THURSDAY, JULY 31, 1873

THE ENDOWMENT OF RESEARCH

IV.

IN accordance with the heading, deliberately adopted for this series of articles, the main object of them has been to insist upon the national importance of a direct endowment of research, and to indicate a way whereby scientific investigators, relieved from any incidental duties, may be placed upon a footing of security and competence. In justice, however, to the letter from Professor Flower, published in our last number, it is necessary to give some explanation why the indirect endowment of scientific men by means of the existing professoriate has been comparatively ignored.

Though it is very far from our intention to quarrel with the main drift of that letter, yet it were vain to attempt to disguise the real point of disagreement between Prof. Flower's proposals and those herein advocated. To augment the salaries of distinguished men of Science, whether government officials, executive members of the wealthy scientific bodies, or University professors, and to increase their numbers, is no doubt an object to which certain classes of the public require that their attention should be drawn, as it is also one of the means by which original scientific work would be encouraged. At this point, apparently, Professor Flower would for the present stop; yet we think that there are many and weighty reasons why those who are not content with such a scheme as final, should hold that a favourable time has now arrived for putting forward a more complete system sufficiently elastic to comprehend within its future development the liberal subsidy of all forms of unremunerative Scientific Research.

As to the funds at the disposal of scientific bodies, it is well known that they are so small as to form a scarcely appreciable element in the consideration of the present question, nor is it likely that they will receive much increase; but yet it would be desirable that the method of their distribution should form an example to guide the application of a more complete system. Again, with reference to the Government appointments, the prospect does not appear more encouraging. Our practical politicians are not unnaturally offended by the anomaly that the holders of these offices should confessedly receive pay, not for the work they do, but in honour of their general scientific attainments. The Mastership of the Mint has not been saved even by the illustrious character of its previous occupants, and along with it have gone several subordinate posts which also were honoured by the scientific men who held them. It only remains for some Chancellor of the Exchequer or Minister of Public Works to arise, wholly given over to the less noble doctrines of Political Economy, and Science will lose the remainder of those places which open competition could fill so much more cheaply, and then the public scientific work of which the popular voice demands the accomplishment, may be resigned to the enterprise of pushing newspaper proprietors. This sort of indirect endowment of research may be said to have had its day; it was of a piece with the public sinecures which used to be awarded indiscriminately for literary or other ill-recognised merit. It was extremely useful when no particular kind of work was required in return, and when the national benefits arising from the advancement were less thought of than they are now.

It is, without doubt, to the professoriate at the Universities that the advocates of indirect endowment must turn in the first place, both for the wealth and the organisation they require, and it is on this ground that issue with them must be joined. It is our purpose, therefore, to point out, first, that the Science professors at the Universities are already in a fair way to get both the position and the emoluments which they deserve, and secondly, that to subordinate original research to the paramount duty of teaching is a clumsy expedient which should not, on principle, be systematically adopted.

In the first place it hardly needs to be said that all the tendency of ancient and modern endowment has been in favour of the Professoriate, so that the interests of teaching are already in possession of the field. In the old days when all instruction was of necessity oral, to found a chair was the one means by which the highest forms of new learning could be promoted; and the force of this tradition, acting in harmony with the practical character of Englishmen, who always expect visible results from money spent, has been a guarantee that modern Science, while growing to its present dimensions, should not fail to receive this sort of attention at the Universities. At Oxford, for example, the present holders of the three leading chairs of Chemistry, Physiology, and Physics receive from various academical sources endowments of 800*l.* each per annum, and if the other Science Professorships are inadequately endowed, the same may be said of many of those subjects which enter directly into the course for the Arts degree. According to a rough estimate of Mr. M. Pattison's, Science on the whole receives nearly 5,500*l.*, whereas Philology, the next highly endowed faculty, gets but 4,000*l.** It must also be borne in mind that the University Commission of twenty years ago gave a stimulus in this direction which has not died away. Both the University and College authorities are not unmindful of the duty of extending the Professoriate, and endowing it worthily: new chairs are even now in process of foundation, and at Oxford at least it is the rule rather than the exception to confer a full fellowship upon a hardworking professor in whatever department of knowledge, whose statutable endowment is comparatively small. From these statements it would be manifestly wrong to draw the inference that either the physical sciences or the other branches of scientific study are as yet fully represented or adequately endowed at Oxford and Cambridge: the purport of them rather is to show that teaching at the Universities in Science as in other matters has gained a position which can well take care of itself. If the plan were adopted which has worked so well at Glasgow, viz. to allow the professors an official house, and to leave to the fees of their pupils the further augmentation of their salaries, Prof. Flower's demand for a simple competency would be completely satisfied.

The real difficulty, however, will yet remain, for on the one hand we have not yet attained any assurance that we

* It should be noticed that the anomalous chairs of Divinity have been throughout excepted from these calculations.

shall get from our endowments anything more than first-rate teaching, and on the other hand we have a large proportion of the University revenues yet to dispose of. It would, of course, be a possible alternative to endow so large a number of professors as to reduce their teaching duties to a vanishing point, and thus avoid the appearance of a radical change and escape the reproach which apparently attaches to the direct endowment of Research. It is not to be supposed that the advocates of indirect endowment intend deliberately to take up with such a subterfuge, yet on any other hypothesis it is as certain as anything can well be, that the original investigation which they put in the second place will come off second best. It were invidious to allude to particular instances, but it is past denial that the original discoveries in Science which once made England famous, and now more or less maintain that fame, neither were nor are achieved by the holders of teaching posts, and it is equally clear that many of the forms into which modern Science is developing are not of such a character as to be capable of being transmitted by oral instruction. The truth seems to be that the intimate connection sought to be established between original investigation and professorial teaching is borrowed from the artificial institutions of another country. It is the chief characteristic of a German University that the full professor, the extraordinary professor, and the *privat docent* make up the class which is there engaged in scientific study not less than in academical teaching, a peculiarity which may be partly attributed to the laborious character of the people, but yet more to the pecuniary poverty of the Institutions. It is in fact from the want of endowments that the emulous spirit of German patriotism has been compelled to exact double work from a single instrumentality. The renowned University of Berlin is indebted for the whole of its resources to the state, and that, a state which is the most frugally administered of any in Europe: and from this cause it has learnt to elaborate an organised system of student teachers and inchoate professors, from whom research is expected as a duty co-ordinate with instruction, while the natural docility and perseverance of the German character have caused these expectations to be abundantly realised. Yet one of the most celebrated of modern German professors is reported to have said, that "the life of a professor would be a very pleasant one, if it were not for the lecturing." No doubt there are many English professors who secretly to themselves would re-echo the sentiment; yet what could sound more absurd if regarded from the ordinary point of view which is popular in this country? Germany indeed has set an example of the novel forms of scientific industry which should flourish at a living University, but the attempt to transfer to Oxford and Cambridge the German system in its integrity would in some respects be a backward step, and would probably prove a failure. The history of our Universities is against it, and their wealth alone serves to vitiate any analogy borrowed from the parsimonious Teuton. They possess, however, a large number of appointments, unconnected for the most part with teaching duties, and originally destined to be held on the condition of study. It would be easy by means of amalgamation and modification of tenure to make these appointments worthy of the acceptance of those who devote their lives to scientific research; nor ought it to be styled "a visionary ideal," to recognise that natural division of labour, which is permitted to us by the magnificent wealth at our disposal, agreeable to English precedent, and in close accordance with the intentions of the founders of Colleges.

C.

CARNÉ'S "TRAVELS IN INDO-CHINA"

Travels in Indo-China and the Chinese Empire. By Louis de Carné, Member of the Commission of Exploration of Mekong. With a Notice of the Author by the Count de Carné. Translated from the French. (London: Chapman and Hall, 1872.)

THE work, a translation of which is before us, is a history of the expedition despatched in 1865, under the auspices of the French Government, for the purpose of exploring the river Mekong, of which expedition Mons. Louis de Carné was a member. In consequence of his death the work has been carried through the press by his father, the Count de Carné. Mons. Louis de Carné, with every allowance being made for a father's very natural expressions of eulogy and admiration, seems to have been a young man of rare ability and promise, and his untimely death at the early age of twenty-seven, the result of the hardships he had to encounter during the expedition, marks a devotion to the cause of Science worthy of the emulation of all those who are desirous of helping forward scientific inquiry and research. The expedition, the history of which is here detailed, originated in a suggestion by the Governor of the French colony of Cochin-China to his Government, that the river Mekong, at the mouth of which Saïgon, the capital of the colony, is situate, might be made the principal route for the commerce passing between Europe and China. There can be no doubt that, could this route be satisfactorily established, the advantage to Europe would be immense, for in addition to a saving of about 1,200 miles in point of distance, the perilous navigation of the China seas, so much dreaded on account of the terrible monsoons by which they are periodically ravaged, might be entirely avoided. Accordingly, in the year 1865 the Marquis de Chasseloup, the French Colonial Minister, sanctioned the scheme of an expedition which should serve the interests of Science, as well as those of the colony, and which, ascending the Mekong from its mouth, where it empties itself into the Indian Ocean, to its sources amid the mountains of Thibet, should report fully on the navigability of that great river, which was then almost unknown beyond the Lake of Augeor, through which the boundary line between the kingdoms of Siam and Cambodgia passes. M. de Carné thus sums up the objects of the expedition:—"It was desired, first, that the old maps should be rectified, and the navigability of the river tried, it being our hope that we might bind together French Cochin-China and the western provinces of China by means of it. Were the rapids, of whose existence we knew, an absolute barrier? Were the islands of Khon an impassable difficulty? Was there any truth in the opinion of geographers who, with Dumoulin, believed that there was a communication between the Meïnam and the Mekong? To gather information respecting the sources of the latter, if it proved impossible to reach them; to solve the different geographical problems which would naturally offer, was the first part of the programme the Commission had to carry out. We were required, besides, to report any miscellaneous facts which might throw light on the history, the philology, the ethnography, or the religion of the peoples along the great river, which was to be as much as possible the guiding-thread of our expedition. We had instructions to seek for a passage from Indo-China to China; an enterprise in which the English have always failed as yet."

M. Drouyn de Lhuys, the Minister of Foreign Affairs, heartily approved of the scheme, and appointed young de Carné to represent his department on the expedition. The exploration party started from Saïgon in June, 1866, but they were doomed to disappointment, so far as regarded their main object, for it was ascertained that the Mekong abounded in rapids, cataracts, and obstructions of various kinds, which precluded all possibility of a route being found to China in that direction, and after encountering severe sufferings and hardships to which some of their number succumbed, including M. de Lagrée, the chief of the expedition, they returned to Saïgon after an absence of about two years and a half.

M. de Carné claims, as the actual results of the enterprise, so far as it was successful, to have "corrected the errors, and set at rest, by lifting the veil from the doubts which had hitherto led geographers to false and uncertain conclusions in describing the eastern zone of the Indo-Chinese peninsula. The capricious windings of the Mekong; the prolongation of its course to the west, at the 18th parallel of latitude; the importance of its affluents; the strength and volume of its waters, and, if I may venture to say so, the proof of its individuality, which, contrary to the received opinion (viz. of the union of the waters of the Mekong and Meïnam), continues to the end of its course; the certainty of its entry into Yunan, where it receives the waters of Lake Tali, and into Thibet, where it has its source—all these points were cleared up. In a word, we brought back precise information respecting the whole course of an immense river, which rises amidst the snows, and completes its course under a burning sun. On the other hand, there are the exact observations and seemingly well-founded information respecting the other rivers of Indo-China; as to their position in different parts of their course, and the limits of their basins; and, in addition, many particulars respecting a part of China itself, which had been hitherto the least known."

We understand that an official report of the expedition is in course of preparation, and we have no doubt the present work will be found to form a very useful supplement to it. The volume would, however, be rendered more valuable and complete by the addition of a few maps, the only one it at present possesses being a somewhat rough sketch of the route followed by the exploring party. Whether France will be able, as M. de Carné suggests, to establish a communication between her colony and China by the river Songkoï, which flows along the north of the Annamite peninsula, is a problem which yet remains to be solved.

G.I.F.C.

"MOTHER EARTH'S BIOGRAPHY"

Chronos: Mother Earth's Biography. A Romance of the New School. By Wallace Wood, M.D. (Trübner and Co.)

THERE can be but few with active minds who have not occasionally found, after having grasped the essential points of any inclusive theory, that in moments of ease and quiet thought, it is far from unpleasant to attempt to apply it, by a running analogy, to some series of phenomena entirely different from those to which it was originally intended to relate, and by taking detail after detail, rebuild it on a fresh foundation. Few, however, have the confidence to put their results on paper, and fewer still to submit them to the criticism of a ruthless public.

The theory of evolution has an intrinsic fascination of this kind, especially to those with a cynical turn of mind; for though developed on a purely physical basis, nevertheless its entire applicability to the intricacies of society, puts the facts of every-day life in a manner so bold, and yet so evidently truthful, that, as it were, scales fall from the eyes of its disciples, and the panorama of moral philosophy flashes out in a manner so vivid and unmistakable as never to be effaced. The picture is a monochrome, and negativism is the colour!

As the title of this work indicates, the history of the world from the beginning of time has to be sketched, and the author commences with a vivid exposition of the nebular hypothesis, and the cooling down of the earth to the commencement of geologic time, under the headings of its Birth and Infancy. He then describes the commencement and development of vegetable and animal life. Just as in a tree all life is found in the terminal twigs, so "the species of animals we see on the earth are the twigs of the great animal tree, the body and branches of which have long since perished," and the struggle for existence by which the present forms have been arrived at, leads to the adoption of the fundamental maxim, "Be hungry and you will be great," which is proposed in place of the old adage—"Be virtuous and you will be happy." Further on the same principle is illustrated in a very different manner: "only iron-clad and zinc-covered trunks are seen on the Western American railroads, all others being smashed up by the remorseless pitching of the baggage-men, employed, it would seem, for the purpose; this is the *Survival of the Fittest.*"

After the world had passed through the early ages of only protoplasmic and invertebrate forms, the vertebrate era commences with "the fishy period." From the amphibian type was developed the reptile, as we are told, thus: "The lizard differed from the frog, and the newt &c., chiefly by breathing entirely through lungs instead of gills, and thus dispensing with water, except as a beverage; forced to magnificent temperance by long ages of death; driven to it by the great propelling power to which we are all more or less victims—the force of circumstances. Thus a second nature is given, and a new type is created. The fish became a reptile. There was no more longing for the good old times; a more glorious prospect in life the world has never seen. The untrod earth was a garden of thick fleshy plants; whole oceans of appetising insects and delicious worms awaited only the eating. And the new-comers grew and throve as never has any immigrant race before or since." The tendency in animals, as we ascend in the scale of life, to assist one way or another in the further maintenance of their offspring, either by development of a nutritive yolk or by feeding them after they are hatched, is certain. "The explanation of this is very simple. As the population of the earth ever increases and competition grows sharper, it is those who have this assistance in their younger days that are enabled to succeed in the world, and to arrive at maturity. And these possess the inheriting tendency to do the same, or very likely a little more, for the new generation than their parents had done for them. 'If I could only give John a thousand dollars when he is twenty-one, I shall be satisfied,' says the sire; 'my father was only able to give me a hundred and a freedom suit.'"

