THURSDAY, JULY 14, 1870

THE UNION OF THE ELEMENTARY TEACHING OF SCIENCE AND MATHEMATICS

ATTENTION is being more and more given to the teaching of science as a means of education. The object of education may be regarded as being to help people to think for themselves; and our duty in practically educating people is two-fold; we must supply them both with the materials for thought and with the method of thinking. It is in this latter respect that the scientific education which has as yet chiefly been given appears to us to have been somewhat deficient. Yet this is the most important part; for materials for thought are supplied by nature itself, whereas the method of scientific thought and reasoning is the result of the world's progress, and to inculcate that method ought to be our chief aim. The excellence of science as a means of teaching people how to think, consists in two things: first, that the facts with which it has to deal are real tangible things, and secondly that the method of reasoning which it applies to these facts is accurate; for if, in any part of science, the accuracy of mathematical reasoning is not attained, at least we can always put down our finger distinctly on the places where it is and where it is not attained. Now the error which has been made, and which is constantly being made, in the teaching of science throughout the country, is that these accurate methods of reasoning, and these tangible facts, have been separated from one another. In our boys' schools and elsewhere (with very few exceptions), mathematics are taught wholly as applied to hypothetical cases, if even so much as that. They are thus the driest bones of method, or like a mill grinding without any corn in it. A man who learns method in this way is like a man who learns anatomy from diagrams and not from the human body itself. On the other hand the facts with which science has to deal are, with very few exceptions, almost everywhere brought forward as isolated facts, or their connection is treated in such a way that the true scientific accuracy of reasoning, by which that connection is demonstrated, is either omitted, or receives an altogether unimportant place. In the more advanced walks of science and mathematics the University of Cambridge is perhaps primarily to blame for this separation between the scientific method and the facts of science. But that University is rapidly making amends for its previous errors, and is, perhaps, pursuing as direct a course as possible towards the reunion of these two. Perhaps it may be that our teachers of schools and others, coming mostly from the University, have carried this unfair dichotomy into their own teaching. The fact is that we are only just beginning to awake all through the country to the immense benefit to be gained from scientific teaching, and we could not expect that, until this awakening had occurred, science should have been properly taught. At the University there are certainly the most unbounded facilities for the true teaching of science, in the maturer minds and more advanced mathematical acquirements of our students; but at the same time, the problem of uniting at schools the study of mathematics with that of the facts of nature is exactly the same problem. It is often objected, both by those engaged in teaching and by others, that an extensive knowledge of mathematics is required before we can apply it to Physical Science. This belief is perhaps one of the things which stand most in the way of the true teaching of science; but the belief is entirely erroneous. Of course a person must first acquire a knowledge of the technical forms of mathematical expressions which are to be used; but it has been found that at the very earliest stages of the learning of any mathematical subject, the application of it to the facts of nature may be taught. In this way a new life is given to the whole study, and a comprehension of it is attained, which is, at least in most cases, otherwise quite unattainable. Let us take an instance.

The subject called "Variation" in Algebra is exceedingly uninteresting as it is usually taught; a sharp boy or girl regards it as, for the most part, but a poor and unnecessary substitute for the rules of proportion, and the applications of the rules for variation are learned from such cases and exercises as have no connection with any subject of interest. Yet, as every one knows, in every part of physical science the facts which are eventually to take the form of equations present themselves as problems in variation; as, for instance, "Ohm's laws." It has been observed that where the subject of electricity is taught (and we are happy to say that it is now sometimes taught to boys and girls) such a matter as the establishment of Ohm's laws is left out; if taught they are taught as results which have been arrived at, as of immense importance of course, but, at the same time, the train of reasoning whereby they are demonstrated is omitted, just *because* it involves the use of the rules of variation. And so indeed it will always be, so long as the plan is adopted of teaching people these and similar rules as abstract things. For when rules so taught come to be applied, the mind has to fit itself first into quite a new way of looking at things, and waste of both time and trouble is the result. On the contrary, it has been found that a child whose knowledge of algebra extends only to the elementary rules, may be successfully introduced to the subject of variation by an experimental demonstration of Ohm's laws. By this means both subjects are much better understood, and the child feels himself in possession of quite a new power, whereby not only the intellectual, but also the moral benefits resulting from education are intensified. This is only an individual example, but in all cases it seems clear that the teaching of the rules of mathematics and of the facts of nature should be, and can be, even in the most elementary instances, so adapted as to go hand in hand. An opportune and careful assistance enables the pupil to elicit for himself these rules as the most distinct form of expression of the facts of nature, and these rules enable him to trace the connection between these facts. The elementary parts of physical science teem with opportunities for the elucidation and application of the elementary rules of mathematics, the full force of which is not rightly understood until they are applied.

A question which continually arises in the mind of an intelligent pupil is: "How did this or that piece of mathematics come to be used?" This is a question the need of which ought not to exist; for the pupil should be introduced to each piece of mathematics by the process of, as it were, himself discovering that it is the proper mode of dealing with some natural fact. By this method of teaching, that best of all faculties, originality, is fostered, whereby we mean not the making of new discoveries, but the habit of taking our own view of everything; in other words, the habit of independent thought, which habit is the nurse of Freedom. It is precisely because of its immense power of fostering this habit that we believe the teaching of science properly conducted is such a very desirable part of education. The difficulty of carrying out the joint teaching of the facts of science and the methods of mathematics, lies chiefly in the difficulty of getting men who are able to do it, that is to say, men who have a sufficient acquaintance with both subjects. This is the great temporary drawback to the spread of true scientific education. But as soon as the true character of that education is recognised, this drawback will only be temporary. Here it is, however, that the progress of these ideas at the University is so much to be desired. And here it is that the University is, let us hope, hastening to confer a great benefit on the country, by providing, for the teaching of others, men whose education has itself been carried out on this principle. This is one of the reasons for which we hope that there will be no long delay in the establishment of lectures there of an experimental nature on Physical Science. The benefit conferred by these, however, will not be complete, until it is arranged that the taking of a mathematical degree shall have ensured the knowledge of such subjects; and the possession of a certificate of attendance on some such course of lectures might well be imposed as a necessary preliminary to taking a degree with mathematical honours. Indeed we may hope that the day may not be far distant when some experimental knowledge of Physical Science will be demanded from all Cambridge students at the "little-go," along with the present Latin, Greek, and Mathematics, for this, more than anything else, would conduce to that which is now so eminently desirable—the existence of a body of persons able to carry out this joint teaching.

FORMS OF ANIMAL LIFE

Forms of Animal Life; being Outlines of Zoological Classification, based upon Anatomical Investigation, and illustrated by Descriptions of Specimens and of Figures. By George Rolleston, D.M., F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford. (Oxford: Macmillan and Co., 1870; Clarendon Press Series.) II.

THE second part of this work consists of elaborate descriptions of fifty preparations in the New Museum at Oxford, designed to illustrate some of the typical specimens of the several animal classes. Thus, among Vertebrata, we have a dissection of the common rat, the skeleton of the same, separate vertebræ of the rabbit, the dissection and the skeleton of a pigeon, the bones of the head and trunk of a fowl, a dissection of the common English snake, vertebræ of a python, dissections and skeletons of a frog and a perch, and vertebræ of a cod.

These descriptions will no doubt be exceedingly useful to the author's pupils, but for others statements that "a black bristle has been passed under the aorta," or "a slip of blue paper under a fascicle of one of these muscles" do not afford much help. Moreover, there is a singular absence of directions how the student is to make these preparations for himself; directions which no one could give better than Professor Rolleston. It would surely have been a more desirable course to print this second part separately in the Museum catalogue; and, instead of mere descriptions of plates, drawn from ready-made dissections, to have given a full account of how first to catch, then to kill, and then to dissect and preserve the several animals mentioned in the third part of the book. The satisfactory way in which careful methods of dissection will preserve the whole of an animal for demonstration was lately well shown by the Curator of the Hunterian Museum, who prepared from a single very poor specimen of *Proteles cristatus* the complete skeleton (articulated so as to allow of each bone being removed without disturbing the rest), the stuffed skin, and all the important viscera. Now, methods of dissection of the so-called lower animals, are just what students of comparative anatomy want; and monographs of the anatomy of a single species like those of Bojanus, or of Krause, are rare even in Germany. Therefore knowing the admirable way in which practical zootomy is taught by Professor Rolleston, we had hoped that, following out his motto παντὸς προσθεῖναι τὸ ἐλλεῖπον, he would have described the steps of the several dissections so that other students might profit by his experience. The plates in the present work would serve very well to illustrate such a manual of dissection, especially if aided by such rough diagrams of relations of parts as every lecturer makes for himself on the black board.

Most of the plates in the third part are copied from actual preparations, and are evidently done with great pains; but it would have been well if some notion of the scale on which they are drawn had been added. The bibliography at the end of each description in Parts II. and III. is very valuable; indeed references are fully given throughout the book.

In the account of the dissection of *Helix pomatia* (pp. 48-54), we look with interest for any new facts as to the existence of a capillary systemic as well as pulmonary circulation in the Gasteropoda; since it has been stated that Mr. Robertson (who prepared all the specimens described) succeeded in demonstrating by injections that the supposed systemic lacunæ are only due to extravasation. This, however, appears not to be the case. We may particularly recommend the account given both of the shell and the soft parts of *Anodonta cygnea* (pp. 54-66, and also the description of Pl. V.), and the comparison with it of *Ascidia affinis* which follows. But perhaps the most valuable description is that of *Astacus fluviatilis* (pp. 90-119, and 205-210*), and especially three tables, of which the first compares the post-oral ganglia in *Astacus*, *Scorpio* and *Sphinx*, both at an early and at the adult period; while the second makes a similar comparison between the same ganglia in the *Amphipoda*, *Isopoda*, and *Orthoptera* (an order of insects which Prof. Rolleston regards as the least differentiated, and approaching nearest to Crustacea); and the third gives a view of the homologies of all the post-antennary segments and appendages in the four Arthropod classes. The views of Prof. Huxley are followed where he differs from M. Milne-Edwards, and the grounds of the several comparisons are clearly stated.

* In Pl VII it ought to have been noted that the longitudinal division of the body is not quite complete, so that the *left* eye, antenna and antennule, are seen—all the other appendages belonging to the *right* side.

The Entozoa are illustrated by the *Coenurus* of the rabbit's muscle, which is regarded as probably of "the same species as the one individuals from which are, when in the cystic state, lodged usually in the brain of the sheep, and are the cause of the disease commonly known as the 'sturdy,' 'gid,' 'staggers,' or 'turnsick'" (p. 136). So that *C. cuniculi* is identical with *C. cerebralis*. There are also some diagrams of parts of Tænia in the cestoid state (pp. 246-252), from Leuckart and Van Beneden, with descriptions.

