THURSDAY, JUNE 30, 1870

NATURAL SCIENCE AT THE ROYAL ACADEMY

I AM afraid the words of the title of this article will sound like a harsh discord to many ears. Let not the reader suspect that they are in any way akin to that incongruous phrase "art manufacture."

I do not presume to address persons versed in the language of the artistic schools, nor the vaguely vapouring sentimentalists who are leaders of æsthetic cliques.

I know a class of men more worthy of respect who are happily more ignorant: men who are accustomed to attach a defined significance to words: men who, when they hear noises that they do not understand, or when they detect the characteristic haziness of artistic cant, stand meekly on one side, careful and troubled only to pursue the orderly and laborious track of mere matters of fact. It is remarkable that the chasm which separates those groups of men who work in the adjacent fields of art and science is one of the most distinct in the whole system of modern life; as marked as that which divides the official from the commercial mind, for instance, or the sacerdotal from the legal caste. There is no common ground between them. They do not teach their young in the same language; the scientific men generally talking in grammatical and reasonable speech, whilst those who speak on behalf of art generally "gas," if I may be allowed to use an expressive americanism.

A small section of the crowd who throng the Academy galleries at this time of year, persons who habitually associate reason with observation, have been known to complain that the exertions of the painters are to a great extent wasted upon them for want of rudimentary knowledge of the principles upon which fine art is founded, and that the information which probably exists somewhere upon this subject seems to be so mingled with rhetorical flourishes and anecdotes, that if not altogether inaccessible, it demands far more time than most students can afford, to sift it out, so that when they go to look at pictures, they do so in the idlest possible temper, expecting to be merely amused with a pleasant but transient sensation like the smelling of a sweet odour, yet all the while not without misgivings that they might be deriving nobler entertainment or more permanent good. Are pictures mere accidents, or are they produced upon known principles? Are their excellencies estimable by definite methods and referable to known standards, or is it all caprice?

Such questions are very pertinent, and ought to be philosophically answered by able painters, but it happens that such men have not generally cared to spend their energies in speech. If a living painter offers one or two hints concerning the Academy, such as can be written in a weekly journal, they must needs be of a general kind, and with little, if any, reference to particular pictures. In any case, if they do not harmonise with the prevailing tone of dilettantism, they will stand as landmarks of heresy to be kicked against by critics. In the columns of NATURE, however, they will only be read—if at all—by clear-headed and simple-souled naturalists.

The central motive of fine art may be most compactly expressed by the simple term beauty. We will not stop to define the word now. Go to the Academy to seek for it. Do not expect much of it; for amongst the four or five hundred essayists on canvas there represented, a good many, perhaps more than half of them, would repudiate that fundamental principle. Be content to take your beauty in small doses: about in the same proportion as pure gold to the pebbles in the bed of an African river, or sense in a railway novel. When found, enjoy freely in your own way. That is the first stage in artistic culture; some people say the whole—beginning, middle, and end of it. But this will hardly satisfy the scientific student. He will want to find out the relation between beauty and the common world, so as to determine what position this exquisite sensation ought to occupy in the order of his experiences.

Let him then proceed to examine the pictures analytically, and, by way of making a beginning, let him apply a test question all round. Let him ask himself, for instance, whether any example of beauty has been offered to him which depends on a violation of natural laws; whether he has come across anything like a lovely monster. The question may appear to be an idle one, but there is a principle involved in it of the very highest importance, though there is no time to illustrate it here. Let him think it over carefully, and apply it not merely to pictures of monstrous animals and vegetables, but to any abnormal effect of the sun's rays, any deviation from the simple laws by which surfaces govern shadows, and so on; any monstrous clouds, apparently constructed out of featherbeds, and yet beautiful; any monstrous draperies, apparently made of bent metal, or cut in horn, or forced into novel shapes as if by explosive gases, and yet beautiful; any monstrous contortions of the human countenance amongst the large variety of attempts to represent the pleasanter, or more repulsive, or more permanently interesting phases of human life. Let him take note whether he is likely to carry with him to his grave one image or suggestion of loveliness that will have had its origin in a picture produced in ignorance of or defiance of natural law; because, if he find any, the fact will be well worth knowing, and I hope he will not hush it up.

Another hint that I will venture to give the scientific student is, to remember that in these days art, in England, is in its infancy. By the time that the play-going public have sickened themselves with displays of trivial sentimental incident; by the time the new-born professors of art at the universities have taught all the growing boys how life may be treated artistically after it has been earned by doing their day's grinding at the mill; by the time these boys have travelled and studied the art of the ancient world and its relation to the modern; by that time there will have become established a school of original painters, able to represent with ease and accuracy any visible fact, and free to choose out from the procession of physical phenomena such special incidents or moments of visible history, such elements of passing scenes as shall be most profoundly beautiful and delightful to men, and to preserve in comparative permanence their evanescent charms.

I took up a scientific periodical the other day, and lighted upon a letter to the editor concerning the colour of the sky, written of course by a scientific man (one who seems to have read that silly little book by Chevreul on complementary colours), in which he suggests that as the sun looks orange, *therefore* the sky looks blue. He says that upon this theory the planets of Sirius and Vega must have a black sky, those of Betelgeuse a green sky, and so on. But if my readers go to the Royal Academy, some of them will probably be a little surprised to find that they are positively living on a planet which enjoys the advantage of a solid lemon-yellow sky! a sort of information not to be picked up every day. In the same sort of way, ethnological students may inform themselves that the ancient Greek heroines were shaded with streaks of treacle about their "great marbly limbs." But the judicious investigator will take note that the chief end of most of this canvas, and the purpose of most of this paint, is to indulge the artists and their disciples with reminiscences of antecedent art—an exemplification of the cud-chewing tendencies of his species. Many of them have drank deeply at the fountain of second-hand beauty; they have drank perhaps intemperately, and their speech must needs be somewhat sublime to uninstructed ears.

Let not the student carry away the impression that artistic faculties and wanton inaccuracy are necessarily connected. If the scientific writer who offered suggestions to account for the blueness of the sky had acquired the habit, which the practice of painting gives, of comparing the intervals between different colours, and estimating the intensities as well as the extent of the several elements that go to make up any given scene in the out-of-doors world, he could hardly fail to notice that the sun at mid-day would in all cases be the least coloured object in the whole field of view; that the colours of other objects would neither gain nor lose perceptibly where exposed to his direct rays; whereas, the shaded and overshadowed parts of them would suffer various enhancements or modifications from the coloured rays reflected from surrounding surfaces; and that, in the case referred to, in a mid-day cloudless sky, the most widely extended and most purely coloured of all the surrounding spaces being blue, all the shadows and many of the shades would be dyed blue, so to speak, over their own proper colours; and that the whole scene would be steeped in the "blue flood of light" of the poets. The colours of the face of our planet, both native and derived, present a large field for study, and a good deal more can probably be ascertained about them than about many less pleasant yet less neglected subjects; and when the scientific investigators have done with the blue sky, I hope they will let us hear what they have to say of the blue sea.

What is the moral of all this? Simply that the scientific men pay too little attention to the broader aspects of the visible world; while the artists on their part pass by the clear fountain of natural beauty, and content themselves with dreamily sipping lukewarm water from the corroded vessels of their forefathers; the one group of doers standing apart from the other; whereas, if either would go to school with the other, they would, in my opinion, each stimulate and aid the labours of the other, and divide between them a far larger share of the spoils of the world.

JOHN BRETT

ON THE NATURAL LAWS OF MUSCULAR EXERTION

AMONG a multitude of profound and happy suggestions to be found in Mr. Babbage's Economy of Manufactures, are some remarks on the relation between fatigue and the rapidity or degree of muscular exertion. Coulomb, it appears, had previously investigated the most favourable load for a porter, and had ascertained by experiment that a man walking upstairs without any load, and raising his burden by means of his own weight in descending, could do as much work in one day as four men employed in the ordinary way with the most favourable load. Mr. Babbage clearly points out (p. 30) that the exertion necessary to accomplish any kind of work consists partly of that necessary to move a limb of the body, and partly of the force actually utilised in the work. The heavier the work done, the larger the proportion, therefore, of the power utilised. But there is a limit to this mode of increasing the useful effect, because, by the natural constitution of the muscles, they can only develop a limited amount of force in a given time, and the fatigue rapidly increases with the intensity and rapidity of exertion. Hence there is in every kind of work a point of maximum efficiency, which is in practice ascertained more or less exactly by frequent trial.

This subject appeared to me to possess interest for at least two reasons: it might be made to throw some light upon the chemical and physiological conditions of muscular force; it might also point out how we could make some commencement, however humble, of defining the mathematical relations upon which the science of economy is founded. I have therefore attempted to add precision and certainty to the ideas put forth by Coulomb and Babbage, by some experiments of a simple kind.

The first and least interesting series of experiments consisted in ascertaining the comparative distances to which various weights could be thrown upon level ground. The product of the weight and distance was taken as the measure of useful work, and it was the object to ascertain according to what law this varied, and at what point it was a maximum. The weights employed varied from ½ lb. up to 56 lbs., and were thrown as nearly as possible in a uniform manner and at the most advantageous angle. About 57 experiments at different times were made with each weight, or 456 experiments in all; and it was quite obvious that good average results were obtained, the correspondence of different sets being very satisfactory. The results are as below:—

Weight in pounds	56	28	14	7	4	2	1	½
Average distance thrown in feet.	1·84	3·70	6·86	10·56	14·61	18·65	23·05	27·15

A little consideration showed it to be probable that these numbers would agree with an equation of the form—

$$x = \frac{p}{w + q}$$

in which x = distance thrown,
w = weight thrown,
q = constant weight representing about half that of the arm,
p = constant amount of force exerted.

The experiments give us eight distinct equations by which to determine the two unknown quantities p and q; and by the method of least squares we determine their most probable values to be—

$$p = 115{\cdot}7$$
$$q = 3{\cdot}9$$

The formula thus becomes—

$$x = \frac{115{\cdot}7}{w + 3{\cdot}9}$$

And calculating thence the distances for the several weights, they are:—

Weights ...	56	28	14	7	4	2	1	½
Calculated distances	1·93	3·63	6·46	10·61	14·65	19·61	23·61	26·30
Differences from experiment	+·09	−·07	−·40	+·05	+·04	+·96	+·56	−·85

The correspondence is so close as to show that the formula is in all probability the true one, and the quantity 3·9 does not differ much from half the weight of the arm, which might be expected to enter into the question. The fact is that the correspondence is embarrassingly close, and I am inclined to attribute it partly to chance. The experiments could hardly have been expected to give results accurate to an inch or two in some cases, and though the formula must be considered true on the ground of experiment, I do not quite see how to explain it on mechanical principles.

If we regard the useful effect as the moving of the greatest amount of matter, it is $x \times w$, and, theoretically speaking, increases continually with w. For the different weights and calculated distances, it is as follows:—

Weight...	56	28	14	7	4	2	1	½
Useful effect	108·1	101·6	90·4	74·3	58·6	39·2	23·6	13·2

But in reality it was not possible to raise the larger weights without exerting additional force unconsidered in the formula, so that the practical maximum of efficiency is probably about 28lb., in the case of my own right arm. With different people it would, of course, vary somewhat.

The above experiments completely confirmed Mr. Babbage's remarks, but did not seem to lead to any further results. I proceeded, therefore, to other experiments upon the rate of exhaustion of muscular fibre. One mode of trial was to raise and lower various weights by a pulley and cord through the convenient range of the arm, continuing the motion with unrelaxed rapidity until the power of the muscles was entirely exhausted. The results of more than fifty experiments were as follows:—

Weight lifted ...	56	42	28	21	14
Average number of times	5·7	11·9	23·0	37·6	111·0
Useful effect ...	319	500	644	790	1554

These numbers show that the total greatest amount of labour can be done with small rather than large weights in this case; but they fail to give any regular law, owing probably to the weight of the body being brought into use with the larger weights.

The mode ultimately adopted was to hold out various weights in the hand at the full stretch of the arm, and to observe the times during which they could be supported. No two experiments were made with the same arm, without allowing, at least, one hour to elapse, so that the vigour of the muscles might be restored. With the smaller weights there was naturally some uncertainty as to the time, but in the case of the large ones the time was very definite. Altogether 238 experiments were made, an equal number with each arm. Uniting all the experiments for the same weight, the results are:—

Weight ...	18	14	10	7	4	2	1
Times in seconds	14·8	32·5	60·3	87·4	147·9	218·9	321·2

These results are pretty satisfactory averages; thus the probable error, for two cases indifferently chosen, was, for 18lbs. in the left hand about ·5, and for 4lbs. in the right 2·7, and the error of the combined results would be less. With the exception of the results for 10lbs. in the left arm, which appear to be somewhat in excess, these numbers are very regular, and point to a systematic law governing the rate of fatigue. The useful effect, or the product of the weight and time, shows a decided maximum, about 7lbs., as follows:—

Weight	18	14	10	7	4	2	1
Useful effect	266	455	603	612	592	438	321

If the weight held be very small, much power is lost in merely sustaining the arm; if the weight is large, there is comparatively little loss on that account, but the power of the muscles is soon run out, and no sufficient opportunity for restoration is allowed. The weights chosen for dumb-bells and other gymnastic exercises appear to be about those which give the maximum efficiency.

I have made several attempts to explain these numbers by reasonable suppositions as to the conditions of exhaustion and restoration of muscular power. It seemed reasonable to suppose that the supply of new matter from the blood would increase in some proportion to the vacancy or want of it, but all such conditions led to integrals of a logarithmic form, which could not be easily compared with experimental results. No formula that I obtained could be made to agree properly with the figures, and all that can be said is that the curve representing the results has a certain appearance of a logarithmic character, so far agreeing with the formulas obtained. Those who are acquainted with the physiology of the subject might succeed better; I am not sure, for instance, how far the failure of strength is due to the exhaustion of the original substance of the muscle, how far to the inadequacy of the current supply of blood. It is a question again how far in any case of muscular action the supply is promoted by the increased action of the heart, or checked by the possible constriction of the arteries. If these questions have not been or cannot be otherwise decided, they might, perhaps, be indirectly solved by experiments of the kind described.

My own object, however, was not to intrude into the domain of physiology, but to show that definiteness might possibly be given by degrees to some of the principles and laws which form the basis of the science of political economy. In some speculations upon the mathematical theory which must underlie that science (read at the British Association in 1862, and published in the *Journal* of the Statistical Society for June 1866, p. 282), I endeavoured to show that it was only the excessive difficulty of determining the character of the functions involved, which prevented economy from taking the mathematical form and standing proper to it. There is little doubt as to the principles of the subject; but when we try to put them into figures, the data are found to be so deficient, complicated, variable, and subject to disturbances of all kinds, that any hope of accuracy soon dies away in most cases. In the above experiments I have attempted to determine the exact character of the functions connecting the amount of work done with the intensity and duration of labour in certain simple cases. These cases, however unimportant in themselves, represent principles which have innumerable applications in common life.

W. STANLEY JEVONS

THE NEW ZEALAND INSTITUTE

Transactions and Proceedings of the New Zealand Institute, 1868. Vol I. Edited and published under the authority of the Board of Governors of the Institute, by James Hector, M.D., F.R.S., Wellington. (London: Trübner and Co.)

OUR brother philosophers at the Antipodes have set us an example that we should do well to follow in this country; those who reside at head-quarters (Wellington) and who form the New Zealand Institute, having affiliated to themselves under a special Act of the Legislative Government, the various other societies engaged in similar pursuits that exist in the New Zealand Islands; and who in consequence transmit their papers, or abstracts of them, to the Institute for incorporation in their Transactions. It is to such an organisation as this that we must look for relief from the overwhelming pressure of miscellaneous scientific literature under which the British naturalist now groans. No matter what branch of science he affects, or in how narrow a groove of it he walks, the number of Transactions, Proceedings, Journals, &c., with which he must keep *au courant*, is the great obstacle to his progress; and if he at all takes a broad view of his science, he must be content that it should be a superficial one, and increasingly so as new societies and journals spring into being.

The rules and statutes of the New Zealand Institute appear to be under parliamentary control, and have little analogy with the charters of our free-born societies:—they were published in the *New Zealand Gazette* of March 9, 1868, and the following is a summary of them:—

Art. 1. Any society desirous of incorporation must consist of twenty-five or more members, and subscribe 50*l.* annually for the promotion of the branch of knowledge it professes.

Art. 2. Incorporation ceases on the failure of these conditions.

Art. 3. Any such society must expend either one-third of its annual revenues in or towards the support of a local library or museum, or one-sixth of its revenues to the extension and maintenance of the museum and library of the Institute.

Art. 4. Failure of this condition is followed by cessation of incorporation.

Art. 5. All papers read at such societies shall be regarded as communications to the Institute, and be published by it, under the following regulations:—

(*a*) The publication shall consist of a current abstract of the proceedings of the incorporated societies, and of papers read before them; and shall be entitled "Transactions of the New Zealand Institute."

(*b*) The Institute has the power to reject papers, but (*c*) must return them. (*d*) A proportional contribution for the cost of publishing the Transactions may be demanded of the societies in respect of the papers they contribute; and (*e*) a proportional number of copies of the Transactions will be sent to each society; which may also (*f*) have as many copies as it pleases, at cost price.

Art. 6. Funds and properties derived from the societies shall be vested in the Institute, and applied by its governors to public uses.

Art. 7. The incorporated societies shall conduct their own affairs, making their own bye-laws, &c.

Art. 8. Certificates of incorporation are granted upon application and compliance with the foregoing conditions, under the seal of the Institute.

The museum and library of the Institute are under the management of the Board of Governors; the laboratory is under the exclusive management of the manager of the Institute.

The governors are nine in number: the official ones are the Governor of the Colony, the Colonial Secretary, and the Superintendent of Wellington; joined to six men, eminent for their scientific attainments or love of science; amongst whom are Dr. Hector, who is likewise manager, and the Hon. Col. Haultain, who is likewise hon. secretary and treasurer. There are four affiliated societies: the Wellington Philosophical Society, the Auckland Institute, the Philosophical Institute of Canterbury, and the Westland Naturalists' and Acclimatisation Society.

With regard to the contents of this first volume, it opens with a capital purpose-like address from the Governor, Sir. G. Bowen, which is followed by a series of papers, for the most part of very great scientific value and interest, and which give a very favourable idea indeed of the spread of scientific knowledge in the Colony, and the number of earnest workers it contains.

Of these, those comprising the Transactions contain no fewer than twenty-two articles on Geology, Physics, Botany, Ornithology, Applied Sciences, Wave and Earthquake phenomena, &c.; these are followed by thirty-nine papers and verbal descriptions of scientific phenomena; and these again by ten essays of great research and merit, respectively entitled:—

"On the Geographical Botany of New Zealand," "On the Leading Features of the Geographical Botany of the Provinces of Nelson and Marlborough," "Remarks on a Comparison of the General Features of the Flora of the Provinces of Nelson and Marlborough, with that of Canterbury," "Sketch of the Botany of Otago," "On the Ornithology of New Zealand," "On the Botany, Geographic and Economic, of the North Island of New Zealand," "On the Cultivation and Acclimatisation of Trees and Plants," "On the Geology of the North Island of New Zealand," "Short sketch of the Maori Races," "On the Maori Races."