The Reptilian Period is followed by "the Age of Brutes," wherein the maxim "might is right" was the ruling power. This is followed by "the Anthropological Age," that of the present time; a time of advance according to evolution, and not of decadence, for all we know tends to show that "the course of history is one of progress, and that consequently man is an elevated and not a fallen being; that he is a perfected creature and not a degraded divinity; that his course is Excelsior, onward and upward, and not downward." And if we consider the age of Man, in contradistinction to that of brute and reptile, to have been that in which man first appeared on earth, what may the present be considered—but the age of Woman. "Historically considered, her case is very strong. If the position of woman continues to become exalted in the future at anything like the rate it has advanced in the past—granted that she began as the slave of a brute—that future will show not an equality, but woman the ruler, the subordinate man; and these are advantages in her favour which none but the naturalist dreams of."

"A complete equilibrium—when for every desire there shall be a gratification," is the author's deduction as to the future, things being as they are; but "it would seem that life on earth is doomed to die a violent, and not a natural death. Man proposes, but the attraction of gravitation disposes," and so "we must be resigned, remembering that after all we are but a mere speck in the *great* celestial economy, which will lose nothing by our death."

The above short account of this eccentric and amusing work, which excels more by the quaint way in which well-known facts are put, than by anything original in itself, will be best supplemented by a perusal of the original.

OUR BOOK SHELF

The Elements of Chemistry. Theoretical and Practical. By William Allen Miller, M.D. D.C.L. LL.D., late Professor of Chemistry in King's College, London. Revised by Herbert M'Leod, F.C.S., Professor of Experimental Science, Indian Civil Engineering College, Coopers Hill. Part I. Chemical Physics. Fifth Edition, with additions. (London: Longmans, 1872.)

ALTHOUGH Parts II. and III. of this well-known manual have needed frequent alteration and revision as the science advanced, Part I. has, until quite recently, experienced but little change from its well-known form. The recent great advances which have been made in what is now so well known, or at least so often heard of, as solar chemistry, have necessitated considerable additions to the edition of 1867, the last that left the hands of the lamented author.

The name of Mr. M'Leod is a guarantee that the work has fallen into good hands. At page 196, a most complete and well-condensed statement of the present aspect of the subject will be found. The early Indian observations of Captain Herschel and others are referred to, and an account of the discovery of the method of observing the chromosphere without an eclipse is given, and also a sketch of the nature of the phenomena thus observed. A very good statement of the present state of our knowledge with regard to the thickening of the F line, and of Frankland and Lockyer's researches on that subject, is also given, and reference is made to their remarkable observation of the different lengths of the metallic lines above the pole, an observation which has since lead to such important results in connection not only with solar and stellar, but with terrestrial spectroscopy. The additions conclude with a very clear and succinct account of our knowledge of the movements of the gaseous masses on the surface of the sun, and the means of measuring their rapidity and direction. The nature of the spectroscopic phenomena of sun-spots is also described, but somewhat briefly. The added portion is illustrated with twelve woodcuts.

Mr. M'Leod's hand is again visible in the chapter relating to atomicity, where he has added in notes several important points in modern chemical theory, which had not been sufficiently explained in the original work of Dr. Miller; and we also notice in the body of the book a short explanation of the graphic and symbolical formulæ now so much used in explaining chemical facts to the student. We most cordially welcome this new and improved edition of an old friend, and congratulate the present editor on the share he has had in producing it.

R. J. F.

The A B C of Chemistry. By Mrs. R. B. Taylor. Edited by W. Mattieu Williams, F.R.A.S., F.C.S. (London: Simpkin, Marshall, and Co., 1873.)

THIS little book is intended apparently for the use of very young children. The attempt to explain the nature of the elements by analogy with the letters of the alphabet is somewhat obscure, though it would perhaps be difficult to find a different method. The book is divided into lessons, and each lesson followed by questions which are, on the whole, well selected. The same cannot, however, be said of the experiments at the end of the book, which all smack strongly of the "conjuring trick." We cannot coincide with the editor in recommending the book to artisans and business men, who, we think, might attempt something a little more advanced, even as a first book. For those, however, who wish to teach children chemistry, it will no doubt be useful.

Third Annual Report of the Wellington College Natural History Society, December 1870 to *December* 1872. (Wellington College: George Bishop, 1873).

IT is disappointing that the first words of this report, as in the case of the Rugby Society which we noticed recently, should be a confession of partial failure: "Natural History," the Preface begins by telling us, "does not flourish at Wellington College . . . The chief reason undoubtedly is, that during the past two years the older Fellows—and in particular the Sixth Form—have ignored the existence of the Society altogether." Judging from what is said at p. 36, the apathy of the older members of the school is owing to some antagonism which exists between the Natural History Society and the Debating Society attached to the school. But, with Mr. Penny, we cannot see that there is any reason why the two societies should be in the slightest degree antagonistic. On the contrary, they might be mutually helpful, both having ultimately the same end in view—to teach the boys to examine, think, and act for themselves. Of course it ought to be remembered what a great innovation a society like that of Wellington College is on the traditional methods of instruction belonging to a school. The work is entirely voluntary, not clearly defined, as in the regular task-work of the school; and the only rewards held out, rewards which it is difficult to get the traditional schoolboy to understand and appreciate, are, besides the direct acquisition of knowledge and the pleasure attending it, development of the power of observation, keenness of insight, and general intellectual vigour. A debating society, with all its undoubted advantages, is apt to become a nursery of boyish vanity; the reward of successful speaking is immediate and very sweet to a tyro, and can be obtained without much labour. The work of a Natural History Society involves much plodding patience, with very little glory to follow; the rewards are intangible, invisible, especially to the boys themselves, and it will take the training of a few generations to teach boyish human nature to love knowledge for its own sake. One of the most valuable means to accomplish this purpose in a school is a society like that of Wellington College, and therefore we would counsel those who are anxious for its prosperity not to be discouraged, but to work on so long as they can get any boys to work with them, using all possible means to insure success. We hope the merely local obstacles will be overcome, and that the next report will have a more lightsome beginning; also that it will contain many papers by the boys themselves, nearly the whole of the papers in the present report being by Mr. Penny and Mr. Lambert, and not one by a boy, though we are glad to see that some papers by boys were read at the meetings. The Rev. C. W. Penny, president of the Society, deserves the greatest credit for the interest he displays in the Society, and the amount of work he does to help on the objects for which it is established. A large number of the papers, full of instruction and interest even to boys, are by him; his predecessor in the presidentship, Mr. Lambert, has also contributed much to make the meetings of the Society attractive and instructive. Appended to the report are pretty full botanical, zoological, and entomological lists.

Familiar History of British Fishes. By Frank Buckland, Inspector of Salmon Fisheries of England and Wales, Corresponding Member of the "Deutscher Fischerei Verein," &c. &c. (London Society for Promoting Christian Knowledge.)

THIS is a new edition of the above work, Mr. Buckland having found it necessary, he says, almost to re-write the book. It may be described as a free-and-easy gossip about fishes, the book being largely made up of extracts from all quarters, *Land and Water* especially being very fruitful in material. As might be expected, Chapter xv., treating of *Salmonidæ*, and occupying upwards of 100 pages, a fourth part of the volume, is the most original and valuable. The chapter will be found useful to all who take an interest in the rearing and preservation of salmon. The numerous illustrations are very fairly executed, and the general reader will find the book entertaining and informing.

LETTERS TO THE EDITOR

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

Endowment of Research

Direct and Indirect Endowment

I SHOULD like to make one or two remarks on Prof. Flower's letter in your last number.

He modestly suggests that his views respecting the endowment of research unencumbered with teaching, or as he felicitously calls it, the *direct endowment of research*, may be considered by members of the Association for the Organisation of Academical Study as "heretical." I venture to think that he is orthodox on the main theoretical position that, *in the long run*, research must be endowed directly as well as indirectly (by the subsidy of teaching professors) and with an *equally* liberal hand. He is at issue with us only, if I take him rightly, as to the *time* when it will be desirable or possible to make a claim for such direct endowment. We contend that *now is the only time* for making such a claim, and for a reason which I will give presently. Mr. Flower, on the contrary, says that while *indirect* endowment of research, by raising the salaries of teachers, may be carried out at once with less opposition from old prejudices, "the far more difficult question will follow more appropriately and [the endowment] be carried out more efficiently when the body of educated scientific men in the country is larger than it is now, and the public generally, especially those in high places, have more appreciation of the claims of Science for its own sake," *i.e.* in the more or less indefinite future.

In answer to this I would say:—

(1) The "public in high places," by which I suppose is meant Mr. Lowe, who make a conscience of Political Economy, appear to appreciate the fact that the support of an useful and necessary but essentially unremunerative employment like research, out of public money is economically a sound investment; whilst the subsidy of a remunerative employment like teaching, out of public funds, though perhaps unavoidable, is nevertheless, economically speaking, an unsound one. We have no fear of Mr. Lowe's opposition.

(2) If by "the opposition of old prejudices" is intended the attitude of the Conservative party towards the claims of knowledge, I would call Mr. Flower's attention to the fact that some of the warmest supporters of "direct" endowment are political Conservatives. It is, indeed, one of the soundest elements in the Conservative consciousness, the distrust of immature generalisations resting upon insufficient inquiry; and the suspicion that, if we insist too much upon exposition, and throw the weight of our endowments into that, and if we make it every man's duty to be continually expounding, instead of insisting upon research and throwing the weight of our endowments into study, the heads of the rising generation run the risk of being inflated with immature and windy generalisations. Depend upon it, the Conservatives are prepared for keeping the endowments of our colleges for the support of that lifelong and uninterrupted study for which the founders originally intended them.

(3) Thirdly, Mr. Flower desires to wait till the demand for these supports of knowledge is much increased, and the body of scientific men wanting them is larger than it is now. But has he ever asked himself whether it is likely, that when this millennium of expectancy arrives, there will still be any university or college endowments undistributed, out of which this increased demand is to be satisfied? If Reformers of our old Institutions content themselves with sketching merely a teaching organisation on the German model, and with asking to have that amply endowed, and take no thought for the morrow when this larger body of trained investigators shall have come miraculously into existence—and I think this would be a real miracle, the emergence of a set of phenomena for which the conditions do not previously exist—if, I say, they are afraid of asking *now* to have a large fund gradually put in reserve, to be gradually drawn upon as the occasion arises, for the support of study and of those engaged in it—does Mr. Flower imagine that the remainder of the College endowments which are not taken up by the teaching establishment upon the German model, will be allowed to lie dead? That no claim will be put in for them by the county towns for the erection of more teaching establish-

ments, or for the support of the lectures to ladies, or as Mr. Walter Morrison desires, for the improvement of the incomes of village schoolmasters?

Assuredly all these claims, and more, will be put in for the residue of the funds—and I think it will be more than half—which will remain unemployed when we have pulled down our old Universities and set up our German teaching establishments in their stead. And shall we be able to offer any resistance to such de ands, unless we can come forward *now* with the courage of our opinions, and present the whole of our scheme for a scientific as well as a teaching organisation, the former on a no less complete scale than the latter, instead of keeping half of our scheme, and the more important half of it, in our pockets? Mr. Flower will remember the old lines:—

> "When land is gone and money spent
> Then learning is most excellent."

In conclusion, I would refer for a moment to Mr. Flower's fifth paragraph, in which he seems to say that the interruption of research and study by teaching work or by official duties, is rather an assistance to them. As this statement is very often made, but always without the addition of any reasons for the opinion, I would respectfully ask Mr. Flower to let us know why an interrupted employment is more likely to prosper than a continuous one? what is the precise advantage of distracting intellectual force from the work it has to accomplish? and why the members of the Government, or, say, the jury in the Tichborne case, should not also be compelled to deliver at least one course of lectures during the London season?

July 25 C. E. APPLETON

Method of Endowment

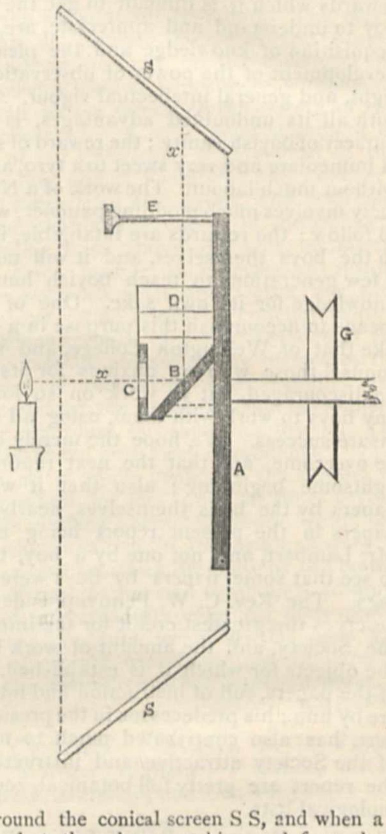
I HAVE read with much interest the three articles which have appeared in NATURE under the above title. The author of these articles has not as yet indicated the manner in which the object which he proposes is, in relation to the Universities, to be attained. He may intend to do this hereafter; but as the absence of any really practical scheme has been mentioned in the public journals as an objection in the way of such endowment as that proposed, I may perhaps be permitted to offer one or two suggestions on the matter. First, it appears certainly desirable that the Fellowships at the Universities should not be abolished, but that the conditions of their tenure should be changed. Scholarships of considerable value, and tenable for a limited number of years, might still be awarded after strict examination; but the Fellowships should be reserved exclusively for the recognition of a capacity for original research, proved by the publication of memoirs, or otherwise. Under such a system there would be little need for an Order of Intellectual Merit. The title of "University Fellow" might well suffice. I have used the expression "University Fellow," for though it would still be desirable that a certain proportion of the Fellows should be required to reside at the several colleges, yet it would probably be considered preferable that the power of election should be transferred from the colleges to a University Council. Such a Council would have to discharge a function similar to that annually performed by the Council of the Royal Society. To prevent favouritism and nepotism, it would be requisite that the names of all candidates should be published, together with the grounds on which each bases his candidature. Similarly the names of the selected candidates should be published, together with the reasons by which the Council have been influenced in their selection. But, it will probably be said, supposing that the Council have in their selection exercised a wise and unbiassed judgment, what is there to prevent the Fellowships from degenerating into mere sinecures? How is the continuance of original research to be secured? Probably there would be, in this respect, little danger in the case of those who have already proved their capacity for original work. But if it be contended that the danger is real, it would not be difficult to provide against it by granting Fellowships, not for life, but for ten or fifteen years, and by renewing them, on the expiry of the original term, only to those who have given strict proof of the continuance of their researches, making exception, of course, in the case of persons disqualified from work either by age or disease.

Such a scheme as that I have suggested would, I venture to think, be both practical and useful, though many matters of detail would still remain to be considered.

July 24 M. A.