Several new terms are introduced in this work, and most of them are likely to be useful additions to nomenclature. Among them are the words "proctuchous" and "aproctous," which we have only seen before used to distinguish the *Turbellaria* with an anus from those without one. Would it not be possible to substitute *Brachionopoda* for the barbarous word "Brachiopoda," which is moreover too near to "Branchiopoda?" Several terms in common use are given by Dr. Rolleston in an improved form; and we think him quite justified in substituting *Myriopodia* for Myriapoda, *Annulata* for Annelida (which is only a pseudo-classical form of "Les Annelides," the name invented by Lamarck), and *Hedriophthalmata* for Edriophthalmata —though if this last change be made, it will be as well to write "*Hedræophthalma*," since the root must be ἑδραῖος, not the doubtful diminutive ἕδριον, and the analogy of μονόφθαλμος, μυριόφθαλμος points out the true form of the termination. In the same way the word *Echinodermata* (which was first correctly applied by Stein to the *shells* of Echini) and *Pachydermata* ought to be written Echinoderma and Pachyderma, especially as the latter form is actually used by Aristotle. But there are probably too many words of this termination for the change to be easily accomplished. Useless synonyms, however, ought undoubtedly to be abandoned; such troublesome words, for instance, as Actinozoa, Ascidioidea, and Bryozoa, ought to yield to *Anthozoa*, *Tunicata*, and *Polyzoa*, for reasons of convenience, euphony, and priority.

The student will find it useful to note the following errata, in addition to those indicated in the book itself.

P. xiv. *monophydont* for *monophyodont* and *Montremata* for *Monotremata;* p. li., *furculum* for *furcula*, repeated p. 21 *bis* and p. 22; p. ciii., *classes Brachiopoda* for *class Brachiopoda;* p. 21, *obtusator* for *obturator;* p. 25, *Wirbel thiere* for *Wirbelthiere;* p. 35, *Körperban* for *Körperbau;* p. 78, *differs from the imago* for *differs from the larva;* p. 136, and again pp. 241 and 251, *Caenurus* for *Coenurus;* p. 160, *coenosare* for *coenosarc;* p. 224, *two sets of them and two rays respectively* for *two sets of three, &c.;* p. 252, *Thudichen* for *Thudichum;* and, in the index, *Vesicular* should read *Vesiculae*, while the reference to Prof. Turner will be found at p. 1. of the second part instead of p. i. of the Introduction. Lastly, at p. 251, line 10 from the bottom of the page, the figure 6 is misprinted for the letter *b*.

It is much to be hoped that a second edition of this thorough and painstaking work will soon be called for; since nothing would better prove the increased number of serious zoologists in this country. Should its learned author then see fit to unite the second and third parts together, and to add somewhat full directions for dissection, I venture to think that the book will be made even more useful than it already is.

P. H. PYE-SMITH

NEW ATLASES

The Complete Atlas of Modern, Classical, and Celestial Maps, together with Plans of the principal Cities of the World, constructed and engraved on steel under the superintendence of the Society for the Diffusion of Useful Knowledge, and including all the recent geographical discoveries, compiled from the latest and most authentic sources. Accompanied by alphabetical indexes to the modern and classical maps. 218 maps and plans.

The Family Atlas, containing 80 Maps, constructed by eminent Geographers, and engraved on steel under the superintendence of the Society for the Diffusion of Useful Knowledge, including the Geological Map of England and Wales, by Sir R. I. Murchison, F.R.S.; the Star Maps, by Sir John Lubbock, Bart., F.R.S.; and the Plans of London and Paris, with the new discoveries and other improvements to the latest date, and an Alphabetical Index.

The Cyclopædian, or Atlas of General Maps, with an Index of the Principal Places in the World. 39 maps.

(London: Edward Stanford, 6 & 7, Charing Cross. 1870.)

AT the present time not a week passes which does not more and more enforce the necessity of everyone of us having an atlas of some sort or another to refer to, and, in fact, it may be said that in these days of rapid locomotion and intercommunication with every part of the planet, an atlas is the corner-stone of a library, even if that library otherwise consist merely of a Dictionary and a Blue Book, Imperial Calendar, or Post-office Directory.

Take the last six months. We have all of us been burning to know the ins and outs of that part of Africa, so far as they have been mapped, which we hope that Livingstone is still exploring for us. The Suez Canal has not only, to the amazement of school boys, made Africa an island as well as a continent; but children of a larger growth have had to talk about Port Saïd, the Bitter Lakes, and the Sweet Water Canal, thereby opening up a new region of minute geography; while the Pacific Railroad, now a *fait accompli*, has carried us at once into a part of the world about which the majority of us had thought but little; and we might easily go on multiplying other instances.

Geography, in fact, is now not only "one of the eyes of history," but it is the "eye" of every-day life among all Anglo-Saxon communities; and from this point of view it is satisfactory to see our foremost geographers and purveyors of maps keeping pace with the times, including "all the recent discoveries" in their publications, and bringing this very important information fairly within the reach of all. Let us add, that it is also satisfactory to see them little by little approaching the German standard of map-making, which, to speak candidly, they have not all reached.

We shall clear the ground for what we have to say of the atlases now under notice, if we state that the maps in all of them belong to the series brought out some time ago by the Society for the Diffusion of Useful Knowledge; that the two first on our list are of the size of the maps, and are for the library, while the third contains the maps folded, and will suit either a school-boy or a family of small geographical requirements. The Complete Atlas differs from the Family one in having classical maps and a large number of plans of cities; the modern maps being pretty much the same in both; both also contain maps of the stars. Sir Roderick Murchison's geological map of England and Wales, however, is to be found in the Family Atlas only. All of them contain a most valuable index of places, so that we have on the whole a very practical gradation to suit all requirements, the *quality* being the same but the *quantity* varying.

There is one very admirable point in the arrangement of the Complete Atlas which at the same time reminds us that it is not so complete as we are sure Mr. Stanford will make some edition of it in the more or less remote future. Side by side with the modern (politically divided) map we have the ancient (politically divided) map of the same area, and in this point the Complete Atlas will commend itself to all scholars; but we miss very much indeed the physical maps of the larger areas, and in the interests of physical geography we feel bound to insist strongly on this point, because we are convinced that the importance of such maps to those who want a large atlas is becoming so great that it will not be borne that they shall be relegated to a separate volume.

By many, and those especially who are content with the modern world, the Family Atlas will commend itself by its index-like arrangement, by which the names of all the maps are visible down the side, and the sides of the foremost maps being cut away, any map may be at once turned to.

This much premised, we may state that we have examined the maps and plans very carefully, and find them as a rule as good as any English maps extant, and honestly brought down to date. Mr. Stanford deserves great credit for the admirable and careful way in which this has been done, and we say this the more strongly because we know the immense labour and expense involved in altering map plates from time to time. Of course, in some cases, it has been simply impossible to alter the plates, the alterations have been too great. Take for instance the plans of New York, extending to Forty-second Street only, and Boston, in which the waste space shown in the map, west of the public garden, is now covered with houses. In other cases all the care has been displayed in the detail map, the general map having escaped revision, or *vice versa; e.g.*, in the map of British North America, Russian America is retained, while in the general map it is correctly omitted; in the general map of Canada and the United States the chief town of Iowa is shown as Iowa City, while, in the detail map, Des Moines is correctly given. We could have wished too to see Patagonia, a time-honoured name, divided, as it really is, between the Argentine Republic and Chili; and we have an idea, too, that by an Order in Council, or some such terrible enactment, the "improper" name of Van Dieman's Land has been altered to Tasmania! We notice these points, not as blemishes by any means, but as indications of a more or less minute revision which we are sure Mr. Stanford would have otherwise undertaken, of a collection of maps of which English geography may be proud.

OUR BOOK SHELF

Birds of Marlborough, being a Contribution to the Ornithology of the District. By Everard F. im Thurn. (12mo. pp. 117. Marlborough and London, 1870.)

THIS unpretending little book affords an additional piece of evidence, if more were needed, that science in some form or other is making its way into our schools. A few years ago it was well remarked by one who had given no small attention to the matter, that the relations of the universities and public schools, as regarded science, formed a "vicious circle"—on the one hand the public schools demurred to its encouragement because it did not "pay" their pupils when they reached the university, and on the other the universities hesitated about rewarding scientific studies because they were pursued by intellects comparatively inferior to those which were devoted to the older branches of learning. This state of things clearly admitted of a remedy; either great power of itself could make the first step; but it was certainly the duty of the universities to take the lead in moving. It must depend on them, and on them alone, to alter and improve the whole higher education of our countrymen, for the curriculum of any public school is almost exclusively prepared with reference to the requirements of the universities and the rewards for proficiency that they offer.* They have but to declare that their emoluments and privileges are accessible to excellence in every branch of human knowledge, instead of confining these encouragements to some very few alone, and leave the public schools to respond to the call. With skilful gardeners these nurseries will speedily grow the plants required; the germs are already there, and under the sunny smiles of pedagogic favour and the golden rain of prizes, vigorous saplings will be transplanted to the Groves of Academe, there to hold their heads as high as their rivals from the primæval forests of classics and mathematics, and (may we say?) to be finally of greater utility.

If Mr. im Thurn's book, as might be expected from the performance of so youthful an author, does not contain any addition to science, it will, of course, be interesting to Marlburians as the work of one who has just ceased from being a schoolboy; but its chief value lies in the fact of its indicating the presence of the promising germs we have mentioned above, of the excellent forcing pit found in the Marlborough College Natural History Society, and of the skilful gardener, Mr. T. A. Preston.

Gymnastics for Ladies. Madame Brenner.

ALTHOUGH many of our large towns are now provided with gymnasiums at which ladies' classes have been established, the subject is but little appreciated, especially, in some more important cases, among the ladies themselves. There can be no doubt that for growing girls a large airy room, provided with suitable apparatus, and where a loose easy dress is a necessary condition, must be advantageous, if the exercises performed are such as to induce emulation without over-exertion. When we consider at how much earlier an age "romping" is prohibited to girls than to boys, and how little there is in the routine of a girl's life to correspond to the cricket and rowing which form the best part of her brother's recreations, we think the fact offers a very probable explanation of the increasing languor and delicacy of the ladies of the period. Breadmaking and other manual duties are being superseded by reading and preparing for examinations, and we must, therefore, look to artificial means to preserve a just balance between mental and physical development.

Madame Brenner's book is little more than an advertisement of her class in Bruton Street, being a description of those exercises which she teaches, enlivened by rather severe criticisms of those which others teach. Still we hope her book will find many readers, as the graceful

* See Rep. Brit. Assoc. Dundee, p. xliv.

illustrations, the strains of lively music which we are told accompany every movement, and, above all, the repeated assurance that the ladies need do no more than they like, will all tend to persuade parents and daughters that gymnastics are very pleasant and desirable.

On Eozoön Canadense. By Professors King and Rowney. 8vo. (Dublin, 1870.)

THIS reprint from the Proceedings of the Royal Irish Academy, treats of a controverted subject of considerable interest to geologists and zoologists, namely, the nature of certain Canadian and other serpentinous limestones in which Logan, Dawson, Sterry Hunt, Carpenter, Jones, Gumbel, and others believe they find definite traces of a foraminifer known as *Eozoön.* Great difference of opinion on the subject under notice has been expressed during discussions before learned societies and in memoirs written by geologists, some seeing under the microscope good proofs of the presence of foraminiferal structure; and these observers are mainly rhizopodists well acquainted with the peculiar structures of shelled protozoa, others finding nothing but inorganic fibres, globules, flocculi, &c., of mineral matter in both the Canadian and any other similar serpentinous marbles. Among the latter disputants are Doctors King and Rowney; and in the paper before us there are some new descriptions and figures of specimens illustrative of the structure of certain ophitic rocks from different countries, and likely to be of use to "eozoönal" students, enlarging their field of observation, and aiding them, perhaps, in arriving at definite conclusions. The figures, however, are little better than diagrams, and cannot help the student much. The paper is largely composed of criticisms on the researches and remarks of others, in a highly disputatious form, and not enriched with anything new to those who have thoroughly studied the matter, either mineralogically or from a zoological point of view. The following important facts do not appear to be recognised by the authors: first, that ophites, on the one hand, may not be really "eozoönal" and yet have mineral structure resembling in one point or another what occurs in *Eozoön;* secondly, that true eozoönal rock is often so greatly crumpled up in its metamorphic state, that patches only of the organic structure are found here and there amongst the somewhat similar ophitic mass of granules and fibres.