The last of these is the result of the life-long labours of one of the most accomplished and industrious of the early missionary settlers in the North Island, Mr. Colenso; whose botanical researches have been as important as his ethnological labours. The typography of the work is very good, but the illustrations are unequal, and often much larger than they need be for all they demonstrate.

From such an excellent beginning who can doubt that important results will quickly follow? To us the experiment of affiliation is a very interesting one, the success of which we hope to witness. That there are in the New Zealand scheme sources of failure is obvious, but we see none which may not be overcome by firmness, judgment, and singleness of purpose, on the part of the head Institute, and by the avoidance of petty jealousies, and a due regard to the interests of Science on that of the affiliated societies. The power vested in the governors of rejecting unfit papers is a most valuable one, if conscientiously exercised; it cannot be too vigilantly watched by the contributors to the Institute, nor too carefully conducted by the governors. In this country the power vested in the councils of our societies, of suppressing papers on the advice of competent referees, works on the whole well, though we could indicate societies, and some of high position, too, which have quite recently published in their Transactions worthless matter, that has evidently never been examined by a council, nor reported on by competent advisers; thus squandering the property of the members, and doing discredit to the science which the members represent.

Is it too much to expect that our powerful and comparatively wealthy societies should affiliate some of their weaker brethren, who devote themselves to the same branches of science? applying their own tests to the admission of contributions to their Transactions, and leaving to lesser societies the freedom to govern themselves as they think best. Of such an affiliation many of the lesser societies would be proud, and the greater could not but benefit by the arrangement.

To one other point only we shall allude in reference to the Transactions of the New Zealand Institute, viz., the advisability of breaking it up, now or at some future time, into series treating of certain branches of science, for the convenience both of workers and purchasers. By so doing, the members would set another example well worth following by some of the oldest, gravest, and most learned of the English, Irish, and Scotch scientific bodies, which publish ponderous quartos of mixed sciences, that are worth neither their shelf-room nor their cost of binding to the mass of the members and purchasers. But we shudder at the thought of suggesting to such august bodies as the Royal Societies of London and Edinburgh, or the Linnean Society, or the Royal Irish Academy, the propriety of breaking up their Transactions into series confined to definite branches of science, and husbanding their funds by giving to their members and correspondents such only as they care to have.

OTHER WORLDS THAN OURS

Other Worlds than Ours. By R. A. Proctor. (Longmans, 1870.)

THAT Mr. Proctor could write, if he were so disposed, an excellent and instructive book on certain branches of Modern Astronomy, we have no manner of doubt; but for the successful accomplishment of such a work, it would be necessary, in the first instance, to settle in his own mind what particular classes of persons the work was intended to suit; and, in the second place, it would be essential for the author to allow himself sufficient time for the completion of his project. It is from a failure in these two particulars that what would otherwise have been a truly remarkable book, is brought down to a level much below Mr. Proctor's actual abilities.

A large portion of the book is devoted to the not very fruitful question of the habitability of the planets or their satellites by beings not generically very different from the human race; but we do not think the question in Mr. Proctor's hands is advanced on either side of the argument, materially beyond where Whewell and Brewster left it. The chief novelty consists in the hypothesis that Jupiter and Saturn may possibly serve as suns to at least some of their satellites; and, by the partial emission of heat (and light?), may compensate for the extreme remoteness of the central orb of the solar system. We can neither follow nor admit the validity or consistency of Mr. Proctor's arguments. The question whether the larger planets have or have not as yet cooled down, by radiation, to a sort of normal temperature, is one of considerable interest, and lies within the scope of astronomical physics; but we cannot say that Mr. Proctor's remarks either remove or elucidate the difficulties of the points at issue. On the contrary he speaks fitfully, and hesitates between doubt and something bordering on dogmatism. For instance, in page 140, he writes thus: "We seem led to the conclusion that Jupiter is still a glowing mass. . . ." Two pages onward he remarks: "He (Jupiter) is not an incandescent body;" and then after he has given his own opinion, that Jupi er is still . . . . "fluid probably throughout, and seething with the intensity of primæval fires, ..." he forthwith falls foul of the late accomplished Dr. Whewell, in terms such as these: (p. 145) . . . . "Surely no astronomer, worthy of the name, can regard this grand orb as the cinder-centered globe of watery matter, so contemptuously dealt with by one who, be it remembered thankfully, was not an astronomer." Our readers will probably feel that such language is to be regretted, and is alike unworthy of Mr. Proctor, and inapplicable to Dr. Whewell.

We have been induced to select the foregoing passages, because they are in fact typical of the style in which Mr. Proctor's volume is written. Laplace, the elder and the younger Herschel, Humboldt, Admiral Smyth, Sir William Thomson, Mr. Tait, Dr. Balfour Stewart, Professor Tyndall, and other honoured names, all in their turn come under Mr. Proctor's adverse criticism with greater or less severity. Mr. Lockyer is the object of our author's especial censure; simply because his opinions do not square with those of our author on the nature of the solar corona visible during a total eclipse, and on which subject wise astronomers confess their profound ignorance, therefore Mr. Lockyer's opinion is condemned as "erroneous," which it may or may not be, and Mr. Lockyer himself is represented as impeding the progress of science! This strong and repeated reference to what our author regards as the scientific peccadillos of an eminent observer, seems to us all the more unnecessary, from the fact, of which Mr. Proctor must be well aware, that one main purpose of the proposed British expedition to view the forthcoming solar eclipse, is to ascertain, if possible, what is the real nature of the mysterious light which forms the corona, and which often streams from the solar photosphere in extended and fantastic forms. We venture to suggest that, for the promotion of that calmness of spirit which is essential to the successful investigation of scientific truth, the refutation of scientific error should in all cases be free from even the appearance of strife and personality. But, passing from the criticisms on Mr. Lockyer's theory of the solar corona, the fate of Mr. Lassell, the present accomplished and experienced president of the Astronomical Society, is not more fortunate. Mr. Lassell, after years of careful observation with a magnificent instrument, unique of its kind, comes to the conclusion that not more than *four* satellites of Uranus have ever been certainly observed. Mr. Proctor, however, thus writes: "I have very little doubt that Uranus has at least eight satellites." And again, although our author admits that Sir William Thomson seems to have abandoned his theory of the probable supply of the sun's heat and light by a battery of meteors, nevertheless he thus writes: "*I am quite certain* * . . . . that at least an important proportion of the sun's heat is supplied from the meteoric streams which circulate in countless millions around him" p. 205). We may fairly ask whence has Mr. Proctor this *certain* knowledge of *countless millions of meteoric streams* impinging on the sun? And how can he, with reference to the satellites of Uranus, venture to set his *opinion* in antagonism with the results of the protracted observations of so accomplished and experienced an astronomer as Mr. Lassell?

We may call Mr. Proctor's attention to the probable inadvertency of his description of the hydrogen spectrum as *white*, which it is not in any sense; and to his inexact description of Mr. Carrington's remarkable observation of what may have been a so ar outburst. Mr. Carrington did not use, and he could not have used, as Mr. Proctor assumes, a dark glass in projecting the solar image on a screen, and consequently the alleged *breaking of this dark glass* by the presumed solar outburst, could have occurred in our author's imagination alone. These mis-statements, however, are easily corrigible in a second edition, presuming they are not typical of much else in the volume itself.

We think so highly of Mr. Proctor's astronomical knowledge and general ability, that we have ventured to point out what strike us as blemishes in a work which contains so much that is suggestive and valuable. Among the portions that are most suggestive are Mr. Proctor's remarks on the distribution and motions of the stars *in streams*; we are far from satisfied that our author has as yet made good his case; but whether his theory be correct or not, the suggestions are valuable, and afford ample scope for the astronomy of the future. Overlooking, however, and forgetting the blemishes, we feel no hesitation in recommending this volume to the perusal of all who are interested in the progress of one of the noblest and most fascinating of the sciences. Some of the plates which illustrate the volume are a decided advance upon all their predecessors of a like kind, and augur well as promises of yet further improvements.

C. PRITCHARD

* The italics are ours.

OUR BOOK SHELF

Balfour's Class-book of Botany; being an Introduction to the Study of the Vegetable Kingdom. With upwards of 1,800 illustrations. Third edition. (Edinburgh: A. and C. Black, 1870.)

PROFESSOR BALFOUR'S "Class Book of Botany" is too well and favourably known to botanists, whether teachers or learners, to require any introduction to our readers. It is, as far as we know, the only work which a lecturer can take in his hand as a safe text-book for the whole of such a course as is required to prepare students for our university or medical examinations. Every branch of botany, structural and morphological, physiological, systematic, geographical, and palæontological, is treated in so exhaustive a manner as to leave little to be desired. The illustrations also form, when enlarged, the very best set of diagrams that a lecturer can have. After this, it may seem hypercritical to find any fault with the new edition. We cannot, however, but regret that the opportunity was not taken of rendering the book still more complete by bringing it down to our present state of knowledge. As stated on the title-page, the additions and corrections are entirely confined to the department of organography, and, as far as they go, are valuable. In particular, the treatment of the subjects of carpology, inflorescence, and phyllotaxis, is rendered much more complete. In other departments we have no such additions, and we miss any reference to the recent labours of Hildebrand, Parlatore, and others, following those of Darwin in the department of fertilisation; of King, Strasburger, and many others in the structure of the reproductive organs of Cryptogams, or to the remarkable observations of Prillieux, Rose, and Brongniart on the movements of chlorophyll. In the department of vegetable palæontology in particular, Unger, Schimper, Heer, Ettingshausen, Dawson, and Carruthers have rendered the science of 1854 scarcely recognisable in 1870; and yet we find not a word added, even to the first edition. The fault appears to be that the book was "stereotyped." Scientific works ought never to be stereotyped. The author has evidently been exceedingly cramped in the insertion of new matter, and the correction of errors has been rendered impossible. Thus we find repeated the old account of the mode of fertilisation in *Parnassia*, which has been shown by both English and Continental botanists to be erroneous, and illustrated by a drawing which is quite incorrect. The work is one, however, which is indispensable to the class-room, and should be in the hands of every teacher. A. W. B.

Proceedings of the London Mathematical Society. Vols. I. and II.

THIS Society met for the first time in January, 1865. At that time the only society in London which received mathematical papers was the Royal Society, while the Philosophical Society of Cambridge naturally took its tone from the university examinations, and paid more attention to the proposal and solution of problems than to the working out of new principles. The present society was formed mainly in order that investigations carried on independently over a wide range of subjects might be compared, and that from the comparison there might grow up some new calculus which should bear to the analysis of the present day the same kind of relation that integration bears to Wallis's elaborate investigations of the properties of the centre of gravity. There were other collateral objects of scarcely inferior importance, such as the improvement and extension of the language of mathematics, the simplification of demonstrations of known truths, and the study of the history of mathematics. There is scarcely a branch of pure mathematics in which the society has not advanced the boundaries between the known and the unknown, but the attention of the leading members has been chiefly devoted to the higher curves.

The introduction of a new word or phrase has often marked an epoch in the history of a science; many of the theories systematised by Darwin are to be found in the writings of others before the happy phrase "natural selection" gave them a simple and enduring shape. Of like importance is Mr. Grove's expression, "correlation of forces," and we find in these proceedings several words, the introduction of which appears likely to play an important part in the development of the science.

The word "quantic" for a "rational and integral function" has been for many years in use, but "deficiency" for the number by which the double points of a curve (including cusps) fall short of the maximum $\frac{1}{2}(n-1)(n-2)$, and "unicursal curves" for those curves in which the deficiency $= 0$, or, in other words, curves in which the co-ordinates (x, y, z) can be expressed as rational and integral functions of a parameter θ, were first used by Prof. Cayley before this society.

The history of mathematics is enriched by an interesting paper by Mr. Merrifield, showing that the Arabs were acquainted with the property of the radical axis.

Prof. Sylvester's proof of Newton's celebrated rule for the discovery of imaginary roots, hitherto undemonstrated, and Prof. DeMorgan's simple proof that every function has a root (which we should be glad to see in a fuller form) are the principal gains in the mere demonstration of known truths. We could wish to see in print the other communications made to the society by Professors Sylvester and DeMorgan, especially those by the former relating to unicursal derivation of successive points on cubic curves, and to residuals.

We heartily congratulate the society on the vitality and enthusiasm evident in every page of its proceedings, and join with them in their hope that they may shortly obtain a tenement of their own worthy of the great, albeit unostentatious, work in which they are engaged.

H. A. N.

Jahrbuch der K.-K. Geologischen Reichsanstalt. Band XIX. Nos. 3 and 4. 1869.

Of this admirable repertory of memoirs on the geology of the Austrian Dominions, the last two numbers for 1869 have lately reached us. This publication contains the cream of the communications made by members of the Imperial Geological Institution, to which the carrying out of the survey of that great and varied tract of country subject to the Austrian sovereign is entrusted; it always includes many papers of great importance to the student of general geology, and the portions now before us present no falling off in this respect. Of strictly geological papers Prof. D. Stur is the principal contributor. He describes the occurrence of brown coal in the district of Budafa, in Hungary; reports at considerable length on the results of the geological survey of the environs of Schmöllnitz and Göllnitz, also in Hungary; and contributes two other papers of more strictly local interest; but the most important of all the geological memoirs is F. von Hauer's notice of the geology of the Western Carpathians, a most interesting district in every respect. Dr. M. Neumayr's contributions to the knowledge of indigenous fossil faunæ contain descriptions of the univalve shells of the freshwater marls of Dalmatia, and of the Congerian strata of Croatia and Western Sclavonia; the species, many of which are new, are well represented on four plates. Five plates are also devoted to the illustration of another palæontological paper, which will probably possess the most interest of all for extra-Austrian geologists—namely, Dr. E. von Mojsisovics's memoir on the Cephalopod-fauna of the Alpine Muschelkalk, some of the species included in which are remarkable for their wide geographical distribution, especially the characteristic species of the zone (*Arcestes* or *Ammonites Studeri*), which ranges to the Himalayas in one direction, and to Spitzbergen in another.

LETTERS TO THE EDITOR

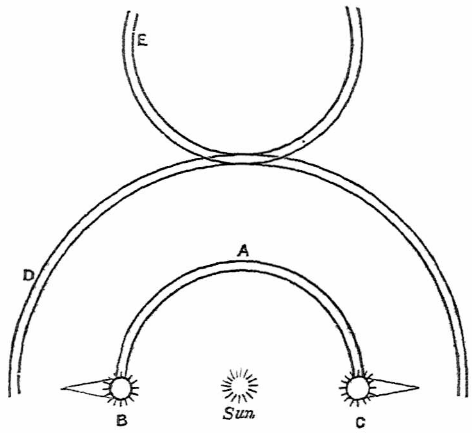
[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Parhelia

(1.) *Seen near Llandudno on 23rd of June, by* Joseph Paget

This evening the phenomenon of which a sketch is enclosed was seen here, viz.:—

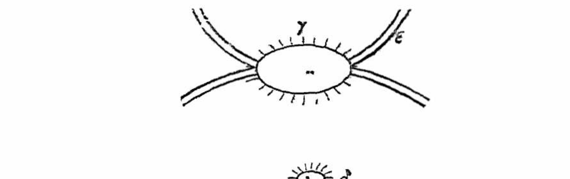
A B C, a portion of a circle of 45° diameter, resting at B and C on mock suns from which a sheaf of light proceeded outwards.



D, a portion of another circle of 90° diameter, at the apex of which was an inverted portion of another circle E of 45° diameter.

(2.) *Seen at Highfield House, near Nottingham, on 23rd of June*

At 7^h 36^m P.M., there was an extraordinary appearance in the heavens. Immediately above the true sun, at a distance of 23°, was an oval-shaped mock sun δ, colourless and not bright; at the distance of 90° from the true sun, and on its horizontal level were two double mock suns, α β, strongly prismatic and very brilliant. They were oval, and from each a flame-like ray extended in the opposite direction to the true sun, portions of a circle of 90° in diameter passed through these mock suns and



also through an unusually large mock sun γ, situated 45° immediately above the true sun, and which was prismatic and almost too brilliant to look at; from this mock sun there was also a portion of a circle of 45° in diameter.

The phenomenon faded away at 7^h 53^m P.M. The weather had been hot from the 13th inst., reaching 86·8° in the shade on the 21st, and 86·6° on the 22nd; whilst on the 23rd (the day of this occurrence) it was only 72°, and on the morning of the 24th, the minimum temperature had fallen to 42·9° at four feet, and to 38·2° on the grass, or a fall of 43·8° in temperature in

36 hours. During daylight on the 23rd there was an Aurora Borealis which continued till 1 A.M. of the 24th; at 7 A.M. rain commenced.

Highfield House Observatory, June 24 E. J. LOWE

(3.) *Seen near Burton-on-Trent, June* 23.

The rare and beautiful phenomenon of parhelia was seen by many observers in this neighbourhood at about seven o'clock on the evening of the 23rd inst., and it continued to be visible for more than a quarter of an hour.

The horizontal bar of light, the coloured halo, and the intensified light at the intersections, as also a portion of the upper bow, were all seen very distinctly. The temperature of the atmosphere at the time was rapidly lowering, and during the night a considerable quantity of rain fell.

I presume that the phenomenon was very local, as I have seen no notice of its occurrence in any of the daily papers to which I have had access.

Burton-on-Trent, June 25 EDWIN BROWN

Natural History of Celebes

It will be, perhaps, not without interest to the readers of your periodical to receive the information that I am leaving for Celebes, on a natural history expedition, at the beginning of July, to stay there for a considerable time with the purpose of exploring first this interesting and little known island as far as possible, and this done, the islands in the neighbourhood.

I shall be happy to learn the wishes and supply the desiderata of any naturalist who takes a special interest in the fauna or flora of the island. Letters will reach me Poste Restante, Menado, Celebes.

I have just finished the translation and publication of Mr. Wallace's "Contribution to the Theory of Natural Selection."

ADOLF BERNHARD MEYER

12, Victoria Road, Kensington, London, W., June 28

Fertilisation of the Barberry

C. K. SPRENGEL, in his *Entdeckte Geheimniss der Natur im au und in der Befruchtung der Blumen*, gives an excellent :count of the structure of the Common Barberry, *Berberis ılgaris*, and points out how it is visited by insects, and how, pon the touch of an insect's limb or proboscis, the irritable filaments move inwards, and press the opened anthers against the stigma. It is needless to recapitulate the details of structure and movements of a plant so well known, but I venture to think that there is a function and a purpose beyond those which Sprengel's ingenuity has pointed out. Sprengel's great object was to show how insects and flowers mutually help one another, and, consequently, when he had shown that the anther could not shed its pollen on the stigma until the filament was touched by an insect, and that, when so touched, the open anther became pressed against the stigma, he was satisfied. He does not seem to have been fully alive to the wider generalisation made by Mr. Darwin, that this relation of insects to flowers serves the purpose of crossing by fertilising the stigma of one flower with pollen taken from another. At any rate, in this case he has been content with the ingenuity of the apparatus for self-fertilisation through the instrumentality of an insect.

But there are one or two circumstances which seem to show that there is something further in this curious motion of the stamens of the Barberry.

In the first place, each filament only moves when touched at a particular spot near its base, where a very slight touch—*e.g.*, with a human hair—will make it start up. The other stamens remain unmoved, and a bee will frequently visit the flowers and carry off the abundant nectar without moving any stamen at all.