Mechanical Combination of Colours

As you have kindly requested me to give a short account in NATURE of the instrument I designed to illustrate the "combination of colours," I have much pleasure in complying with your request. The instrument was designed to show the colour that resulted from the mixture of all or any of the colours of the spectrum given by any light. The construction is as follows:—



To the centre of a disc, A, which can be caused to revolve by the wheel G, a plain mirror, B, is fixed at an angle of 45° to the surface of the disc. In front of the mirror is placed a prism, D. At the edge of the disc there are placed different slides, E, for cutting off any particular rays; also, above the mirror, is a small slit cut in a piece of brass, C, to admit the ray under examination.

x x is a ray of light, which passing through the slit C, is deflected at right angles by the mirror B through the prism D, and is then received in the form of a spectrum upon the screen S S. As soon as the wheel G is set in motion the spectrum also moves round the conical screen S S, and when a certain velocity is arrived at, the colours combine and form the original coloured light which is entering at the slit C. In the same way, by using the slides, any two or more colours may be combined to form the resultant colour.

FREDERICK J. SMITH

On seeing the Red Flames on the Sun's Limb with a Common Telescope

ON observing the partial eclipse of the sun on Dec. 22, 1870, it occurred to me whether it might not be possible to see the red flames on the sun's limb without waiting for a total solar eclipse, or whether it was possible to make an artificial eclipse sufficiently perfect to admit of the red flames being seen. Accordingly I cut out several circular discs of thin brass (blackened on both sides), leaving three arms projecting from the periphery of each of such length that when the ends were bent they should slide into the tube of the eye-piece. I placed one such disc in the eye-tube as near to the field lens as possible to avoid its getting hot; but here a difficulty presented itself which I had not foreseen,—the disc was a trifle too large, and it shut out the sun altogether. I put in a smaller one which admitted too much of the sun's light. I afterwards tried several, and it required a considerable amount of filing and scraping to produce one just the right size to cover the sun's disc and no more; especially as the least jarring or vibration of the telescope would cause the edge of the sun to be seen first on one side and then on the other. After several trials at different times I succeeded on January 16, 1872, in seeing on the south-western limb a red flame. It appeared rather wider at the top than the bottom with a smaller one growing out from the bottom or root close to the sun's limb. There was another tongue of flame a little to the right, which appeared to be detached from the larger flame and also from the sun's limb.

On September 20, 1872, I saw a red flame which went up a little distance from the sun's limb and then divided in three. Close to this, on the edge of the sun's disc, was a group of nine small spots, and a large space was covered with faculæ. The flame—which was of a deep red colour—did not appear to be projected against the sky, but upon a very delicate purple background.

No coloured glass was used in either of these observations, but a sheet of letter paper was held between the eye and the telescope which was removed the instant the sun was brought into the centre of the field of view. R. LANGDON

The Huemul

IN the number of NATURE for July 24, p. 253, I see it is stated that "the Chilian Exploring Expedition has discovered a specimen of the Huemul, an animal that has been altogether lost sight of."

The late Earl of Derby received a female specimen of this animal from Port Famine, in the Straits of Magellan, described and figured by me in the Proc. Zool. Soc. 1849, p. 64, t. xii., as *Cervus leucotis*, which is now in the Derby Museum at Liverpool. Mr. Bates has sent to the British Museum a male and female of the Huemul, which were obtained by Don Enrique Simpson in a valley of the Cordilleras, lat. 46 S. These specimens have been described, the horns of the male figured, and the history of the animal given in detail by me, under the name of *Huamela leucotis*, in the Ann. and Mag. Nat. Hist. 1872, x. p. 445; 1873, xi. p. 214, and p. 308.

The animal, like all the American deer, differs from the stags of the Old World in having no tarsal gland.

British Museum, July 24 J. E. GRAY

Colour of the Emerald, &c.

IN the valuable and important paper given on this subject in NATURE (July 24), the writer has not made it quite clear what kind of emerald was experimented on.

Taken in conjunction with the beryl, it may be assumed that reference is intended to the green beryl, a silicate of alumina and glucina, commonly called emerald, from its colour; but the name of emerald is also applied to green varieties of corundum, which is crystalline alumina.

It would be interesting to understand fully the distinction of colour constituents.

July 25 A. H.

Parasites of the House Fly

SOME of your readers may not be aware that the common house fly is at this time frequently found with from one to twenty parasites on its body. To such I recommend the observation of them as an interesting microscopical study. They are usually on the under part of the fly and can be seen with an ordinary lens of high power. A. R.

Regent Street, July 23

Bees and Aphides

IN his interesting communication respecting the relations supposed to exist between *Trigona* and *Membracis*, Dr. H. Müller appears to have overlooked the Abbé Boisier's observation (Kirby and Spence, "Introduction to Entomology," 7th edition, p. 384) that hive-bees will collect the honey-dew excreted by Aphides. I have also observed the same habit in humble-bees.

Kilderry, Co. Donegal W. E. HART

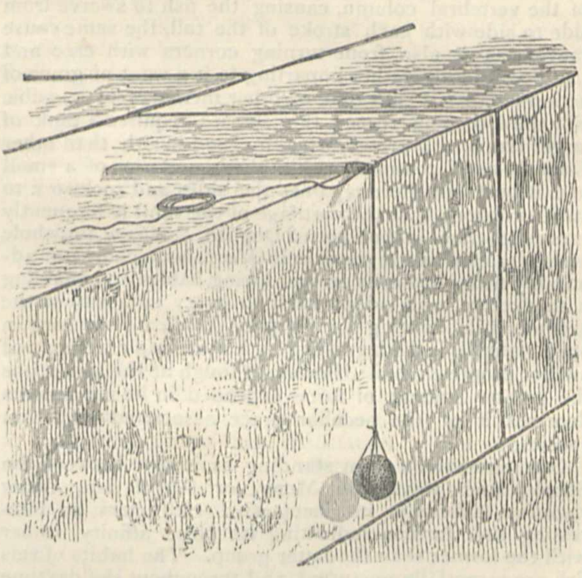
Flycatcher's Nest

SOME flycatchers have built their nest *inside* a temporary shed erected for the masons at present employed upon the rebuilding of Llanfrechfa Church. The nest is now full of young ones, and the old birds fly in and out of the shed with perfect confidence, carrying food to them, and quite regardless of the carving and sawing going on close to them.

July 16 ELIZABETH H. MITCHELL

Relics of the Pyramids

GLANCING over a number of your periodical I find depicted (vol. vii. p. 147) a grey granite ball, recently discovered in the Great Pyramid, and surmised to be an ancient Egyptian weight. It does not seem to have struck the author of the article that this ball could be anything else than a standard weight, but the description he gives leads me to assign to it quite a different use.



I believe it to be a naturally formed granite pebble, selected on account of its nearly spherical form, for a mason's "plumb-bob." The small white spots of lime found on the ball were probably the result of its impact against the narrow cement joints whilst the masonry was in progress and the mortar not yet set.

The bronze hook and cedar rod may have formed part of the same tool, which possibly resembled the accompanying sketch.

Mangalore, June 20 E. H. PRINGLE

FISH DISTINGUISHED BY THEIR ACTION

AS the trained eye of a constant resident in the country enables him to recognise the various species of birds that cross his path by their flight, irrespective of their form and colour, so the observer of fish as they wander at will in the tanks of a large aquarium soon learns to invest them with an additional marked individuality imparted by their mode of action. In some instances these distinctive characters are instructive, as illustrating the varied mechanical principles on which locomotion is effected, while in others they are highly valuable as affording accessory means of discriminating the zoological affinities of the different races and species.

Commencing with the Plagiostomous order, we find in the two primary sub-groups, including respectively the Sharks and Rays, that progression is effected on very distinct principles. With the *Selachoidea*, or shark tribe, the fish move by the even, powerful swaying from side to side of the largely developed and unsymmetrical caudal fin and whole posterior part of the body, the other fins remaining quiescent and being merely subservient as balancers. Descending to the species we find again that each form exhibits a peculiarity of action distinct from its congeners, and one which readily enables us to discriminate between them. Thus in the Smooth Hound, *Mustelus*, the pectoral fins are so largely developed that their balancing powers are highly augmented; comparatively slow motion of the caudal extremity suffices to propel the fish through the water, and the whole body being flexible, it progresses with a measured grace of action surpassed by no other species of its tribe. In the Picked Dogfish, *Acanthias*, the general contour of the body is very similar to that of the last species, but the pectorals being much smaller, more rapid action of the caudal extremity is requisite for supporting it in the water, and to this has to be added a great rigidity of the anterior half of the vertebral column, causing the fish to swerve from side to side with each stroke of the tail, the same cause preventing it also from turning corners with ease and rapidity, and altogether imparting to it a want of grace of action compared with that of other members of its tribe. For the foregoing reason this species requires a tank of larger size for its preservation in good health than other Dogfish, as if confined within the boundaries of a small one, it beats its head against the sides and rockwork to such an extent, that the cartilage of the skull is frequently exposed to view. In the Spotted Dogfish, *Scyllium*, the whole body is more elastic even than in *Mustelus*, a character admirably fitting it for its ground-loving habits, and enabling it to explore, and adapt itself to every sinuosity of the ground while hunting for its prey. When swimming in open water, it is distinguished by a more rapid action and swifter progress than *Mustelus*, though at the same time the greater amount of force expended in its movements deprives it of the peculiar grace associated with that species.

One anomalous form standing as it were between the Sharks and Rays, the Monk, or Angel fish, *Rhina squatina*, affords in its locomotive characters an interesting link further indicating its close affinity rather with the former than the latter group. The habits of this fish are essentially nocturnal, and throughout the daytime it usually reclines sluggishly at the bottom of its tank. Its depressed body and broadly expanded pectoral fins, resemble those of a Ray more than a Shark, and like the former fish it seeks concealment by burying itself beneath the sand or shingle, excavating a hole with the shovel-like action of these broad fins, and thus waits in ambush for passing prey. Immediately the Monk fish rises above the surface of the ground, its true affinities become apparent, progression being effected entirely by the lateral action of the caudal extremity, as in the Sharks, though in a more slow and clumsy manner. The lateral position of the gill openings in this fish forms its chief shark-like anatomical character, and to this has to be added its viviparous habits.

In the Batoidea, or Ray tribe, onward motion is accomplished by a singular, even, and wing-like action of the broad pectoral fins, the attenuated caudal extremity remaining perfectly quiescent, and serving only to preserve the fishes' equilibrium. Swimming towards the surface of the water, these fish present a most remarkable bird-like aspect, their large flapping fins reminding the observer of the flight of the heron or some other unwieldy representative of the Grallian order, while the slender tail dependent in the rear suggests the characteristic mode in which those birds hold their long legs, while pursuing their course through the more subtle medium which they inhabit.

Proceeding to the Teleostean group, we find the means by which the same organs are made subservient to the faculty of locomotion, still more highly diversified ; space, however, will only admit of a few selections.

In the Gurnards, *Trigla*, during rapid movement, all the fins are pressed closely against the body, the broad wing-like pectorals being shut up like a fan, while the fish is propelled swiftly through the water by the vigorous undulations of the tail ; when the fish moves leisurely the pectorals are opened to their full extent, acting as balances. In many species, such as the Striated Gurnard, *T. lineata*, these fins are brilliantly coloured, reminding the observer, especially when regarding them from above, of gorgeous ropical butterflies, gliding along with the smooth action characteristic of the Vanessa tribe. Yet a third property of motion is possessed by these remarkable fish. Settling on the ground at the bottom of the water, they are capable of literally walking over it by means of the three free rays of the pectoral fins, which are situated a little in advance of the others, and are curved and especially thickened, to adapt them for their anomalous office.

The Gemmeous Dragonet, *Callionymus lyra*, a small and beautiful fish somewhat resembling the Gurnards in outward appearance, is distinguished by an essentially different mode of progression. The habits of this species are rather sluggish ; it spends much time reclining on the ground, occasionally moving for short distances just above its surface, by the flitting action of the delicate pectoral fins. On ascending towards the top of the water, its swimming capacities are shown to be very limited, being restricted to the weak vibrations of the pair of fins above mentioned, and which impart to it a peculiar jerky action. The male in this species is recognised by the extraordinary length of the first ray of the anterior dorsal fin, which is raised and depressed at pleasure like the latteen sail of a Mediterranean fishing yawl. This singular appendage appears, from my own observations of the species in confinement, to be subservient to the same end as the wattles, crests, and other abnormal adjuncts of the male in the Gallinaceous birds—for the purpose of fascinating their mates ; to this is added a similar heightening of the colour, which is carried to such an extent in this fish, that the two sexes were long regarded and described as separate species, under the respective titles of *Callionymus lyra* and *dracunculus*.

In the Pipe-fish and Sea-Horses, *Syngnathus* and *Hippocampus*, representatives of the Lophobranchii, the organs of locomotion are reduced to their minimum, being often restricted, in the former genus, to a single median dorsal fin, and being at the most supplemented by a pair of diminutive pectorals and a rudimentary caudal. In all cases this dorsal fin is the chief propelling instrument, and in motion, rapidly undulating from end to end, illustrates the action of the Archimedian screw, driving the fish through the water on the same principle. Dr. J. E. Gray was the first to point out this remarkable peculiarity, in the case of *Syngnathus*, from observing these fish in the Aquarium at the Zoological Gardens. In both *Syngnathus* and *Hippocampus* the animal usually assumes a vertical position while progressing through the water.

The John Dorée, *Zeus faber*, affords us an example of the same principle noticed in the Syngnathidæ, applied to the purposes of locomotion, though to a still more remarkable and extensive degree.

One of these singular looking fish added to the Brighton tanks about two months since, has continued in perfect health up to the present time ; and although of shy and retiring habits, has already yielded many points of interest in connection with its life history. The ordinary position assumed by this fish is the neighbourhood of some projecting rock near the bottom of its tank, and against which it sometimes inclines in a leaning posture, remaining motionless for hours together. Its ordinary progress from place to place is remarkably slow, and it is only when on rare occasions it rises high in the water, that the beautiful mechanism that guides its movements can be appreciated. It may then be seen that the only organs called into action are the narrow and delicate membranes of the posterior dorsal and anal fins, each of which vibrates in a similar manner to the single dorsal of the pipefish ; the long filamentous first dorsal, pectorals, ventrals, and caudal fins meanwhile remaining perfectly motionless. Thus this wary fish, with an almost imperceptible action, silently and stealthily advances upon its intended prey, engulphing it in its cavernous mouth almost before the hapless victim is aware of its enemy's approach.

W. SAVILLE KENT

THE ORIGIN OF NERVE FORCE

TO any one taking a general view of the present position of physiology, there are few things more striking than the deficiency of our knowledge respecting the source of the current which traverses the nervous system, and is brought into play through the instrumentality of its various parts. That the current itself is electricity in some form or another, is almost universally acknowledged, but in what part of the body it originates, or from what store of energy it is derived, is more than most have attempted to answer. The question is made more difficult than it would otherwise be, from the fact that in all those animals which exhibit external electrical phenomena to any extent, such as the Torpedo and Gymnotus, there are large and elaborate special organs for the development of the shocks they produce, but no similar mechanicism, and nothing approaching to it, can be detected in man or other animals, whereby an electrical current or charge might originate. The brain and the various ganglia are often compared to the batteries of a system of electric telegraph, but how they would act if they were such, it is almost impossible to explain.

