fire-damp is sufficient to convert an absolutely indifferent mixture into a highly dangerous one.—In geology there is an adverse criticism of Mr. Mallet's theory of volcanic action, by M. Roth, and an experimental inquiry by M. Hoppe-Seyler into the formation of dolomite. The latter points out that wherever, on a sea-bottom covered with chalk or limestone, eruptions of lava occur, dolomite is a necessary product, the lava supplying the temperature (which must be high), the limestone the calcium and carbonic acid, and the sea-water the magnesium.—From twenty years' observations in St. Petersburg, M. Rikatcheff draws some conclusions as to the influence of cloudiness on the daily variations of temperature.—We further note an abstract of a recent *brochure* by Prof. Lommel, on the interference of reflected light (the author developes variously a well-known experiment of Newton), and a summary of an interesting lecture by M. Löwe to the Physiological Society of Berlin, on the theory of descent.

March.—The formation of cheese has lately engaged the attention of Prof. Ferd. Cohn in connection with his researches on the lowest forms of plant life; and he has made personal observations on the manufacture, as carried on in Switzerland. The phenomena accompanying the process are thus described: The rennet contains a liquid ferment which causes coagulation of the milk; also ferment-organisms (Bacillus), which probably bring on butyric-acid fermentation, and cause the slow maturing of the cheese. It is their resting-spores that, enclosed by the dry cheese substance, resist boiling heat for a long time, and, in a suitable nutritive liquid, may afterwards develop to bacillus rods. (One of Dr. Bastian's results is thus explained.)—In a paper by M. Rosenthal, the action of the automatic nerve-centres is explained as dependent, not on some immanent property of the nerve apparatus, but on the nature of the blood. To account for the rhythmus of the movements in breathing, he supposes a constant resistance opposed to the constant excitation, and illustrates the case by supposing a vertical tube closed below by a plate which is pressed against it by a spring, while a constant stream of water flows in from above. When the liquid reaches a certain height the spring yields, and some water escapes; then the spring forces back the plate, and the process is repeated, thus giving a rhythm. From experiments made by M. Bartoli, in Italy, it is inferred that all solid and liquid substances, whatever their nature, have, in air, a damping influence on the oscillations of a magnetic needle suspended over them, and that this action depends on the air that is between the two surfaces. Among other subjects handled in this number may be mentioned those of irregularities of the sea-level (Hann), the molecules of isomeric and allotropic bodies (Smit), the physical properties of litter in woods (Ebermayer), and decomposition of albuminous matter in animal bodies (Drechsel).

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 4.—Supplementary note "On the Theory of Ventilation" (see NATURE, vol. xi. p. 296). By Francis S. B. François de Chaumont, M.D., Surgeon-Major, Army Medical Department, and Conjoint Professor of Hygiene, Army Medical School. Communicated by Prof. Stokes, Sec. R.S.

In his previous paper the author endeavoured to establish a basis for calculating the amount of fresh air necessary to keep an air-space sufficiently pure for health, taking the carbonic acid as the measure. The results showed that the mean amount of carbonic acid as respiratory impurity in air undistinguishable by the sense of smell from fresh external air was under 0·2000 per 1000 volumes. His object in the present note is to call attention to the relative effects of temperature and humidity upon the condition of air, as calculated from the same observations.

Linnean Society, June 1.—Prof. Allman, president, in the chair.—An interesting series of photographs illustrating coffee cultivation in Ceylon, an enormous banyan tree and other tropical vegetation, were shown by Mr. J. R. Jackson, of the Kew Museum; Mr. W. Bull's exhibition of several fine healthy, growing plants, and the seeds of his lately introduced *Coffea liberica* and of *C. arabica* for comparison came in most *à propos* to the above.—The Rev. G. Henslow read a paper on floral æstivations, in which, after giving the eight kinds, viz., distichous, tristichous, pentastichous, half-imbricate, imbricate proper, convolute, valvate, and open, he explained their origin, and specially dwelt upon the new term *half-imbricate*, which he applied to a very large number of cases ranging from perfect regularity to extremely irregular and zyomorphic flowers of the pea and snap-dragon. The author then showed how that, as well as the fifth and sixth kinds were successively deducible from the third or pentastichous (quincuncial) by merely shifting one edge of the *second* part *under* the adjacent edge of the *fourth* part. The author added a note on a new theory of the cruci-