In the second place, after the stamen has moved inwards, which it does rapidly and with a sort of jerk, it soon begins again to move slowly outwards and backwards, and in a short time recovers its original position. Pollen still remains in the anther cells, and the stamen is ready again to jump up towards the stigma on the visit of another insect.

Now these two facts are not explained, if the sole object of the movement of the stamens is self-fertilisation. Why in that case should each stamen move separately when gently touched on its inner side, and why should it return to its original place, instead of remaining with its whole mass of pollen firmly pressed against its own stigma? Why, too, should it move inwards rapidly, and retreat slowly?

But if the object be to enable the insect to carry pollen to another flower, these facts, as well as the remarkable points of structure and function noticed by Sprengel and others, are all explained.

The separate stamen moves when touched on its inner side by the thin proboscis or limb of an insect, in such a way as to leave its pollen on that limb or proboscis, or on the body of the insect, which will then generally be interposed between the anther and the stigma. And the stamen returns to its place after the insect has departed laden with pollen to other flowers, in such a way as that, when more nectar has been deposited and another insect comes and touches it, it may again spring up and deposit some of its remaining pollen on that insect, to be again carried by it to fresh flowers.

If this be so, the function of the whole apparatus is not to cause self-fertilisation, but to enable insects to carry pollen from one flower to another. The case is curious, because at first sight the very remarkable movement of the stamens in this plant certainly looks like an ingenious device for *self-fertilisation*, and as such, an argument against the crossing theory.

Mr. Darwin, to whom I have mentioned this, tells me in confirmation, that the North American Barberries (*Mahonia*) have become so much crossed that it is almost impossible in this country to procure a true specimen of the two or three forms originally introduced. T. H. FARRER

The Corona

I DO not think Dr. Gould's lucid account of his own views could have been misunderstood. I, at least, who replied to him, understood him in the sense in which he has written to you.

The chief question at issue has been, whether there is or not *anything at the sun* (to use Dr. Gould's expression, as, on account of its very vagueness, most suitable), *outside the prominences*. Mr. Lockyer has said *no*; I and others have said *yes*; and Dr. Gould has helped to prove we were right. It has always seemed to me, however, that the photographs taken by Mr. De La Rue and Fr. Secchi, in 1860, had settled the question.

The question relating to matter outside this brilliant appendage, is of less moment. We know certainly that during totality there is *some* light in our atmosphere, and the question where this begins or ends is more interesting to the meteorologist than to the astronomer. But I would invite Dr. Gould's attention to the fact that in the March number of the Astronomical Society's *Monthly Notices*, I have given a simple mathematical proof of the fact that no atmospheric light *can* come in any considerable total eclipse from any region of the sky within seven or eight degrees of the eclipsed sun.

So far as this proof is concerned, I cannot admit that the matter is one of theory at all. But my views as to the *nature of the material* producing this coronal light are not founded on absolutely certain evidence, though the evidence in their favour is very strong indeed. Moreover, I should expect that precisely those appearances would be seen which Dr. Gould regards as tending to show that the faint coronal light arises from something *which is not at the sun*.

But really where a mathematical demonstration of a fact is extant, the consideration of arguments derived from admittedly doubtful evidence seems a mere waste of time. It may as readily be shown that the three angles of a triangle are not equal to two right angles, as that in the Indian or American eclipses atmospheric glare could have been visible within seven or eight degrees of the eclipsed sun.

In the supplementary number of the *Notices* of the Astronomical Society, I hope to give a further explanation of the extremely simple proof on which my views depend; and (because all questions involving considerations of tri-dimensional space are perplexing to most non-mathematicians) I am having a mechanical figure constructed to make the matter clearer, in illustration of papers I hope to read at the next meeting of the British Association, and next November before the Astronomical Society.

RICHARD A. PROCTOR

South Lambeth, June 24

Euclid as a Text-book

THE suggestion of Mr. Levett with regard to the formation of an Association for securing a general reform in the teaching of geometry, is worthy of being at once carried out. Such an association would show how general amongst teachers is the dissatisfaction with the ancient methods, and might lead to more uniformity of practice by securing a free public discussion of the best methods and most suitable terms. There are abundant materials available as the basis of discussion, and no doubt the aid of the most distinguished geometers would be easily secured by the Association, so as to bring about a decision on controvertible points. Several English text-books are already in existence, and there are many good features as well as many defects in all of them. Excellent series of lectures have lately been given on the subject in London and Cambridge, and some of the lecturers have printed very full notes for the use of their students. The syllabuses of Mr. Clifford, and Professor Hirst, are very suggestive.

After an exhaustive discussion, the Association would doubtless be able to secure the publication of a text-book which would have the approval and patronage of all its members. If the readers of NATURE who approve of the plan, would send their names to Mr. Levett, with contributions, if possible, towards expenses of printing, postage, and advertisement, some practical result would soon ensue.

Brixton RICHARD WORMELL

Storms and Fishes

CAN any of your readers give me any information on the following subjects?

1. What is Le Verrier's law of storms?

I asked this question some months ago, but no one replied to it.

2. Have any articles or special works on Poisonous Fishes appeared since Dumeril's memoir on the subject?

I should also be obliged to anyone who can give me information regarding fresh-water fishes that are in the habit of attacking bathers. Don Paez describes such fishes in some of the South American rivers. M.D.

The Scientific Education of Women

WILL you kindly allow me to add some information to an article which I have just seen in NATURE of June 16, on the Scientific Education of Women? First, however, allow me to correct an error which was made in an article on Lectures to Ladies, which I observed in NATURE some months ago, in which it was stated that the first series of educational lectures to women given under the auspices of any society for such lectures, was given under the direction of the Edinburgh Ladies' Educational Association. The fact is, that the first series of lectures of that kind, in recent years, was given in Liverpool, Manchester, Leeds, and Sheffield, under the auspices of the "North of England Council for the higher Education of Women." These lectures were given in the Autumn of 1867. The subject was Physical Astronomy. Under the auspices of that society, as well as of other societies, many sets of lectures have been given since that time on subjects connected with Physical Science. But I have always regretted, as the writer of your article regrets, that a greater number of the lectures have not been on such subjects, for I have always found that women exhibit a peculiar aptitude for the study of Physical Science. I have also found in my own experience a considerable desire on the part of women for such studies; and I believe that the fewness of such courses of lectures to them is to be put down to the scarcity of people at once competent and willing to teach them such subjects. From all that I have seen there is in my mind no doubt that the desire for true scientific instruction throughout the country, both among women and among men, exceeds the present possibility of supplying that teaching. I called your attention to this fact some months ago, and I believe that we cannot over-estimate its significance.

With respect to the remarks made in your article as to the medical education of women, it may interest your readers to know that Cambridge, which has moved so much in the matter of women's education, has not been behind-hand in this; but that a few weeks ago a petition went up to parliament praying that, in the ensuing legislation for the medical profession, provision might be made to prevent the exclusion of women from that profession. This petition was from resident graduates, and one hundred names were attached, among which were those of two heads of houses, nine University professors, and thirty-eight tutors or assistant-tutors of Colleges.

In your notice of the exhibitions in connection with the lectures to women at Cambridge, you speak of the Cambridge higher examinations for women. These examinations, which were instituted last year for women over eighteen years of age, were suggested to the University by a memorial presented by the North of England Council for the Higher Education of Women, a body to whose exertions the whole of this cause is deeply indebted. JAMES STUART

June 22

ILLUMINATION OF THE SEA

THE following is derived from the *Kölnische Zeitung* of June 19:—

"Gulf of Siam, April 11

"Last night, between two and three o'clock, I had the opportunity of witnessing an illumination of the sea of the most peculiar kind. It had become quite calm, after a sharp breeze which had sprung up from the N.N.W., caused by a passing storm in the distance. Heat-lightning was still very frequent in the west horizon, and the sky was covered with light clouds, through which the moon shone rather brightly. We took in sail and set the engines going. I then noticed in the water large white flakes which I had at first taken to be reflections of the moon; they were about a fathom in diameter, apparently lustreless, and of no particular shape, like objects seen lying deep in the water. By the rising and falling of the sea's surface these flakes floated off to a short distance from the ship without imparting any noticeable increase of brightness to the water illuminated by the moon's rays. After steaming further forward for six or seven knots, a most wonderful spectacle presented itself. On both sides obliquely in front of us, long white waves of light were seen floating towards the ship, increasing in brightness and rapidity till at last they almost disappeared, and nothing was observed but a white lustreless, whirling (*schwirrendes*) light upon the water. After gazing for some time it was impossible to distinguish between water, sky, and atmosphere, all which were but just now clearly distinguishable, and a thick fog in long streaks appeared to be driving upon the ship with furious swiftness. The phenomenon of light was somewhat similar to that which would be produced by the whirling round of a ball striped black and white so rapidly that the white stripes seem to be lost and blended with the dark ones. The light was just as if we were enveloped in a thick white fog. The direction of the waves of light upon the ship was always on both sides obliquely from the front. The phenomenon lasted about five minutes, and repeated itself once more afterwards for about two minutes. Without doubt, therefore, shoals of small creatures in the water were the cause of this luminosity, and the waves of light find their cause, according to my conviction, in the white flakes above described. Yet their moderate velocity of 1½ geog. mile per hour, and the weak light at first emitted by each flake, so weak as not to influence the tint of the surface-water, does not seem calculated to call forth a phenomenon of such magical effect as the one described. The luminous appearance commonly seen in the wake of a ship, or in water disturbed by oars or rudder, is not to be compared with such a phenomenon as the above. In the former the light is lustrous, glaring green and blue, like phosphorus, often very splendid in deep clear water, mingled with a reddish white foam. We saw a beautiful instance of this kind one night, in perfectly still and smooth water, in a lonely bay of Nipon. It was pitch dark and perfectly quiet, when a heavy shower of rain came on, in large but not dense drops. Every drop as it struck the water became illuminated, little drops of fire sprang up in the air, and a little luminous circle formed itself. It seemed as if the bay was suddenly filled with little flowers of fire. This phenomenon was almost immediately dissipated by a puff of wind."

FLIGHT.—FIGURE OF 8 WAVE THEORY OF WING MOVEMENTS

In the Proceedings of the Royal Institution of Great Britain for March 1867, Dr. J. Bell Pettigrew, F.R.S., the distinguished curator of the museum of the Royal College of Surgeons of Edinburgh, announced the startling discovery that all wings whatever—those of the insect, bat, or bird—were twisted upon themselves structurally, and that they twisted and untwisted during their action—that in short they formed *mobile helices* or *screws*. In June of the same year (1867), Dr. Pettigrew, following up his admirable researches, read an elaborate memoir "On the Mechanism of Flight" before the Linnean Society of London, wherein he conclusively proves, by a large number of dissections and experiments, in which he greatly excels, that not only is the wing a screw structurally and physiologically, but further that it is a reciprocating screw. He shows, in fact, that the wing, during its oscillations, describes a figure of 8 track similar in some respects to those described by an oar in sculling. This holds true of the vibrating wing of the insect, bat, and bird, when the bodies of these animals are artificially fixed.

When, however, the creatures are liberated, and flying at a high horizontal speed, the figure of 8, as he points out, is curiously enough converted into a wave track, from the wing being carried forward by the body, and from its consequently never being permitted to complete more than a single curve of the 8. This is an entirely new view of the structure and functions of the wing, and one fraught with the deepest possible interest to the aeronautical world. It promises to solve everything. Dr. Pettigrew's remarkable discovery has received an unlooked-for confirmation within the last few months at the hands of Professor Marey, of the College of France, Paris. This gentleman, whose skill in applying the graphic method to physiological inquiry is unequalled, has succeeded in causing the wing of the insect and bird to register their own movements, and has established, by an actual *experimentum crucis*, the absolute correctness of Dr. Pettigrew's views. Professor Marey's mode of registering displays much ingenuity, and is briefly as follows:—"A cylinder revolving at a given speed is enveloped by a sheet of thin paper smeared with lamp black, and to this the tip of the rapidly vibrating wing of the insect is applied in such a manner as to cause it to brush out its track on the blackened paper, which it readily does. A similar result is obtained in the bird by fixing a registering apparatus to the wing and causing the bird to fly in a chamber. In this case the registering apparatus is connected with the cylinder by means of delicate wires, and the registering is effected by means of electricity. In both cases the figure of 8 and wave movements, originally described and figured by Dr. Pettigrew, are faithfully reproduced. The way of a wing in the air has hitherto been regarded as a physiological puzzle of great magnitude; and well it might be, since some insects (the common fly for example) vibrate their wings at the almost inconceivable speed of 300 strokes per second, that is, 18,000 times in a minute!

It should be added that though Professor Marey endorses Dr. Pettigrew's view as to a figure of 8 movement, and has recently admitted his priority in that observation, he is yet by no means of the same opinion as Pettigrew as to the explanation of the mechanical effect of the movements and the influence of the bird's weight. Pettigrew maintains that the wings act as inclined planes in such a way that the bird actually rises by its own weight. Dr. Marey will not admit this at all, and is at issue with the Scotch anatomist on some other matters of moment, as he recently informed the writer. The beautiful and ingenious experiments which Dr. Marey is now carrying on will place these matters beyond conjecture by the light of experiment.

A FALL OF YELLOW RAIN

On the 14th of February a remarkable yellow rain fell at Gênes. The following details respecting it are given in a letter addressed to M. Ad. Quetelet by M. G. Boccardo, director of the Technical Institute of Gênes, who examined it in concert with Dr. Castellani, professor of chemistry. The quantitative analysis gave the following results:—

Water	6·490	per cent.
Nitrogenous organic substances	6·611	,,
Sand and clay	65·618	,,
Oxide of iron	14·692	,,
Carbonate of lime	8·589	,,

Observed narrowly under the microscope, the presence was revealed of a number of spherical or irregular ovoid substances of a cobalt blue colour; corpuscles similar to the spores of *Peziza or Peronospora*; spores of *Dematiaceæ* or *Spheriaceæ*; a fragment of a *Torulacea* (?); corpuscles of a pearly colour, concentrically zoned, probably small grains of fecula; gonidia of lichens; very scarce fragments of *Diatomaceæ*; spores of an olive brown colour; a few fragments of filaments of *Oscillaria*, *Ulothrix*, and *Melosira varians*; a fragment of *Synedra*; a peltate hair from an olive leaf. If, instead of collecting the earth on the morning of the 11th, when it had already been subjected to the action of rain falling for several hours, I had been able (writes M. Boccardo) to observe the phenomenon during the night, at the moment when it was produced, it is very probable that the microscope would have shown the existence of several kinds of Infusoria, as has been the case in several similar instances.

The author notes that the direction of the wind at Gênes during the night of the 13th and 14th was from the south-east, and without being exactly a hurricane as on the preceding few days, was still very strong. The temperature, previously exceptionally low, had risen, and probably did not fall during the night below $+4°$ R. (5° C. or 41° F.) The journals state that on that date a tempest devastated the coasts of Sicily. M. Boccardo, following P. Denza, proposes the theory that the dust came from the coast of Africa. "We ought not to forget," he writes, "that according to Maury's theory of the circulation of the atmosphere, these clouds of dust may well have travelled a long distance before touching the soil of Italy, coming from beyond the Atlantic, like those which in 1846 spread from Guiana to the Azores, over the south of France and the whole of Italy."

RELICS OF NON-HISTORIC TIMES IN JERSEY

CONSERVATION *v.* DESTRUCTION

On the 18th of May information was received from Jersey of the partial demolition of some tumuli, hitherto undescribed in that island; and, accordingly, two gentlemen, interested in the conservation of all ancient monuments, resolved to make a tour of inspection of the pre-historic remains in Jersey without delay, and the following is the result of the inspection:—The time was necessarily brief, occupying only two days, the party arriving at Jersey at 11 A.M. on the 19th, and leaving the island at 6.45 A.M. on the 21st. A summary of the route taken may be useful to tourists and others who may wish to visit all the pre-historic stone monuments in Jersey, as far as they are known at present. Leaving St. Helier's by the St. Aubin's road, the first attraction is the Ville Nouaux Cromlech, not far from the first martello tower. This structure was examined last year by leave of M. de Quetteville, the proprietor, and described at the time.

As now exposed to view, this cromlech appears to be an elongated *allée couverte*, nearly due east and west, measuring 35 feet in length. Its sides, about four feet apart, are as nearly as possible parallel, although there are indications of the avenue being narrowed towards its eastern extremity, as we should expect to find. The side blocks of stone average from 4 to 5 feet in height, and number eleven on the northern and seven on the southern side, the western end being closed by a fine single slab. The interstices between these blocks are roughly filled up with irregularly shaped smaller stones, evidently built in to prevent the exterior earth and soil of the superimposed tumulus from falling into the sepulchral grotto.

There must have been formerly at least nine cap-stones; of these, two have been removed, as observed above, whilst the whole fabric appears to have been tilted, with an inclination to the south, probably caused either by the unequal pressure of the accumulated sand-drift on the northern side, or by the removal of the ballast from its southern supports. It is difficult to determine whether all the cap-stones are in their original positions, or whether some of them have not slipped between the side blocks from their summits.

Several urns, tulip shaped, &c., with a cylindrical stone muller, were brought back to Guernsey from this cromlech, and are now deposited in Mr. Lukis's museum.

Not far to the north of this spot is a semicircle of stones, which presents a suspicious appearance.

It is much to be wished that the tenant of this field would prevent the causes of the filthy state in which this cromlech is at present. The stones themselves have not been disturbed since the last exploration.

Entering St. Laurence's parish, to the right of the road, on the hill above the vineries, is La Blanche Pierre, a menhir which is fortunately still preserved. The route is next taken to the north-west, through St. Peter's parish to St. Ouen's, and the small hamlet of Trodais. Here the party visited M. Lefeuvre, who in the course of agricultural operations, has removed a large portion of tumuli on his property, and who, six years ago, found within one of them, and has in his possession, a remarkable cinerary urn with four handles, evidently for suspension. The upper portion of this urn, which is hand-made, not turned, is likewise decorated with an ornamental border, consisting of horizontal lines, so arranged as to form three triangles between each handle. It is of a different and later type than the urns discovered in the cromlechs of these islands. M. Lefeuvre accompanied his visitors to the sites of the tumuli. These curious mounds are in two groups, one group being called *Les Hougues de Millais*, on one spur of the hills overlooking L'Etac and St. Ouen's bay, to N.W. of *La Robeline*, and the other on a similar spur to the S.E. of the former, about 800 yards distant, in a line with St. Ouen's windmill; these last are called *Les Monts de Grantez*. A portion only of one of the *Hougues* remains, and exhibits a series of cap-stones, five in number, of which four remain, supported by a dry-walling of smaller slabs, forming a tunnel about 18 feet long, which lies east and west, and was blocked at either end with a broad stone, of which the west one alone is *in situ*. It presents an exact parallel to the *Creux des Faias*, which existed till lately in St. Peter's parish, Guernsey, a few hundred yards west of the menhir at Les Paysans; but which has been swept entirely away. A granite muller was picked up here by the visitors, which also resembled, in a remarkable manner, a similar one picked up but a day or two before by the same gentleman, on the site of the *Creux des Faias*, in Guernsey, showing an identity of manufacture and a contemporaneity of construction of the tumuli in both islands.