Direct evidence, therefore, failing to give a satisfactory solution of the problem as to whence nerve force originates, it is necessary to appeal to the indirect in endeavouring to obtain an answer. The hypothesis of "the survival of the fittest" evidently presupposes that after the struggle for existence has lasted a certain time, the individuals which remain, economise to the utmost all the forces at their disposal, because the more perfect use that a living being can make of the limited forces at its command, the easier will it be for it to continue to live. The Rev. Samuel Haughton from the resulting very strongly marked economy of the animal mechanicism, has deduced the principle termed by him that of "least action in nature." The generalness of this principle makes it necessary, if there is evidence of the existence of any store of energy in the living body apparently unemployed, to endeavour to find whether its effects have not been overlooked, or included with those of some other force; and if, at the same time, a force is at work whose origin is unknown, to try and prove whether the two are in any way related to one another. As shown above, there is a force which is in continuous action, with an unexplained origin; the question then resolves itself into whether there is a source of energy in the living body, whose effects have not been explained, and if so, can it on any known or probable grounds, be considered competent to give rise to the nerve current? An endeavour will now be made to show that both parts of the question may be answered in the affirmative; in other words, that there is an available source of energy, as yet unrecognised, of which the function is therefore not yet explained, and which is quite capable of giving rise to the nerve current.

This physiologically new source of energy is *the differences of temperature between the interior and surface of the living body*. Those who are unacquainted with the principles of the modern doctrines of thermo-dynamics, will readily perceive that a difference of temperature in two bodies is a source of power, when they consider that a low-pressure steam engine depends, for its power of doing work, on the difference of temperature between its boiler and condenser; and that a current may be maintained through a copper wire, if it is connected with a thermo-electric battery of which the two ends are kept at different temperatures. In what are termed hot-blooded animals, that is, in mammals and birds, the difference of temperature between the surface and the interior is considerable under all natural circumstances, and in them there is a regulating action of the skin, by which they maintain a uniform internal temperature, always hotter than the surface, whatever that of the external medium may be. In the sluggish so-called cold-blooded animals, the temperature of the interior of the body is but slightly different from that of the air or water in which they live; that it must be higher is evident from the fact that destruction of tissue is continually going on in their bodies, which is always necessarily attended with the evolution of heat.

Such being the case, it is evident that in the difference of temperature between the surface and the interior of the living body there is an available source of energy, which is almost certainly employed advantageously throughout the whole animal kingdom; and what is more, it may reasonably be supposed to be that which gives rise to the electrical nerve current, as only one assumption is involved, and that not an improbable one, it being that a thermo-electric current is capable of being generated between soft tissues of different composition or structure. Physicists will be able to decide this question experimentally, and if they do so, they will do a service to physiology.

For the distribution of a current so generated, the construction of the nervous system is perfectly suited. Two sets of conductors are necessary, the one to carry the currents from the skin to the central organ, which arranges the direction that they must take, and the other to send them on to their destination; these are to be found in the afferent and efferent nerves. As in the telegraph system, no return conductor is necessary; for as the ends of the wires are put into connection with the earth, by which they are able to communicate, so the terminations of the nerves in the skin, muscle-corpuscles and otherwise where they lose their insulated coverings, place the extremities of the afferent and efferent nerves in communication through the intervention of the mass of body tissue. The brain and minor ganglia would then act like greater and lesser offices for the reception and transmission of currents in the required directions, being in fact the commutators of the system.

There are several of the most important phenomena exhibited by the nervous system which are very satisfactorily explained on the above hypothesis. For instance, in cold weather the impulse to action is much more powerfully felt, than in summer when the air is hot, and therefore the temperature of the surface is higher. It is well known that it is impossible to remain for more than a very short time in a hot water-bath, of which the temperature is as high as, or a little higher than, that of the body, on account of the faintness which is sure to come on, and this may be reasonably supposed to be the result of the cessation of the nerve current, which is consequent on the temperature of the surface of the body becoming the same as that of the interior. This faintness is immediately recovered from by the application of a cold douche. When great muscular exertion has to be sustained, as in running or rowing, it is always necessary to have the clothes very thin, and it is felt during the time that it is necessary for the continuance of the effort, that the surface of the body must be kept cool.

As the termination of the nerves in the skin must correspond, on this hypothesis, with the cooled end of a thermo-electric battery, therefore the brain, which is very abundantly supplied with blood, and is the part of the body to which most of the nerves are directed, must be compared with the heated end; and as it is by the conversion of heat into electric current that the nerve force is developed, it is evident that heat must, to a certain extent, disappear as such in the brain, and that that organ must consequently be colder than the blood which enters it. This is exactly what Dr. John Davy observed in the case of the rabbits he experimented on, and his results have not been shown to be incorrect.

A paper on this subject by the present writer appeared in the June number of the *Journal of Anatomy and Physiology*.

A. H. GARROD

NOTES FROM THE "CHALLENGER"

V.

On Wednesday, March 26, we sounded (Station 25) in lat. 19° 41′ N., long. 65° 7′ W., nearly 90 miles north of St. Thomas, in 3,875 fathoms. The bottom brought up in the hydra tube was reddish mud, containing, however, a considerable quantity of carbonate of lime. It is singular that the colour and composition of this mud were not uniform. The upper layer, that which had been forced farthest into the tube, was much redder than that which was nearest the mouth of the tube, and which had consequently come from a greater depth. I am inclined to attribute this to the

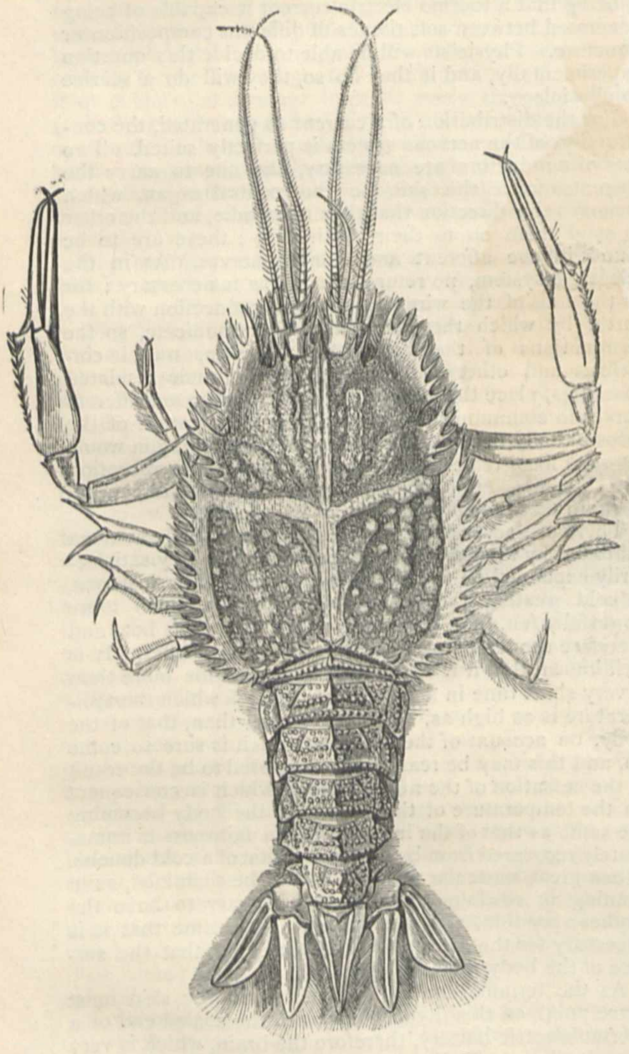


Fig. 1.—Deidamia crucifer, v. W.-S.

steepness of the slope from the plateau of the Virgin Islands. It is easy to conceive that, under the influence of currents varying from time to time in force and direction, the calcareous mud, the product of the disintegration of the coral reefs, may be washed down the incline in varying proportions.

Two thermometers were sent down in this sounding, and a slip water-bottle. The thermometers were unable to bear the extreme pressure, and both were broken. I have already (vol. viii. p. 109) in a former report described the circumstances connected with the loss of these two instruments. The water bottle appeared to have answered its purpose. Mr. Buchanan finds that the bottom water has a specific gravity slightly greater than usual at great depths, but not materially so. The amount of carbonic acid is somewhat in excess.

As this was the deepest sounding which we had taken, we were anxious to try whether the dredge would still prove serviceable. The small dredge was accordingly lowered with the usual bar and tangles, and from the centre of the bar a "hydra" sounding tube weighted with 4 cwt. was suspended about two fathoms behind the dredge. A two-inch rope was veered to 4 400 fathoms; a toggle was stopped on the rope 500 fathoms from the dredge, and when the dredge was well down two weights of one cwt. each were slipped down the rope to the toggle. We commenced heaving in about 1.30, and at 5 P.M. the dredge appeared, with a considerable quantity of reddish-grey ooze, mottled like the contents of the sounding-tube. The whiter portion effervesced freely with acids, the redder only slightly. The mud was carefully examined, but no animals were detected except a few small foraminfera, with calcareous tests, and some considerably larger of the arenaceous type. This dredging, therefore, only confirmed our previous conviction, that very extreme depths, while not inconsistent with the existence of animal life, are not favourable to its development. In the afternoon a series of temperatures were taken at intervals of 100 fathoms from the surface to 1,500. The temperature at the surface was 24°·5 C., and that at 1,500 2°·4 C. The curve constructed from this series indicates a very rapid and uniform fall of about 20 C. during the first 600 fathoms,' and generally a distribution of temperature almost identical with that of some of the later stations on the section from Santa Cruz to Sombrero. In this way we pursued our course northwards under all plain sail.

On the following day we sounded in much shallower water—2,800 fathoms. The bottom was much of the same character, and on the 28th in 2,960 fathoms with a like result, but at our next sounding in 2,850 fathoms on the 29th, the calcareous element in the mud had almost entirely disappeared, and the contents of the tube seemed to be identical with the "red clay" which occupied so large a portion of our first section. The occurrence of this clay is a large and important phenomenon. In the section of the Atlantic, from the Canaries to the West Indies, it occupies about 1,900 miles, a distance twice as great as that occupied by the globigerina mud. What its lateral extension from that line may be, we do not know; but we now find that it extends more or less from over the greater part of the distance between St. Thomas and Bermudas. The nature and source of this deposit, and the causes of its peculiar distribution in the deeper parts of the ocean, are therefore questions of the highest interest.

On the 2nd of April, at a distance of 134 miles from Bermudas, a series of temperature soundings was taken at intervals of 20 fathoms from the surface to 300 fathoms.

The pilot came on board in the afternoon of April 4 and we passed through the narrows, the reefs which make the navigation of this singular little group of islands so dangerous spreading round us in rich purple patches, contrasting with the vivid pale green of the channels of deeper water between them.

The evening was falling as we anchored in Grassy Bay and received our first impressions of Bermudas. On the Monday following we moved from Grassy Bay to the Camber, in the great Dockyard. We remained there till the 21st of April, and employed the interval in taking such a general survey of the natural beauty of the island as our time allowed.

As Bermudas, on account of its isolated position, its structure, and its peculiar conditions of temperature, presents many points of great interest, I will defer giving a detailed account of it until some investigations which we have still in hand are completed.

We met at Bermudas with a singular confirmation and illustration of our view as to the organic origin of the "red clay" of the Atlantic sea-bed.

The Islands of Bermudas consist exclusively of limestone, in some places very compact and hard, almost crystalline; more usually soft and crumbling easily when first quarried, but hardening on exposure to the air. The limestone is very irregular in the direction of its dip. In amount, however, the dip seems never to exceed 30°. The beds are thrown about in a curious way, every quarry or road-cutting showing contortions of all kinds in the strata and every amount of irregularity consistent with uniformly low angle of dip. One would imagine at first sight that the islands exhibited, on a small scale, an epitome of the geological phenomena of a disturbed palæozoic district.

Lieut. (now General) Nelson, R.E., at that time a young man, stationed at Bermudas, communicated to the Geological Society of London on April 23, 1834, a very valuable paper on the geology of Bermudas, which was published in the fifth volume of the Transactions of the Society. Lieut. Nelson pointed out that the great proportion if not the whole of the Rocks of Bermudas are formed simply by the blowing up by the wind of the fine calcareous sand the product of the disintegration of the coral, shells, serpula-tubes, and the other constituents of the Bermudas reefs, that white sand which we found to extend at varying depths through a radius of about 20 miles round the island. The sand is washed in by the sea; it is then caught at certain exposed points by the prevailing winds, blown into sand-hills 40 to 50 ft. in height, which slowly move along, forming shoreward a glacis at the angle of repose of loose sand, on which lamina after lamina is deposited, overwhelming a large tract of country with its fields, gardens, and cottages, in a comparatively short time, and advancing until its progress is stopped by an opposing slope of sufficient height, or by the binding of the sand by vegetation. On these wind-blown beds of lime, aptly called by Lieut. Nelson, Æolian formations, which are originally formed at a considerable inclination, changes in the direction and force of the wind-floods of sub-tropical rain and other transitory and accidental

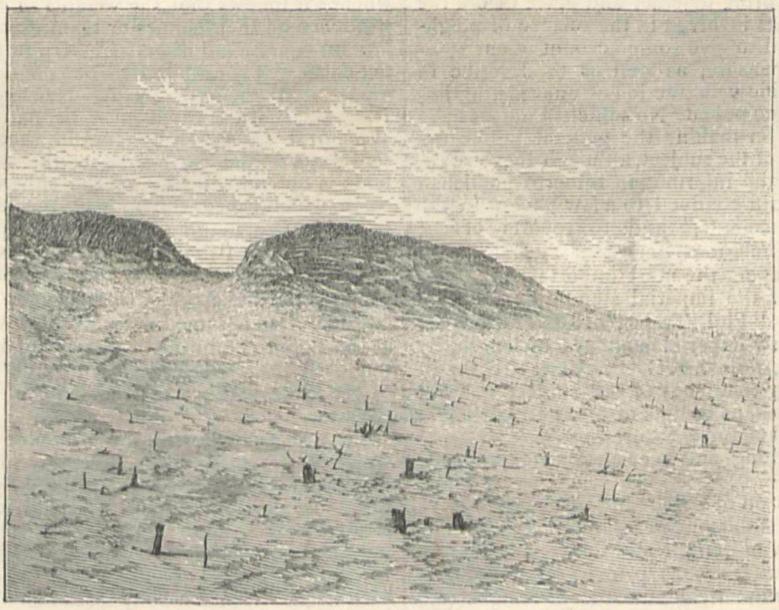


FIG. 2.—Rocks of Coral Sand in Bermudas in process of formation, showing Stratification, and the Stumps of Cedars which have been overwhelmed.

causes produce with great rapidity all the appearances, denudation, unconformability, curving, folding, synclinal and anticlinal axes, &c., which are produced in real rocks, if I may use the expression, by combined aqueous and metamorphic action, extending over incalculable periods of time.