Die Ophthalmologische Physik, und ihre Anwendung auf die Praxis. Von Dr. Hugo Gerold, of Giessen. Part II. (Vienna, 1870. London: Williams and Norgate.)

THE advances in the department of Ophthalmology have of late years been so rapid and important, that either thoroughly-revised editions of the standard works or altogether new books have become a sheer necessity. The volume before us comes under the latter category, and is the work of a gentleman well known as an able physicist. The present part is occupied with the Dioptrics of the Eye; the defects in it that are due to spherical and chromatic aberration; the terminology employed to indicate the different functional relations of the several parts to one another and to light, as æquatorial, median, and sagittal planes, axes, visual lines, field of vision, angle of elevation, &c.; the principles of perspective and of the construction of the microscope, the ophthalmoscopic investigation of the eye, and the adaptation of convex and concave lenses for hypermetropia or myopia, and lastly, a section on light and colour. The parts we have read appear to be clearly and intelligibly given, and with something like French method and order. The mathematical formulæ introduced are not beyond the comprehension of an ordinary well-instructed reader, and the diagrams are numerous (123 in number) and instructive.

H. P.

LETTERS TO THE EDITOR

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

Prof. Pritchard and Mr. Proctor

IT has been pointed out to me that Prof. Pritchard, engaged as he is in many important avocations, may quite unwittingly have misjudged my treatise on the Plurality of Worlds. I readily (eagerly) admit this, and also that, in this case, I owe the esteemed Savilian professor an apology for suggesting that he has intentionally wronged me.

The matter is now reduced to a simple issue. I have submitted considerations which are sufficient to convince Prof. Pritchard that his critique is not just. If he withdraws his unfavourable comments, as resulting from accidental misconception, I shall be bound to apologise for too hastily charging him with deliberate unfairness. If he will not, I cannot truthfully withdraw my objections. I will not endure to be represented as speaking severely (and by inference unfairly) of men for whom I have (and have expressed) a most sincere and unqualified admiration—of such men, to wit, as the Herschels, Tyndall, Lassell, Balfour Stewart, and Sir W. Thomson.

RICHARD A. PROCTOR

Whence Come Meteorites?

I HAVE read, with great interest, in the number of June 2nd of your journal the article which Mr. N. S. Maskelyne has devoted to the examination of my theory on the Origin of Meteorites. I request permission to offer some observations on the criticisms of that learned mineralogist.

Although Mr. Maskelyne concludes by saying that, in his opinion, I have not attained the end which I had proposed to myself, I will attempt to show that my system has, in fact, perfectly resisted his attacks.

In truth, the views I have been led to take on the subject of meteorites are not by any means a simple fruit of my imagination. I have been led to them by the observations of material facts easy of verification; and it is only in the background, so to speak, that I have brought under consideration different consequences, which may certainly be matter for discussion. Now, in Mr. Maskelyne's argument, he has given the place of honour to these secondary considerations, whilst he has left the real substance of the question completely in the shade. A few lines will suffice to justify my assertion.

The chemical and mineralogical study of the specimens which compose the rich collections of meteorites at the Museum of the Jardin des Plantes has made me acquainted with *polygenic* masses—that is to say, masses formed of angular fragments soldered together, but possessing each one such decidedly separate characters that it is impossible to suppose that they were originally produced in the forms and in the relative positions which they present at the present day. These clastic meteorites had been previously studied; but not, as far as I am aware, from the point of view at which I have placed myself.

From the studies and experiments I have made on this subject results the indubitable fact that the fragments, the union of which constitutes various clastic meteorites are, each one, completely identical with well-known monogenic meteorites. It is thus, that the clastic meteorite of St. Mesmin (May 30, 1866) contains angular fragments rigorously the same in every respect as those which would be produced by breaking up the meteorite of Lucé (Sept. 30, 1768); fragments soldered together by a dark coloured cement exactly similar to the substance which forms the principal mass of the stone of Limerick (Sept. 30, 1813). It is thus also that in the same cement, the meteorite of Canellas (May 14, 1861), contains fragments of a rock impossible to distinguish from that of which the mass of Montrejeau (Dec. 9, 1858) is a specimen.

How is it possible to understand these positive facts without having recourse to the explanation, so evidently true of terrestrial fragments? For fragments of two distinct rocks to be found associated in one clastic mass, it is absolutely necessary that these two rocks should come from a region where they were in connection. Thus, on one hand, the rocks of Lucé and of Limerick were in connection; thus, on the other hand, the rocks of Montrejeau and Limerick were in connection; then, in conclusion, the rocks of Lucé and of Montrejeau were in connection.

By the side of this first assemblage of facts, of which the meaning seems to me not doubtful, I find another of at least equal importance—that of meteoric rocks evidently eruptive.

The meteoric iron recently discovered in the cordillera of Deesa, in Chili, having been submitted by me to a careful analysis, both chemical and mineralogical, appeared to me clearly to be formed from the mixture of two meteoric rocks, known, each of them, by masses of which they are entirely constituted. The one, stony and black, fell at Sétif, Algeria (June 9, 1867); the other, metallic, constitutes the mass of iron found in 1828 at Caille, in the south of France. Besides this, the metallic portion of the iron of Deesa, in which the black angular fragments are encrusted, has manifestly preserved the character assumed by the iron of Caille when it is subjected to fusion, so that the mode of formation of the Chilian mass cannot be considered doubtful. We must believe that on a globe, large enough to have been the seat of considerable pressure, masses of iron from Caille, still melted, were injected into superposed layers of Sétif rock so as to give birth to dykes, identical, except in their mineralogical nature, with those which the crust of the earth everywhere presents to our view.

These two orders of facts, which seem to me indisputable, being admitted, there remains to explain how fragments of polygenic conglomerates, or of dykes, can wander through space, and here only it is that the hypothetic part of my work begins.

From what precedes the meteorites in question are, by definition, planetary fragments. It remains to learn how the rupture of the planet whence they come can have taken place. On this it is evidently impossible to argue with any certainty.

Nevertheless, it appears to me that several considerations may greatly facilitate a choice among the different explanations which present themselves to the mind.

In the first place the *unity of composition* of the solar system, mentioned by Mr. Maskelyne, is evident.

Secondly, it is manifest that in the same system there exists a perfect *unity of geological phenomena*.

Lastly, but this, perhaps, has less weight, it appears to me that we should have recourse to accidental causes to explain natural phenomena only when every other means is forbidden.

This said, I observe that without making any other hypothesis than that of Laplace, we arrive at the conclusion that the stars tend of themselves to become broken. The earth is cracked in all directions; these fissures, designated as *faults*, are known to everyone. Little by little, as they form, they become reunited by the injection of an internal melted cement. But if the supply of this cement failed, the molecular operation which has opened the faults would still continue its action to enlarge them; we observe this in the moon, which, far more advanced in refrigeration, manifests by its fissures a phenomenon hitherto unknown in our earth. Evidently if we suppose to have been formed at the same time as the moon, a much smaller globe, that globe will have arrived actually at a state of cold far more advanced than that of the moon; and the fissures, excessively multiplied, and increased in depth and in width, may have finished by reducing the globe into separate fragments.

We have no positive proofs that such events have really happened, but is it not a very simple hypothesis to admit that meteorites, which bear so evidently the impress of a detritic character, may have had such an origin?

It is very probable that once parted from one another, the fragments are scattered along the orbit, and it is evident that they will tend progressively to approach the central star, so as to finish by falling on its surface under the form of meteorites.

Now, whether these fragments have been sorted or whether they have not, whether this sorting, if it exists, be or be not in accordance with that which the facts of observation have seemed to point out to me; I consider the question as entirely secondary as regards the general theory, and I request permission, in order to keep within the limits of the present discussion, to lay it absolutely aside for the present. I will simply repeat, in concluding this note, already somewhat long, that positive facts alone have served as the basis of my theory, and that the different circumstances on which my opponent has so learnedly insisted, possess for me but a secondary importance.

At the same time, I sincerely congratulate myself in the fact that my work has had the good fortune to fix the attention of a scientific observer so well placed as Mr. Maskelyne for submitting the mineralogical and lithological part of it to a severe verification.

DR. STANISLAS MEUNIER, Aide Naturaliste au Muséum
23, rue de Vaugirard, à Paris

Monographs of M. Michel Chasles

PAR une lettre inséréee dans le No. 36 de NATURE, page 190, M. C. Ingleby fait appel aux lecteurs de votre Revue pour obtenir quelques renseignements au sujet de "l'Aperçu historique" de M. Chasles, imprimé à Bruxelles en 1837. Le travail, qui porte pour titre exact: "Aperçu historique sur l'origine et le développement des méthodes en géométrie, particulièrement de celles qui se rapportent a la géométrie moderne," a été publié par l'Académie royale des sciences de Belgique dans le tome xi. de ses "Mémoires couronnés et des savants étrangers" (in 4to.), et il est très-difficile aujourd'hui de s'en procurer des exemplaires. Toutefois, M. Ingleby pourra s'adresser, pour consulter ce mémoire, à la Société royale de Londres, qui doit certainement le posséder dans sa Bibliothéque. Voici d'ailleurs la liste des établissements scientifiques de Londres qui ont reçu cet ouvrage a l'époque de sa publication: Société royale, Société astronomique, Société royale de littérature, et Société linnéenne.

J'espère que ces details pourront être utiles à votre honorable correspondant.

Bruxelles, le 8 Juillet A. LANCASTER,
Attaché au Secrétariat de l'Académie royale des Sciences de Belgique

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IN reply to Dr. Ingleby's note I may state that many papers by M. Chasles on various subjects in the history of Mathematics, are to be found in the volumes of the *Comptes Rendus* for 1837, onwards. His "Aperçu Historique" &c., originally appeared as a special volume of the Transactions of the Brussels Academy, but was sold as an independent work. It appeared in quarto, and was published in 1837. Like his "Traité de Géométrie Supérieure," it is very rare, and fetches an enormous price. Mr. Quaritch is, perhaps, the most likely bookseller in London to be able to procure it. The German translation by Sohncke is comparatively cheap, and may be readily obtained through Messrs. Williams and Norgate.

Torquay, July 9 G. E. DAY

The Specific Heat of Mixtures of Alcohol and Water

IN the report of the papers read at the Academy of Sciences, Paris, June 13, which appears in NATURE for June 30, it is stated that MM. Jamin and Amaury presented a note on the above subject, in which they point out, apparently as if it were something new, that the specific heat of some of these mixtures rises even above that of water.