Leaving Les Hougues, and after visiting Les Monts de Grantez, St. Ouen's church, which is being magnificently restored, was examined, and a worked stone of the Neolithic period picked up in the churchyard. Pursuing the circuit of ancient remains, the route descends towards the sea by St. Ouen's pond, where, in the Val de la Mare, St. Peter's parish, are *Les Trois Roches*, in all probability a portion of a cyclolith. Two only are upright, the third lying prone at a little distance and not visible at first sight, until one approaches close to it. The ground being marshy it has formed a pit for itself. The upright stones have been apparently worked into shape on their summits, whilst their sides are almost polished from cattle rubbing against them. The new road is now traversed through the western portion of St. Brelade's parish, between the dreary *dunes* of *Les Quenvais*, and ascending by *La Pulente* on to the hills of La Moye; a kitchen-midden is to be found at the summit of the ascent, where the soil has been scarped in the formation of the road, a mass of limpet, ormer, mussel, and other shells, at some feet in depth below the surface of the original soil.

The famous menhir of Le Quesnel, which stood so picturesquely to the south of this spot this time last year, has fallen a victim, and in its place a large quarry yawns; but in Le Marais, close by, is a portion of a circle, and an alignement, in connection with the former menhir, is still to be traced. Close to Moye signal staff a natural cropping up of the rock presents a striking resemblance to a cromlech and circle round it. About half a mile from Le Quesnel, and directly above La Corbière Point, is a fine single stone Dolmen called La Table des Marthes, beneath which were found some bronzed implements many years since.

Over the granite quarries of La Fosse Vaurin, is a curious natural work which, aided by the hand of man, presents the appearance of two basins with a channel for emptying one, whilst the fissures to the east resemble a cross, the work, perhaps, of some hermit in mediæval times. Several mullers, worked stones, &c., were found in the locality during this brief visit, and brought back to Mr. Lukis's collection. A seven miles' drive brings one back to St. Helier's. This day's visitation occupied from 11 A.M., until 9 P.M., but much time was spent in sketching and measuring, searching for stone implements, &c. The find for the day was tolerably good, viz.:—Ville Nouaux Cromlech, 1; Les Hougues de Millais, 1; St. Ouen's churchyard, 1; Les Trois Roches, 3; La Moye, 5 stone implements. S. P. OLIVER, Lieut. R.A.

40, Hauteville, Guernsey

SOUNDINGS AND DREDGINGS BY THE UNITED STATES COAST SURVEY

IN the office of the Coast Survey at Washington there are about 9,000 specimens of various kinds of marine animals which were brought up by the sounding lead from the sea-bottom, in the region between the shore of Florida and adjacent States and the outer edge of the Gulf Stream, and descending to a depth of 1,500 fathoms nearly. The dredge has been but comparatively little used along the coast of the United States, and that so many specimens were collected by the lead alone is due to the persevering care of the late superintendent of the survey, Prof. A. D. Bache, and to the instructions which he gave to the hydrographical officers. Of course, specimens brought up by the lead can include the smaller animals only, such as Foraminiferæ, Diatomaceæ, and such like; for the larger animals, the dredge must be employed.

The work thus begun has been resumed by the present superintendent of the Coast Survey, Prof. B. Peirce. The surveying parties are instructed to take observations of the depth, velocity, and direction of the Gulf Stream, the temperature and density of the water at different depths, and of the Fauna from the surface down to the bottom. By these researches we may hope that our knowledge of the phenomena of the Gulf Stream will be increased, particularly as regards its powers of transportation from shallow to deeper water, or along its bed, besides its action in forming deposits in particular localities, and its possible influence on the growth of coral reefs on its shores.

The first operations under the new direction were carried on between Key West and Havana, along the route now occupied by a telegraph cable. Dredgings were made at depths varying from 90. to 300 fathoms, and yielded Crustaceans, Annelids, Mollusks, Radiata, Foraminiferæ, Sponges, a single vegetable specimen, being a minute alga, *Centroceras clavulatum*, and "a number of nodules of a very porous limestone, similar in colour and texture to the limestone forming the range of low hills along the shore of Cuba, but composed apparently of the remains of the same animals which were found living." Among these Deltocyathus, Caryophyllia, and Pteropods were recognised in the stone, and found in various stages of fossilisation.

At the end of a descriptive list of the specimens collected during the cruise, M. de Pourtales remarks:—"It would be premature to compare this deep-sea Fauna with the animals inhabiting the regions of lesser depth on the coast of Cuba or Florida. In the first place, many of the smaller forms—such, for instance, as the Bryozoa or the Hydroid polyps of those shores—are not yet sufficiently known to enable us to say if any of the species dredged exist in any other than the abyssal region. Then, a very different value must be assigned to the different classes of animals under examination. Thus, the dead shells must be left out of the question, at least the smaller ones, for they may have dropped with the excrement of fishes, or, in the case of Pteropods, have sunk from the surface after the death of the animal. The Crustaceans and Annelids being abundant and generally sedentary, will, when better known, afford good characteristics of the regions of unequal depth. The same remark applies to the sponges and the Foraminiferæ; the great abundance of the latter and the ease with which they can be brought up by the sounding lead render them particularly useful."

From this it will be understood that the United States Coast Survey is in good hands, and may be expected, when the time comes, to take part in the suggested dredging expedition all across the Atlantic, when England and the States, after accomplishing each a half, are to meet and shake hands in mid-ocean.

NOTES

PROFESSOR HENRY, the President of the American Academy of Sciences, and Director of the Smithsonian Institution, is now in this country *en route* to the Continent to attend the meetings of the International Commission on Standards.

WHEN presiding over the distribution of prizes for the Faculty of Arts and Laws at University College, London, on Friday last, the Bishop of Exeter made some admirable remarks on the nature of a true system of education, and of the places which ought to be occupied by classics, mathematics, and natural science, and the proper method of teaching them. In all true teaching a scientific method is indispensable; it is because this scientific method has been applied to instruction in Greek and Latin, that such good results have been obtained in this department of education. The introduction of scientific teaching has not hitherto met with the same success because it has not been carried out in the same spirit. In very many instances, those who are endeavouring to promote the study of natural science as a part of education, have made the great mistake of omitting altogether that which is essential to true study, namely, scientific method. The reason why the teaching of the natural sciences still hangs back in our public schools is, in great measure, the unscientific method in which science has been taught by many. To form a part of real education, the study of science must be pursued in the same rigorous manner as that of classics or mathematics; it will then prove as hard work to the learner, and the result of its introduction must be most beneficial. While the exclusive study of mathematics must fail as a complete discipline for the understanding, and the great mathematician may be uncultivated as a man, it is very rarely that you see such a result in the student of external nature; therefore, this study must rank by the side of the other, and must hold a place in no way inferior to it. The practical importance given to these remarks by the experience of Dr. Temple at Rugby, ought to make them carry great weight with all teachers of science.

DR. HENRICI has been elected by the Council of University College, London, Professor of Mathematics, in the place of Dr. Hirst, who resigned the professorship, on his appointment to the Assistant-Registrarship of the University of London. Dr. Henrici had acted as Prof. Hirst's assistant during the whole of the session just ended. He had pursued his mathematical studies at Carlsruhe under Professor Clebsch, and subsequently at Heidelberg, where he attended Prof. Hesse's lectures on mathematics, and those by Prof. Kirchhoff on theoretical physics. While at Heidelberg he took his degree of Doctor of Philosophy in the highest grade, and the Philosophical Faculty of the University considered the dissertation which he wrote on that occasion to have so high a scientific value, that they recommended the government of Baden to recognise its importance by conferring upon Dr. Henrici a special public distinction. Dr. Henrici subsequently prosecuted his studies at Berlin and Kiel, and then came to England, where he has resided nearly five years.

THE completion of the deep-sea cable between Falmouth and Bombay was celebrated last Thursday evening by an entertainment given by Mr. Pender, chairman of the British-Indian Submarine Telegraph Company, at which royalty largely assisted. Complimentary messages were exchanged between the Viceroy of India and the President of the United States, the distance of 8,442 miles being accomplished in forty minutes; between the Prince of Wales and the Khédive, the Prince of Wales and the King of Portugal, the Prince of Wales and the President of the United States, and the Prince of Wales and the Viceroy of India. This is the first instance of direct telegraphic communication between India and America. The comic side of telegraphic communication was presented by the message between the Prince of Wales and the Viceroy, which, though despatched soon after twelve at night, and only nine minutes on its way, reached Lord Mayo at five in the morning, when his lordship was, naturally enough, fast asleep. What will be the result when the earth is completely girded with a telegraphic cable, and a message is sent to the antipodes? The question between night and day will be expanded to one between to-day and to-morrow, to say nothing of yesterday.

THE Royal Commission on Scientific Instruction and the Adancement of Science has held two meetings since our last issue.

THERE will be an election at Magdalen College, Oxford, in October next, to six Demyships and one Exhibition. Of the Demyships, one will be mathematical, one in natural science, four classical. The Exhibition will be in natural science. It is necessary that candidates for the exhibition should prove to the satisfaction of the electors that they cannot be supported at college without such assistance. Evidence on this point will be considered as confidential. No person will be eligible for the Demyships who shall have attained the age of twenty years, and (in the case of candidates in mathematics and natural science) who is not sufficiently instructed in other subjects to matriculate as a member of the college. The stipend of the above Demyships and Exhibition is 75*l.* per annum, inclusive of all allowances; but there are tenable with the Demyships certain College Exhibitions, which raise their annual value, on an average, to about 83*l*. They are tenable for five years. Testimonials of good conduct will be required, and a certificate of birth and baptism, which must be presented to the President on Monday, the third day of October, between the hours of three and six, or eight and nine P.M. The examination will commence on the following day. Particulars relating to the examinations in the various subjects may be obtained by applying to the senior tutor. No entrance fees or caution money are required by the college. The University fees payable on matriculation amount to 2*l*. 10*s*.

IT is refreshing to learn that that reverend and tory institution, St. John's College, Oxford, has just elected to a fellowship a devoted physical investigator, in the person of Mr. Bosanquet. This election is important, not only as a recognition of natural knowledge, but also of the principle of research as against that of mere education.

CONSEQUENT on the death of the Rev. Dr. Luby, one of the Senior Fellows of Trinity College, Dublin, the Rev. Mr. Jellett, B.D., President of the Royal Irish Academy, has been co-opted a Senior Fellow. It is understood that the Professorship of Natural Philosophy held by Mr. Jellett will be given to the Rev. R. Townsend, A.M., author of the "Modern Geometry of the Point, Line, and Circle."

THE Minister of Public Instruction in Italy has promised a grant of 1,600 *lire* towards the expenses of instituting a laboratory of cryptogamic botany in Pavia; and it is hoped that a contribution will also be received from the Minister of Agriculture.

THE death of Sir James Simpson has been followed by another heavy loss to medical science in that of Professor Syme, who died on Sunday last, at the age of seventy. Mr. Syme was for a short time Professor of Surgery in University College, London; for a much longer period he occupied the chair of Clinical Surgery in the University of Edinburgh, which he resigned only quite recently to his son-in-law, Mr. Joseph Lister. He was a voluminous writer on surgical subjects, many of his works being held in very high reputation.

THE *Field* suggests that the drought of the summers of 1868 and 1870 is connected with the rapid increase of drainage in this country, the average summer rainfall having been greatly reduced from 1860 to 1870. Our contemporary also expresses an opinion, in which we cordially concur, that a needless alarm has been raised as to the prospects of the corn harvest, as shown in the rise of 10*s*. to 12*s*. a quarter in wheat at Mark Lane. The harvest of 1868 is the finest on record; and we hope to see the prediction of our contemporary fulfilled, that "the cereal crops in this country will, on the whole, turn out favourably."

NOW that so many, both Londoners and their country friends, are flocking to the national botanic garden at Kew, we may call attention to Prof. Oliver's "Guide to the Royal Botanic Gardens and Pleasure Grounds, Kew," which has now reached its twenty-fifth edition. It is a *multum in parvo* of value and interest far beyond the purpose for which it is designed; indeed we do not know where else, in so small a compass and at so low a price, to meet with so much and varied information respecting the vegetable products of different countries, their economic purposes, and their geographical distribution, illustrated with exceedingly well-drawn woodcuts.

IN a paper in the *Bulletins de la Société Vaudoise*, No. 62, Dr. C. Nicati gives a *résumé* of various researches respecting the peculiar red snow which occasionally falls in the Grisons. Some of this snow fell, mingled with common snow and rain, during a violent storm from the south-west on the morning of January 15th, 1867, in various places. The chemical analysis of the melted snow demonstrated the presence of minute quantities of sulphate of lime or gypsum, sulphate of magnesia, organic matters, chlorine, and iron; and microscopic examination detected vegetable fibre, pollen, spores, with here and there diatoms and small crystals. The colour varies from brick red to a pale yellow. This snow is quite distinct from the red snow of the upper Alpine regions, which owes its colour to the presence of the minute plant, *Protococcus nivalis*. After discussing various theories respecting its origin, Dr. Killias expressed his opinion that it is the dust of the desert of Sahara, transported by a sirocco, which gives the colour to the snow of the Grisons. Dr. Nicati gives many interesting particulars, with analyses, of the Algerian sirocco dust, and of the mud-rain in Naples and Sicily; and Professor C. Cramer states that he has discovered, both in the sand of the Sahara and in the red snow of the Grisons, particles of vegetable organisms (especially polythalamia) and minute fragments of animal origin, such as wool, hair, &c. He considers the presence of gypsum in the red snow an incontestable proof of its containing matter conveyed from the desert of Sahara

PROFESSOR GERARDIN has recently communicated to the Council of the "*Société d'encouragement pour l'industrie nationale*" the results of his efforts to purify the waters of the Croult, a small river which, rising at Louvres, passes through Gonesse, Arnouville, and flows through St. Denis, ultimately falling into the Seine. The stream was poisoned by the drainage from the starch manufactories. The principle on which M. Gerardin proceeded was that water in which weeds and shell-fish cannot live was infected and poisoned. The potatoes from which the starch was made contain 75 per cent. of juice, which itself contains 7 per cent of albumen. The water issuing from the factory is clear, reddish, and inodorous; in motion, it forms coherent masses of coagulated albumen. The river deposited in its course masses of whitish, pitch-like substance, without consistence. The surface was covered with white froth; the mud black and stinking, while the water had a strong odour of sulphuretted hydrogen. M. Gerardin recognised the white masses deposited by the waters as the "baregine" characteristic of the sulphurous waters of the Pyrenees. When the works stop, this "baregine" putrefies, and infusoria are developed in abundance. M. Gerardin thought the best method of remedying the unwholesome state of the water would be to destroy the albumen by the simultaneous action of the air, clay, and the organic fermenting agents always contained in cultivated ground, and he determined to make the waters pass over a soil well drained. The factory of M. Boisseau, at Gonesse, consumed in a day 400 hectolitres (sacks) of potatoes, weighing 28,000 kilog. (27½ tons), and containing 21,000 kilog. of juice, carried off by 130,000 litres of water (say 28,500 gallons); these waters are spread over a field whose area is 500 yards square, in which are placed drains at 6 feet distance from each other, and 2 feet deep. The arrangement has perfectly succeeded. Weeds grow in the Croult from Louvres to St. Denis: *Limnæus* and *Planorbis* find their abode in these weeds, the "baregine" has disappeared, the sulphuretted hydrogen odour is entirely gone, and the water is sweet and limpid.

A GOOD tabular arrangement of the Natural Orders of plants is very much wanted by botanical teachers, where the alient diagnostic characters of allied orders are presented to the eye at a glance. That this desideratum is entirely supplied by Dr. Griffith's "System of botanical analysis applied to the diagnosis of British natural orders," we cannot altogether affirm, as each teacher will probably find it vary in some respect or other from his own ideas of the best mode of classification. We can, however, safely recommend it as a useful help to beginners in getting over the difficulties of systematic botany.

FACTS AND REASONINGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS*

IN all ages it has been believed by many that Living things of various kinds could come into being *de novo*, and without ordinary parentage. Much difference of opinion has, however, always prevailed as to the kinds of organisms which might so arise. And, although received as an article of faith by many biologists—perhaps by most—in the earlier ages, this doctrine or belief has, in more recent times, been rejected by a very large section of them. Definitely to prove or disprove the doctrine in some of its aspects is a matter of the utmost difficulty, and there are reasons enough to account for the wave of scepticism on this subject, which has been so powerful in its influence during the last century. The notions of the ancients were altogether crude, and founded upon insufficient proofs. It was not in their power to settle such a question; and when the inadequacy of the evidence on which they had relied became known, then much doubt was thrown also on the truth of the conclusion at which they had arrived. All this was natural enough. When, therefore, about a century ago, the rude microscopes of the time began to reveal a multitude of minute organisms whose existence had been hitherto unsuspected; when more facts became known concerning the various modes of reproduction amongst living things; and, above all, when the philosophical creeds of the day were supposed to be irreconcilable with such a doctrine, then a growing scepticism in the minds of many gradually developed into an utter disbelief in the possibility of the occurrence of what was called "spontaneous generation."

This was the state of things anterior to and during the time of the celebrated controversy between the Abbé Spallanzani and John Needham. Then it was that the former of these two champions, with the view of accounting for phenomena which would otherwise have necessitated his admission of the doctrine which he rejected, recklessly launched upon the world the *hypothesis* that multitudinous, minute, and almost metaphysical "germs" existed everywhere—ready to burst into active Life and development whenever they came under the influence of suitable conditions. Armed with this all-powerful *Panspermic* hypothesis, Spallanzani argued against the conclusions of Needham. His views on this subject were supported by the still more extravagant theories of Bonnet. The doctrine of "*L'Emboîtement des germes*" was the production of an unbridled fancy, and might, perhaps, never have been elaborated, had not the Leibnitzian doctrine concerning "Monads," as centres of force and activity, been already in existence, and at the time all-powerful in the philosophical world.

The controversy which was initiated by these two pioneers in microscopical research they were unable to terminate—the enigma which they sought to solve has, since their time, still pressed for solution, and still the tendency has been to solve it after one or other of the modes by which they attempted to account for the occurrence of the phenomena in question. It is and has been contended, on the one hand, that Living things can originate *de novo*, and without ordinary parentage; it is contended, on the other, that this is impossible—that every Living thing is the product or off-cast of a pre-existing Living thing, and that those which appear to arise *de novo* have, in reality, been produced by the development of some of the myriads of visible or invisible "germs" which pervade the atmosphere.

Now it is obvious, that of these two opposing doctrines, the one must be true and the other false: either Living beings can originate *de novo*, or they cannot. So long as any doubt remains upon this subject, we have to confess our ignorance concerning one of the very first principles of Biology. In the whole domain of Science, moreover, it is scarcely possible to propose a question which is more replete with interest than that which asks whether Living things can be evolved *de novo*. If settled in the affirmative, what light will be thrown upon the past and present history of our globe! How must our notions concerning Life, health, and disease be influenced in one way or the other by its solution!

Without entering into the history of the long controversy which has taken place upon this subject—details of which may be found in the works of Pouchet* and Pennetier,† and in the writings of Pasteur‡—I shall, before describing my own experiments and their results, merely relate as briefly as possible what conclusions have been come to concerning the degree of heat to which inferior organisms may be subjected with impunity, and what temperature, on the other hand, has been invariably found to be fatal to them. Fortunately there is at present much unanimity of opinion on this subject. As a result of numerous investigations which have been communicated to the French Academy, and to the Société de Biologie during the last ten years, we find that both the advocates and the opponents of heterogeny are, within certain limits, pretty well agreed on this most important aspect of the question. The many disbelievers and opponents of heterogeny who took part in these investigations, naturally desired that the power possessed by inferior organisms, both animal and vegetable, of withstanding the destructive influence of high temperatures, should be shown to be as high as possible. We may, therefore, with much safety, assume that the limits of vital resistance could not then be shown to be higher than that which these experimenters were compelled, after frequently repeated investigations, to ascribe to such inferior organisms.