Rain-water contains a considerable quantity of free carbonic acid. Water thus charged dissolves the lime rapidly, and the solution of bicarbonate of lime percolating through the bed, loses a portion of its carbonic acid, and deposits a cement of carbonate of lime between the particles of the coral sand. This process is kept up not only by the surface rain but by the water of the sea, which, as we shall see, percolates through the porous stones of the islands. As evidence of the universality of this process, we have every crack and fissure of the rock filled with semi-crystalline stalagmite, and every here and there the rock is hollowed out into caves which in some places assume the proportions of magnificent caverns with lofty roofs, supported by huge stalagmitic columns, and fretted and enriched by curtains and fringes of stalactite.

One very striking thing about Bermudas is the total absence of running water. There is not a trace of a stream or pool, or even of a ditch. The rain, which often falls in great quantities, sinks through the soil at the spot where it falls as it might sink through a sieve. The islands are perfectly permeable to water horizontally as well as vertically, so that below the level of the sea the stone is saturated, or filled with salt water. The fresh water lakes and wells, of which there are many, are thus merely catches of fresh water lying upon the surface of salt water, and they are nearly all slightly brackish, and those near the sea rise and fall perceptibly with the tide.

WYVILLE THOMSON

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE

I.

DURING the last few years public attention has been frequently drawn to the subject of our national weights and measures. The administrative and social questions of the improvement of our existing system, and of the proposed introduction into this country of the decimal metric system—first established in France, and now being generally adopted on the Continent of Europe, and indeed extending to the other quarters of the world—have formed the subjects of debate in every Session of Parliament, and are still awaiting solution. The scientific questions involved in the use of weights and measures have for a much longer period engaged the attention of many of our most eminent men of Science, several of whom have been members of the various Standards Commissions from time to time appointed by the British Government. These questions are also at the present time the objects of investigation and deliberation by the large body of scientific men from all civilised countries, who compose the International Metric Commission at Paris. It may, therefore, be useful to bring together and place before the public the several points involved in the science of weighing and measuring, and to give some account of our standards of weight and measure, as well as to describe in some detail the scientific construction of our existing imperial standard yard, and pound. No sufficient means have hitherto been adopted for making the general public acquainted with this part of the subject, although they are directly interested in it, the information hitherto published respecting it having been confined to a few papers in the "Philosophical Transactions," and to reports of the several Standards Commissions, and other Parliamentary Returns. Of these papers the most important are those on the construction of the imperial standard pound, by Prof. W. H. Miller, in "Phil. Trans.," 1856, and on the construction of the new imperial standard of length, by the Astronomer Royal, now Sir G. B. Airy, K.C.B., in 1857. In the following treatment of the subject use will be made of these papers, as well as of other authoritative works relating to weights and measures.

The science of weighing and measuring comprehends the following points :—

The scientific definition of weight and measure.

The authoritative establishment of fundamental units of weight and measure of length and the construction of their material representatives as primary standards, in relation to which all numerical amounts of weight and measure are to be expressed.

The establishment of determinate aliquot parts and multiples of the primary units of weight and measure, and of other units derived from them, such as the unit of measure of capacity, &c. ; and the construction and verification of their material representatives, as secondary standards, by comparison with which the accuracy of all weights and measures in ordinary use is to be determined.

The scientific methods of using standard and other weights and measures in which special accuracy is required, as well as auxiliary scientific instruments, such as balances, thermometers, barometers, micrometers, and other comparing apparatus.

The determination of the just results of weighing and measuring with these scientific instruments, after allowing for all indirect influences affecting the accuracy of the direct results of weighing and measuring ; for instance, differences arising from the physical composition of bodies, variations of temperature and consequently of the expansion or contraction of the several substances, changes of condition in the medium in which the comparisons are made, &c., including also a computation of the probable errors of the final results.

The whole subject will therefore be treated under the following general heads :—

I. Definitions of weight and measure.

II. Standards of imperial weight and measure.

III. Scale of multiples and parts of imperial standard units.

IV. The metric system.

V. Weighing and measuring instruments and their use.

I. *Definitions of Weight and Measure.*

Weight or *gravity* has been defined as the quality in physical bodies by which they tend towards the centre of the earth, in a line perpendicular to its surface ; or it may be defined more generally as a property inherent in all bodies, by which they are drawn to some common point, called the centre of gravity, and with a velocity in proportion as they are more or less dense, and as the medium through which they pass is more or less rare.

In following out his discovery of the theory of universal attraction and gravitation, Sir Isaac Newton demonstrated, first, that the weights of all bodies at equal distances from the centre of the earth are directly proportional to the quantity of matter that each body contains ; whence it follows that the weights of bodies have no dependence on their shapes or textures, and that all spaces are not equally full of matter. Up to the time of Newton the earth was considered to be spherical, but it was demonstrated theoretically by Newton, as well as by Huygens, that the earth must be flattened at the poles. Whence it was shown by Newton, secondly, that on different parts of the earth-surface, the weight of the same body is different, owing to the spheroidal figure of the earth, which causes the body on the surface to be nearer to the centre in going from the Equator towards the Poles ; and that the increase of the weight is nearly in proportion to the versed sine of double the latitude, or, which is the same thing, to the square of the sine of the latitude. He assumed the weight at the Equator to that at the Pole to be in the proportion of 229 to 230, and consequently the whole increase of weight from the Equator to the Pole to be the 229th part of the weight at the Equator.

In accordance with the principle the discovery of which is ascribed to Archimedes, that all bodies immersed in a liquid suffer a loss of weight precisely equal to the weight of the liquid displaced, it was also demonstrated that a body immersed in any fluid specifically lighter than itself loses so much of its weight as is equal to the weight of a quantity of the fluid of the same bulk with itself. Hence a body loses more of its weight in a heavier fluid than in a lighter one, and therefore it weighs more in a lighter fluid than in a heavier one, for instance, more in air than in water.

The foregoing principles laid down by Newton are universally admitted as correct, with the exception of the numerical proportions of the weight of bodies at different parts of the earth's surface ; for it is important to observe that Newton founded his calculation of the earth's ellipticity on the hypothesis of its being homogeneous, which is not the case ; and hence he makes the equatorial diameter greater than the polar axis, as 230 is to 229. But from the numerous experiments since made with the pendulum at different parts of the earth, it has been found that the earth is not homogeneous, or composed of concentric strata of equal density, and that the ellipticity is not so great as Newton supposed.

The method of measuring the intensity of gravity on different parts of the terrestrial spheroid, by means of the seconds pendulum, is said to be due to Borda, as originally described in a Memoir inserted in vol. iii. of the *Base du Système Métrique*. From the results of Borda's experiments, made towards the close of the last century, Laplace computed the ellipticity of the earth to be $\frac{1}{330}$, but later experiments and computations of other men of science concur in making it nearly $\frac{1}{300}$.

In the Philosophical Transactions of the Royal Society for 1818, Capt. Kater has stated the results of his pendulum experiments in London, and determined the length of the pendulum vibrating seconds, or completing one vibration in $\frac{1}{86400}$ part of a mean solar day, when measured in a vacuum at the mean level of the sea and at a temperature of 62° Fahr. to be 39·13842 inches of the Standard yard, which was legalised in 1824 as the Parliamentary Standard of length. The latitude of his place of observation in London was 51° 31′ 4″ N. He subsequently made a slight correction in this determination, making the length of the seconds pendulum to be 39·13929 inches, as shown in Phil. Trans. 1819, and this length, or rather 39·1393 inches, was declared to be the true length in the Standards Act of 1824.

It was, however, discovered by Bessel that the correction which had ordinarily been applied, and was applied by Kater, for reducing the vibrations of a pendulum, as observed in ordinary air, to vibrations in a vacuum, ought to be greatly increased. The experiments were consequently repeated by Capt. (now Sir Edward) Sabine, with special reference to the form of pendulum usually employed in England. In Phil. Trans. 1821, Sir Edward Sabine has shown as the results of his experiments on the acceleration of the pendulum in different latitudes, that the mean diminution of the force of gravity from the pole to the equator was 0·0055138, in other words, that a weight of 100 lbs. at the equator would be less by 0·55138 lb. at the pole; whilst the resulting mean ellipticity of the earth deduced from his pendulum observations, was $\frac{1}{313 \cdot 6}$. Sir Edward Sabine has also shown as the result of his experiments on the length of the seconds pendulum in Greenwich Observatory, that its length vibrating 86,400 seconds in the 24 hours, at the temperature of 62° F., and in a vacuum, was found to be 39·13734 inches.

In his paper on the Yard, the Pendulum, and the Metre, Sir J. Herschel has observed that the true measure of the earth's attraction (independent of centrifugal force arising from its rotation), is best to be derived from an ideal seconds-pendulum supposed to vibrate at the extremity of the earth's polar axis; and that the mean length of the polar or of the equatorial pendulum must be derived from the general result of observations of the lines of oscillation of one and the same invariable pendulum at a multitude of geographical stations in all accessible latitudes in both hemispheres; but that no two combinations agree in giving the same precise length, in consequence of the local deviations of the intensity of gravity, due to the nature of the soil or crust of the earth, and the configuration of the ground immediately beneath and around the places of observation. And further, that since the pendulum cannot be observed at sea, the whole sea-covered surface of the globe is of necessity excluded from furnishing its quota of observations to the final or mean conclusion. Water being on the average not more than one-third the weight of an equal bulk of land, such as the earth surface consists of, and only $\frac{2}{11}$ of the mean density of the globe, the force of gravity at the surface of the sea is less than at the sea-level on land by the attractive force of as much material taken at twice the specific gravity of water (or at $\frac{4}{11}$ that of the globe), as would be required to raise the bottom to the surface.

With regard to the determination of the earth's ellipticity, as shown by actual measurements of the dimensions of our globe, and the relative length of the equatorial diameter and the polar axis of the earth, the most recent determination is that by Major Clarke, as stated in his "Comparison of Standards of Length," published in 1866. This memoir has been declared by Sir J. F. M. Herschel, to be the most complete and comprehensive discussion yet received on the subject of the earth's figure, and to be held as the ultimatum of what scientific calculation is as yet enabled to exhibit as to its true dimensions and form.

Major Clarke's results were computed, not from pendulum experiments, but from the combination of all the separate measurements of arcs of meridians in Peru, France, Prussia, Russia, Cape of Good Hope, India, and in the United Kingdom. They are as follows:—

	Feet.	Inches.	Metres.	Metres according to Capt. Kater's equivalent.
Length of Polar axis.	41,706,858	500,482,296	12,712,136	12,712,020
Longer equatorial axis (long. 15° 34′ E.).....	41,853,700	502,244,400	12,756,588	12,756,470
Shorter equatorial axis (long. 105° 34′ E.)...	41,839,958	502,079,496	12,752,701	12,752.588
Length of meridian quadrant of Paris....	32,813,524	393,762,292	10,001,472	10,001,381
Length of minimum quadrant (long. 105° 34′ E.).....................	32,808,772	393,704,064	10,000,024	9,999,953

In computing these equivalents, Major Clarke takes the metre at the temperature of 32° F. from his own measurements to be equal to 1·09362311 yard at 62°, that is to say to 3·28086933 ft., or to 39·37043196 in., instead of the more generally received determination by Capt. Kater of 39·37079 in. The metric length according to both these equivalents is here given.

From the determination of the earth's dimensions, it may be easily computed, that the earth's ellipticity in the longitude of Paris, is $\frac{1}{288}$, whilst its mean ellipticity in all longitudes is $\frac{1}{298}$.

Hence also the mean length of a degree of latitude in the longitude of Paris is $\frac{32,813,524 \cdot 38}{90} = 364,591$ ft., or 69·05 miles. The mean diameter of the earth is 41,800,173 ft., or $7216\frac{2}{3}$ miles, and its mean circumference 23,871 metres.

Thus not only each longitudinal meridian, but also the equator is slightly elliptical.

Sir H. James has shown in his preface to Major Clarke's paper that the longest meridian in 15° 34′ east longitude, nearly corresponds to the meridian in the eastern hemisphere which passes over the greatest quantity of land; and in the western hemisphere to that which passes over the greatest quantity of water, as it passes through the centre of the Pacific Ocean. The shortest meridian in 105° 34′ east longitude nearly corresponds to that which passes over the greatest quantity of land in Asia; and in the western hemisphere, and that which passes over the greatest quantity of land of North and South America.

The connection here shown to exist between the definition of weight and the measurement of the dimensions of our globe, leads naturally to the definition of the second principal head of the subject, viz. of measure.

Measure is generally understood to mean the determinations of a body with relation to a fixed standard unit, or the measure of extension; and it is in this sense that it will now be taken in discussing the "science of measuring." The measure of extension comprehends

The measure of length, or linear extension;
The measure of surface, or square measure;
The measure of volume, or solid or cubic measure;
The measure of capacity, or the cubical quantity contained in any vessel for measuring dry goods, liquids, or aëriform fluids.

All these measures of extension are based upon one fixed standard unit of length; and as all measures of length vary according to their temperature from expansion or contraction, the length of the standard must be fixed at a normal temperature.

Strictly speaking, measure includes weight, which is the measure of the gravitation of bodies towards the centre of gravity. And measures of capacity also are almost universally derived, not from their cubical dimensions, but from the weight of pure water contained in them under determinate conditions as to temperature and atmospheric pressure.

The measure of temperature is based upon the observed rate of linear expansion by heat of a body selected for this purpose, generally mercury, taking as constant units the temperature of melting snow or ice, and of water boiling under determinate atmospheric pressure.

In defining measure, it should be added that it is also applied to the measure, or (as it is termed) *admeasurement* of the tonnage of ships, being a determination of the weight a ship is capable of carrying, with relation to its measure of cubic capacity; to value in relation to a monetary unit; to time and duration in relation to the unit of a mean solar day or a second, its 86,400th part; to velocity, by combining the measure of extension with that of time or duration; to mechanical work, the unit of which is a horse power, as it is commonly termed, or more properly the power of raising 33,000 lbs. one foot in one minute, thus combining the measures of linear extension, weight, and time; to angles, the unit being a degree or the 360th part of a circle described from the point of junction of the two straight diverging lines forming the angle; &c. &c. It is not, however, proposed here to refer further to these measures or to the scientific questions connected with them.

The measure of volume, or bulk of a body, as compared with that of another body differing in volume but equal in weight, is shown by its density, and is also expressed in terms of a fixed standard unit. The densities of bodies are in the direct ratios of their masses, or quantity of matter, and in the inverse ratios of their volume.

The density of a body is defined to be the mass contained in a unit of volume, when referred to a uniform standard. The specific density is to be distinguished from its specific gravity, which shows its weight in relation to its volume, also when referred to a uniform standard. The specific gravity of a body is defined to be the *weight* of a unit of its volume.