Now, more than two years ago, March 26, 1868, we communicated a paper to the Royal Society giving the specific heat of various mixtures of alcohol and water, and drawing special attention to the remarkable fact that the specific heat of these mixtures is not only above the calculated mean specific heat, but that in all those of less strength than 36 per cent. of alcohol, it is higher than the specific heat of water itself. A knowledge of this fact should therefore be old by this time.

An abstract of our paper is printed in Proc. R. S., vol. xvi., p. 337. Subsequently we examined this and various other properties of similar mixtures more in detail, and communicated our results to the Royal Society in a second paper, an abstract of which is printed in Proc. R.S., vol. xvii., p. 333, and the paper in full in Phil. Trans. for 1869, Part II., p. 591.

The insertion of the above in the next number of your valuable journal will greatly oblige A. DUPRE & F. T. M. PAGE

Westminster Hospital, July 2

Geographical Prizes

HAVING been chiefly instrumental in causing prize medals to be offered by the Geographical Society for competition among the chief public schools, I do not like Mr. Wilson's letter in your last number to pass without comment.

Geography may be, to use his words, a subordinate branch of education, but I maintain that it is so only in the sense that it underlies a large part of liberal knowledge. It underlies the study of history. For example, I do not see how a boy could thoroughly understand Bible history without having acquired a very vivid conception of the geography of Palestine, and the same is true for all other histories, ancient and modern. It follows, as a matter of fact, that geography is incidentally taught to a considerable extent in schools, and I am sorry to say it is sometimes very ill-taught, as we learn from the reports of our examiners, but through some omission, not easily to be explained, if it be not the effect of a mere accident, geographical proficiency has never hitherto been adequately encouraged. Consequently, the Geographical Society has thought it right to step in to supply the needful encouragement. There is another good reason for the interference of the Society, in the fact that facilities of travel have rendered our interests much more cosmopolitan than formerly, while the public schools of the old-established type, have made no corresponding change in their curriculum. Mere youths now-a-days have exhausted the grand tour of two generations back, and a year or two of early manhood is often spent in America, Australia, and India, while books of travel load our library tables. It seems monstrous that a so-called liberal education should not qualify men to journey themselves, or to read the journeys of others, in an intelligent manner.

Mr. Wilson remarks, and his remark deserves respect, that the masters of Rugby were almost unanimous in rejecting the invitation of the Geographical Society, but I can fairly retort that other scholars no less practised in education and no less competent to decide, pronounced our system of prizes to be a valuable and much-needed institution.

It would be easy to write at great length in support of what we have done, and I might perhaps be expected to say something on the respective objects of the political and physical geography prizes, but I do not wish to provoke a discussion in your pages, because I am on the point of going abroad and should be unable to take further part in it. FRANCIS GALTON

"Kinetic" and "Transmutation"

1. WHEN, in 1864, I wrote for the *Reader* the history of the Baconian Philosophy of Heat, I found in use, in connection with the subject, the term "dynamical theory of heat," in English, which was employed as an equivalent for the expression "mechanische Warmetheorie," current in German. The word "dynamical," already so vague from frequent abuse, corresponded but little, when used in its proper meaning, to the real intent of the theory in question; and the same remark applies, with at least equal force, to the word "mechanisch," even wider in its scope and as often misused. I was thus led to adopt the word "Kinetic," to supersede the above; and that in preference to the current word, "cinematic," which, in conjunction with "theory," would imply a tautology.

I am glad to see that Sir W. Thomson and Professor Tait, in their treatises on Natural Philosophy and on Heat, as well as in some remarkable papers on Atoms which have appeared in NATURE, frequently make use of the same word, "Kinetic," in connection with the theory of heat and of gases, as also in conjunction with "energy." Instead of the expression, "actual energy," originally introduced, I believe, by Mr. Rankine, Sir W. Thomson and Mr. Tait employ the term "Kinetic energy;" and from various motives, linguistic as well as strictly scientific, I venture to think that the original wording of Mr. Rankine in the case of "potential energy," should be likewise superseded, viz., by "dynamic energy."

2. In the *Philosophical Magazine*, I have been rated, indirectly, by Professor Challis, (for no mention is made of my name in connection with the subject), for having applied the word "transmutation" to rays, without recalling the fact of his having done so before me. I considered the expression "transmutation of rays" as the abbreviated and thoroughly English rendering of the words, "change of the refrangibility of rays, or light," used by Professor Stokes; and as such, requiring no authority but the precedent furnished by the existence of the analogous expression of "transmutation of matter." If, however, an authority had to be cited, it would have been Euler, in whose "Nova theoria lucis et colorum" (Opusc. var. argum.) the following passage occurs:—"Cum igitur a corporibus rubris radii tantum rubri, et a violaceis violacei ad nos pertingant, etiamsi radii albi in ea incidissent, manifestum est istam transmutationem a sola reflectione proficisci non posse."

As I have returned to this subject, I may be permitted to express my astonishment that Professor Challis, who thought it due to him that his name should be mentioned for being the author of the expression "transmutation of rays," should have on his part omitted, in speaking of the transmutation of Herschelic rays into Newtonic, a reference to my own share in the *res gestæ*. When I see the same thing being done in so widely circulated a treatise as that of Mr. Brooke on Natural Philosophy, and in one intended for even more popular reading, reproducing the teaching of the Polytechnic, I might think of entering a protest, if experience had not convinced me of its uselessness.

C. K. AKIN

Parturition of the Kangaroo

I BEG leave to call your attention to certain comments in your issue of the 23rd of June on the proceedings of the last meeting of the Royal Geological and Zoological Societies of Ireland. It is usual when parenthetical observations are made in any journal without the customary affix "Ed." to ascribe them to the printer's devil. Now, your devil, in commenting on an *imperfect* report of your Dublin correspondent, would lead your readers erroneously to infer that I had adopted the ideas which he has been pleased to call "absolute nonsense," and takes me to task for saying "that the actual passage of the fœtal kangaroo from the uterus to the pouch was not yet proved;" he himself stating that my remarks were "in contradiction to the facts observed by the late Earl of Derby's father or by the present Professor Owen." Now, a critic calling in question the words of others should be careful of his own. No facts on the subject were observed by the late Earl of Derby's father, and Professor Owen, after elaborate arrangements for the observation, states that "as parturition took place in the night, the mode of transmission to the pouch was not observed." (Phil. Trans. for 1834, p. 344.) There have been four observers in this matter especially worthy of being noticed:—(1) the keeper at the Zoological Gardens, Knowsley, who, according to Lord Derby's statement, saw the young kangaroo born, and that it was placed in the pouch by the paws of the mother (Proceedings of Zoological Society for 1833, p. 132); (2) Professor Owen, as referred to above; (3) Mr. E. G. Hill, who, at thirty yards' distance, saw the kangaroo with her mouth take up what he thought was a stone, open the pouch with her paws, and place it in the marsupium, and that he shot the animal and found a newly-born fœtus in the pouch (Proceedings of Zoological Society for 1867, p. 476); (4) M. Jules Verreaux, who is mentioned by M. E. Alix as having seen the kangaroo remove the fœtus from the vulva with her mouth, and place it in the pouch (Annals of Natural History for 1866, p. 316). These all differ as to the actual facts observed, and would seem sufficient to justify me in the statement I had made. That Professor Owen does not consider the question settled, may be inferred from his concluding observations on the subject, "whether the circumstance of the parturition is constant, viz, the dropping on the ground, or whether the fœtus may occasionally be received by the mouth from the vulva, I am disposed to regard as a matter for further observation; but the main fact of the conveyance of the fœtus to the pouch by means of the mouth may now be held as the more probable (at least the more usual, if not the constant) way in the genus Macropus" (Proceedings of Zoological Society for 1866, page 382). I refrain from any comments, but I thought it right to remonstrate against statements which I felt were injurious to me, to the Society to which I have the honour to belong, and to the advancement of science. JOHN BARKER, M.D.

Dublin, July 1

The Extinction of Stars

IF you will kindly permit an amateur to rush in where astronomers fear to tread, I shall be glad to offer a few remarks on the above subject.

The progress of science enables us to trace, with a probability almost amounting to certainty, the career of a star from its birth; from the most diffused condition of its parent nebula; through the stage of primary agglomeration when it shines as our sun; through the process of cooling into a dim and cloudy spheroid, such as Jupiter or our earth; until cold rules supreme, and the once glowing orb rolls on, barren as our moon.

But when we have reached this stage, we have by no means done with the star. It must continue on its course, and, though in obscurity, it must retain its momentum and its attractive force. Our sun will thus one day travel in darkness, attended by a cohort of funereal planets, and perpetual night will reign over the solar system. This result appears to be but a question of time, and we are, therefore, led to the consideration that many systems must, in all probability, be already extinct, and wandering unnoticed. But as extinction is a gradual process, there will be multitudes of stars in various stages of dimness, and the brilliancy of any orb, its "magnitude" in fact, will therefore depend on its age, quite as much as on its size or distance. On this view, Sir W. Herschel's method of "star gauging" cannot be relied on for a correct determination of the actual shape of the cluster called the "Milky Way," as instead of taking the average of brightness only, as an indication of the average of distance, we have to superadd the average of age. Now, the smaller the star, the more quickly will its light die out, and, therefore, the necessary extent of our galaxy is immensely reduced; in other words, it appears that while the space separating us from the nearer stars, for which parallax has been obtained, remains of course unchanged, the computed distances of those hitherto considered to be farthest off, will be much lessened, as there appears to be no reason for concluding that telescopic stars are necessarily more distant than bright ones for which we cannot obtain parallax, but simply that they are older, or smaller, or both, and therefore dimmer.

Mr. Proctor, in "Other Worlds than Ours," argues that as telescopes barely reach the outermost stars of our own cluster, therefore it is impossible that they should reach to and resolve clusters constituting other systems and lying at distances enormously greater, and therefore that the resolvable nebulæ must lie within our galaxy. If my idea that the stars of our cluster which the telescope shows with difficulty, are not distant but dim, be correct, Mr. Proctor's argument appears to lose its force.

It will be readily allowed that if the light of the stars be fading away, a vast number may have already become extinct, and that it is indeed possible that the orbs now visible may be but a small surviving remnant of far greater multitudes which once illumined the heavens. If our cluster then be much reduced in extent, and its constituents be largely increased in number, it would follow, I imagine, that the chances against collision would be much reduced, and it then becomes less difficult to conceive the possibility of such an event having occurred in the case of the recent outbreak in τ Coronæ Borealis, especially if it were caused by the unobserved approach of an extinct body. This outbreak is usually ascribed to a sudden conflagration of hydrogen, the star being, as Prof. Roscoe says, "on fire." But a star self-luminous surely must be always on fire, and if it contain hydrogen, that gas must be in a state of constant conflagration. The temporary brilliancy of the star seems rather such as would be occasioned by a collision with some comparatively small body, whose impact was yet sufficient to generate heat enough to accomplish its own disintegration and ignition. Let us suppose that collisions are possible, and that their frequency is merely a question of the chances. What would be the consequences of such an event? I imagine that they would depend chiefly on the relative momenta of the colliding bodies; that if one were very much larger than the other, and the velocities high, the temperature would be raised sufficiently to dissipate the smaller into gas, while merely heating, or possibly liquefying the larger. If the bodies were nearly of a size, and their momenta were great, possibly both would be reduced to a gaseous condition; in either case their tendency would be to form ultimately a body equal in weight to the sum of its two constituents. Either the larger body would annex the smaller, or, if both became nebulous, the fervid gases would radiate their heat and contract anew into a system possibly containing a sun and planets.