In *dry air* or in a vacuum, organisms are capable of withstanding a notably higher temperature than when they are immersed in fluid. According to the direct observations of M. Pasteur, the spores of certain fungi belonging to the family *Mucedineæ*, seem to possess this tenacity of life to a very great extent; but even these, he says, though they still remain capable of germinating after having been raised, for a few minutes in dry air or *in vacuo*, to a temperature of from 120° to 125° C., lose this power absolutely and entirely after an exposure for half an hour, under similar conditions, to a temperature varying from 127° to 130°C. And the labours of the commission (consisting of the following members—MM. Balbiani, Berthelot, Broca, Brown-Séquard, Dareste, Guillemin, and Ch. Robin) appointed in 1869 by the Société de Biologie, to inquire into the subject, led them to the conclusion that the lower animals which were the most tenacious of life§—the rotifers, the "sloths," and the anguillules of tufts of moss or lichen—succumbed at even a much lower temperature than this. In dry air or *in vacuo*, therefore, we may look upon the temperature of 130°C. for thirty minutes, as marking the extreme limit of vital endurance under such conditions, so far at it has been hitherto possible to fix such a limit. There is, at present, no evidence forthcoming to upset this conclusion. When immersed in *fluids*, however, the power possessed by the inferior organisms of resisting the destructive influence of heat is not nearly so great. Comparatively few, whether animal or vegetable, have been found capable of resisting a temperature of 75°C.; and with regard to that of 100°C., it has been admitted, by MM. Claude Bernard and Milne-Edwards, by M. Pasteur, and by all the other most influential opponents of the doctrines of heterogeny, that such a temperature, even for one minute, has always proved destructive to all the lower organisms met with in infusions ‖—so far as these had been made the subjects of special and direct experimentation. And, amongst all the diversity of form presented by the lowest Living things, there is so much of uniformity in property—living matter, as we know it, agrees in so many of its fundamental characters—that biologists and chemists alike may feel a reasonable assurance as to the probable universality of any such rule which has been proved to hold good for a very large number of organisms, more especially when, amongst this large number of cases, no exceptions have been encountered.

Practically, however, it will be found that, in order to appreciate the bearings of the experiments which I shall have to relate, it will be necessary for us more especially to know what are the limits of vital resistance to high temperatures, possessed by *spores* of Fungi on the one hand, and by *bacteria* and *vibrios* on the other.

I am not aware of any experiments tending to show that *spores* of Fungi can survive after exposure for even a few seconds in fluids raised to a boiling temperature (100° C.); whilst, on the

* This paper was originally intended for presentation to the Royal Society, but it was finally not presented, when I understood that, owing to the accumulation of many papers and other causes, no evening could be allotted on which it might be read and discussed. Its appearance *in extenso*, and at once, was thought preferable to the reading of its mere title before the Royal Society, with the probability of a very considerable delay in its publication. —H. C. B.

* Hétérogenie, Paris, 1859. † L'Origine de la Vie, Paris, 1868.
‡ Annales de Chimie et de Physique, 1862.
§ This extreme tenacity of life is perhaps due in part to the chitinous integument with which such animals are provided.
‖ It is quite fair to make this limitation, since we are only concerned with the origin of such organisms. Seeds of higher plants, provided with a hard coat, may—especially after prolonged periods of desiccation—germinate even after they have been boiled for a long time in water.

other hand, there is the concurrent testimony of many observers to the fact that, after such exposure, germination would never take place, because the spores were no longer living. This was the result obtained in many experiments made by Bulliard, and related in his "Histoire des Champignons." Mere contact with boiling water was found sufficient to prevent germination; and H. Hoffmann* similarly ascertained that an exposure for from four to ten seconds to the influence of boiling water sufficed to prevent the germination of all the fungoid spores with which he experimented. The experience of other observers has been similar to that above quoted, and amongst these we may cite M. Pasteur himself. Speaking of his experiments with boiled milk in Schwann's apparatus, M Pasteur says:—"Je n'ai jamais vu se former, dans le lait ainsi traité autre chose que des Vibrions et des Bacteriums, *aucune Mucédinée, aucune Torulacée, aucun ferment végétal.* Il n'y a pas de doute que cela tient à ce que les germes de ces dernières productions ne peuvent résister à 100° au sein de l'eau, ce que j'ai d'ailleurs constaté par des expériences directes."†

The evidence which we at present possess concerning the tenacity of life displayed by *bacteria* and *vibrios* in fluids whose temperature has been raised, is just as decisive as that concerning the spores of fungi. M. Pouchet's observations have lead him to believe that vibrios, in common with all the kinds of ciliated Infusoria, are killed by raising the temperature of the fluid which contains them to 55° C. M. Victor Meunier, also, never found any of these organisms alive after they had been similarly subjected to a temperature of 60° C. I have myself invariably found that vibrios were not only killed, but were broken up and more or less disintegrated, after the fluid had been boiled for even one minute. There is every reason also to believe that an exposure to similar conditions kills their less developed representatives—the primordial monads and bacteria. With reference to these organisms, however, one caution is necessary to be borne in mind by the experimenter. The movements of monads and bacteria may be and frequently are of two kinds. The one variety does not differ in the least from the mere molecular or Brownian movement, which may be witnessed in similarly minute non-living particles immersed in fluids; whilst the other seems to be purely vital—dependent, that is, upon their properties as living things. These vital movements are altogether different from the mere dancing oscillations which non-living particles display, as may be seen when the monad or bacterium darts about over comparatively large areas, so as frequently to disappear from the field of the microscope. After an infusion has been exposed for a second or two to the boiling temperature, these vital movements no longer occur, though almost all the monads and bacteria may be seen to display the Brownian movement in a well-marked degree. They seem to be reduced by the shortest exposure to a temperature of 100°C., to the condition of mere non-living particles, and then they become subjected to the unimpaired influence of the physical conditions which occasion these molecular movements.

Such is the evidence existing as to the power of resisting the destructive influence of heat, manifested by the organisms about which we are at present most interested. It is certainly harmonious enough with our ordinary experience, and is, therefore, not difficult for us to believe. Eggs of higher animals containing an embryo may fairly enough be compared with the lower organisms of which we have been speaking, so far as the matter of which they are composed is concerned; and knowing the profoundly modifying influence of water at a temperature of 100° C. upon the comparatively undifferentiated matter of the embryo and of the egg—and also, we may add, even upon the differentiated tissues of the parent fowl—need we wonder much that the same temperature should have been found hitherto to be destructive to the simple and naked living matter entering into the composition of fungus-spores, and of bacteria and vibrios? If any other result had been ascertained, would there not have been much more reason for surprise?

We must therefore be very cautious how we attempt to set aside the conclusions which have been arrived at on this subject, based as they have been upon direct evidence of a most positive character, on account of other evidence which is indirect and more or less ambiguous. Concerning the legitimacy of such an attempt which has been made by M. Pasteur, I shall have more to say hereafter.

Passing on, then, to the more immediate consideration of our subject, it should be distinctly understood that in all the discussions which have hitherto taken place on the possibility of the evolution of Living things, pre-existing *organic matter* has always been supposed to furnish the materials entering into the composition of the new organisms. New combinations and re-arrangements have been supposed to take place amongst the molecules of this pre-existing organic matter, under the agency of some mysterious force or forces—which new combinations of previously uncombined or differently combined molecules have been supposed to result in the production of such primordial living specks as monads and bacteria. The observations of preceding inquirers have also been conducted for the most part on infusions containing organic matter *in solution*; and since the molecules of such matter are then invisible, observers have, of course, been quite unable to follow, by any magnifying power at present attainable, variations in the modes of collocation of such invisible molecules. The minutest specks of living matter—the germs of monads and bacteria, and of the spores of fungi, less than $\frac{1}{50000}''$ in diameter—may be seen gradually appearing under the microscope in previously homogeneous solutions containing none of them.* But although microscopical investigation enables us to adduce evidence of just the same kind in elucidation of the mode of origin of certain low organisms, as we possess in explanation of the mode of origin of crystals,† this evidence is not deemed adequate in the case of organisms. A living thing has been supposed to be a something altogether different, incapable of arising out of a mere collocation of matter and of motion; and, therefore, under the influence of this theoretical assumption, whilst chemists and physicists have thought that they could in a measure account for the genesis of crystals by reference to the affinities and atomic polarities of the ultimate constituents of such crystals, they have, for the most part, declined to adopt a similar mode of reasoning in order to account for the appearance of the minutest living specks in solutions containing organic matter. The same reservation is likewise made by the major part of the biologists of the present day. Whilst it is not an article of faith—whilst such a surmise scarcely crosses our minds—that crystals always proceed from pre-existing germs, in the case of Living things, on the contrary, the doctrine *omne vivum ex vivo* has become almost one of the "forms of thought." Principally owing, therefore, to certain theoretical views concerning Life, and in order to account for facts which would otherwise be adverse to these, biologists and others have been accustomed to make the most extensive postulations concerning the supposed universal distribution of "germs" of all the lower kinds of living things; whilst they have recourse to no parallel hypothesis to account for the appearance of crystals, although we know no more—can drive our knowledge back no further into the phenomena attendant upon the birth of crystals than we can into the phenomena which usher in the appearance of organisms. In each case, under suitable conditions, they appear at first as minutest visible specks, in solutions which were previously homogeneous. In the one

* Etudes mycologiques sur les fermentations.

† Annales de Chimie et de Physique, 1862, p. 60.

* A more complete account of this part of the subject will be given in a work on *The Beginnings of Life*, shortly to be published.

† Or, better still, concerning the mode of origin of those modified crystals which appear on mixing solutions of gum and carbonate of potash, as described by Mr. Rainey (*On the Mode of Formation of the Shells of Animals*, &c., 1858). The malate of lime contained in the gum is decomposed, but owing to the slow mixing of the solutions in the presence of gum, the insoluble carbonate of lime does not appear in its usual crystalline condition, but in globular modifications, resembling calculi. When portions of the two solutions are mixed under the microscope, Mr. Rainey thus describes what takes place:—"The appearance which is first visible is a faint nebulosity at the line of union of the two solutions, showing that the particles of carbonate of lime, when they first come into existence, are too minute to admit of being distinguished individually by the highest powers of the microscope. In a few hours exquisitely minute spherules, too small to allow of accurate measurement, can be seen in the nebulous part, a portion of which has disappeared, and is replaced by these spherical particles. Examined at a later period, dumb-bell-like bodies will have made their appearance, and with them elliptical particles of different degrees of excentricity" (p. 9). Mr. Rainey made use of one of Ross's $\frac{1}{8}''$ object glasses. These modified crystals are produced with no more rapidity than the lowest living things seem to be in other solutions, during hot weather; and the shapes of the products in the two cases are remarkably similar, judging from Mr. Rainey's figures. The protraction of the process, brought about by the presence of gum, serves to bring out more clearly the real relationship existing between the formation of crystals and that of the lowest organisms, in homogeneous solutions.

That there are very strong reasons accounting for this belief I do not attempt to deny. There is, however, much evidence to show that the very same organisms which do propagate their kind after this acknowledged method, may themselves originate *de novo*. Whilst allowing, therefore, the widest generality to any given rule, we may well hesitate before, on this account, we reject certain other alleged facts which are complementary rather than contradictory.

case we have to do with *crystallisable* matter, in solution, and in the other with those big-atomed, unstable compounds which constitute the so-called *colloidal* states of matter. And it is well to call attention to the fact that, concerning these latter states, the late Professor Graham, one of the most cautious and philosophical of chemists, wrote:*—"Another and eminently characteristic quality of Colloids is their mutability. Their existence is a continual metastasis. . . The Colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition. The colloid possesses ENERGIA. *It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.* To the gradual manner in which colloidal changes take place (for they always demand time as an element) may the characteristic protraction of chemico-organic changes also be referred."

Granting, then, that microscopical evidence alone may not be quite satisfactory for settling the mode of origin of such primordial living things as monads and bacteria, it becomes obvious that we must endeavour to throw light upon this evidence by other methods of investigation. Still it should also be kept steadily in mind that microscopical evidence is equally powerless to throw light upon those primordial collocations which initiate the formation of crystals. The problem concerning the primordial formation of crystals and living things is essentially similar in kind. Any difference in degree between our present knowledge on these two subjects must not blind us as to their essential similarity. Monads and bacteria are produced as constantly in solutions of colloidal matter as crystals are produced in solutions containing crystallisable matter. Crystallisable substances are definite in composition, and give rise to definite statical aggregations; whilst colloidal substances, much more complex and unstable, give rise on the contrary to dynamical aggregations. These dynamical aggregations, though they at first make their appearance in the form of monads and bacteria, are, by virtue of the properties of their constituent molecules, endowed with the potentiality of undergoing the most various changes, in accordance with the different sets of influences to which they are submitted. They are dynamical aggregates, in fact, in a condition of unstable equilibrium, and are capable of being diverted into new modes of current and reciprocal molecular activity in response to changes in their medium or environment. These differences between the products met with in solutions containing crystallisable and colloidal matter respectively, may, however, be due simply to the original difference in nature between such kinds of matter. Respecting the origin of the first visible forms which appear in either kind of solution, the evidence which we possess is precisely similar in nature. If such microscopical evidence does not enable us to get rid of the doubt that the smallest visible specks of living matter may have originated from *invisible* "germs" of such organisms, neither does it any more enable us to dispense with the supposition that the smallest visible crystals may have originated from pre-existing *invisible* "germs" of crystals. There is, in fact, so far as actual scientific evidence goes, almost as good reason for a belief in the universal distribution of invisible "germs" of crystals, as there is for our belief in the universal distribution of invisible "germs" of monads and bacteria. The very existence of the one set of invisible "germs" is, in fact, just as hypothetical as the existence of the other. Monads and bacteria we do know; but concerning the existence of invisible "germs" of monads and bacteria we know just as little as we do concerning the existence of invisible "germs" of crystals.

And yet almost all the difficulties in finally settling the question of the truth or falsity of the doctrines of "spontaneous generation" are centered in this question as to the mode of origin of monads, bacteria, and such fungus-spores as similarly originate in homogeneous solutions in the form of the most minute specks of living matter.† Given the existence of such primordial living particles, and we can easily watch changes taking place in aggregations of them, which lead to the production of much larger and altogether different organisms. We can then trace out with the microscope various kinds of evolution—processes of so-called "spontaneous generation," in fact—the establishment of the reality of which is just as much in opposition to generally received biological notions, as is the supposition that the primordial units themselves are able to come into being *de novo* after particular modes of collocation of colloidal molecules hitherto invisible. The amount of difference between such invisible organic molecule and the speck-like organism less than $\frac{1}{50000}''$ in diameter, which appears in the previously homogeneous solution, may be no more real or striking than is the difference between some of the visible monads or bacteria and the much larger and higher kinds of living things, whose mode of origin I am about to describe, and which may be seen to arise after particular sets of changes have taken place in aggregations of such monads and bacteria. Of two things previously deemed alike improbable, the one which can come within the range of our vision may be shown to take place—the other being, unfortunately, beyond our ken, admits of no such proof. The unmistakeable upsetting of our preconceptions on the one subject should, however, make us cautious how, on theoretical grounds, we pronounce that to be impossible in the case of organisms which we, nevertheless, believe to be possible and actual in the case of crystals: especially when, in these two sets of cases, the amount of actual evidence which we possess is almost equal and similar.

Waiving, then, for the present the consideration of additional evidence as to the mode of origin of the primordial living particles, the monads and bacteria, and of the apparently similarly originating fungus-spores, I will describe some of the evolutional changes by which higher organisms may be seen to arise in a pellicle formed by an aggregation of the simpler kinds of living particles.

I.

The Mode of Origin of Unicellular Organisms and of Spores of Fungi in the "proligerous pellicle" of organic solutions.

What Burdach named the *proligerous pellicle* of organic solutions is made up of an aggregation of monads and bacteria in a transparent jelly-like stratum, on the surface of the fluid. It constitutes at first a thin scum-like layer, and although the monads and bacteria entering into its composition are motionless, M. Pouchet and others were not warranted in assuming from this fact alone that they were dead. There is, indeed, good reason for believing to the contrary, since, as pointed out by Cohn, when any of these particles are set free from the broken edge of a pellicle they always resume their movements. Motion, therefore, may simply be prevented by the presence of the transparent jelly-like material in which they are imbedded, although the particles may be undoubtedly living.

My observations on this subject have been principally carried on throughout the winter months; and this is a time not favourable for the appearance of ciliated Infusoria in organic infusions. Hence it is, perhaps, that I have not been able to witness any of those changes in the pellicle which have been described by Pouchet, as resulting in the evolution of *Paramecia*, *Kolpodæ*, and other ciliated Infusoria. The changes which I have observed, however, have been so indubitable in nature, have been seen so frequently, and have had such a close general resemblance to those which have been described as leading to the evolution of *Paramecia*, that I am quite disposed to believe in the correctness of the observations that have been made by Pouchet and others on this part of the subject.

My own observations have been conducted principally on the pellicle of hay infusions, and one of the commonest processes of what may be termed *secondary organisation* takes place in the following manner. In a pellicle which previously presented a uniform appearance, certain areas, altogether irregular in size and shape—though always presenting outlines bounded by curved lines—gradually make their appearance. These are, at first, distinguishable from the general ground-work of the pellicle only by their somewhat lighter aspect. On careful microscopical examination with high powers, it may be seen that the boundary of such an area—measuring it may be as much, or more than $\frac{1}{300}''$ in diameter—is pretty sharply defined from the surrounding unaltered granular stratum. The immediately contiguous granules of this are occasionally somewhat more tightly packed, though at other times no such change is observable. In either case the unaltered portion of the pellicle is quite different from the included lighter area, because in this an increase has, apparently, taken place in the amount of jelly-like material between the granules, and, as well, there is a certain alteration in the refractive index, and occasionally in the size of the granules (monads and bacteria) themselves. The next change observable is, that the included area shows lines crossing it here and there, which at first tend to map it out into certain larger divisions. These intersecting lines gradually increase in number, till at last

* Philosophical Transactions, 1862. Capitals and italics are employed as we give them in the memoir itself.

† Such, for instance, is one of the modes of origin of *Torula* cells.

the mass becomes divided into an aggregation of rounded or ovoid bodies each about $\frac{1}{5000}''$ in diameter. As these subdivisions are taking place, the mass as a whole separates from the unaltered pellicle by which it is surrounded. Occasionally there is

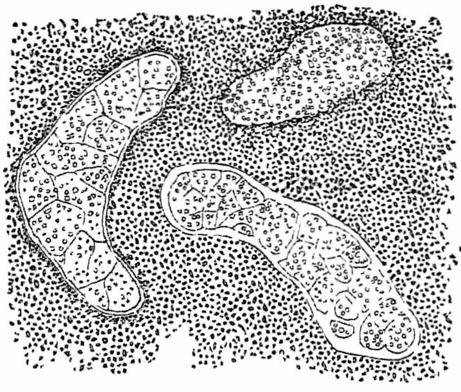


FIG. 1.—Development of Unicellular Organisms: three areas of differentiation showing different stages.

the most distinct interval, at a certain stage, between the parent pellicle and this differentiating mass, whose subdivisions also gradually separate from one another. These subdivisions now appear as independent unicellular organisms, bounded by a delicate membrane, and containing, perhaps, from four to eight of the altered monads and bacteria in their interior.