The specific gravity of a body is the quotient of its density when divided by the density of that substance which is considered as unity. Pure water is generally adopted as such unity. But since both these densities vary with the temperature—because the same invariable quantity of matter which the body contains is always distributed over its whole volume, and this is variable with the temperature; so that, generally speaking (with some exceptions, pure water, for instance, at certain temperatures), the body, at a higher temperature, has less density than at a lower temperarure—we must fix a certain temperature at which the body, as well as the water, must be considered. It is not necessary that this fixed temperature should be the same for the body and the water, its choice for both being quite arbitrary.

For bodies the most convenient standard temperature for expressing their density seems to be that of one of the fixed points of the thermometer; and the temperature of melting ice or snow (32° F. or 0° C.) is generally adopted. For pure water, there is a maximum of density which occurs at nearly 39° F. or 4° C., and this maximum density of pure water is generally adopted as the unit of density.

The sign $\triangle$ prefixed to the symbol of any weight, with its numerical value following, denotes the ratio of the density of the weight at the temperature of melting snow to the maximum density of pure water.

The relation of the bulk or volume of a body to its weight is expressed both by its density and its specific gravity, these terms being often used indiscriminately. But the former term is more strictly applicable to solid bodies, and the latter to liquids and gases.

To ascertain the density of a body, it is requisite that its volume should be determined, as the density cannot be directly found. The actual volume may be determined—

1. Either by cubic measurement, when the form of the body admits of this measurement being actually made; but this occurs but rarely.

2. Or by ascertaining its specific gravity, from determining the difference of its weight when weighed in air and in water. This is the readiest and most accurate mode of determining both its volume and its density, but the immersion of a body in water is not always practicable, or it may be injurious to the body under experiment.

H. W. CHISHOLM

(*To be continued.*)

NOTES

At the Meeting of the Paris Academy of Sciences, M. Ferdinand de Lesseps was elected an "Academicien libre" in the place of M. de Verneuil, deceased. M. de Lesseps obtained 33 votes; M. Breguet, 24; MM. du Moncel, Jacquemin, and Sedillot 1 each. M. de Lesseps thus obtained 2 votes beyond the absolute majority required to render an election valid, and was therefore declared elected. The number voting, 60, was large.

The forty-first Annual Meeting of the British Medical Association will be held in King's College, London, on Tuesday, Wednesday, Thursday, and Friday, August 5th, 6th, 7th, and 8th. The President-elect is Sir William Fergusson, Bart., F.R.S. The following are the six sections into which the meeting will be divided, and in each section a very large number of papers is already entered to be read:—Section A, Medicine; B, Surgery; C, Obstetric Medicine; D, Public Medicine; E, Psychology; F, Physiology. The sections will meet in rooms of the College appropriated for the purpose, and the Annual Museum of objects of interest in connection with medicine, surgery, and their allied sciences will be arranged in the Library of the College. The President's address will be delivered at 3 P.M. on August 5, and in the evening the Lord Mayor will hold a reception at the Mansion House. The following public addresses will be delivered:—On August 6, an address on Medicine, by Prof. E. A. Parkes, M.D., F.R.S.; on August 7, an address on Surgery, by Prof. John Wood, F.R.S.; and on August 8, an address on Physiology, by Prof. Burdon Sanderson, F.R.S. The President and Council of the Royal College of Surgeons hold a reception on the evening of August 6, and several excursions have been arranged to take place during the meeting. Altogether, to judge from the programme, the meeting promises to be a very successful one.

The Royal Archæological Institute commenced its annual session at Exeter, on Tuesday, when the Mayor and Corporation held a reception at noon. The President, the Earl of Devon, thereafter delivered his inaugural address on the advantages of the study of Archæology, and in the afternoon an excursion took place to Rougemont Castle. In the evening, again, the Mayor held a reception in the Albert Museum. The Sectional Meetings commenced yesterday, and several interesting excursions have been arranged. The Sections are, Antiquities, Architecture, and History. One of the most attractive accompaniments of the Exeter meeting is the formation of a temporary Museum and Portrait Gallery.

The French Association for the Advancement of Science commences its second session at Lyons on August 21, the concluding meeting to be held on August 28. As was the case a Bordeaux, there will be General Meetings, Meetings of Sections or Groups, Scientific Excursions, and Public Lectures. A large number of papers has already been entered to be read at the Sectional Meetings, by well-known scientific men, and several interesting excursions have been planned, including one to the famous pre-historic station at Solutré. So far, this year's meeting of the Association promises to be very successful. Immediately after the session of the Association is concluded, the Geological Society of France holds its annual meeting at Roanne.

DR. GÖPPERT, of Breslau, the veteran writer on the subject of fossil plants, is desirous of disposing of his immense collection, in securing which he has spent more than thirty years, and made it perhaps the finest in the world, embracing, as it does, type specimens of 94 different works and 400 minor essays, represented on about 1,000 plates. The number of specimens exceeds 11,000, and includes Sigillaria from sixteen to twenty feet in length, and other specimens of equal magnitude. There are also 200 specimens of different kinds of amber with their inclosed plants, and also a series of diamonds, with various objects included in them. In addition to the fossil objects there is also a very large collection of recent plants, which serves to illustrate the first-mentioned series, such as palms, tree-ferns, cycades, bamboo, algæ, sections of wood, fruits, seeds, &c. Numerous original drawings also accompany the collection, which add much to its value.

MR. SMITH gives some very interesting details in the *Daily Telegraph* of his excavations at Nimrod. We think, however, the main result of his expedition is to show the necessity of a more thorough and longer continued exploration of the ruins of Assyria than Mr. Smith has been able to give; and the sooner such an exploration is undertaken, the more fruitful are the results likely to be.

THE *New York Herald* of the 17th inst. publishes a letter from Dr. Petermann, the eminent German geographer, to Dr. Strasnecky, the Secretary of the American Geographical Society. In it he says:—As at the departure of the expedition much stress was laid on its prospect of reaching the North Pole, the public at large, which has no idea of the difficulties surrounding the solution of geographical problems, might look upon it as a complete failure. It should not be made a reproach to Captain Hall that he held out such a prospect, for without it he would not probably have obtained either ship or money, or any other support. Placed in a similar condition, the same thing has happened to me and my friends in Germany, and it will always remain thus as long as the civilised Governments of the world devote their millions principally to the increase of their armies, and the scientific objects only figure in the Budget for the crumbs, and as long as people who are willing to add to the little knowledge we have of our own earth have to go begging for small contributions. To me the geographical results of the expedition appear of an extraordinary value. At any rate they are the highest that any vessel among the numerous expeditions of all nations to the North and South Poles have ever accomplished for many centuries. I shall speak of the subject at greater length in my next Arctic report (No. 80).

AT the commencement of 1874, says the *Deutsche Zeitung*, one or two ships of the German navy are to be sent on a scientific mission to observe the transit of Venus. These vessels will have to submit their observations, which are to be extended to ocean currents and tides, to the hydrographic office of the German Admiralty.

THE first three numbers of a work on indigenous and exotic Lepidoptera have been issued by Mr. Hermann Strecker, of Reading, Pennsylvania, U.S. the object of the author being principally to bring to the cognizance of the public the many new species from all parts of the world embraced in his very extensive cabinet. While the preference will be given to those from North America, he, unlike Mr. William H. Edwards, includes some species from other countries. The illustrations, which occupy one plate for each number, are all drawn, printed, and coloured by Mr. Strecker himself in the intervals of his daily labours, and the whole work is extremely creditable to him. The work is in quarto, and it is proposed to publish one number every two months, each with a single plate, crowded as fully as possible with figures. The enterprise is well worthy of commendation, and persons desirous of obtaining the work can do so by addressing Mr. Strecker, as above. A few copies only are printed, and the drawings then erased to make way for a new set.

PROF. MEEK announces the existence of primordial species among the fossils collected by Dr. Hayden, in 1872, from near Gallatin City, Montana, U.S.—a very important geological fact. He has also found carboniferous fossils in various localities. Some of these are from the "divide" between Ross's Fork and Lincoln Valley, Montana, embracing many of the same species as occur in the noted Spurgen Hill locality, in Indiana, of the age of the St. Louis limestone.

AT noon of July 8 Prof. Agassiz formally opened the Anderson School of Natural History on Penikese Island, thus bringing to a practical beginning the great idea of a summer school of natural science as first suggested by Prof. Shaler. Our readers are sufficiently familiar with the details of the circumstances which led to the establishment of this magnificent educational enterprise—first, the donation by Mr. John Anderson, of New York, of Penikese Island, one of the Elizabeth group, situated at the entrance of Buzzard's Bay, and valued at 100,000 dollars; then his endowment of it in the sum of 50,000 dollars to meet the current expenses; and subsequently the presentation to the professor by Mr. Galloupe, of Swampscot, of a yacht worth 20,000 dollars, for use in deep-sea dredgings and other explorations in connection with the school. In a circular Prof. Agassiz gives notice to the public that the island affords no accommodation to strangers, and that no guests can be received excepting those who have been accepted as members of the school. The limit of fifty has long since been made up, one-third of them being ladies, while more than a hundred have been rejected in consequence of the limitation. A caterer has been engaged, who will provide for the table, and keep the rooms in order. There is to be no charge whatever for tuition, and as the dormitories have been built at the expense of the fund, no rent will be charged beyond a percentage of the value of the bed-room furniture. The board is to be charged at cost. Should any persons desire to make collections of specimens to carry away with them, cans and alcohol will be furnished at cost to those who are not already provided.

THE Russian astronomers have decided upon occupying twenty-four stations on the important occasion of observing the Transit of Venus. It is found that the weather will probably be highly favourable to astronomical observation at all the stations in Siberia and on the Pacific coast, as there is an average of only three cloudy days in the month of December in these parts of the Russian possessions. The extreme cold of November is well regarded as an almost insuperable hinderance to the proposed work. The following very complete outfit has been ordered for use on this occasion, viz., three heliometers and three photoheliographs, for use in measuring the position of the planet on its passage across the sun's disc; ten equatorials, for observing the apparent contacts of the limbs of the planet and sun by the use of the spectroscopic method, and for the determination of the same moments by observations with the filar micrometer; ten telescopes, for simply observing the instant of each contact; and besides these, there is for each station a complete outfit of clocks, chronometers, and instruments for determining the local time. The observers are all to practise beforehand at the Imperial Central Observatory at Pultowa. The geographical positions of those stations at which the observations result successfully will be afterwards determined by a special geographical expedition by the Russian navy. To perfect this portion of the work, a line of telegraph will be built through Siberia to Nicolaevsk.

WE have received the programme for Session 1873-74 of the University of Durham College of Physical Science, at Newcastle-on-Tyne. It contains ample information as to the amount and kind of instruction to be obtained at the Newcastle College, and full details as to the arrangements, fees, examinations, exhibitions, and scholarships. There are three exhibitions of 15*l*. each to be awarded after examination in October, one scholarship, the T. Y. Hall scholarship, of 20*l*, yearly value, tenable for three years, and two scholarships offered by Mr. Hugh Taylor, consisting of the expense for maintenance and education at the Newcastle College, for two years: those last are for sons of overmen, deputies, or pitmen, who are engaged in coal mines in the counties of Northumberland or Durham, and are between sixteen and eighteen years of age. So far as it goes, the Newcastle College seems to furnish a thorough training in scientific knowledge and method.

WE have received from Mr. F. Abbott a paper read before the Royal Society of Tasmania, giving the result of his recent observations at the Private Observatory, Hobart Town, Tasmania, of η Argus. He thus summarises the results of his most recent observations. In the eye draft of the object η Argus, Feb. 1873, the principal stars appear to have retained their relative position as shown in the drawing of last year. The dark spaces are extending and becoming more undefined, gradually filling up with small stars, fully half as many again as shown in last year's drawing; the whole field of the telescope when directed to η is studded with stars from the 7th to the 12th magnitude, too numerous to count. I have on the present occasion omitted to make a drawing of the object, as in all probability before long photography will be applied both to this and other portions of the dense Nebula between it and κ Crucis—a thing much required.

A MAGNIFICENT work, in the shape of a Photographic Album of Ethnology ("Anthropologisch-Ethnologisches Album"), from the collections of the Berlin Anthropological Society, is about to be published in parts, by Wiegandt and Hempel, of Berlin, the photographs by C. Dammann, of Hamburg. Each part will contain five leaves 48 centimetres in length by 64 centimetres in breadth, each part in a separate portfolio. The contents will be arranged in tables containing from ten to twenty photographs each, and the price of each part is twelve thalers. The first part contains two tables illustrative of the East Coast of Africa, and three tables for Asia, illustrating Eastern Siberia, Japan, Siam, &c. Appended to each portrait is a brief description indicating the country, particular district, sex, and age of the original. The immense value of such a work to ethnologists is evident.

FROM the "Report of the Radcliffe Observer to the Board of Trustees," we see that a considerable amount of regular observatory work has been done during the past year, and that the establishment is in good condition.

IN a letter to the *British Medical Journal*, Mr. J. C. Galton refers to a specimen of a human heart in which the "moderator band" recently found by Prof. Rolleston in the Cassowary, and long known to be well developed in Ruminants as a strong fibrous cord, running in the right ventricle between its outer wall and the septum, is well developed as a thick muscular band. But he remarks that from it "some of the chordæ tendineæ of the tricuspid valve take origin." Prof. Rolleston also considers that one at least of the columnæ carneæ in man, which are unattached in the middle of their course, and are in connection with the musculi papillares of the tricuspid valve, is homologous with it. In the Ruminant, however, the band is quite free and of fibrous structure, and is apparently a much more specialised development than the uncertain muscular cords found in the human heart.

THE report of work contained in the "Proceedings of the Liverpool Naturalist's Field Club for the year 1872-3," appears to us, on the whole, gratifying. The Society made nine field excursions during last summer, and, considering the unsettled state of the weather, these were well attended. The working members of the Society, during these excursions, devote themselves mainly to botanical collecting, though the majority of those who make up the parties spend their time in visiting places of antiquarian and historical interest. Prizes are given for botanical collections, and we are afraid the Society do not take the precaution of urging upon collectors the danger of extirpating the rare plants of the districts visited in their eagerness to make up prize-taking collections. Several evening meetings were held during last winter, at the first of which Mr. Fisher gave a *résumé* of the Botanical gains of the Society during the excursions. The following valuable papers were also read at these meetings:—"On the Respiration and Germination of Plants," by Dr. Carter; "Corals and Coral Islands," by the Rev. H. H. Higgins, President; "On the Intimate Relations between the Animal and Vegetable Kingdoms," by Mr. Chantrell; "On the Sap of Plants, the Physical Causes of its Ascent, and its Composition," by Mr. Davies. We have also received an "Appendix to the Flora of Liverpool," containing a considerable number of additions to that valuable work, which we noticed on its appearance about a year ago.

IT is said that the scheme which has been on foot for some time past, having for its object the closer connection of St. Andrew's University with the neighbouring town of Dundee, by the establishment of an affiliated college there, on the same principle as the Science College at Newcastle is connected with the University of Durham, has fallen through, several of the St. Andrew's professors being of opinion that if this arrangement were entered into it would ultimately end in the University being transferred across the Tay.