ferous flower, based on a quinary type, and which, by *symmetrical reduction* (*i.e.*, the fifth part of each whorl would be suppressed) the remaining fours would, by further arrest, due to adaptations to insect agency, form the normal flower. He also disputed the tenability of *Chorisis* in the pairs of long stamens, regarding their occasional union as indicative of evolutionary advance and not retrogression; as cohesion is a subsequent stage to freedom, except in the rare cases of atavism indicated by solution and dialysis. The author called in question the justness of Pfeffer's view of the corolla of primula, being an outgrowth of the Andrœcium, by showing (*a*) the position of the stamens to be explained by the staminodia of Samolus, (*b*) that the corolla appearing subsequent to the stamens is no anomaly, (*c*) that the fibro-vascular bundles are ten in number, of which five are intermediate, and (*d*) that phyllotactical æstivation were those of true leaves; so that all these facts conspired to render the theory untenable. Mr. J. G. Baker read a paper on a collection of ferns made by Mr. Wm. Pool in the interior of Madagascar. Altogether 114 species have been obtained, of which fifteen are entirely new and twenty-eight prove to be varieties of already known forms. Some examples, *e.g.*, *Asplenium trichomanes*, *Nephrodium felix-mas*, and *Aspidium aculeatum*, are thoroughly temperate types.—Mr. Francis Darwin read an account of some researches of his on glandular bodies on *Acacia sphærocephala* and *Cecropia peltata*, serving as food for ants. The structures in question were discovered by Mr Belt (Nicaragua), and subsequently further observations made by Fritz Müller (Brazil), while Mr. Darwin has more particularly entered into their minute composition. In Acacia they are of two kinds (*a*) nectar-secreting glands situate at the base of the petiole, (*b*) small, somewhat flattened, pear-shaped bodies, which tip six or seven of the lowermost leaflets of the bipinnate leaves. In Cecropia cylindrical bodies are developed in flat cushions at the base of the leaf-stalk. Mr. Darwin shows the microscopical structure of all of these to be homologous in kind, cellular, protoplasm, and containing oil globules. He infers, moreover, they bear a relation to the serration-glands of Reinke, in certain cases afterwards being converted into stores of nutriment, which undoubtedly the ants live on, and in their turn protect the trees from the ravages of the leaf-cutting ants.—A notice of the lichens of Madagascar collected by Mr. W. Pool, by the Rev. J. M. Cerombie, was taken as read.—Prof. Wyville Thomson, of the *Challenger* Expedition, addressed the meeting, giving the results of two communications by him; one on new living Crinoids belong to the Apiocrinedæ, the other on some peculiarities in the mode of propagation of certain Echinoderms of the Southern Seas.