Again, supposing that two bodies approach each other in such a manner as to avoid a collision, that is, so that their mutual gravity causes them to leave their paths and revolve round each other, we should have the explanation of the existence of double, treble, multiple stars; we should also understand how it happens that some stars (Sirius, for instance,) are accompanied by non-luminous orbs. Also, it would seem that if extinct stars are really far more numerous than is generally supposed, the theory which regards the revolution of attendant dark bodies as one cause of the variability of certain stars, receives fresh support.

Thus, in the course of time, nebulæ would form suns, suns would grow cold, or, while yet glowing, would come into contact and combine with other suns, till gradually space would be peopled with suns, larger and larger, but less and less thickly strewn. Pursuing the idea, we arrive at a period when all the stars of each galaxy shall become agglomerated into one mighty globe—nay, when all these vast galactic suns shall come together and form one solitary orb, in which all the matter once scattered through space shall be collected, accomplishing its successive fates as a sun without a system—a world without a sun—a cold and naked ball.

EARDLEY MAITLAND

Why is the Horse Chestnut Tree so called?

DURING the spring this tree is the ornament and pride of our public and private parks. In "Woodland Gleanings" it is stated to be a native of the north of India, and is supposed to have been introduced into England about 1575.

Our observant forefathers have given it the very significant name of *Horse* Chestnut (*Castanea Caballina*),* to distinguish it from all other species of chestnuts. The *reason* for so doing I have never seen stated in print; but from the three specimens of cuttings from a branch of this tree which I enclose, it will be very manifest. All over its branches, at every bud, can be seen what at a glance will be taken for an exact conformation of the *foot of a horse*, exhibiting the hoof, the nails of the shoeing, the fetlock-joint, &c., in marvellous miniature, some, of course, better developed than others. This curious freak in nature's vegetable kingdom, has, no doubt, been the origin of our nomenclature of this tree; and it would be an interesting point of philological inquiry to ascertain *whether or not its native Asiatic name has incorporated or associated with it that of the horse?*

I write with the view of eliciting information on this point, and with the hope, too, that some of your botanical contributors will throw further light on this peculiarity.

EUGENE A. CONNELL

Fall of an Aerolite

A LETTER of the year 1628, "sent by Mr. John Hoskins, dwelling at Wantage, in Berkshire, to his son-in-law, Mr. Dawson, a gunsmith dwelling in the Minories without Aldgate," and preserved among Nehemiah Wallington's Historical Notices (i. 13) contains the following narration:—

"On Wednesday before Easter, being the ninth of April, about six of the clock, in the afternoon, there was such a noise in the air, and after such a strange manner, as the oldest man alive never heard the like. And it began as followeth:—First, as it were, one piece of ordnance went off alone. Then after that, a little distance, two more, and then they went as thick as ever I heard a volley of shot in all my life; and after that, as if it were the sound of a drum, to the amazement of me, your mother, and a hundred more besides; yet this was not all, but, as it is reported, there fell divers stones, but two is certain, in our knowledge. The one fell at Chalows, half a mile off, and the other at Barking, five miles off. Your mother was at the place where one of them fell knee deep, till it came at the very rock, and when it came at the hard rock it broke, and being weighed, all the pieces together, they weighed six-and-twenty pound. The other that was taken up in the other place weighed half a tod, 14 pound."

I do not know whether there may be any other record of this remarkable aerolite, so simply but graphically described. Is it not just possible that some of the fragments may yet be preserved in the neighbourhood of its fall? At any rate a search would involve but little trouble.

T. W. WEBB

ANDERSON'S UNIVERSITY

WE extract the following from the *Evening Citizen* (Glasgow) of June 22:—

"The annual meeting of the trustees of Anderson's University was held this afternoon within the institution. Mr. William Ewing, in the absence of the president, was called to the chair. In the annual report which was submitted, reference was made to the death of Dr. Penny in November last, and the appointment of a successor. Mr. Young, of Kelly, who had arranged to set aside 10,000 guineas for the endowment of a Chair of Technical Chemistry in connection with the University, had, it was stated, no further proposal to make, he leaving it to the trustees to make what alteration in the deed of trust they may think proper. Under these circumstances, the managers recommended that advertisements be issued for a successor to Dr. Penny; that the chair should in future be styled the Chair of Scientific Chemistry; and that in electing the professor power should be reserved to the trustees to create such other chair or chairs of Chemistry in connection with the University, and elect such additional professor or professors to fill said chairs as the trustees may see fit; and also to arrange and define, from time to time, the respective departments of the subject to which each professor, including the Professor of Scientific Chemistry, should devote himself. Regarding the mode of electing professors, the

* The scientific name of the horse-chestnut is *Æsculus hippocastanum* it has no relationship to the *Castanea*, or sweet chestnut.—[ED.]

managers recommended that no change should be made. The earlier practice was to appoint the professor only for a course of lectures, and upwards of fifty years ago a bye-law was passed that the election of a professor should be only for one session. With a few exceptions, the election of professors has been annual from that date down to the present time, and the power of not re-electing has been of great service, says the report, in the management of the University. Dr. Steven introduced a motion to abolish the annual election of professors. He spoke of the present system as most degrading. It struck at the root of the institution's claim to be a university; and while it was evidently contrary to the will of the founder, Dr. Anderson, he had heard of no case in which it had been of benefit. Mr. Kidston, secretary, remarked that many years ago it had been the means of causing the protessors to pay up their rent. Dr. Steven said he had heard it remarked that the trustees might come to look upon security for rent as a qualification in their professors of more consequence than educational ability. The professors, he contended, should be elected *aut vitam aut culpam*.—Dr. Pirrie seconded the motion. An amendment was moved by Mr. M'Lelland for having no alteration in the present system. Dr. Adams supported the motion in a speech of some length, in which he characterised the annual election of professors as somewhat disreputable. The chairman recommended that no alteration should take place. All the other officials, he argued, were elected annually, and why not the professors? Dr. Weir asked an explanation ot the paragraph in the report which stated that the system had been found to be of great service. Mr. Kidston, by way of reply, again instanced the refusal of the professors to pay rent. On a division, the amendment was carried by 35 as against 6. The meeting was proceeding to some routine business when our reporter left."

We had hoped that the trustees of Anderson's University would have made good use of the opportunity which exists at the present time, in consequence of the vacancy in the chair of Chemistry and of Mr. Young's munificent offer to endow a professorship of Practical Chemistry, to make some alterations in the status of the professors of the institution, but we seem doomed to disappointment. The professors are still to be appointed yearly, to give one course of lectures, and to have the privilege of paying rent for their laboratories and class-rooms in the meantime. The consequence of this will be that no chemist of eminence will be induced to undertake the duties of a post in which he will find himself on the same footing with *other officials*, doorkeepers, and laboratory man, we suppose; and we shall be much surprised if anyone will be fcund to apply himself solely to the duties of the appointment if he is to be liable to find himself turned out at the end of a year. The principal portion of his time must necessarily be devoted to commercial work and other means of obtaining a living, to the great detriment of scientific research, and certainly not to the credit of an institution which claims to be a university.

THE MICROSCOPE

CHOICE OF A MICROSCOPE.—Medical and other students are at this time of the year purchasing a microscope with which to begin the investigation of animal and vegetable structures. Others who would wish to invest in an instrument are deterred by the expense on the one hand and by the fear of obtaining a worthless thing on the other. Too strong a protest cannot be made against the notions prevalent with regard to microscopes, and encouraged by most of the makers in this country. The handsome-looking instrument of great size, with its long tube and innumerable wheels, is not to be recommended to the would-be observer, even should he feel justified in the expenditure. The microscopes which are used in most of the German laboratories where so much thorough work is done (to the writer's knowledge in Prof. Stricker's and Prof. Rokitansky's laboratories at Vienna, in Prof. Schweigger Seidel's at Leipzig, and in Prof. Claude Bernard's at Paris), are the little instruments of Hartnack, which do not stand above ten inches high, with a simple but large stage without any movement, no rackwork to the tube, but a sliding motion and a fine adjustment. The instrument is used in the vertical position with complete comfort, and when liquid is on the stage, this position being necessary, it is of considerable advantage to have a small microscope over which one can easily bend the head. Large microscopes, with their complicated machinery, are made to suit the optician who sells them, and not for the convenience of the observer. Those who wish to get a microscope should insist either on having one of these small and handy instruments made, or order one from M. Verick or M. Hartnack in Paris. Such a body having been purchased at a very minimum of cost, a larger sum may be expended on the really essential part of the apparatus, namely, the lenses. And here it will be found of great advantage to have the tube of the microscope not more than three-and-a-half or four inches in length, for then the objectives of the continental makers can be used with the greatest advantage, though, with proper care as to the ocular or eye-piece, they may be used on our ordinary long-tubed awkward English microscope. It is almost incredible that the English makers of object-glasses continue to demand three, or even four, times the price for their lenses which foreign makers do for lenses in every respect as good. For two pounds an object-glass may be obtained of M. Verick or M. Hartnack, of Paris, No. 8, which is quite as good a glass and in some respects more pleasant to use than the one-eighth, for which English opticians demand eight guineas. Many persons anxious to work with the microscope are deterred by the price of really first-rate instruments in this country. What we urge upon them most earnestly is to purchase such a body with eye-piece as that described above—simple but strong and steady- -for between two and three pounds, and to equip the instrument with the objectives of MM. Verick or Hartnack, say No. 2, No. 5, and No. 8, which can be obtained for another four pounds. We shall have occasion again to speak of the merits of English and foreign objectives, especially of the immersion object-glasses. At present we speak from personal experience, and desire to point out the convenience and cheapness of the small microscope-body, and the thorough excellence and immensely diminished cost of the French makers' object-glasses.

Cutting Sections of Tissues.—The method of "embedding" first practised by Stricker and Klebs is now extensively used in Germany, and is of very great assistance to the practical histologist. It consists simply in surrounding the object from which sections are desired, with either paraffin, stearine, or a mixture of wax and oil. This latter is preferred at Vienna by Prof. Stricker and Dr. Klein, his assistant, and can be obtained of the exact consistency which may be desired; usually equal parts are to be used. A little tray of paper is made, and some of the wax composition in a melted state is poured in. The object to be cut is then placed in the tray, and more composition added, till the object is thoroughly enclosed. When hard, sections of the mass can be cut, the advantage being in the case of thin laminæ or processes, that a complete support is offered by the surrounding composition, and a uniformly thin cutting may be obtained. For some purposes the microtome of Dr. Ranvier, of Paris, is very useful: it is similar to one recently brought out by Mr. Stirling, of the Anatomical Museum, Edinburgh. In this little instrument we have a flat piece of brass with a hole in the centre, leading into a cylindrical chamber, at the bottom of which a screw works. A piece of elder-pith is excavated, so as to hold the tissue to be cut; and when this has been well fixed in it, the pith is squeezed into the cylindrical box through the hole in the brass plate. A razor drawn along the surface of the brass plate cuts through the pith and the tissue it embraces, leaving a surface perfectly smooth and continuous with that of the plate. A turn of the screw, which works into the cylindrical box, now causes a certain very small thickness of the pith and tissue to project above the plate, and the razor again drawn across and pressed on to the surface of the brass plate, cuts a fine section, the exact thickness of which may be nicely regulated by the screw which pushes up the pith. This little instrument may be obtained at a small cost from M. Verick, 2, Rue de la Parcheminnerie, Rue St. Jacques, Paris. It is not unlike an instrument described in English books on the microscope for cutting sections of wood, but its application with the use of pith, previously much in use for making sandwiches with delicate tissues which had to be cut, increases its value greatly. As to knives to be used in making sections, though some large knives are made on purpose, there is nothing better than a first-rate broad-bladed razor. Dr. Meynert has cut his immense collection of brain preparations with a common razor.