THE first four parts of an "Illustrated International Review of the Universal Exhibition of Vienna, 1873," have come to hand. It is a handsome and well-illustrated folio, printed in French, German, and English, and promises to be an "absolutely complete encyclopædia of the Vienna Exhibition of 1873, at once descriptive, artistic, scientific, anecdotic, and biographical." If the prospectus is faithfully carried out, the work will be very valuable both in a scientific and an industrial point of view.

ADVICES to the 12th of June, dated Denver, U.S., make mention of satisfactory progress in the explorations conducted by Professor Hayden and his parties. One of the divisions of the survey at that time was established near Central City, in charge of Mr. Jackson, and consisted of Mr. Coulter as botanist, Mr. Carpenter as naturalist, and Mr. Cole as assistant naturalist. They had already obtained a large collection of plants and zoological objects, having spent two weeks high up in the mountains. Mr. Jackson had made about fifty negatives of the higher peaks, principally in the vicinity of Long's Peak. They expected to proceed shortly to the "Garden of the Gods." Mr. Gardner has been occupied in establishing his base line of triangulation. He has already erected three signal monuments thirty feet high, and twelve miles apart, all of which can be seen from the main range of mountains. One party is at work on Long's

Peak, in charge of Mr. Marvin, accompanied by Mr. Gardner, and another under Mr. Gannett, accompanied by Dr. Peale, as geologist, and Mr. Batty as naturalist. According to the *Denver News*, the cattle, finding these constructions extremely convenient places for scratching, and thinking them apparently erected for their accommodation, have at once commenced appropriating them to that purpose, and evidently with great satisfaction, as it is said that they concentrate in their vicinity for miles around.

"ANNALEN des Physikalischen Centralobservatoriums" is the German title of the record for 1871 of the work done at the great Physical Observatory of St. Petersburg. It is a very thick quarto in Russian and German, and contains full and well-arranged meteorological statistics for fifty-five Russian towns for the year 1871.

THE following are the principal additions to the Brighton Aquarium during the past week:—10 Thornback Rays (*Raia clavata*), 1 Large Tope (*Galeus canis*), 1 Large Smooth Hound (*Mustelus vulgaris*) 3 Three-bearded Rockling (*Motella tricirrata*), 1,000 Sticklebacks (*Gasterosteus spinosus*), 1 fine group of *Actinoloba dianthus* (orange variety); a Smooth Hound (*Mustelus vulgaris*) gave birth to seven young ones, which died immediately, or were born dead.

THE additions to the Zoological Society's Gardens during the past week include two Maugé's Dasyures (*Dasyurus maugæi*) from Australia, presented by Mr. George Heath; a Tytlers Paradoxure (*Paradoxurus tytleri*) from the Andaman Islands, presented by Mr. J. S. Campbell; a Bactrian Camel (*Camelus bactrianus*) from Asia; a Gibbon (*Hylobates* sp. ?); a Crowned Eagle (*Spizaëtus coronatus*) from Senegal; three Blue crowned hanging Parrakeets (*Loriculus galgulus*) from Malacca; an Egyptian Fox (*Canis niloticus*); an Egyptian Vulture (*Neophron percnopteros*), purchased; an Ocelot (*Felis pardalis*) from America; a Hobby (*Hypotriorchis subbuteo*) from this country, and four red-billed Tree Ducks (*Dendrocygna autumnalis*) from America, deposited.

ON THE TEMPERATURE AT WHICH BACTERIA, VIBRIONES, AND THEIR SUPPOSED GERMS ARE KILLED*

WHILST a heat of 140° F. (60° C.) appears to be destructive to *Bacteria*, *Vibriones*, and their supposed germs in a neutral saline solution, a heat of 149° or of 158° F. is often necessary to prevent the occurrence of putrefaction in the inoculated fluids when specimens of organic infusions are employed. What is the reason of this difference? Is it owing to the fact that living organisms are enabled to withstand the destructive influence of heat better in such fluids than when immersed in neutral saline solutions? At first sight it might seem that this was the conclusion to be drawn. We must not, however, rest satisfied with mere superficial considerations.

The problem is an interesting one; yet it should be clearly understood that its solution, whatever it may be, cannot in the least affect the validity of the conclusion arrived at in my last paper, viz., that living matter is certainly capable of arising *de novo*. We were enabled to arrive at the conclusion above mentioned regarding Archebiosis by starting with the undoubted fact that a heat of 158° F. reduces to a state of potential death all the *Bacteria*, *Vibriones*, and their supposed germs which an organic infusion may contain. The inquiry upon which I now propose to enter, therefore, touching the degree of heat *below this point* which may suffice to kill such organisms and their supposed germs in an organic infusion, and touching the cause of the delayed putrefaction apt to take place in inoculated organic infusions which have been heated to temperatures above 140° and below 158° F., is one lying altogether outside the chain of fact and inference by which the occurrence of Archebiosis is proved.

* Extracts from a paper by Dr. H. Charlton Bastian, F.R.S., read before the Royal Society May 1, 1873.

It seems to me that the solution of the problems which form the subject of the present communication can only be safely attempted by keeping constantly before our minds two main considerations:—

Thus, in the experiments whose results it is now our object to endeavour to explain, the fluids have been inoculated with a compound consisting partly (*a*) of living units, and partly (*b*) of a drop of a solution of organic matter in a state of molecular change; so that in many cases where putrefaction has been initiated after the inoculating compound has been heated to certain temperatures, there is the possibility that this process of putrefaction may have been induced (in spite of the death of the organisms and their germs) owing to the influence of *b*, the dissolved organic matter of the inoculating compound; that is to say, the heat to which the mixture has been exposed may have been adequate to kill all the living units entering into the inoculating compound, although it may not have been sufficient to prevent its not-living organic matter acting as a ferment upon the infusion.

And there are, I think, the very best reasons for concluding that in all the cases in which turbidity has occurred after the organic mixtures have been subjected to a heat of 140° F. (60° C.) and upwards, this turbidity has been due, not to the survival of the living units, but rather to the fact that the mere dead organic matter of the inoculating compound has acted upon the more unstable organic infusions in a way which it was not able to do upon the boiled saline fluids.

The reasons upon which these conclusions are based are the following:—

1. Because the turbidity which has occurred in inoculated organic infusions that have been subjected to a temperature of 140° F. has always manifested itself appreciably later, and advanced much more slowly than in similar mixtures which had not been heated above 131° F.; whilst it has commenced even later, and progressed still more slowly, when occurring in mixtures previously heated to 149° F. Such facts might be accounted for by the supposition that exposure in these organic fluids to the slightly higher temperature suffices to retard the rate of growth and multiplication of the living units of the inoculating compound, although the facts are equally explicable upon the supposition that the later and less energetic putrefactions are due to the sole influence of the mere organic matter of the inoculating compound.

2. So far as the evidence embodied in the Tables goes, it tends to show that the more unstable different specimens of similar infusions are (that is, the stronger they are), the more rapidly and frequently does late turbidity ensue, and the more this late turbidity approaches, both in time of onset and in rate of increase, to that which occurs when inoculated infusions are not heated to more than 131° F.—when both living and non-living elements of the inoculating compound act conjointly as ferments. Such facts show quite clearly that where the intrinsic or predisposing causes of change are strong, there less potent exciting agencies are more readily capable of coming into play; but they still do not enable us to decide whether the exciting cause of this delayed turbidity is in part the living element whose vitality and rate of reproduction has been lowered by the heat, or whether the effects are wholly attributable to the mere organic matter of the inoculating compound.

So far, therefore, we have concomitant variations which are equally compatible with either hypothesis. But it will be found that each of the three succeeding arguments speaks more and more plainly against the possible influence of the living element, and in favour of the action of the organic matter of the inoculating compound, as an efficient exciting cause of the delayed putrefactions occurring in the cases in question.

3. As stated in my last communication,* when single drops of slightly turbid infusions of hay or turnip previously heated to 140° F. are mounted and securely cemented as microscopical specimens, no increase of turbidity takes place, although drops of similar infusions heated only to 122° F. do notably increase in turbidity (owing to the multiplication of *Bacteria*) when mounted in a similar manner. Under such restrictive conditions as these, in fact, a drop of an inoculated and previously heated organic infusion behaves in precisely the same manner as a drop of a similarly treated ammonic-tartrate solution. In each case, when heated to 140° F., turbidity does not occur, apparently because there are no living units to multiply, and because in

* See NATURE, vol vii. p. 435.

these mere thin films of fluid dead ferments are as incapable of operating upon the organic fluids as they are upon the ammonic-tartrate solutions.

4. Because, in the case of the inoculation of fluids which are not easily amenable to the influence of dead ferments, such as a solution containing ammonic tartrate and sodic phosphate, this delayed turbidity does not occur at all. Such inoculated fluids become rapidly turbid when heated to 131° F., though they remain clear after a brief exposure to a temperature of 140° F. When the living units in the inoculating compound are boiled, there is nothing left to induce turbidity in such solutions. The mere fact that these fluids do not undergo change when exposed to the air proves conclusively that they are very slightly amenable to the influence of the ordinary dead organic particles and fragments with which the atmosphere abounds. The absence of delayed turbidity in these fluids serves, therefore, to throw much light upon the cause of its occurrence in the organic infusions.

5. And, lastly, I can adduce crucial evidence supplied by the "Method of Difference," speaking with its accustomed clearness. Two portions of the same hay- or turnip-infusion can be inoculated in such a manner as to supply us with the information we require. In the one case we may employ a drop of a turbid ammonic-tartrate solution previously heated to 140° F., in which, therefore, the living units would certainly be killed: whilst in the other we may add an unheated drop of the same turbid saline solution to the organic fluid, and then heat this mixture also to the temperature of 140° F. The comparative behaviour of these two inoculated fluids (placed, in the ordinary manner, in previously boiled corked phials) should be capable of showing us whether the living elements of the inoculating compound were able to survive when heated in the organic infusion. If they did survive, the fluids inoculated in this manner ought to undergo putrefaction earlier and more rapidly than those inoculated with the drop of turbid fluid, in which we know that the *Bacteria*, *Vibriones*, and their supposed germs would have been reduced to a state of potential death. With the view of settling this question, therefore, the following experiments were made:—

Description of Experiments.	Results.	Inferences.
A. Boiled ammonic-tartrate solution, inoculated with an unheated drop of a similar solution turbid with *Bacteria*, &c.	Turbid in 40 hrs.	That boiled ammonic-tartrate solution is a fluid inoculable by living *Bacteria*, &c., and favourable for their growth and rapid multiplication.
B. Boiled ammonic-tartrate solution, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Clear at expiration of 8th day.	That *Bacteria*, *Vibriones*, and their supposed germs are either killed or deprived of all power of multiplication when heated to 140° F. in this fluid.
C. Boiled turnip- and hay-infusions, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	The precisely similar behaviour of the turnip- and hay-infusions of series C and series D respectively shows that *Bacteria*, *Vibriones*, and their supposed germs are as inoperative in series D as they are known to be in series C; whilst the behaviour of the hay-infusions shows that they are little amenable to the influence of the drop of the saline fluid when its living units are killed.
D. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated turbid saline solution, the inoculated fluid being subsequently heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	
E. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated saline solution, the inoculated fluid being subsequently heated to 131° F.	Turnip-infusions turbid in 28 hrs. Hay-infusions turbid in 38 hrs.	Shows that a heat of 131° F. is not sufficient to kill *Bacteria*, *Vibriones*, and their supposed germs in organic infusions, and, again, that turnip-infusions are more rapidly influenced by such an inoculating agent than some hay-infusions.*

No experiments could speak more decisively. Those of series B show that *Bacteria*, *Vibriones*, and their supposed germs are either actually or potentially killed when heated to 140° F. in the neutral saline fluid, which the experiments of series A show to be eminently favourable for their growth and reproduction. Being certain, therefore, that the living units are killed in the drops with which the fluids of series C were inoculated (because they were drops of the same fluid as was employed in series B), we may be equally certain that the turbidity and putrefaction which did ensue in the turnip-solutions of series C were due to the influence of the mere dead constituents of these drops of the turbid saline fluid; whilst, seeing that the behaviour of the fluids of series D was precisely similar to those of series C, we have a perfect right to infer that this series of fluids (D) was as devoid of living units as those of C are known to be—that is, that *Bacteria*, *Vibriones*, and their supposed germs are killed by the temperature of 140° F. in organic fluids, just as they are in saline fluids, although, as shown by the experiments of series E, they do not succumb to a heat of 131° F.

The evidence now in our possession shows, therefore, that whilst the temperature at which living ferments cease to be operative varies within very narrow limits (131°–140° F.), that which destroys the virtues of non-living ferments varies within much wider limits, and depends not only upon the amount of heat employed, but also upon the nature of the putrescible or fermentable liquid to which such ferment is added, in conjunction with the degree of heat and other conditions to which the mixture is subsequently exposed.* Here, therefore, we have evidence as to the existence of a most important difference between living and not-living ferments, which has always been either unrecognised or more or less deliberately ignored by M. Pasteur and his followers.† This difference is, moreover, thoroughly in accordance with the broad physico-chemical theory of fermentation which has been so ably expounded by Baron Liebig and others, and the truth of which may now be regarded as definitely established. According to this theory "living" matter, as a ferment, would take rank merely as a chemical compound having a tolerably definite constitution; and this, we might reasonably infer, would, like other chemical compounds, be endowed with definite properties, and amongst others that of being decomposed or radically altered by exposure to a certain amount of heat. Looked at also from this essentially chemical point of view, it would be only reasonable to expect that the molecular movements of living ferments with a lowered vitality might not be more marked or energetic than those which many not-living organic substances are apt to undergo; and this being the case, we might expect that there would often be a great practical difficulty in ascertaining whether a ferment belonging to the arbitrary and artificial (though, in a sense, justifiable and natural) category of "living" things had or had not been in operation.

Dr. Bastian then refers to certain statements made by M. Pasteur, and afterwards classifies the various fermentable fluids under three main divisions:—I. Self-fermentable fluids; II. Fluids which will not ferment without the aid of unheated organic matter, either not-living or living; III. Fluids which will only ferment under the initiating influence of living matter.

Dr. Bastian's conclusions from these investigations are thus expressed:—

Thus it can now be proved, by evidence of a most unmistakable nature, that the process of putrefaction which invariably occurs in previously boiled putrescible infusions contained in flasks with narrow but open necks is not commonly (is, perhaps, only very rarely) initiated by living germs or organisms derived from the atmosphere; it can also be proved that putrefaction and the appearance of swarms of living organisms may occur in some boiled fluids when they are simply exposed to air which has been filtered through a firm plug of cotton-wool or though the narrow and bent neck of a flask, to air whose particles have been destroyed by heat, or even in fluids hermetically sealed in

* These experiments of series C, D, and E were many times repeated with specimens of the same turnip- and hay-infusions, the specific gravity of the former being about 1008 and that of the latter 1005. Different specimens of hay especially vary so much that it becomes absolutely essential to use portions of the same infusion for the comparative experiments of these different series.

* See "The Beginnings of Life," vol. i. p. 437.