Royal Astronomical Society, June 9.—William Huggins, D.C.L., president, in the chair. A paper by Prof. Simon Newcomb was read on a hitherto unnoticed apparent inequality in the longitude of the moon. The inequality was, it appeared, brought to light in the course of an investigation which has recently been made by Prof. Newcomb, of the corrections to be applied to Hansen's "Tables de la lune," in order that they may be used for the determination of the longitudes of the transit of Venus stations. Prof. Newcomb set himself to compare the places derived from Hansen's Tables with the series of lunar observations made at Greenwich and Washington between the years 1862 and 1874. The residual errors of the moon's place showed a systematic inequality which could not be got rid of by any new assumption as to the value of the corrections of the lunar elements. There can be no serious doubt about the existence of the inequality, because both the Greenwich and Washington observations agree in showing it, and a close investigation shows that the errors are periodic and depend upon the moon's longitude. In order to make the investigation more complete, Prof. Newcomb has determined the corrections for the years 1847 to 1858, for which period the residual errors of Hansen's Tables are given in the Greenwich observations of 1859. A table of the resulting corrections is given in the paper, and it appears that the period of the chief term of the new inequality is 16⅔ years with a probable error of half a year. The corresponding period of the inequality in longitude is 27·4304 days ∓ 0·0040 days, and there is a large preponderance of probability against the real period being less than 27·42 days, or more than 27·44 days. No known term in the moon's longitude falls within these limits. The moon's sidereal period is 27·32 days and the anomalistic period is 27·55 days, so that the new term falls half way between the two. The non-accordance of this period with any term heretofore sought for, is the probable reason why this term has not before been noticed; a term if unknown would not be remarked unless its value was such as visibly to effect the individual comparison of theory with observation, and Hansen's tables as corrected are the first of which the residual errors are so small that a term of 1″·5 would be remarked in the comparison with observations. Prof. Adams said that he was at a loss to imagine what the cause of this inequality can be, he was rather inclined to suppose that it may have something to do with the effect of the figure of the earth on the motion of the moon, but this was only an idea thrown out on the spur of the moment.—Lord Lindsay exhibited an adaptation of the ordinary altazimuth instrument designed to give a rough equatorial motion; to the base of the altazimuth pillar is fixed an iron bar, through a hole in which a string or wire is attached to the object-glass end of the telescope. The only adjustments that are necessary are that the horizontal bar shall be placed approximately north and south, and that the distance from the base of the altazimuth pillar to the hole in the bar through which the string passes shall be equal to the height of the pillar into the cotangent of the latitude of the place of observation.—Mr. Plumber read a paper on photometric experiments upon the light of Venus. By comparing the shadow of a wire cast by the light of the planet with the shadow of a similar wire cast by a candle at a known distance, and again by comparing the light of the candle with the light of the full moon, he came to the conclusion that the light of Venus at its greatest brilliancy was equal to $\frac{1}{799{\cdot}5}$ of the brightness of the full moon, and by a similar method found that the light of Jupiter at mean opposition was equal to $\frac{1}{6430}$ of the light of the mean full moon.

Chemical Society, June 15, Dr. J. H. Gladstone, F.R.S., vice-president, in the chair.—A large number of communications were read, this being the last meeting of the season. The first paper, by Prof. Dewar, entitled "Chemical Studies," was chiefly devoted to an account of several interesting lecture experiments.—Dr. H. E. Armstrong then gave a short account of his elaborate researches on the reduction of nitric acid and on the oxides of nitrogen, part i., on the gases evolved by the action of metals on nitric acid, made in conjunction with Mr. Accworth.—Mr. C. T. Kingsett then read a paper on the composition and formula of an alkaloid from Jaborandi.—There were also papers on the simultaneous action of iodine and aluminium on ether and compound ethers, by Dr. J. H. Gladstone and Mr. A. Tribe; on compounds of antimony pentachloride with alcohols and with ethers, by Mr. W. C. Williams; on the volatility of barium, strontium, and calcium, by Prof. J. W. Mallet; on the action of chlorine on acetamide, by Dr. E. W. Prevost; note on the perbromates, by Mr. M. M. P. Muir, and a communication on a new and convenient form of areometer for clinical use, by Dr. J. G. Blackley.