Staining and Mounting Tissues.—The method which is now very extensively used in German histological laboratories for the study and preservation of all kinds of delicate tissues, such as sections of the developing hen's egg, morbid growths, fine injections, nerve tissues, &c., is as follows: The section, either from a fresh specimen or from one preserved in alcohol, is placed in a solution of carmine in ammonia, from which all excess of ammonia has been allowed to evaporate, as tested by the smell. The solution is also carefully filtered before use, and diluted to a small extent. After from three to ten minutes or more in the carmine solution, the section is placed in distilled water and thoroughly washed for some time by blowing into the water with a small pipette. From this the section is removed momentarily to a watchglass containing distilled water and two drops of acetic acid, and then is placed in absolute alcohol. The water is thus removed, and in five or ten minutes the section may be placed in oil of cloves, which renders it very transparent. From this it is removed to the glass slip, and is mounted in a solution of gum damara in turpentine, such as is sold by artist's colourmen. At any stage in this process we can proceed back again by the same steps, ammonia being used in place of acetic acid, and re-stain, re-wash, or re-acidify as the case may be. If the staining is carefully managed and the subsequent washing a thorough one, most cellular structures are very beautifully and clearly brought out. Where rapidity is desired, and for the purpose of inspecting a specimen, it may be simply mounted in glycerine after the staining. The process above described is that of Gerlach and Stieda, and is preferred to any other by some observers of great experience. Thus Dr. Meynert, of the lunatic asylum at Vienna, who is throughout Germany regarded as the great authority on the histology of the brain, uses this method for mounting his sections of cerebrum, cerebellum, &c. It is very convenient to have little glass dishes with covers for each of the above-mentioned re-agents, so that the sections may be passed from one to the other and left covered up, if desired, for a day or two—the waste of re-agents involved in filling watch-glasses each time they are required being also avoided. If preparations have been preserved in chromic acid, they must be very well washed before staining, and very often cannot be made to stain well at all. Various methods are useful in various cases, but, as one of great general use, the carmine staining and oil of cloves clearing may be strongly recommended. Staining tissues with nitrate of silver, chloride of gold, and with bile-pigment are most important aids to the histologist, the merits of which have been recently much discussed, and of which we shall have a word to say from experience.

Glycerine Jelly.—This composition, which has been lately introduced, melts at a lower temperature than Deane's medium, and has a greater clearing action on the objects mounted in it. A small piece of the jelly put on a glass slip and warmed, soon liquefies, and is ready to receive any object, after which the cover is directly applied. For objects which do not require any great amount of "clearing," it is a most useful medium. Insects, worms, small crustacea, &c., may be mounted in this way excellently. E. RAY LANKESTER

METEOROLOGY OF JUNE 1870

I BEG to send you a few particulars of the weather of the past month (which was characterised by unusual atmospheric phenomena), deduced from daily observations with standard instruments, the place of observation being in latitude 51° 27′ N., longitude 0° 18′ W., height above sea level 64 feet.

The barometrical readings have been corrected for capillarity, index error determined by comparison at the Royal Observatory, Greenwich, and certified by James Glaisher, Esq., F.R.S., and reduced to 32° Fahr. and mean sea level.

The thermometrical readings have been corrected for index error determined by comparison at the Kew Observatory of the British Association.

Time of observation, thermometer $7^h\ 45^m$ A.M., barometer $8^h\ 0^m$ A.M., wind direction $8^h\ 30^m$ A.M., daily (approximate).

The following are the calculated monthly means, &c.

Mean height of the barometer (corrected)	30·135 in.
Highest observed reading	30·551 in.
Lowest observed reading	29·747 in.
Monthly range	0·804 in.
Mean temp. air ($7^h\ 45^m$ A.M.)	60·8°
,, ,, of evaporation	55·3°
,, ,, of dew point	50·6°
Relative humidity (dry air = 0, saturation = 100)	70
Mean of the maxima	75·1°
Mean of the minima	51·2°
Mean diurnal range of temperature	23·9°
Extremes: Highest reading (June 22)	91·4°
Extremes: Lowest reading (June 6)	41·6°
Monthly range of temperature	49·8°
Mean estimated force of wind (0 to 6)	1·5
Total rainfall	0·597 in.
Days on which rain fell	5
Evaporation on 22 days	3·652 in.
Mean intensity of ozone (24h)	2·5

*** Sun at greatest meridional altitude (year) or greatest N.D. June 21st.

A lunar halo (or portion of a circle) was observed on June 9 shortly after 10^h P.M. (or 10^h astronomical time). Its estimated extent was 270° of a circle whose diameter was 60°. Estimated altitude of the moon at time of observation, 35°.

A thunderstorm occurred on the 16th, with very vivid lightning, yielding 0·355 inch of rain, which was equivalent to 7987·5 gallons, 1288·65 cubic feet, or 35·9 tons per acre, assuming the rainfall to be equally distributed, which may be done with some degree of truth, as the amount measured at the Kew Observatory, one mile distant, agrees with mine to the second decimal.

The atmosphere was moderately charged with moisture during the month, which must have been an assistance to vegetation in spite of the excessive drought.

The rainfall during this month was 0·558 inch less than that registered during the corresponding period last year.

Wind directions in the lower regions of the atmosphere were observed on 12 out of 16 points, the prevailing directions being between W. and S.W. points.

Richmond, Surrey, July 7 JOHN J. HALL

THE ROTUNDITY OF THE EARTH

"PARALLAX" is not dead yet. His backer, Mr. John Hampden, has again brought his sophisms and his misstatements before the public in the form of a periodical called the *Armourer*, which has already had one period of existence, having been discontinued about four years since, "amidst the regrets of hundreds of its readers," as the editor asserts. When Mr. Hampden speaks of the recent experiment by which the falsity of "Parallax's" views was exposed, as "the Bedford Canal swindle," of Mr. Wallace's victory as having been obtained by "Scotch knavery and cunning," and of the conduct of the editor of the *Field* as umpire as having been "false, unfair, and fraudulent," we may well leave these charges to be replied to by these gentlemen themselves, or by the law. As, however, "Parallax" repeats unblushingly his assertion that he has for years propounded his views by lectures in various parts of the country without their having been once refuted, we may call to his remembrance a circumstance which he has probably found it convenient to forget. During the recent experiments at the Bedford Level, "Parallax" carefully concealed the fact that the very same test had been previously applied. In the year 1856, however, after a lecture by "Parallax," at Norwich, two gentlemen challenged him to an experimental proof of his views. He accepted the challenge and was invited to witness the experiment, which invitation, however, he did not respond to, but prudently left the town in the interim. The nature and result of the experiment are detailed in a printed slip which was inserted at the time in the local papers, and a copy of which we append:—

COPY OF AGREEMENT.—We, the undersigned, "Parallax," of No. 61, Upper North Place, Gray's Inn Road, London, on the one side, and John Weir, of No. 14, Suffolk Street, Union Place, Norwich, and Charles William Millard, of Prince's Street, Norwich, on the other side, having different opinions as to whether the Earth be a Plane or a Globe, agree to test the accuracy of our respective opinions in the following manner, that is to say, to place four flags in a straight line, intersecting the River Yare between Strumpshaw or Bradestone and Norton, for a space of not less than four miles, or six miles if possible. The flags to be at the same height above the water except the last or fourth flag, which is to be placed close behind the third flag, at a height of three feet above it; if we can see the fourth or furthest flag above the tops of the other three flags, the Earth is a plane, or if the second flag from the telescope be above a line joining the tops of the first and third flags, the Earth is a globe.—(*Signed*)—"PARALLAX;"—JOHN WEIR, C. W. MILLARD, Engineers and Surveyors.

Dated November 24, 1856 *Witness*—R. F. HINDE

COPY OF CERTIFICATE.—We, the undersigned, hereby certify and declare, that on the eleventh day of December, one thousand eight hundred and fifty-six, we accompanied Messrs. Weir and Millard, and assisted in placing the flags in the manner above mentioned, and that upon looking at the flags with a powerful telescope, the top of the second flag was fifteen inches and one half of an inch above a line joining the tops of the first and fourth flags, and twenty-four inches and one quarter of an inch above a line joining the tops of the first and third flags, thereby proving that the earth is a globe, and that from the results of this experiment, "Parallax" is bound, by the before-mentioned agreement, to renounce, for ever, his theory of the earth being a plane.—(*Signed*)—R. F. HINDE, Sussex-street, Norwich, manufacturer; ALEX SANDERSON, Magdalen-street, Fye-bridge, tobacconist; W. H. DAKIN, Davey place, Norwich; JAMES NEWBEGIN, St. Andrew's, tobacco manufacturer.

Will nothing stop "Parallax's" mouth?

TEA

THE word "Tea" is applied to the leaves of numerous plants from which infusions are made in their several native countries. Thus in Paraguay they use a species of Holly, in Abyssinia and Arabia the leaves of *Catha edulis*, and in Labrador those of *Ledum latifolium*.

We propose, however, in this paper, to say a few words about that article which is generally and popularly known as tea, and which forms such an important commercial commodity between China, India, and our own country. How long tea had been used in China before its introduction into Europe early in the seventeenth century no one can venture to say, but it appears to have been first known in England about the year 1660, and no article of commerce, perhaps, presents a parallel history of such rapid development. In 1678 the East India Company imported into England 4,713lb. Tea, however, continued to be a rarity for many years after that date, fetching a high price, and consequently remaining beyond the reach of all but the more wealthy. The demand for it increased so rapidly that in 1725 the consumption in the United Kingdom reached 370,323lb. Since then tea has been more and more in demand, until we find the returns for last year show as much as 139,223,298lb. imported, and 111,889,113lb. entered for home consumption, the computed real value of the tea imported during eleven months of 1869 being 9,115,823*l*.

The plant from which this large source of wealth is obtained is a shrub, the native country of which is still not definitely known. Although it has been cultivated for many hundreds of years in China, and its use alluded to in ancient Chinese legends, it has not been discovered in that country in a wild state, but truly native tea occurs in the jungles of North-eastern India.