† See, for instance, all M. Pasteur's celebrated experiments in which he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," as recorded in chaps. iv. and v. of his memoir in "Ann. de Chimie et de Physique," 1862. M. Pasteur was engaged in an investigation, one of the avowed objects of which was to determine whether fermentation could or could not take place without the intervention of living organisms, which M. Pasteur held (in opposition to many other chemists) to be the only true ferments. In his inoculating compound (dust filtered from the atmosphere), there was, as M. Pasteur was fully aware, a large amount of what his scientific opponents considered non-living ferment, whilst *possibly* there existed a certain number of living ferments. In explaining the results of his experiments, however, M. Pasteur and others thought he was pursuing a logical and scientific method when he attributed these results to the action of the possibly existing element of the inoculating compound, whilst he ignored altogether the other element which was certainly present in comparatively large quantity, and the testing of whose efficacy was the ostensible object of his research.

flasks from which all air has been expelled. The evidence in our possession is therefore most complete on this part of the subject: it shows beyond all doubt, not only that putrefaction may and does very frequently occur under conditions in which the advent of atmospheric particles, whether living or dead, is no longer possible, but also that living particles derived from the atmosphere can only be very rare and altogether exceptional initiators of the putrefaction which invariably occurs in previously boiled infusions exposed to the air.

Again, the evidence which we now possess with reference to the influence of heat upon *Bacteria*, *Vibriones*, and their supposed germs is no less decisive. It has been unmistakably proved that such organisms and their imaginary germs are either actually or potentially killed by a brief exposure to the temperature of 140° F. when in the moist state; and it had also been previously established that they are invariably killed by desiccation even at much lower temperatures.*

But if living germs do not come from the air to contaminate the previously boiled fluids, and if it is not possible for any of them to have escaped the destructive influence of heat in the boiling fluid or on the walls of the vessel in which the fluid is contained, what can be the mode of origin of the swarms of living things which so rapidly and invariably appear in such infusions when contained in open flasks, and which so frequently appear when the infusions are contained in flasks whose necks are closed against atmospheric particles of all kinds? They can only have arisen by the process which I have termed Archebiosis.

CONCLUSIONS

If a previously boiled ammonic-tartrate solution remains free from *Bacteria* and *Vibriones* when exposed to the air, it is because the air does not contain living organisms of this kind or their supposed germs, and because mere dead organic particles are not capable of initiating putrefaction in such a fluid.

And if ordinary organic infusions previously boiled and exposed to the air do rapidly putrefy, though *some* of the same infusions when exposed only to filtered air remain pure, it is because such fluids are, in the absence of living units, quite amenable to the influence of the dead organic particles which the air so abundantly contains, although they are not self-fermentable.

Whilst if other more changeable fluids, after previous boiling, when exposed to filtered air or cut off altogether from contact with air, do nevertheless undergo putrefaction or fermentation, it is because these fluids are self-fermentable, and need neither living units nor dead organic particles to initiate those putrefactive or fermentative changes which lead to the evolution of living organisms.

* See the experiments and conclusions of Dr. Burdon Sanderson in Thirteenth Report of Med. Officer of Privy Council, p. 61. This fact of the inability of these organisms and their germs to resist desiccation shows the futility of some objections which have been from time to time raised by those who thought that *Bacteria*, *Vibriones*, and their germs might resist the destructive influence of heat by adhesion to the glass above the level of the fluid, or even in the fluid itself, just as dried and very thick-coated seeds have been known to do. Dry heat would seem to be even more fatal to such organisms and their germs than a moist heat of the same degree, owing to their extreme inability to resist desiccation; if they become dry they are killed at a temperature of 104° F., whilst if they remain moist they succumb, as we have seen, to a temperature of 140° F.

SCIENTIFIC SERIALS

THE June number of the *Journal of Anatomy and Physiology* contains several papers of special interest, as well as the excellent summaries by Profs. Turner and Rutherford, of the progress of Anatomy and Physiology during the last six months. Prof. Turner describes, for the first time, the Visceral Anatomy of the Greenland Shark (*Læmargus borealis*) from two specimens caught near the Bell Rock. The larger was 11 feet 8 inches long, and the other 8½ feet: they were both females. The most important peculiarities of this fish, wherein it differs from other sharks are, that the *bursa entiana* is not developed; that there are two large duodenal cæca, one of which is closely adherent to the pyloric tube, as well as a true pancreas, corresponding with the similar condition found by Alessandrini in the Sturgeon; and that there are no oviducts, so that the ova must be discharged into the peritoneal cavity. From these peculiarities the author places *Læmargus* in a family by itself, named by him *Læmargidæ*.—Prof. Turner also, in a short paper on the so-called claw at the end of the tail of the lion, shows that no true claw exists, but that the tip of the tail is hairless, and becomes hard on drying.—Prof. Rutherford tabulates experiments proving that the retardation of the pulse in the rabbit, which follows closure of the nostrils, depends on the obstruction of the respiration, and not as Drs. Brown-Séquard and Sanderson supposed, on direct reflex action. Mr. Dewar and Dr. McKendrick describe experiments on the Physiological Action of Light, an account of which has already appeared in this journal.—Mr. Blake, of San Francisco, has a paper on the action of the salts of the metals sodium, lithium, cæsium, &c., when introduced directly into the blood. Mr. A. H. Smee, in a paper on the physical nature of the coagulation of the blood, endeavours to prove that it coagulates in obedience to a purely physical law, namely, the power of soluble colloid matter to pectinise, or spontaneously to coagulate. Mr. Garrod, on the law which regulates the frequency of the pulse, proposes as a substitue for that given by Marey, the following:—the heart re-commences to beat when the arterial tension has fallen an invariable proportion, this being the only possible explanation of the facts that pulse rate varies with arterial resistance and not with blood pressure. He also gives a new theory of the source of nerve force.—Dr. Charles, Prof. Curzon, and Prof. Drachmann, record peculiarities in anthropotomy, the first in the arterial system, the second in the muscular and nervous system, and the third in the muscular.—There is an excellent and very careful review, by Mr. Trotter, of the Rev. Samuel Haughton's "Principles of Animal Mechanics," which will be very valuable to many physiologists, who here have the opportunity of seeing the opinion of a mathematician, who is also a biologist, of a work which might by itself lead them to think that the physiogical basis for work was in a better position than it really is.

Bulletin Mensuel de la Société d'Acclimatation de Paris for June. A great portion is devoted to the description of the best modes of rearing silkworms and the more suitable kinds of food for feeding them. A paper is devoted to the Japanese Mulberry (*Morus japonica*), which is being introduced into France as producing a superior food for the silkworm.—The cultivation of various kinds of beans and melons is advocated by M. Bossin, and his paper might be read with advantage in this country, where these vegetables are not sufficiently valued as an article of diet. Not only the acclimatisation of useful, but the destruction of hurtful animals, plants, and insects, forms part of the programme of the society, and we have therefore some remarks on insecticides and on the preservation of insectivorous birds.—The American notes on pisciculture, on the grey wolf, and the commerce of Chicago are interesting. A black monkey from Sumatra has just arrived at the Jardin d'Acclimatation, but it is not expected to live.

SOCIETIES AND ACADEMIES

LONDON

Quekett Microscopical Club, July 25.—Dr. Braithwaite, F.L.S., president, in the chair.—This being the annual meeting, the report of the committee for the past year was read, and testified to the continued prosperity of the club, which now numbers 570 members.—The president delivered the annual address, in the course of which he noticed the progress of microscopical investigation in Botany and Zoology during the past year.—The ballot then took place for the election of officers. Dr. Braithwaite was re-elected president; Dr. Matthews, Messrs. B. T. Lowne, T. W. Burr, and C. F. White, vice-presidents; and Messrs. Bywater, Crisp, Hailes, Hind, Waller, and T. C. White, were elected to fill the six vacancies on the committee. Mr. J. E. Ingpen succeeded Mr. T. C. White, who retires from the office of hon. sec. (owing to increase of his professional duties), after four years of unremitting and valuable service. The proceedings terminated with the usual *conversazione*.

BELGIUM

Royal Academy of Sciences, May 13.—Reports were given in on the following papers:—On the Superficial Tension of Liquids considered in reference to certain movements observed on their surface, by M. G. Van der Mensbrugghe, which it was resolved to print in the *Memoires*.—On the Osculatory Sphere, a note by M. L. Saltel, which is printed in the *Bulletin*.—On the chloric acetonitriles, by M. L. Bisschopinck, also printed in the *Bulletin*.—Essay on the state of vegetation at the epoch of the Heersien Marls of Gelinden, by Count G. de Saporta and Dr. A. F. Marion. It was resolved to print this paper with the plates in the *Memoires*.—The following communications were made:—On frozen alcoholic drinks carried to very low temperatures, and on the cooling and freezing of ordinary or sparkling wines, which will appear in the *Bulletin* for June.—Third addition to the synopsis of the Caloptergines, by M. de Selys Longchamps. His first list was published in 1853, and additions in 1859 and 1869; the present long list contains descriptions of many new species, as well as corrections of and additions to species already described. The author is indebted for the greater part of his material to Mr. MacLachlan.

PHILADELPHIA

Academy of Natural Sciences, May 6.—Dr. Carson, vice-president, in the chair. Double Flowers in *Epigæa repens*. —Mr. Thomas Meehan observed, that on several occasions, during the few past years, it had been noticed among the *variations in nature*, that the tendency to produce double flowers was, by no means, the special prerogative of the florist to originate. Many of our commonest wild flowers, which no one would think of cultivating, had double forms in cultivation which were no doubt originally found wild. Thus we had a double *Ranunculus acris*, *R. bulbosus*, *R. Ficaria*, *R. repens*, and some others. There were, in plants, two methods by which a double flower was produced. The axis of a flower was simply a branch very much retarded in its development, and generally there were, on this arrested branch, many nodes between the series forming the calyx or corolla, and the regular stamens and carpels, which were entirely suppressed. But when a double flower was produced, sometimes these usually suppressed nodes would become developed, in which case there was a great increase in the number of petals, without any disturbance in the staminal characters. But at other times there was no disturbance in the normal character of the axis. The stamens themselves merely became petaloid. This was the case in the *Epigæa*, recently found by Dr. Darrach.—Influence of Cohesion on Change of Characters in *Orchideæ*.—Mr. Meehan also said that in the early part of the winter he had exhibited some flowers of *Phaius Tankervillæ*, in which, by the mere cohesion of one of the dorsal petals with the column, a flower differing very much from the general condition was the result. Since that time Dr. Maxwell T. Masters, in the issue of the *Gardener's Chronicle* for April 12th, notices the receipt of a *Phaius Wallichii* in which there had been produced three spurs and regular petals, looking, Dr. M. says, rather like those of a gladiolus than of an orchid.

May 13.—Dr. Ruschenberger, president, in the chair. The following paper was presented for publication:—"Observations on Nests of *Sayornis fuscus*," by Thos. G. Gentry.—Prof. Cope exhibited and described some extinct turtles from the Eocene strata of Wyoming.

May 20.—"Descriptions of new species of Orthoptera, collected in Nevada, Utah, and Arizona, by the Expedition under Lieut. G. M. Wheeler," by Cyrus Thomas.—"Observations on the Habits of the Neuters of *Formica sanguinea*," by T. G. Gentry.—*Lilium Washingtonianum*.—Mr. Thomas Meehan referred to a paper by Prof. Alphonso Wood, entitled a "Sketch of the Natural Order of Liliaceæ," of the Pacific coast, published in the volume of the Proceedings for 1868, in which he describes a "new species" of Lilium, as *L. Washingtonianum*, giving, as a reason for the name, that it was generally known as the "Lady Washington" by the miners. Prof. W. said, in his paper, that it was remarkable so fine a plant had been overlooked by other botanists. It so happens that it had not been overlooked, but had been described ten years previously by Dr. Kellogg, in the Proceedings of the California Academy for 1858.—"On a Species of Delphinus," by Dr. H. C. Chapman.

PARIS

Academy of Sciences, July 21.—M. de Quatrefages, president, in the chair.—The following papers were read:—Note on changes of rate in isochronous regulators, by M. Yvon Villarceau.—Third note on guano, by M. Chevreul.—New researches tending to prove that the co-ordinating power over bodily movements lies in the cerebellum, &c., by M. Bouillaud.—The laws of friction and concussion on the thermo-dynamical theory, by M. A. Ledieu.—On the movement of a spherical segment on an inclined plane, by Gen. Didion.—On the spectra of iron, and some other metals, by Father A. Secchi. The author had failed when examining the iron spectrum given by a battery of fifty cells, to observe the line 1474K, and he gave, in the present paper, an account of a further search for it. The same battery power, with new acids, was used; various samples of iron were burnt in the arc, either as iron poles or placed in hollow carbon points, and the sunlight was reflected into the spectroscope with a heliostat. The line in question could not be found in any sample of iron used. His other observations are on the "structure" spectra of carbon and aluminium; he observes that each line of the columnar bands is itself resolvable into a mass of fine lines.—On the permeability of the Fontainbleau sands, by M. Belgrand.—On the movement of the wash produced in artificial canals, and on causing water to rise along an inclined bank to a sensibly constant height. A letter from Mr. Nordenskiöld, dated Mossel Bay, latitude 79° 54′ N. was read by M. Daubrée.—New spectroscopic observations of the sun which do not agree with certain sun-spot theories, by Father Tacchini. The theories are those of M. Faye and Father Secchi. The author describes watching a facula over the sun and observing its appearance on the limb which was accompanied by the reversal of large numbers of metallic lines in the chromosphere. This, Tacchini considered as evidence of an eruption, and as militating against Faye's theory because he considers that theory not to allow of eruptions, and against Secchi also, he having stated that faculæ were eruptions, and spots the erupted matter, and yet this facula had no spots during half a revolution.—On Euler's constant and Binet's function, by M. E. Catalan.—Researches on electric condensation, by M. V. Neyreneuf.—Studies on nitrification in soils, by M. T. Schlœsing.—On a combination of picric acid, with acetic anhydride, by MM. Tommasi and David. The authors considered this body as a picrale, in which one atom of metal is replaced by acetyl.—On pyrogallic acid in the presence of iodic acid, by M. Jacquemin.—On a natural combination of ferric and cuprous oxides, and on the production of atacamite, by M. C. Friedel.—On the spontaneous changes of eggs, by M. Gayon.—An attempt to determine, by comparative embryology, the analogous portions of the intestines in the superior vertebrata, by M. Campana. During the meeting, an election was made to the place of *Membre libre*, vacant by the death of M. Verneuil. M. de Lesseps obtained 33 votes, M. Breguet 24 votes, MM. du Moncel, Jacquemin and Sedillot, 1 each. M. de Lesseps was therefore declared duly elected.

BOOKS RECEIVED

AMERICAN.—Views of Nature: Ezra C. Seaman (Scribner & Co., N.Y.).

FRENCH.—Traité Générale de Photographie. 6th ed.: D. v. Monckhoven (G. Masson, Paris).

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ERRATA.—P. 201, col. 1, 1st line below table, after = insert λ. P. 246, title of Fig. 2, for *Salenica* read *Salenia*.