Geological Society, June 7.—Prof. P. M. Duncan, F.R.S., president, in the chair.—John Thos. Atkinson, Edmund Clark, Frederick Derry, Walter S. Gervis, Thos. Jones, Baldwin Latham, and Edward Sewell, were elected Fellows of the Society.—On the British fossil cretaceous birds, by Prof. H. G. Seeley, F.L.S. In this paper the author gave an account of the remains of birds which have been collected from the Cambridge Upper Greensand. The bones are so fragmentary that the size of the animal can only be given roughly as similar to that of the Diver, but with a shorter neck. The affinities of the animal are strongest with *Colymbus*. It also closely resembles Prof. Marsh's cretaceous genus *Hesperornis*, and like that genus may be supposed to have had teeth. The species were described as *Enaliornis Barretti* and *E. Sedgwicki*. Some bones were also described thought to indicate birds in which the extremities of the bones remained unossified throughout life.—On two chimæroid jaws from the Lower Greensand of New Zealand, by E. T. Newton, F.G.S., of H.M. Geological Survey. The two jaws which were the subject of this communication form part of the collection of fossils from the Lower Greensand of New Zealand deposited in the British Museum by Dr. Hector. One of the specimens, a right mandible, was referred by the author to *Ischyodus brevirostris*, Ag., a species from the Gault of Folkestone, hitherto known only by name, no description or figure of it having been as yet published. The second specimen, a small right maxilla, possessing but one tooth, and this of a peculiar form, was compared with the corresponding form in *Ischyodus*, *Edaphodon*, *Elasmodus*, *Ganodus*, *Chimæra*, and *Callorhynchus*. Reasons were given for

believing that it differed generically from all other known forms of Chimæroid jaws; and the author therefore proposed to call it, in allusion to the form of the tooth, *Upsilodus Hectori*.—On a bone-bed in the Lower Coal-measures, with an enumeration of the fish-remains of which it is principally composed, by J. W. Davis, F.L.S. In this paper the author described a thin bed composed chiefly of remains of fishes, which rests immediately upon the "Better-bed coal" of the Lower Coal-measures in Yorkshire.—Note on a species of Foraminifera from the Carboniferous formation of Sumatra, by M. Jules Huguenin. Communicated by Prof. Ramsay, F.R.S., V.P.G.S. The author described some globular Foraminifera, belonging or allied to *Fusulina*, from a carboniferous deposit containing *Productí* and *Phillipsiæ*, which occurs north-east of Padang and south of the lake of Singkarak in Sumatra. The author described the structure of these fossils, which he compared with *Fusulina cylindrica* and *F. depressa*, and arrived at the conclusion that they belong to a new genus, to which perhaps the North American *Fusulina robusta* also belongs.—On the Triassic rocks of Somerset and Devon, by W. A. E. Usher, F.G.S. The author stated that the Trias of Devon and Somerset was divisible into three groups, occupying distinct areas. The first lies north of the Mendip Hills, where the Trias is thinnest and assumes its simplest characters, consisting of marls and Dolomitic conglomerate. The second area embraces the country south of the Polden Hills as far as a north and south line through Taunton. The chief portion of the Trias in this area, as in the northern, consists of marls. The third area, bounded on the north by the Bristol Channel, on the south by the English Channel, on the east by the Blackdown range, and on the west by the Culm and Devonian highlands, presents the most complex relations of the Trias in the south-western counties.

Victoria (Philosophical) Institute, June 19.—A paper by Prof. Morris, M.D., of Michigan University, on the theory of unconscious intelligence as opposed to theism, was read. The paper discussed the theories which have been put forward on the subject. The professor laid down the proposition that consciousness and intelligence imply one another, and that, therefore, "unconscious intelligence" is a self-contradictory phrase.