At one time botanists were inclined to the opinion that black and green teas were furnished by two distinct species, the former by *Thea bohea* and the latter by *T. viridis*. So little difference exists between them that there seems no doubt as to their being mere varieties, and both are now usually referred to one species, the *Thea chinensis* of Linnæus. Though tea is now largely grown in Assam and some also in Japan, the plants cultivated in both countries are varieties introduced from China. The black and green teas of commerce may be prepared from either form of the plant according to the pleasure of the tea farmer, the colour in a great measure depending upon the rapidity of the artificial drying of the leaf, and also upon the length of time the freshly gathered leaves are exposed to the air before heating. There are, however, districts in China called respectively the Black and Green tea districts, in which the plants are grown specially for each purpose. For the preparation of either sort the leaves are gathered by hand, and the younger ones should alone be taken. If they are intended for the manufacture of black tea they are exposed to the air for a short time, after which they are placed in iron pans and submitted to a gentle heat for a few minutes. By this process much moisture is thrown off, and the leaves are rendered pliable, so that they are easily pressed or rolled between the hands, by which the characteristic twist or curl is given to them. Before, however, they are fit for market, they are exposed to the air for two or three days, and finally dried in iron pans over a slow fire. The chief difference in the preparation of genuine green tea is, that it has to be more quickly dried after undergoing the curling or twisting process in the hands, black tea being allowed to remain in heaps in a flaccid state, before the final drying or roasting, which, in itself, is much slower. A great deal, however, of the green tea consumed in this country, is artificially coloured by the Chinese, chiefly with Prussian blue, gypsum, and turmeric. Of course it is only inferior teas that are so treated, a good face being thus given to them. They can mostly be detected by placing a handful of the tea on a sheet of white paper; a thick, greenish dust will not only be left on the paper, but will rise every time the tea is shaken. By breaking a few leaves also with the finger nails this coloured tea will show a brownish fracture, while genuine uncoloured tea is more or less green throughout, and consequently little or no dust is deposited from it. As the leaves of true tea vary very much in size and form, adulteration with the leaves of some other plants is not so easily detected. The nearest approach, however, to the form of the true tea leaves are those of *Camellia sasangua*. This plant itself is a near botanical ally to the tea, and the leaves are moreover used by the Chinese for scenting many of their teas. Most other leaves which have been found as adulterants may be detected by their forms.

We give a figure of a leaf of true tea.

If a leaf of black tea be soaked in cold water, spread out, and inspected through a microscope of ordinary power, it will present the appearance shown in the cut, the older and larger leaves will be of a dullish green, and the younger ones of a light semi-transparent green. It will not serve us to examine the internal structure of the leaf, as it has many points in common with other leaves, and would moreover require minute examination. The best black tea, then, should present the appearances above indicated, and the same may be said of green tea, with this exception, that after being soaked it is of a paler green colour than the former.

Amongst the commercial varieties of tea the following are the best known:—Congou: this constitutes the bulk of black tea from China. It is that which is usually sold as black tea, and of course varies much in price according to its purity; a really good tea of this description ought to be had at the present time at 2*s*. 6*d*. per lb.

Souchong and Pekoe are both finer kinds of black, and fetch higher prices. Another kind of black called Orange Pekoe may be known by its long, wiry leaves, which are mostly genuine; it is artificially scented, and is generally used by grocers for mixing with inferior kinds. A fine Pekoe, however, ought to be obtained for about 4*s*. per lb.

Caper is a common black tea, artificially scented; the leaf as we see it in commerce has the form of the Gunpowder leaf, but these are made up of tea-dust and other matters agglutinated.

Amongst green teas, genuine Gunpowder is the finest; the qualities and prices however vary very much; the leaves of the best are in fine, close curls, and are the younger ones gathered from the tops of the plants. The lower qualities of this tea are almost all coloured artificially, and many contain no perfect or whole leaf at all, but are made up of broken tea-leaves; 4*s*. 6*d*. per lb. may be considered a fair price for a good quality Gunpowder tea. In Hyson the leaf is longer than Gunpowder; it is mostly composed of the true leaf, but is very frequently artificially coloured.

Oolong is really a green tea, but with so black an appearance that its colour is only developed by putting it in hot water. It is artificially scented, and is used for mixing with other kinds of tea.

The cultivation of tea in Assam has sent several good kinds into our markets, the Congou, Souchong, and Flowery Pekoe of these plantations being, as hitherto imported, all genuine teas. We regret, however, to see that in the course of the past few weeks a quantity of artificially coloured green tea has been imported from the Indian plantations. Many of the Assam teas have a fine malty flavour, which is so much esteemed that it is frequently imitated and imparted to other teas in London.

A great deal that has been said and written for many years past on the subject of adulteration of food we are bound to admit as truth, but, on the other hand, there has been some exaggeration. With regard to tea, the great demand amongst all classes has led to a very keen competition, not only amongst retail dealers, but also amongst importers themselves. The system of mixing inferior articles with those of better quality must not be wholly laid to the charge of the British tradesman or merchant, for the natives of the several countries producing the various commercial products, practise a great amount of deception. The importation of several chests of such rubbish as the "fine Moning Congou," about which so much talk was made a few weeks since, as well as the numerous cargoes of "tea-dust," a sample of which is now before us, composed of small fragments of various kinds of vegetable matter and other substances, with little or no tea, are proofs that others than the retail dealers are the most culpable. We are ashamed to own that in many instances this system of deception has been taught the natives by our own countrymen; but such is not always the case, and other articles besides tea, as we shall have occasion to show in the course of these papers, are equally subject to native adulteration. A system of manufacture of spurious tea, called "Lie Tea," is openly known to exist in China, and was at one time profitably carried on in England. It consisted in converting the leaves of numerous plants into imitation tea for the purposes of adulteration. Though teas of varied qualities are imported from China, those of the very finest kinds seldom leave the country, except a small quantity which is carried overland to Russia, where they sell for as much as 50*s*. per lb., and the same price is even paid by the princes and mandarins of China in the very country where the tea is produced. It is said that these fine teas would deteriorate in quality in such a journey as that from China to England. A fine variety of Assam tea called Flowery Pekoe, is now chiefly imported for the Russian trade, very little of it being sold in this country. It is worth about 7*s*. 6*d*. per lb., consequently there is little demand for it. Though the Russians boast, and with good reason, of the quality of their tea, a vast quantity of rubbish is sent to that country from China for consumption by the poorer classes. This is known as Brick Tea, and is frequently made up of the sweepings of the manufactories and warehouses mixed with bullock's blood and other refuse, and compressed into hard cakes or bricks; for use it has to be boiled. In some parts of India the natives use a similar kind of brick tea, making, instead of a clear infusion, a thick kind of drink more like soup.

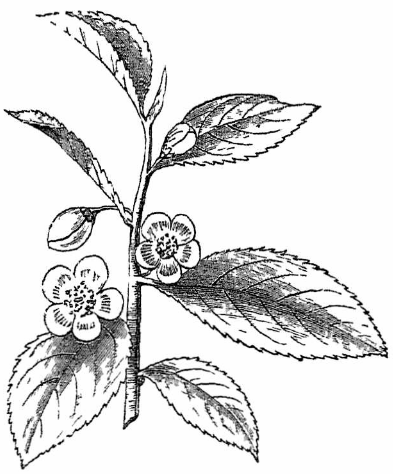

FIG. 1.—Tea (*Thea chinensis*, L.)

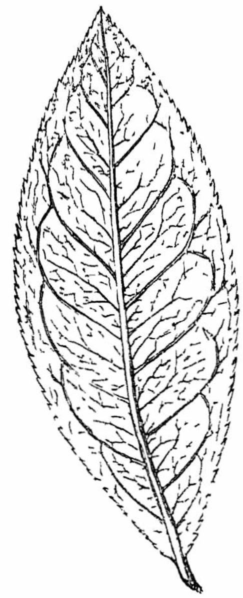

FIG. 2.—Leaf of the Tea Plant—natural size of a full-grown leaf

Tea contains an active principle called "theine" and a volatile oil, it also contains about fifteen per cent. of gluten or nutritive matter, and about twenty-five per cent. of tannin or astringent matter. The effect of theine upon the human system is to excite the brain to greater activity, but whether or not it soothes the vascular system by preventing the rapid waste of the body, is a point upon which physiologists are not quite agreed. Theine, however, if taken in excessive quantities produces tremblings, irritability, and wandering thoughts, it has been recommended that when these symptoms show themselves, cocoa should be used as a beverage for a few days. The volatile oil is narcotic and intoxicating; it is to this oil that the flavour and odour of tea are due, it is of course present in larger quantities in new teas than in old, therefore the fresher the teas are the fuller is their flavour and odour, consequently no kind of tea improves by being kept exposed to the air or even in paper, so that tea weighed at the time of purchase should be preferred to that sold in packets, the buyers of such tea having to risk the length of time it has been packed; and, moreover, the teas themselves are usually of an inferior description.

Since writing the above, I have had two samples of green tea sent me which have been offered for sale in London during the past week. One sample is composed of nearly or quite half its weight of the young fruits of the tea-plant about the size of small peas, the remainder being made up of broken tea-leaves agglutinated and rolled together, and enclosing fragments of various matters, mineral as well as vegetable, which, of course, are included for the purpose of increasing its weight and bulk. The other sample consists principally of leaf stalks, a few leaves, rice husks, and the pappus fruits of some Compositæ. Truth compels me to say that all the leaves I have examined out of these samples have been leaves of the true tea-plant, or rather fragments of such, but all artificially coloured, and so superficial is the colouring that it can be easily wiped off with the dry finger. These teas have been offered for sale, one at $1\frac{1}{4}d.$ and the other at $1\frac{1}{2}d.$ per lb., the duty paid on them being equal to that charged on the best teas—namely, 6*d*. per lb.

This class of tea can, of course, only find a sale amongst unscrupulous tradesmen, who buy it to mix with good teas, and where a comparatively small proportion of this rubbish is mixed with a large quantity of good tea, but yet in sufficient bulk to increase the tradesman's profits, it is difficult for the purchaser to detect a few hundred or more such leaves in the thousands which go to form a pound of tea. It is high time there was some regular system of examination of such articles directly they come into port.

J. R. JACKSON

NOTES

PROFESSOR HELMHOLTZ has left Heidelberg for Berlin, to occupy the position left vacant by the death of Magnus, but with the title of Professor of Physiology.

AT the meeting of the French Academy on the 4th inst., Professor Brandt was elected a correspondent of the section of Anatomy and Zoology. In the final election he received twenty-two votes out of thirty-eight, the remaining sixteen being in favour of Mr. Darwin. In the first ballot Professor Huxley received three votes, and M. Loven one.

ONE of the improvements in the management of the Hunterian Museum of the Royal College of Surgeons, introduced by the present Conservator, has been the publication of an annual report of the progress and condition of the collection, and the exhibition in the theatre of the College, of the specimens that have been added to the Museum during each twelvemonth. As the College year ends at Midsummer, this exhibition has just taken place, and has enabled those interested in the Museum to judge of the nature and value of the additions, and the mode in which they have been prepared. The new specimens include fifty-five specimens of pathological anatomy, one hundred and eleven of normal human and comparative anatomy; the latter chiefly prepared from animals which have died in the Zoological Society's Garden, and a considerable series of skeletons and skulls. We propose to refer more fully to Professor Flower's Report on a future occasion.