Throughout the winter months, such areas of differentiation and such resulting unicellular organisms were frequently met with. The unicellular organisms seem during such weather to persist for a very long time in this condition, merely, perhaps, increasing somewhat in size, and most of them ultimately become disintegrated without undergoing further development. They were always seen in a completely motionless condition, and presented no trace of a cilium, so that they were altogether different from the creature known as *Monas lens*. In one solution of hay in which such organisms had been present for some time, after a few days of warmer weather, several of them were found to have become spherical, and to have undergone a considerable increase in size. Some of these were as much as $\frac{1}{2000}''$ in diameter, and on one occasion a stage in the actual transition of one of these unicellular organisms into an *Amœba* was seen with the most perfect distinctness. One

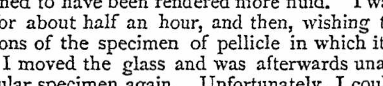


FIG. 2.—Representing gradual enlargement of Unicellular Organisms, and conversion of one of them into an *Amœba*.

half of the organism was distinctly amœboid in character, whilst the other half was almost unchanged, containing large granules like those in the unaltered cells. As slow alteration in shape, of a slug-like character, took place in the anterior diaphanous protoplasmic portion, slow rolling movements occurred amongst the granules in the posterior cell-like portion, whose matrix seemed to have been rendered more fluid. I watched this organism for about half an hour, and then, wishing to examine other portions of the specimen of pellicle in which it had been contained, I moved the glass and was afterwards unable to find this particular specimen again. Unfortunately, I could discover no other Amœbæ or transition states.*

In other cases the areas of differentiation, commencing in a somewhat similar way, terminate in the production of spores of fungi, and I will now describe the mode of evolution of such spores as I observed it taking place in portions of a pellicle having a brownish colour, from an old infusion of hay. The development of this brownish tinge in the earlier stages made it more easy to unravel the nature of the earlier changes. The areas which began to differentiate were generally not very large. They were at first quite colourless, and the granules were separated from one another by a notable amount of transparent jelly-like material. The granules themselves were mostly shaped like the figure 8, and each half was about $\frac{1}{30000}''$ in diameter. A later stage was seen, apparently, in other areas which had assumed a very faint brownish tinge, and which presented evidences that

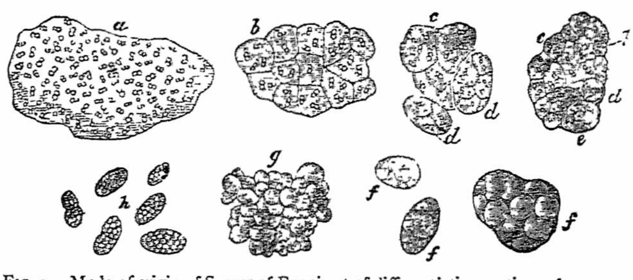


FIG 3.—Mode of origin of Spores of Fungi out of differentiating portions of a Pellicle formed on an infusion of hay.

subdivision was taking place. As the process of subdivision progressed, so the brown tinge became gradually deeper. Ovoid masses were frequently seen about $\frac{1}{2000}''$ or $\frac{1}{1000}''$ in diameter, of a decidedly brown colour, with from 8 to 12 or more ovoid subdivisions within the common envelope. As multiplication advanced, the individual products lost all trace of their original granular condition. They became quite homogeneous and highly refractive masses of a brown colour, looking almost like large brown fat globules. At last, multiplication still proceeding, the distended, and always thin, cyst-like, general envelope becomes ruptured and disappears, leaving only an irregular mass of spherical or ovoidal bodies of various sizes. The individual segments, as soon as this process of multiplication has ceased, increase in size, and then gradually become less refractive and lighter in colour. A slight differentiation of their contents also again takes place, marked by the appearance of faint lines within, as they assume the appearance of ovoid bodies about $\frac{1}{4000}''$ in diameter.* Even when they have attained this stage of development, they may again undergo a process of division; though generally, after a time, they give origin to ordinary mycelial filaments.

Similar changes in the refractive index have been frequently noticed in other cases when a protoplasmic mass is at the same time differentiating and undergoing a process of multiplication,† whilst the mode and frequency of the sub-division is exactly comparable with what so frequently happens to the gonidia of lichens.

The changes which I have described represent, I think, only two extreme types of a mode of metamorphosis which is apt to take place in portions of the pellicle. In the one case a certain area of the pellicle, after undergoing some changes, resolves itself into a number of ovoid bodies, which collectively are about equal in bulk to the altered area itself; whilst, in the other case, at different stages, the segments of the altered area undergo a process of growth and sub-division, so that ultimately the mass of spores which results far exceeds in bulk that of the original area when it began to undergo change.

At other times intermediate processes are met with, and then fungus-spores are produced after a fashion more closely resembling that which leads to the production of the unicellular organisms above described. The areas of change are then larger than those last described, and colourless throughout, whilst the processes of growth and multiplication are less marked at the different stages. Where fungus-spores result after this fashion, the changes in the refractive index, and the homogeneous appearance previously alluded to, still generally manifest themselves at the ultimate stage of division, though nothing of this kind shows itself in the more simple process leading to the production of the unicellular organisms.

Now, however mysterious the nature of these changes may be, which take place, as it seems to us, "spontaneously" in the pellicle on the surface of a solution of organic matter, they are exactly comparable with other changes occurring within the terminal dissepiments of a kind of submerged mucor, named *Achlya prolifera*, similar to those which occur within the thecæ of certain other fungi and of certain lichens, and altogether analogous to that which, as Prof. Haeckel says, takes place in

* Prof. Hartig has, however, described a similar mode of origin of Amœbæ from unicellular organisms, in his observations on the phytozoa of *Marchantiæ*. See *Journal of the Microscopical Society*, 1855, p. 51.

* The markings of these spores are more obscure and less regular than they are represented (*h*) in the woodcut.

† See Nicolet, in Thompson's *Arcana Naturæ*, 1859.

some of the simplest *Amœbæ*, after they have encysted themselves. In all these cases, formless and apparently homogeneous or merely granular living matter, resolves itself more or less rapidly into a number of individualised segments, which are

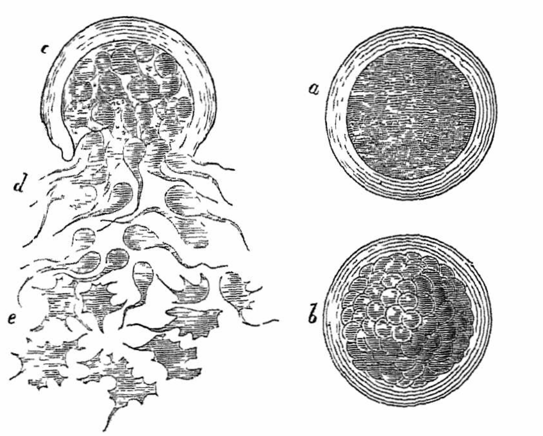


FIG. 4.—Representing subdivision of formless living matter within encysted *Protomyxa*, and exit of products from cyst as active tailed Zoospores, ch subsequently become converted into reptant Amœbæ (Haeckel).

capable of existing as independent living things.* These changes occur in the formless matter of definite organisms, and the products of subdivision tend to reproduce organisms of a similar kind; but the changes which take place in portions of the pellicle are changes occurring in fortuitously aggregated living matter, and the resulting products are, as might have been expected, more variable in kind. There is every reason to believe that the changes which take place in the homogeneous living matter of the encysted *Protomyxa* occur by reason of the molecular properties of this living matter, and are not occasioned by any occult influence exercised by the mere inert cyst-wall, which is but a product of the living matter that it

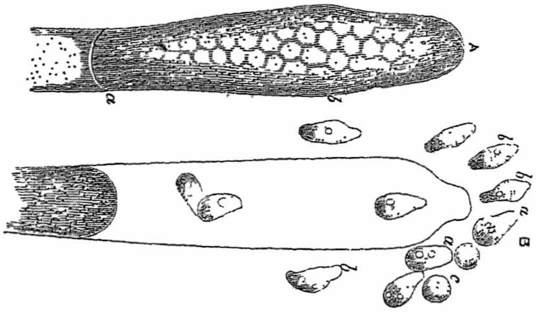


FIG. 5.—Showing the mode of origin of motile Zoospores within the terminal dissepiment of *Achlya* (Tulasne).

encloses. And so we have good reason for supposing that the changes which take place in the mere granular mucilage of the rapidly-formed terminal segment of an *Achlya*, by which this in the space of less than two hours resolves itself into free-swimming zoospores, is to be ascribed to the molecular properties of the mucilage itself which undergoes the change. In the pellicle, on the other hand, we have an aggregation of granular living matter also, and the observations which I have adduced simply go to show that those molecular properties of living matter which lead to its differentiation and further organisation, are not limited to the living matter that is contained within organisms of a definite type. Just as the changes which take place in the structureless living matter of these organisms seem to be due to the forces acting upon, and to the reactions amongst, the several molecules of which it is composed, so do the changes which occur in given areas of the pellicle seem referrible to the influence of physical forces upon the living molecules of which it is composed, and upon the mutual inter-action of these upon one another when under the influence of such incidence. In both cases the changes take place in living matter; in both cases they are the results of molecular activity: in the one set of cases they take place in a fortuitous aggregation of living matter, and the products are accordingly very variable in nature, whilst, in the other set of cases, just as they take place in living matter which constitutes part of a definite organism, so are the products more definite in kind.

But this process, which most certainly occurs in the living matter of a pellicle, is of a kind not hitherto generally recognised as one of the modes by which unicellular organisms, or spores of fungi, may originate. These are, in fact, instances of what has been called "spontaneous generation," or what we may better term heterogenous evolution. The majority of biologists would be as much inclined to believe that these processes did not take place as they are inclined to disbelieve that a monad or a bacterium may be born *de novo* in a solution of organic matter. The occurrence of the one process has been thought to be about as improbable as that of the other. Yet the one can be seen undoubtedly taking place with the aid of the microscope alone. Unfortunately, however, the organic molecules, which are supposed to coalesce in the solution of organic matter, in order to form the smallest visible Living particle, are themselves invisible. We cannot, therefore, trace the genesis of one of these particles with the aid of the microscope, any more than we are able to trace the genesis of a crystal beyond its *minimum visible* stage. In the one case physicists and biologists willingly assume that such ultimately visible particles are the products of a "spontaneous" coalescence of molecules, which are themselves invisible, whilst in the case of organisms they will grant no such assumption. They require us to prove, in fact, that such organisms have not been produced from pre-existing though perhaps invisible "germs," before they will grant for organisms that probability which they at once concede in the case of crystals. This difference which is made between the two cases seems due, in great part, to some theoretical views which are held concerning the nature of Life And yet it would not be difficult to show that the metaphysical or vitalistic theories in question, to which they commit themselves, are directly opposed to some of the most accredited scientific doctrines of the day.* The doctrine of the Conservation of Energy and of the Correlation existing between the Vital and the Physical forces do, if pushed to their ultimate issues, inevitably bring us to the conclusion that the forces acting within all Living bodies are molecular forces, and that such forces are derived from the physical forces of the outside world just as surely as the matter of the organism formerly existed outside itself. The most careful interpretation of scientific evidence, moreover, would lead us to the conclusion that what is called the Life—or in other words the aggregate set of phenomena displayed by one of the simplest bodies which we call a living thing—is as much the essential and inseparable attribute of the particular molecular collocation which displays it, as the properties of the crystal are essential to the kinds and modes of aggregation of the molecules which enter into its composition. It may be maintained, therefore, that all *à priori* presumptions, based upon the best scientific evidence, would lead us to disbelieve the "vitalistic" theories which are still held by many at the present day. It is the vitalist, however, who alone has any logical reason for ins sting that what may be a good and valid mode of accounting for the origin of crystals cannot be considered to hold good in the case of organisms. Those who believe that the forces acting in Living things manifest themselves in the individual molecules of which these are composed, and that such forces are convertible with the ordinary physical forces, have, on the other hand, strong *à priori* reasons for believing that Life will manifest itself wherever particular collocations of complex organic molecules occur. It rests, then, in reality, with the vitalist, who assumes the truth of a mere theory, in favour of which he can adduce no scientific evidence, to show why a different rule should be presumed to

* All this part of the subject will be much more fully treated in the work on *The Beginnings of Life*. The possession or not of the property of motility seems to be an altogether unessential characteristic. The products of subdivision of such an encysted *Amœba* as *Protomyxa aurantiaca* are motile zoospores, and so are those of the fungoid *Achlya prolifera*; but the reproductive products arising from the subdivision of the formless matter within the spore-cases of *Pezizæ* and other fungi are motionless, and this is the case also with the gonidia of the *Saprolegniæ*, which are fungoid organisms, otherwise almost undistinguishable from *Achlya*.

* This I shall attempt to show fully elsewhere.

hold good in the birth of crystals and of organisms respectively.* Those who hold opposite opinions need only suppose that molecules of "organic" matter, or some such complex molecules, may aggregate and arrange themselves after certain modes to produce a Living thing—just as a crystal, endowed with its particular properties, is producible by other modes of aggregation—because with them Life is considered to be a product of molecular collocation and of molecular change. And if the "vitalist" wishes to establish the existence of a more fundamental difference between crystals and organisms than we are prepared to grant, seeing that the scientific evidence seems to be against him, it remains for him at least to endeavour to show good grounds for the establishment of such difference.

It should be remembered, then, that in the present state of science all theoretical considerations seem favourable to the views of the evolutionists, and that the only thing which can be opposed to them is the *assumption* that those processes of reproduction which take place amongst all known varieties of living things are the *only* processes by which such living things can arise. But now, already, by means of microscopical evidence alone, it has been shown that Living things may arise by a process of heterogenesis—not as products of a pre-existing organism like themselves, but by a process altogether different from those which have been hitherto supposed to be general. And it is worth remembering, as we have before pointed out, that the supposed coalescence of invisible molecules and the changes which lead to the production of the minutest living monad in organic solutions, if they could be shown to be true, would not be one whit more startling than those very changes which, before disbelieved in, can now be easily shown to take place in the "proligerous pellicle." Have we not seen that out of a mere fortuitous aggregation of living particles, and the subsequent metamorphoses taking place therein, organisms appear which are much larger, and of a much higher type than those which preceded them, although such a mode of origin was formerly regarded almost as "impossible"?

* I will here make two quotations in order to show that the opinions of two of our leading scientific men (many others might have been quoted) are not at all opposed to the comparisons I have been instituting. They both, in fact, declare emphatically that the phenomena of Life are phenomena of molecular physics.

In his address to the Mathematical and Physical Section of the British Association in 1868, Prof. Tyndall, as president, speaking of a grain of corn, said:—"But what has built together the molecules of the corn? I have already said, concerning crystalline architecture, that you may, if you please, consider the atoms and molecules to be placed in position by a power external to themselves. The same hypothesis is open to you now. But if in the case of crystals you have rejected this notion of an external architect, I think you are bound to reject it now, and to conclude that the molecules of the corn are self-posited by the forces with which they act upon each other. It would be poor philosophy to invoke an external agent in the one case and to reject it in the other. . . . . But I must go still further, and affirm that in the eye of science the animal body is just as much the product of molecular force as the stalk and ear of corn, or as the crystal of salt or of sugar. . . Every particle that enters into the composition of a muscle, a nerve, or a bone, has been placed in its position by molecular force. And unless the existence of law in these matters be denied, and the element of caprice introduced, we must conclude that, given the relation of any molecule of the body to its environment, its position in the body might be predicted. Our difficulty is not with the *quality* of the problem, but with its *complexity*." (Pp. 4 and 5.)

Prof. Huxley, again, in his article on "Protoplasm," in the *Fortnightly Review* for February 1869, says:—"Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life. I see no break in this series of steps in molecular complication, and I am unable to understand why the language which is applicable to any one term of the series may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen, and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed. . . . Is the case in any way changed when carbonic acid, water, and ammonia disappear, and in their place, *under the influence of pre-existing protoplasm*, an equivalent weight of the matter of life makes its appearance? . . . What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative or correlative in the not living matter which gave rise to it?" (The passage I have marked by italics indicates the extent to which Prof. Huxley stops short of the views I have been endeavouring to support.)

Now I maintain that the logical outcome of such doctrines as these is that Life *may* manifest itself whenever certain particular collocations of complex molecules occur, just as surely as crystalline properties will be manifested by chloride of sodium whenever the molecules of this substance combine to form crystals. The *à priori* presumptions being rather in favour of (certainly not opposed to) the occurrence of the new evolution of Living things, we have to show, as well as we are able, that there *is* a tendency to the occurrence of such clusterings as will lead to the formation of bacteria or of fungus-spores, just as we feel compelled to believe that there is a tendency to the occurrence of those particular clusterings of molecules which result in the formation of crystals.

II.

On the probable Evolution of Living Things in Organic and Saline Solutions, which have been previously exposed to high Temperatures, in airless and hermetically sealed vessels.

We must now come to the consideration of all the experimental evidence which can be adduced in support of what the microscope teaches us as to the mode of origin of the lower kinds of Living things.

The method of experimentation which has been principally relied upon, has, since 1837, always been that introduced by Schwann. Sometimes the correspondence has been exact, and sometimes his experiments have been repeated with some slight modification. In this method, the solution of organic matter is first boiled in a flask, the neck of which is securely connected with a tube closely packed with portions of red-hot pumice-stone, or other incombustible substance: after the solution has been boiled for some time, and all the air of the flask has been expelled, the flask itself is allowed to cool whilst the tube containing the closely-packed red-hot materials is still maintained at the same temperature, in order that whatever air enters into the flask may be subjected to a calcining heat as it passes through the tube. When the flask has become cool it will then contain only the previously boiled solution in contact with air at ordinary atmospheric pressure, which has been calcined. Since it has been hitherto settled that the lower kinds of organisms which may be contained in the solutions, are destroyed when these fluids are raised to a temperature of 100° C., and that no organisms have been known to survive after having remained for thirty minutes in air raised to a temperature of 130° C., the boiling of the fluid for a time and the calcination of the air has generally been supposed to be a sufficient precaution to ensure the destruction of all organisms in the experimental media. Experiments conducted in this way have been said to yield negative results by some, whilst others have maintained that in spite of all such precautions, destined to destroy pre-existing Living things, they do, nevertheless, obtain low kinds of organisms, after two or three months, if not before, in their experimental fluids.