PARIS

Academy of Sciences, June 12.—Vice-Admiral Paris in the chair.—The following papers were read:—Experimental critique on Glycemia (continued). Physico-chemical and physiological conditions to be observed in searching for sugar in the blood, by M. Cl. Bernard. The sugar found normally in blood of animals ranks among glycoses. M. Bernard shows how its properties may be demonstrated after coagulation of the blood, by superheated steam, by alcohol, or by sulphate of soda. He then details his mode of finding the amount of sugar.—On the absorption of free and pure nitrogen and hydrogen by organic matters, by M. Berthelot. White filter paper, slightly moist, placed in pure nitrogen, under influence of the effluve or silent discharge, absorbs a considerable quantity in eight or ten hours. Oxygen does not hinder this (in 100 vols. air, 2·9 hundredths of nitrogen and 7·0 of oxygen were absorbed in about eight hours). Hydrogen is absorbed even more rapidly than nitrogen by benzene, terebenthene, acetylene, &c.—On the formation and the decomposition of binary compounds by the electric effluve, by M. Berthelot. In principle the reactions are the same as those with the spark, but the longer duration of the spark and the heating it produces are adverse to the formation of condensed products, such as arise under the effluve.—Presentation of solar photographs of large dimensions, by M. Janssen. In these the disc is 22 centimetres in diameter, yet there is great distinctness. M. Cornu hopes shortly to have photographs from the focus of a telescope of 36 centimetres aperture.—On electric transmissions through the ground, by M. du Moncel. From experiments he shows how unequal moisture about the electrodes, unequal heating of these, and unequal size, are physical causes which intervene, more or less, causing variations in intensity of currents transmitted through the ground. A general conclusion is, that it is not advantageous to interpose earth in a circuit unless when its resistance exceeds 10 or 15 kilometres of telegraph wire.—On some new experiments made with Crookes's radiometer, by M. Ledieu. In the first experiment rotation was obtained from a beam of luminous rays falling parallel to the axis (though less rapid than when it falls at right angles). In the second, the two sides of the vanes were kept bright; and here the vanes moved as if repelled by the luminous ray meeting them. (The ray should be made to strike the vane next the light at a small angle, and the two opposite vanes, with reference to the plane of the ray and the axis, be shaded by a screen. The place should be quite dark.)—On amber, by M. Reboux.—On the law of Dulong and Petit, by M. Terreil. The product of specific heat by chemical equivalent is a constant, provided all the bodies are taken with the same gaseous volume, and before any condensation. The specific heat of simple bodies, taken with the same volume and gaseous state, is inversely proportional to their chemical equivalents; so is that of compound bodies, and it is proportional to the condensation of the gaseous volumes of the constituent simple bodies in combining. Simple or compound bodies which have lost the gaseous state have a specific heat double that which they have in this state.—Letter to M. Dumas on Phylloxera, by M. Fatis. The cycle of metamorphoses may, in certain circumstances, occur entirely under ground without intervention of the perfect winged form.—On the employment of sulphide of carbon against Phylloxera, by M. Allies.—Another on the same subject, by M. Marion.—On the pantanemone, an apparatus acting in all winds, without orientation and without reduction of surfaces, by M. Sanderson.—Ephemerides of the planet (103) Hera, for the opposition of 1877, by M. Leveau.—On the presence of magnesium in the sun's limb, by M. Tacchini. The magnesium gains in intensity and elevation where the flames of the chromosphere present most vivacity. While there is at present a minimum of spots, protuberances, hydrogenic clouds, and metallic eruptions, the circulation of magnesium still retains a certain energy capable of rising to a maximum as in previous years.—Phenomena of electric oscillation, by M. Mouton.—On the propylenic chlorhydrines and the law of addition of hypochlorous acid, by M. Henry.—Elementary analysis of electrolytic aniline black, by M. Goppelsroeder.—On anthraflavone and an accessory product of the manufacture of artificial alizarine, by M. Rosenstiehl.—On the internal membrane of a chicken's gizzard as an osmotic partition, by M. Carlet. Interposed between water and alcohol in the normal conditions of osmose, this membrane is always traversed by a dominating current from the water to the alcohol; it is therefore not (as generally supposed) an exception among animal membranes.

VIENNA

Imperial Academy of Sciences, Feb. 17.—The following (among other) papers were read:—Further observations on the formation of a rational space curve of the fourth order, on a conical section, by M. Weyr.—On the distribution of the colouring matter in ovules during the process of division, by M. Schenk. The ovaries and testicles of *Echinus saxatilis* are commonly yellowish, but some species have reddish violet ovaries; M. Schenk studies the changes wrought by artificial fecundation of the ovules in these latter with sperma from the yellow testicles.

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