Negative results in these experiments can of course prove little or nothing; they may be explained equally well by either party: either no organisms have been found, because they or all the germs which could give rise to them have been killed; or it is just as fair for the evolutionists to explain the absence of organisms on the supposition that the particular fluids employed have not yielded them because of the severity of the destructive conditions to which the particular organic matter in the previously boiled fluids had been subjected. When organisms *are* found, however, in fluids which have been legitimately subjected to the conditions involved in Schwann's experiments, then one of two things is proven: either the amount of heat which hitherto was deemed adequate to destroy all pre-existing organisms is in reality not sufficient, or else the organisms found must have been evolved *de novo* as the evolutionists suppose. Unless, therefore, the standard of vital resistance to heat can be shown to be higher than it was formerly supposed to be, any single positive result when Schwann's experiment has been legitimately performed, is of far more importance towards the settlement of the question in dispute than five hundred negative results. It would tend to show that in the particular fluid employed organisms might be evolved *de novo*. And yet positive results in the performance of these experiments have been obtained again and again by Schwann himself, by Ingen-housz, by Mantegazza, Pouchet, MM. Joly, and Musset, Jeffries Wyman, Dr. Child, and, not to mention any others, even by M. Pasteur himself.*

But even this is not all; organisms have been found in fluids which had been contained in closed vessels, after exposure to conditions still more severe. Prof. Jeffries Wyman, of Cambridge, U.S., published an account (which I am sorry to say I have been unable to obtain from any of our libraries) in 1862 of experiments in which he had boiled fluids containing organic matter for a period of two hours under a pressure of two atmospheres, that

* We have the testimony of M. Pasteur to the fact that organisms may almost always be met with when milk or some other alkaline fluid is made use of in Schwann's experiment. He says he has always met with negative results, however, if such fluids have been raised to a temperature of 110° C. rather than 100° C. Concerning his inferences from these experiments I shall have more to say hereafter.

is to say, at a temperature of 120·6° C. To the fluids so treated no air was allowed access, except what had passed through the capillary bores of white-hot iron tubes. And yet, when the flasks were broken, after a certain time, organisms were found in the fluids which had been submitted to these conditions. Prof. Mantegazza has obtained organisms from the fluids of hermetically closed flasks, after these, containing the putrescible fluids and common air at ordinary atmospheric pressure, had been subjected for some time to a temperature of 140° C.; and Prof. Cantoni, of Pavia, has obtained bacteria and vibrios in the fluids of similarly prepared closed flasks, after these had been exposed in a Papin's digester to a temperature of 142° C. for four hours. And still, though no positive evidence has been forthcoming to show that the standard of vital resistance can be raised—though nobody has shown that any Living thing which has been made the subject of experimentation has been found alive after an exposure for a minute or two in a fluid raised to 100° C., or after an exposure for thirty minutes to a temperature of 130° C. in dry air or *in vacuo*, many scientific men are as much disinclined as ever to admit that the organisms found by the above-mentioned observers could have been evolved *de novo*.

For all those, however, who form their opinions on such matters in accordance with scientific evidence, rather than in obedience to theoretical preconceptions, it must be admitted that the balance of evidence is at present altogether in favour of the supposition that organisms can arise *de novo*. And it only remains for those who are opposed to the notion from an *à priori* point of view to bring forward positive evidence tending to show that the standard of vital resistance, for the organisms in question, is much higher than it has been hitherto shown to be.

Some of the additional evidence I have now to bring forward, therefore, only tends to strengthen the validity of the conclusion which was already deducible from the experiments of Pouchet, Wymann, Mantegazza, Cantoni, and others.

Hitherto, in speaking of the experiments of Schwann, I have only incidentally referred to the destructive influence of heat upon the organic matter contained in the solutions, though very strong evidence could be adduced to show that such organic matter is notably altered after these solutions have been raised to the temperature of 100° C. The disruptive agency of heat is fairly enough supposed by the evolutionists to destroy some of the more mobile combinations in each solution—to break up, more or less completely, in fact, those very complex organic products, whose molecular instability is looked upon as one of the conditions essential to the evolutional changes which are supposed to take place. I shall postpone for the present the consideration of the question as to how far this destructive agency of heat is affected by the alkalinity, neutrality, or acidity of the fluids, though I shall towards the close of this paper bring forward evidence tending to show that organic matter in acid solutions is more damaged by the temperature of 100° C. than is that which is contained in neutral or slightly akaline solutions, when these are heated to the same extent.

To any one looking boldly at the problem, the question which now seems to suggest itself is, whether any other substances can be employed in place of the organic matter, such as would not be injured by a temperature of 100° C., and which, whilst containing the necessary elements for the formation of an organism, might also permit the occurrence of those peculiar molecular re-arrangements which result in the formation of Living things? Postponing, however, the consideration of this question for the present, I will first refer to the influence of another of the detrimental conditions involved in Schwann's experiments.

In those instances where the results were positive, and in which calcined air and previously heated organic solutions were shut up in hermetically-sealed vessels, nothing but the lowest forms of living things ever appeared—mere monads, bacteria, and vibrios—and these generally not till after the expiration of two, three, or more months. There seems reason to believe that the delay, and the very low forms of the organisms met with, are attributable, in great part, to the increased tension which almost invariably occurs within the closed vessels. In some cases the tension becomes so great that it ultimately bursts the flasks. This took place several times in the course of Dr. Child's experiments. After reflection upon these facts, it seemed to me that there was not much room for surprise, looking at it from the evolutionist's point of view, that the results should have been so unsatisfactory. The small amount of space above the level of the fluid, is already occupied, at the time that the flask is hermetically sealed, with air under ordinary atmospheric pressure. But, when putrefactive or evolutional changes begin to take place in the fluids containing organic matter, such changes are almost sure to be attended by the liberation of gases either simple or compound. And in direct proportion to the extent of the liberation, so does the tension and consequent pressure upon the fluid increase within the flask. This pressure upon the solutions might and probably would tend to prevent, in proportion to its extent, those life-giving re-arrangements which are presumed to take place amongst the molecules of the organic matter contained therein. Having come to this conclusion, and as there also seemed to be good reason for the belief that atmospheric air was not needed for the development of bacteria and vibrios, it occurred to me that it would be possible so to modify the experiment of Schwann that it might be repeated under conditions more satisfactory to the evolutionists, and at the same time in a way which would be not less in accordance with the views of the panspermatists. The withdrawal of all air from the flasks in which the boiled solutions were contained, rather than the admission of calcined air, seemed to be the kind of modification which was desirable.* Then the contamination of the boiled fluids with possible atmospheric germs would be as effectually provided against as if air had been only allowed to enter after it had been calcined, and the seemingly obvious advantage would be attained that there would be even greater freedom than usual for the commencement of evolutional changes, on account of the diminished pressure upon the fluids contained *in vacuo*. It was presumed, also, that changes might go on for a certain extent before the evolution of gases had been sufficient to exercise such a repressive influence as to prevent their continuance.

The results of experiments conducted upon these principles have been most uniform, and have been of such a nature as to tend to support the truth of the reasonings which dictated them.

The flasks employed have generally been of small size, capable of holding about two ounces of fluid. These have proved to be quite large enough, and their small size made it easy for me to manage the whole process with a very slight amount of assistance. The method adopted was as follows:—After each flask had been thoroughly cleaned with boiling water, three-fourths of it was filled with the fluid which was to be made the subject of experiment. With the aid of a small hand blow-pipe and the spirit-lamp flame, the neck of the flask, about three inches from its bulb, was then drawn out till it was less than a line in diameter. Having been cut across in this situation, the fluid within the flask was boiled continuously for a period of from ten to twenty minutes. At first ebullition was allowed to take place rapidly (till some of the fluid itself frothed over) so as to procure the more thorough expulsion of the air; then the boiling was maintained for a time at medium violence over the flame of my reading-lamp, whilst the greatly attenuated neck of the flask was heated in the flame of a spirit-lamp placed at a corresponding level. The steam for a time poured out violently into the flame of the spirit-lamp; and whilst my assistant (my wife) turned down the flame of the reading-lamp so as to diminish still further the violence of the ebullition, I directed the blow-pipe flame upon the narrow orifice of the neck of the flask, and so sealed it hermetically. Immediately that the orifice was closed, the heat was withdrawn from the body of the flask.

After a little practice I soon became able to procure in this way an almost perfect vacuum. Even though the vessels were

* I was actually led to adopt this important modification, perhaps, by a mere chance. In the spring of last year Mr. Temple Orme, of University College, had kindly undertaken to perform some experiments with me bearing upon the subject of "Spontaneous Generation." We at first proposed to repeat, with some very desirable variations which he suggested, Schultze's experiments. One day, however, he told me he had boiled an infusion of hay for four hours, and had then hermetically sealed the neck of the flask whilst ebullition continued. In this way a more or less perfect vacuum was procured. This he did as a sort of tentative experiment, and it was then, on thinking over the subject, that I resolved to give the plan a thorough trial, as it appeared to me that by so doing I should be working under conditions which were most in accordance with the theory of evolution. I performed four experiments at that time in concert with Mr. Temple Orme, with hay infusions which had been boiled for four hours and had then been sealed up *in vacuo*. In each of these fluids organisms were found after a comparatively short time. These were the first experiments performed under such conditions. In my subsequent work I have not had the benefit of Mr. Orme's personal assistance, although I have frequently profited by suggestions which he has made.

so small, momentary ebullition could generally be renewed again and again for the space of five minutes after they had been hermetically sealed, by the mere application of one of my fingers, which had been dipped into cold water, to a portion of the glass above the level of the fluid. The water-hammer effect was also very obvious in several which were tested in this fashion.

I believe that an almost perfect vacuum can be produced in this way; in the first violent ebullition the air is driven out of the flask by the fluid, and as ebullition is continuously kept up after this till the flask is hermetically sealed, there is always an outpouring of heated vapour, and no opportunity for a re-ingress of air. But, even if in any given case the vacuum should not prove to be absolute, it does not seem to me that there would be any material abatement from the severity of the conditions which the panspermatists have a right to demand. If, on the one hand, absolutely the whole of the air had not been expelled from the flasks during the process of ebullition, what remained would necessarily be mixed up with a very much larger quantity of continually renewed aqueous vapour, and the effect would probably be that any living things would be just as effectually and destructively heated as if they were lodged in the boiling solution itself; whilst if, on the other hand, the boiling had been arrested for one or two seconds before the complete closure of the almost capillary orifice at the mouth of the flask, even if any air entered, it would have had first to pass through the blow-pipe flame, and then through the white-hot capillary orifice—it would in fact have been calcined as in Schwann's experiment. The conditions of the experiment would then have been no less severe, and the only effect would be that the vacuum with which I prefer to work would have been rendered by so much the less complete. Although I make these remarks with the view of meeting criticisms, I am inclined to think that the vacuum in my experiments has been complete; and it should be remembered that M. Pasteur always adopted this method when he wished to preserve solutions for a time *in vacuo*. Whenever he desired to make comparative trials with the air of different localities, the solutions which had been prepared in this way were assumed by him to be contained *in vacuo*, so that the flasks could then be taken to the localities, with the air of which he wished to experiment. There the necks of the flasks were broken, in order that they might become filled with the air of the respective localities. After this had been done the flasks were resealed, and kept for future observation of their contained fluids. M. Pasteur, M. Pouchet, and others who adopted this method, carried away their experimental fluids *in vacuo*, during a two or three days' journey to the Alps or to the Pyrenees, and it never seemed to have occurred to either of them that evolutional changes might be taking place during their journey. M. Pasteur, in fact, habitually shut his eyes to all such possibilities, they did not come within the range of what he considered possible; such thoughts might, however, have suggested themselves to M. Pouchet and others, although this does not seem to have been the case.

After the flasks had been prepared in the way above mentioned, they were suspended beneath the mantelpiece in my study. During the day there was always a fire in the room, and at night I put my reading lamp underneath them with the flame properly turned down. So far as I have been able to ascertain, the temperature to which they have been subjected has mostly ranged between 23°—29° C. (75°—86° F.) Sometimes they have been exposed to the lower temperature and sometimes to the higher, and I suspect that a variation of this kind may perhaps be more favourable for the production of evolutional changes than maintenance at a constant temperature.

In detailing the results of the following experiments, I shall not enter into any minute description of the organisms found. My main object throughout has been to obtain evidence on the subject as to whether a *de novo* evolution of Living things could or could not take place. The demands upon my time have been so serious in the carrying on of these investigations, that occasionally it has only been small portions of the experimental fluids which have been examined. If, for instance, what I found in the first few drops of the fluid left no doubt in my mind as to the nature and abundance of some Living things contained therein, the remaining portions of the fluid were frequently not examined. Other bodies, therefore, may have been contained in the solutions, which were never seen at all.

H. CHARLTON BASTIAN

(*To be continued.*)

SCIENTIFIC SERIALS

THE *American Naturalist* for June contains several excellent articles. The first is by Prof. J. S. Newberry, "On the Surface Geology of the Basin of the great Lakes and the Valley of the Mississippi." In the northern half of this area down to the parallel of 38° to 40° N. lat., are found, not everywhere, but in most localities where the nature of the underlying rocks is such as to retain inscriptions made upon them, the unmistakeable indications of glacial action. Some of the valleys and channels which bear the marks of glacial action, evidently formed or modified by ice, and dating from the ice period of an earlier epoch, are excavated far below the present lakes and water-courses which occupy them. These valleys form a connected system of drainage at a lower level than the present river system, and lower than could be produced without a continental elevation of several hundred feet. Upon the glacial surface are found a series of unconsolidated materials, generally stratified, called the drift deposits. These consist in the lowest stratum of the Erie clays of Sir William Logan, above which are sands containing beds of gravel; and near the surface elephants' teeth have been found, water-worn and rounded. Upon these stratified clays, sands, and gravel of the drift, are scattered boulders and blocks of all sizes, of granite, greenstone, siliceous and mica slates, and various other metamorphic and eruptive rocks, generally traceable to some locality in the Eozoic area of the lakes. Among these boulders many balls of native copper have been found, which could have come from nowhere else than the copper district of Lake Superior. Above all these drift deposits, and more recent than any of them, are the "lake ridges," corresponding to our raised sea-beaches, embankments of sand, gravel, sticks, leaves, &c., which run imperfectly parallel with the present outlines of the lake margins, where highlands lie in the rear of such margins. The general conclusions drawn are the existence of a glacial epoch over the northern half of the continent of North America, probably contemporaneous with that of Europe, and with a climate comparable to that of Greenland; that the courses of these ancient glaciers correspond in a general way with the present channels of drainage; and that at this period the continent must have been several hundred feet higher than now.—"A Winter's Day in the Yukon Territory," by W. H. Dall, refutes the prevalent idea, perpetuated even by "official" reports, that the island of St. Paul is surrounded in winter by immense masses of ice, on which the polar bears and arctic foxes sail down from the north and engage in pitched battles with the wretched inhabitants. The fact is that there is no solid and very little floating ice near St. Paul in winter; the arctic foxes found there as well as on most of the islands were purposely introduced by the Russians for propagation, a certain number of skins being taken annually; and there is no authentic evidence that the polar bear has ever been found south of Behring's Straits. The country of Alaska comprises two climatic regions, which differ as widely as Labrador and South Carolina in their winter temperature. One contains the mainland north of the peninsula of Alaska and the islands north of the St. Matthew group; the other includes the coast and islands south and east of Kadiak, while the Aleutian Islands, with the group of St. Paul and St. George, are somewhat intermediate. A day's excursion during the winter season in the northern and more inhospitable of these two regions yielded a considerable number of interesting animals.—Articles of a popular character are "Our native Trees and Shrubs," by Rev. J. W. Chickering, Jun.; and "A Few Words about Moths," by A. S. Packard, Jun. A review of Principal Dawson's article in the *Canadian Naturalist* on "Modern Ideas of Derivation," criticises, favourably on the whole, that writer's strictures on the Darwinian theory of Natural Selection.—The Natural History Miscellany contains many interesting notes, either original or culled from English scientific journals.

The fourth part of the *Jenaische Zeitschrift für Medicin und Naturwissenschaft*, June 1870, contains the following important articles. 1. Gegenbauer on the skeleton of the limbs of Vertebrata, and of the Selachia and Chimæra in particular. 2. Abbe on a spectrum apparatus for the microscope. 3. Dr. Dohrn: Further researches on the structure and development of the Arthropoda, especially bearing on the Zoea stage of Crustacea; and lastly, a long and interesting paper by Ernst Haeckel on the "Plastiden-theorie," in which he treats fully of the deep-sea life brought to light by the dredgings of Drs. Wallich, Carpenter, Wyville Thomson, Huxley, and others, describing the Bathybius, Coccoliths, Globigerina, &c. He confesses himself unable to solve the problem of the origin of the immense quantities of protoplasm that form a bottom to the sea, but is disinclined to regard it as consisting of the mycelium of sponges, an opinion advanced by Wyville Thomson. He finds the well-known yellow cells of Radiolaria to contain starch, the reactions of which are not distinguishable from those characteristic of starch derived from vegetables. These starch granules make up more than half of the entire mass of the Radiolaria.

The *Bulletin de la Société Imperiale des Naturalistes de Moscow*, 1869, No. 2 (received June 15, 1870), contains, amongst other valuable papers, a carefully-worked-up description of the anatomy and development of the *Pedicellina*, by B. Uljanin, which is accompanied by two plates illustrating the changes undergone as far as he had an opportunity of observing them.

In the last number (Heft iv. Band lx.) of the *Sitzungsberichte der K. Akad. der Wissenchaften zu Wien* is a long paper by Dr. A. Polotebnow on the origin and mode of increase of Bacteria. These, as most of our readers are aware, consist of very small rods, which present a kind of tranverse striation at tolerably regular intervals, like an extremely diminutive sugar-cane of from two to six or seven joints, and which exhibit irregular vibratory movements. They have been, like other lowly organised forms, sometimes considered, as by Dujardin, to belong to the animal kingdom; sometimes, as by Cohn, to represent a form of vegetable life; and sometimes, as by Pertz, to occupy an intermediate position on the confines of the two kingdoms. Dr. Polotebnow finds that an unbroken series of forms can be observed between the minute round cells which form the mycelium of *Penicillium*, and probably other fungi, and fully-developed Bacteria. In regard to their multiplication, he thinks this can only occur from the cells above mentioned, and that when they have once become fully formed Bacteria they are no longer capable of further multiplication.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 8.—Mr. Joseph Prestwich, F.R.S. president, in the chair.

Mr. Henry G. Vennor, of the Geological Survey of Canada, Montreal; Alexander Kendall Mackinnon, Memb. Inst. C. E., Director-General of Public Works, Montevideo, South America; and Mr. Arthur Roope Hunt, Quintella, Torquay, were elected Fellows of the Society.

1. "On the Superficial Deposits of the South of Hampshire, and the Isle of Wight." By Thomas Codrington, F.G.S. This paper treated of the gravel deposits covering the tertiary strata of the country between Portsmouth and Poole, and of the Isle of Wight. The strikingly tabular character of the surface is best seen on the east of the Avon, where from the coast for more than twenty miles inland a gravel-covered plain can be followed, rising gradually from 80 feet to 420 feet above the sea, at the rate of about 20 feet per mile. The high plains of the New Forest, to the eye perfectly level, and indented by deep valleys, are portions of this table-land. The plateau between the Bournemouth Cliffs and the Valley of the Stour, and detached gravel-capped hills further inland, are the remnants of a similar table-land on the west of the Avon, while eastwards the same character prevails up to Southampton Water. Sections parallel with the coast show the level nature of the country, broken only by well-defined river-valleys. On the east of Southampton water a similar tabular surface, sloping at a steeper angle towards the shore-line, and cut through by the valleys of the Itchen, Hamble, and Titchfield rivers, remains; and in the Isle of Wight the gravels capping the flat-topped tertiary hills coincide with a corresponding plain sloping northwards. The gravel covering these table-lands is composed chiefly of subangular chalk-flints, with a varying proportion of tertiary pebbles. Sarsen stone blocks are found everywhere, and on Poole Heath granitic pebbles; and in the gravel of Portsea large boulders of granitic and palæozoic rocks are met with. In the Isle of Wight, chert from the Upper Greensand, and materials from the Lower Cretaceous beds also occur. The colour of the gravel is generally red; and the origin of the white gravel, which often overlies the red, is to be ascribed to the bleaching action of vegetable matter. Brick-earth is generally associated with the gravel at all levels but the highest; but the contorted appearances attributed to glacial action only occur at low levels. No organic remains have been found in the gravel covering the plains, while the valley-gravels of the district have afforded mammalian bones and teeth of the usual species. Flint implements have been found at Bournemouth at 120 feet above the sea; at Lymington, near Southampton, at 80 and 150 feet; and also along the shore between Southampton Water and Gosport, at 35 feet above the sea, from gravel forming part of the covering of the tabular surface, and unconnected with the river valleys. The gravel capping the cliffs of the south coast of the Isle of Wight, in which the remains of *Elephas primigenius* have been found near Brook and Grange, was probably deposited in the same river-basin as the mammaliferous gravel of Freshwater; and the cutting back of the coast-line by the sea has given the tributaries of a river which flowed by Freshwater northwards to the Solent, a direct outfall to the sea; and the streams thus intercepted at a high level, under the changed condition of flow, have originated the *Chines*. The gravel cliff of the Foreland, at the eastern end of the Isle of Wight, consists principally of raised shingle, which towards the south thins out, and is overlain by a thick deposit of brick-earth, a continuation of which caps the cliffs up to the chalk, and in which a flint implement was found by the author at 85 feet above the sea.

General Considerations.—The marine gravel, with granite boulders covering the south of Sussex, is continued westward by the gravel with similar boulders covering Portsea Island; and this again by the Hill-head gravels, with large blocks of Sarsen stone, these lower gravels being bordered on the south by the raised shingle deposits of the Isle of Wight; and on the north by the higher marine gravels of Avisford, Waterbeach, and Bourne, from which the lower gravel is divided by a well-marked step, extending beyond Portsdown Hill to Titchfield, and traceable on the west of Southampton Water. The Hill-head gravels are considered to be an estuarine deposit, of the same age as the marine gravels of Sussex, and the low-level gravels of the river-valleys; they are supposed to have been formed when the Isle of Wight was still joined to the main land, and all the rivers now reaching the sea by Poole Harbour, Christchurch Harbour, Southampton Water, &c., were affluents of a river communicating with an estuary opening to the sea in the direction of Spithead. The gravels lying above the step, such as those of Avisford and Waterbeach, Titchfield Common, Beaulieu Heath, and Bournemouth, are looked upon as equivalent in position and age to the high-level valley gravels. The level of the gravels on the highest parts of the table-lands is such as to indicate an age far greater than that of the highest gravels of the river-valleys; but the uniform surface from the 400-feet level downwards points to a long continuance of similar conditions, during which the gravel from the highest levels to that of the Bournemouth Cliffs was deposited. The area that can with any probability be assigned to the catchment basin of a river such as that which has been before alluded to, is only three-quarters of the basin of the Thames above Hampton, within which it is difficult to imagine that such an extent of gravel could have been spread out; and the inclination of the flattest of the table-lands is for a river such as only mountain-streams have, and quite incompatible with the spreading out of large even surfaces more than twenty miles across. It is considered more probable that the materials of the gravel were brought down from the chalk country on all sides by rivers, and spread out in an inlet of the sea shut in on the south, and opening out eastwards. This view is not without difficulties; it involves a gradual upheaval of the land, which, when the highest gravels now remaining were being spread out at or near the sea-level, must have stood more than 400 feet lower; and a considerable part of this upheaval must have taken place since the formation of the gravel in which implements fashioned by man are imbedded.

2. "On the relative position of the Forest-bed and the Chillesford Clay in Norfolk and Suffolk, and on the real position of the Forest-bed." By the Rev. John Gunn, M.A., F.G.S.

The author commenced by stating that both at Easton Bavent and at Kessingland the Forest-bed is to be seen forming part of the beach, or of the foot of the cliff, and underlying the Chillesford Clay. He considered that the soil of the Forest-bed had been deposited in an estuary, and that after its elevation the trees, of which the stools are now visible along the coast, grew upon it, and the true Forest-bed was formed. After the submergence of this first freshwater, then fluvio-marine, and finally marine deposits were formed upon it; and the author proposed to give the whole of these deposits the name of the "Forest-bed series." The author suggested that the Forest-bed itself is represented inland by the stony bed which lies immediately upon the chalk and between it and the Fluvio-marine and Marine Crags, his theory being that the surface of the chalk, after supporting a forest-bed fauna, was gradually covered up by successive crag deposits.

3. "On *Proterosaurus speneri*, von Meyer, and a new species, *Proterosaurus huxleyi*, from the Marl-slate of Midderidge, Durham." By Albany Hancock, F.L.S., and Richard Howse. Communicated by Prof. Huxley, F.R.S., F.G.S.

In this paper the authors described a specimen which they referred to *Proterosaurus speneri*, von Meyer; and one of a smaller form, which they regarded as new, and described as *Proterosaurus huxleyi*. Both were from the same part of the marl-slate of Midderidge, Durham. The two species agree in having the limbs and tail long and the neck long, and composed of seven vertebræ, in the number of dorsal vertebræ, in the number and character of the bones of the hand, and in some other particulars, sufficient, with these, in the opinion of the authors, to justify the reference of both to the genus *Proterosaurus*. In *P. huxleyi* the ribs are flattened instead of rounded at the proximal extremity, and less widened and grooved at the distal extremity than in *P. speneri*; the hind limb is considerably longer in proportion to the fore limb: and the distal extremity of the humerus is only twice as wide as the constricted part, instead of three times, as in the old species.

Chemical Society, June 16.—Prof. Williamson, F.R.S., president, in the chair. L. A. Lucas and A. W. Bickerton were elected Fellows. Mr. James Bell read a paper on "Fermentation." The author has instituted a series of experiments to determine: 1. The forms of natural ferment which various albuminous bodies will give rise to in solutions of cane sugar, and of cane sugar and glucose. 2. The relative fermentative powers of various ferments, especially of those occurring in malt extract and in the grape juice. 3. The influence of change of soil upon the fermentative organisms. From among the manifold results obtained in these experiments, the following may be mentioned: (*a*) Addition of glucose to fermenting liquids, especially to the juice of grape, is advantageous, inasmuch as it assists to exhaust the juice of its fermentative element, and thus imparts to the wine a greater keeping power. (*b*) Each ferment has its favourite soil. The President, in proposing a vote of thanks to the author, took occasion to give a brief *résumé* of the present state of knowledge of the yeast plant. Though called a "plant," the yeast organism appears in all its functions rather animal than vegetable; the products of its secretion are less complicated than those it takes in; it does not, like plants, require light for its vital process, neither does it absorb heat, but on the contrary gives it off. Alluding, then, to Liebig's recent memoir on fermentation, Prof. Williamson observed that that distinguished chemist had entirely dropped his former notions regarding the process of fermentation.—Dr. Heisch communicated a paper "On organic matter in water." The author was some time ago called on to assist a large manufacturer of lemonade, who suddenly found it impossible to make lemonade that would keep. After a day or two it became turbid, and its odour anything but agreeable. On investigating the liquid under the microscope it was found full of small spherical cells with, in most cases, a very bright nucleus. After examining all the materials employed, the fault was detected to be with the water. On putting a few grains of pure crystalline sugar into some of the water, it became turbid in a few hours, and contained the cells above mentioned. On inquiry it turned out that the well from which the water used in the preparation of the lemonade was obtained, had been slightly contaminated with sewage. This led the experimenter to mix a minute quantity of sewage with a sugar solution; the cells very soon made their appearance. Filtration through the finest Swedish paper does not remove the germs. Boiling for half an hour in no way destroys their vitality. Filtration through a good bed of animal charcoal seems to be the only effectual mode of removing them, but it is necessary to air the charcoal from time to time, else it loses its purifying property.—Mr. Perkins read a letter from Prof. Strecker, wherein the latter claims the priority of having published the true formula of alizarin as early as 1866, a fact which was not mentioned by Mr. Perkin in his recent lecture on alizarin. Mr. Perkin said that this omission was due to an oversight on his part, certainly not to any intention to deprive Professor Strecker of his merits.—Mr. Herman read a paper "On the methods for the determination of carbon in steel." Several samples of steel were analysed according to different methods, with the view of ascertaining which of the usual processes for determining the carbon in iron is the most advantageous. A large number of careful experiments led to the conclusion that the direct burning of the iron filings in a stream of oxygen (Wöhler's process) is the most expeditious and accurate method. In the following table the means of the results obtained by the different methods are given, the quantities of ferric oxide obtained by combustion in oxygen are almost identical with those required by theory.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
By Eggertz's method	1·319	·789	·701	·587	·486	·349	·283	
By combustion in oxygen	1·1656	·7602	·635			·3594	·273	·9215
By Elliott's method	1·248	·8065	·724	·6701	·5025	·4772	·349	·9427

PARIS

Academy of Sciences, June 13.—The following mathematical papers were read:—Demonstration of Jacobi's method for the formation of the period of a primitive root, by M. V. A. Le Besque; on the construction of the axis of curvature of the developable surface, enveloping a plane of which the displacement is subjected to certain conditions, by M. A. Mannheim, communicated by M. Chasles; and on a certain family of curves and surfaces, by MM. F. Klein and S. Lie, also presented by M. Chasles.—MM. Jamin and Amaury presented a note on the specific heat of mixtures of alcohol and water, in which they show by the examination of numerous mixtures that the specific heat of such fluids is not only higher than the mean specific heat of their constituents, but that in certain proportions it even exceeds that of water.—M. Bussy remarked that M. Buignet and himself had previously ascertained that a mixture of equivalent parts of alcohol and water had a specific heat greater than the mean of its elements.—MM. C. Martins and G. Chancel presented a second note on the physical phenomena which accompany the rupture of hollow projectiles of various calibres by the congelation of water, in reply to observations made by General Morin and MM. Dumas and Élie de Beaumont on their former communication.—A note by M. J. Violle, on the mechanical equivalent of heat, was presented by M. H. Sainte-Claire Deville.—M. A. Houzeau communicated some experiments on the electrolisation of the air or of oxygen as a means of producing ozone. He stated that the production of ozone is greater at the negative than at the positive pole, that it increases with the electrical intensity but only to a certain extent with the duration of the experiment, that its formation is not prevented by the envelopment of the poles in glass tubes, and increases considerably with diminution of temperature, and that the ozone produced in air contains small quantities of nitrous compounds, which do not occur in that furnished by pure oxygen.—M. de Saint-Venant presented a note by M. J. Boussinesq on the theory of the flow of a liquid through an orifice in a thin wall.—The ephemeris of the newly-observed comet was communicated by M. Le Verrier from a letter of M. Winnecke's.—An extract from a letter from M. Bourgogne, giving an account of a storm which burst on the 29th of May in the neighbourhood of Alais, was communicated by M. Dumas. The hailstones were as large as small walnuts, and the damage done to the vegetation was immense.—A note on the spirting (*rochage*) of the carburets of iron, and on the sparks produced by these metals, with remarks on some new properties of iron, was read by M. H. Caron.—Some researches on platinum, by M. P. Schutzenberger, were presented by M. H. Sainte-Claire Deville. This paper related to two compounds previously described by the author, namely, chloro-platinite of carbonyle, and chloro-platinite of dicarbonyle, $C\,O\,Pt\,Cl^2$ and $C^2O^2\,Pt\,Cl^2$, which he stated may be regarded as the chlorides of two diatomic compound radicals, platoso-carbonyle (C O, Pt) and platoso-dicarbonyle $\left(\begin{smallmatrix}CO\\CO\end{smallmatrix} > Pt\right)$. He described the behaviour of these compounds when treated with ammonia and ethylene, and also the action of protochloride of phosphorus upon subchloride of platinum, and of perchloride of phosphorus upon platinum.—A long and elaborate paper on tribromhydrine, by M. L. Henry, was read, and a note by M. Gobley, on the action of ammonia upon lecithine, was presented by M. A. Wurtz. He has found that lecithine in presence of ammonia gives origin to margaramide, to phosphoglyceric acid, and to choline.—M. C. Robin presented a note by MM. Lebert and Cohn, upon a new species of *Peronospora*, parasitic on cacti. The species, which the authors named *P. cactorum*, attacked several specimens of *Melocactus* and *Cereus giganteus*, in General

Jacobi's collection, and affected them with a putrefactive disease analogous to that of the potato, in which a *Peronospora* also takes part.—A note by M. E. F. Marey, "On the mechanism of the flight of birds," was communicated by M. H. Sainte-Claire Deville, to which we have adverted in another column.—A note by M. E. Perrin, presented by M. de Quatrefages, contained observations on the scissiparous reproduction of the Naidea, as evinced in the genus *Dero*.—Other papers communicated were a note by M. Yvon Villarceau, "On the decimal division of angles and of time;" a note by M. Morache, "On the use of creosote in the treatment of typhoid fever;" one by M. Pegrani, "On the relation of the sympathetic nerve to the secretion of urine;" and one by M. Duboux, "On a new sign of death."

BOSTON

Natural History Society, March 16.—The secretary read the following observations of Mr. L. Trouvelot, upon the tendency of trees to bend toward the east. In the *Scientific American*, of March 5th, 1870, is inserted a paragraph headed "The Growth of Tree Trunks." It is there stated that a French naturalist had been measuring the tree trunks in a forest, and had found them all broader in the east-west than in the north-south direction, while another arborist of Toulouse, similarly gauging the trees, found the greatest swelling of their trunks towards the east-south-east; the former attributing this want of symmetry to the rotation of the earth, while the latter thinks that it is due to the early action of the sun upon the sap. As this paragraph reminded me of some observations which I made some five or six years ago, and which bear closely upon the same subject, I will present them to the society, thinking they may have some value in a scientific as well as in a practical point of view. While in the country, if we observe attentively the tree tops, we shall soon perceive that many species seem affected by a steady wind, though there is not the least breeze to be felt. Soon we notice that the branches of a great many trees have a general tendency to obey an unknown force which bends their extremities towards the east, or perhaps more correctly, in a direction perpendicular to the magnetic meridian. This bending of small branches cannot be observed so plainly upon all kinds of trees; some species having it well marked in every instance, while other species have it less visible, and even some others not at all noticeable. Most prominent for this peculiarity is the cherry tree, sometimes bending its branches towards the east, from head to foot. Next to this come the maple, the button wood tree (*Platanus*), then the pear tree, then the oak, etc. In the last named it is not always noticeable, though if the tree is isolated from others it is very plain in every instance. With the cherry tree it is so certain, that one could almost invariably determine the cardinal points by looking at the direction of its branches. At first I thought this might be due to the action of the prevailing winds, but this hypothesis was somewhat shaken, when I saw in many instances cherry trees sheltered entirely from the west winds by high blocks of houses within a few feet of them, exhibiting the same phenomenon. Whether this direction of the branches of trees is to be attributed to the prevailing winds, or to the rotation of the earth upon its axis, or to the heat or light of the sun, or again, to terrestrial magnetism, I shall not inquire at present, not having sufficient data to establish any theory. It would be of value, I venture to say, if observers would direct attention to this subject, and see if the direction is the same all over the globe, or if it is a local phenomenon, and also ascertain what species of trees obey this unknown force. It is not only in a theoretical point of view that this observation has some value; there is in it a practical lesson for the cultivators of shade and fruit trees. Soon after my observations, it struck me that something practical could be derived from this truth. All country people know by experience—sometimes dearly bought—that the transplantation of trees does not always succeed, and especially when the transplanted trees have arrived at a certain age. Fruit growers tell us that the cherry tree is one of those least likely to live when transplanted, while the apple tree will almost invariably succeed. My observations on many thousand cherry trees have shown me that this tree is very sensitive to the unknown force, while the apple tree is a great deal less so, and it is very seldom that an indication of bending will be seen. Has not this anything to do with success in transplanting? If, without regard to the direction of the branches of a cherry tree, we set this tree in a position contrary to the one it occupied before, its branches now bending towards the west, then it is plain that the force which gave it the bend is acting in an opposite direction, in consequence of which the tree is suffering. But with the apple tree it is different, as this is far less sensitive; therefore it will not suffer much. Ten years ago I bought a fine cherry tree and transplanted it to my garden, of course without regard to direction; the tree is now living; it has not grown a particle: there has not been one inch of new wood added to the length of its twigs since it was put there; the branches have no bend. Five years ago another cherry tree from the same place was also transplanted in my garden; the tree is now treble the size of the other, its branches are strongly bent east. Why this difference? Was the one set in a suitable position, and the other not? I could not tell. But here is something more positive. Three years ago I saw in Malden twenty beautiful pear trees transplanted with the greatest care; all these trees were of pretty good size, being some years old, and they all bent very strongly. They were set without regard to direction; five or six of these trees happened to be placed in about the position which they must have had when growing, the remainder were set in all directions. I went many times that way to watch the success of this small orchard. The very first year about one half were completely dead. The second year took five more, which had been languishing all the summer. and now five out of the twenty are living and in good condition, and strange to say, these five are those which were set with their branches dipping east. Do we owe their life to the fact that after being transplanted they occupied the same relative position with regard to the points of the compass as before, or is it only a curious coincidence? It is more than I can tell. My experience is not sufficient to allow an opinion in this matter; time will throw light upon the subject.

DIARY

FRIDAY, JULY 1.

GEOLOGISTS' ASSOCIATION, at 8.

SUNDAY, JULY 3.

SUNDAY LECTURE SOCIETY, at 8.—On Man's Cruelty to Man: Rev. Allen D. Graham.

MONDAY, JULY 4.

ENTOMOLOGICAL SOCIETY, at 7,
LONDON INSTITUTION, at 4.—Botany: Prof. Balfour.
ROYAL INSTITUTION, at 2.—General Monthly Meeting of Members.

BOOKS RECEIVED

ENGLISH.—Guide to the Western Alps: J. Ball (Longmans).—Treatise on the Astrolabe of Chaucer: A. E. Brae (J R Smith).—On the Manufacture of Beet root Sugar: W. Crookes (Longmans)—A Glance at some of the Principles of Comparative Philology: Lord Neaves (Blackwoods).—Technological Dictionary (English-German-French), edited by E. Althaus (Williams and Norgate).—Astronomical Observations taken during the years 1865-1869 at the Private Observatory of J. G. Barclay (Williams and Norgate).—Westward by Rail: W. F. Rae (Longmans).

FOREIGN—(Through Williams and Norgate).—Petit traité de physique; 1re fascicule: M. J Jamin.—Bryozoi fossili Italiani; 3rd Contribuzione: D. A. Manzoni.—Reactions Schema für die qualitative analyse zum Gebrauche in chemischen Laboratorium zu Berlin.—Jahresbericht uber die Fortschritte der Chemie: A. Strecker.—Archiv für mikroskopische Anatomie: M. Schultze.—Vorweltliche Pflanzen aus den Steinkohlengebirge der preussischen Rheinlande und Westphalien: Dr. C J. Andrea.—Phanologische Beobachtungen aus dem Pflanzen und Thier-reiche: Karl Fritsch—Annales del Museo Publico de Buenos Aires: F. Savy ed Autor.

CONTENTS