THE naturalists of Switzerland have decided to form a scientific congress devoted to the study of the natural phenomena of the Swiss Alps, to include the geologists and palæontologists of France, Germany, and Italy, who have paid special attention to this subject, to be held in Geneva on the 31st of August and 1st and 2nd of September, and to be called the Congress of Alpine Geologists. Among the promoters of the congress are Prof. Studer, of Berne; Prof. Mérian, of Bâle; Prof. Escher de la Linth, of Zürich; Prof. Desor, of Neuchâtel; Prof. Favre, of Geneva; Profs. de Loriol, Heer, and Mousson, of Zürich; Prof. Rütimeyer, of Bâle; Prof. Renevier, of Lausanne; Profs. Vogt and Pictet, of Geneva. A committee for the organisation of the congress has been formed at Geneva, with M. Pictet as president, M. Alphonse Favre as vice-president, and MM. Ernest Favre and E. Sarazin as secretaries. All geologists interested in the subject are invited to be at the president's reception on the evening of August 30th, and anyone wishing to communicate any address or paper is requested to write to M. Ernest Favre, 6, Rue des Granges, Geneva.

WE are pleased to hear that the Government of Demerara has re-considered its resolution for discontinuing the geological survey of that colony, and has now resolved to complete it, under the direction of Mr. Charles B. Brown, an associate of the Royal School of Mines.

A SPECIAL extra meeting of the Syro-Egyptian Society of London will be held on Tuesday, July 19, at half-past seven P.M., for the exhibition of a collection of drawings of Egyptian antiquities, by the late R. Hay, F. Arundale, and C. Laver, Esqs. Messrs. Simpson and Bonomi will give explanations.

AT a meeting of the trustees of Owens College, Manchester, held on Thursday, the 7th inst., the vacancy caused by the resignation by Professor W. Jack, M.A. of the Natural Philosophy Professorship, was filled up by the appointment of Dr. Balfour Stewart, F.R.S., superintendent of the Kew Observatory, to the Senior Professorship, and of Mr. James Thomson Bottomley, M.A., F.C.S., Demonstrator and Lecturer in Natural Philosophy in King's College, London, to the Junior Professorship of Natural Philosophy. Dr. Stewart was also appointed Director of the Physical Laboratory which is about to be established in the college. We are informed that Mr. Bottomley has since withdrawn.

M. CLAUDE BERNARD has been elected a member of the Imperial Council of Public Instruction in France for the year 1869-70; M. Briot has been appointed Professor of Mathematical Physics and the calculus of probabilities in the Faculty of Sciences at Paris; and M. Emery, Professor of Geology, Mineralogy, and Botany in the Faculty of Sciences at Dijon.

THE annual public meeting of the Paris Academy of Sciences for the distribution of prizes and rewards, which should have been held in December last, was postponed till the present month, and is now again put off for some unexplained cause.

IT is with great pleasure we hear that the London Institution, in Finsbury Circus, has appointed Mr. John Cargill Brough to the post of Librarian. The library of this Institution is so valuable that it is fitting it should be under the care of a man who combines literary and scientific qualifications in so eminent a degree. It was right that an office once filled by such men as Maltby and Brayley, should have fallen into good hands.

WE understand that Dr. B. H. Paul has been appointed editor of the *Pharmaceutical Journal*, the new series of which we recently announced.

THE list of members of the Institution of Civil Engineers, corrected to July 1, 1870, contains the names and addresses of 16 honorary members, 702 members, 999 associates, and 177 students, making together 1,894 of all classes.

THE House of Commons decided on Friday last that the land belonging to the Thames Embankment shall be kept entirely free from building. The Natural History Museum will therefore occupy the ground already indicated by us on the space adjoining the Royal Horticultural Gardens.

Apropos of this subject, one of our daily contemporaries (and by no means the worst informed on scientific subjects) makes ludicrous blundering. While generously affirming that "any back street will do for a museum," it maintains that "Englishmen cannot fairly be charged with undervaluing Natural History; witness the crowds that throng the great Grecian temple in Bloomsbury, to pause at the cases of stuffed monkeys, and to marvel at the *horns of the megatherium!!*" as indeed well they may. Whether Englishmen can be charged with undervaluing natural history or not, it is clear that newspaper leader writers can be charged with expressing a confident opinion on subjects in which they are unacquainted with the most elementary facts. Clearly there is room for more science teaching.

AT a recent meeting of the Franklin Institute of Philadelphia Professor Morton resigned the office of Resident Secretary, he having received an appointment in a distant city. The resignation was accepted, and the President stated that Professor Morton had consented to continue his charge of the *Journal of the Franklin Institute*, which had achieved so desirable a position under his management. The new position accepted by Professor Morton is that of President of a College of Mechanical Engineering, to be established in Hoboken, opposite New York. He is succeeded at the Institute by Dr. W. H. Wahl.

THE Council of the Institution of Civil Engineers has awarded the following premiums:—1. A Telford Medal and a Telford Premium, in books, to Edward Dobson, Assoc. Inst. C.E., for his Paper on "The Public Works of the Province of Canterbury, New Zealand." 2. A Watt Medal and a Telford Premium, in books, to R. Price Williams, M. Inst. C.E., for his Paper on "The Maintenance and Renewal of Railway Rolling Stock." 3. A Watt Medal and a Telford Premium, in books, to John Thornhill Harrison, M. Inst. C.E., for his Paper on "The Statistics of Railway Income and Expenditure, and their bearing on future Railway Policy and Management." 4. A Telford Medal and a Telford Premium, in books, to John Sopwith, jun., M. Inst. C.E., for his Paper on "The Dressing of Lead Ores." 5. A Telford Medal and a Telford Premium, in books, to James Nicholas Douglass, M. Inst. C.E., for his Paper on "The Wolf Rock Lighthouse." 6. A Watt Medal and a Telford Premium, in books, to George Berkley, M. Inst. C.E., for his "Observations on the Strength of Iron and Steel, and on the Design of parts of Structures which consist of those Materials." 7. A Watt Medal and a Telford Premium, in books (to consist of the second series of the Minutes of Proceedings, vols. xxi. to xxx. inclusive), to Robert Briggs, of Philadelphia, U.S., for his Paper "On the Conditions and the Limits which govern the proportions of Rotary Fans." 8. A Watt Medal and a Telford Premium, in books, to Edward Alfred Cowper, M. Inst. C.E., for his Paper on "Recent Improvements in Regenerative Hot Blast Stoves for Blast Furnaces." 9. A Telford Premium, in books, to John Grantham, M. Inst. C.E., for his Paper "On Ocean Steam Navigation, with a view to its further development." 10. A Telford Premium, in books, to Daniel Makinson Fox, M. Inst. C.E., for his "Description of the Line and Works of the Sao Paulo Railway, in the Empire of Brazil." 11. The Manby Premium, in books, to Emerson Bainbridge, Stud. Inst. C.E., for his Paper on "Coal Mining in Deep Workings." The Council have likewise awarded the following prizes to students of the Institution:—1. A Miller Prize to Robert William Peregrine Birch, Stud. Inst. C.E., for his Paper on "The Disposal of Sewage." 2. A Miller Prize to Henry Thomas Munday, Stud. Inst. C.E., for his Paper on "The Present and the Future of Civil Engineering." 3. A Miller Prize to William Walton Williams, jun., Stud. Inst. C.E., for his Paper on "Roads and Steam Rollers." 4. A Miller Prize to Sidney Preston, Stud. Inst. C.E., for his Paper on "The Manufacture and the Uses of Portland Cement." 5. A Miller Prize to Edward Bazalgette, Stud. Inst. C.E., for his Paper "On Underpinning and making good the Foundations of the Irongate Steam Wharf, St. Katherine's, London." 6. A Miller Prize to Josiah Harding, Stud. Inst. C.E., for his Paper on "The Widening of the Liverpool and Manchester Railway between Liverpool and Huyton, and on the Construction of a Branch Line to St. Helen's." 7. A Miller Prize to the Hon. Philip James Stanhope, Stud. Inst. C.E., for his Paper on "The Metropolitan District Railway."

THE fabled alligator captured in the Thames some months ago has been surpassed by a hippopotamus disporting itself in the Seine. The scarcity of water has been so great in the Jardin des Plantes, that his majesty had been taken by his keepers to the river for his daily bath, securely held, as was thought, by his chain. One day, however, he snapped his chain during his gambols, to the no small dismay of the *blanchisseuses* and steamboat passengers, one lot of whom he threatened to demolish at a mouthful. Several keepers who attempted to board him were treated to a playful ducking, but after the upsetting of a good number of small boats, he was at length captured and hauled ashore. The poor brute must have thought that the good old times of the pre-glacial epoch had returned.

A SPECIAL general meeting of the Fellows of the Royal Geographical Society was held on Monday last at the Royal Institution, Albemarle-street, to consider the question of purchasing a large freehold house for the society's map rooms, library, and offices. The president, Sir Roderick Murchison, explained the necessity for further accommodation, and stated that the Council had at length concluded to purchase the large house No. 1, Savile Row. The meetings, however, would still be held in the great hall of the University of London. The resolution to purchase the property in Savile-row for 14,400*l.* was carried without opposition.

THE Second Report of the King's School Natural History and Natural Science Society, Sherborne, shows the activity with which the study of natural science is followed in the school. As might be expected in so rich a neighbourhood, geology appears to be the branch of science most zealously cultivated.

WE have received the First Annual Report of the American Museum of Natural History. Until 1869 New York was behind Boston, Philadelphia, Washington, and Chicago in possessing no Natural History Museum, a position which a number of gentlemen then determined their city should no longer occupy. Rooms were therefore secured in a building in the Central Park, and large subscriptions were at once collected, and employed in purchasing collections in Europe. Baron Osten-Sacken and other gentlemen also made large donations of specimens and books; and thus, without any assistance from the State except a grant from the duplicate specimens of Natural History belonging to it, the New York Museum of Natural History is in a fair way to become one of the most important on the continent.

THE Natural History Association of Natal has issued its second report. Papers were read during the past session by the Rev. Dr. Callaway, on phenomena occurring among the natives akin to mesmerism, the basis of their faith in divination, in which many interesting and striking facts were brought forward. Mr. Windham's paper on "The Game-birds of Natal," was illustrated by a collection of mounted specimens, chiefly shot by himself, and generally identified by Mr. Layard, whose work on the birds of South Africa forms the recognised authority on the subject. In entomology, valuable papers were read by Mr. H. C. Harford, on the larvæ and pupæ of some Lepidoptera of Natal; by Mr. Morant, Notes of a collecting trip in the Transvaal, during which several new and interesting species were captured; and by Dr. Seaman, on protective resemblances in some local forms of insect life. In botany there are records of a new climbing Scrophulariaceous plant, *Buttonia natalensis*, discovered by Mr. E. Button, and of a new date-palm, detected by Mr. M'Ken, curator of the Natal Botanic Gardens. The colony may be congratulated on possessing so energetic a society.

THE Portuguese Consul-General at Bangkok, a hale man and an excellent swimmer, while bathing in the River Menam, suddenly sank from having come into collision with an electric eel, and was drowned. The Siamese say such deaths are not uncommon.

WE have received from Mr. M. J. Barrington-Ward a syllabus of a course of botanical lectures recently delivered at Clifton College, accompanied by the following gratifying remarks:—"My lectures were attended, I should say, by 100 ladies, and I can well bear out what was said in NATURE a few weeks ago as to the ability which women display at such classes. I never met with so much diligence and real skill in pupils before, and I only wish I could send you some of the papers written by these ladies to show how well and logically they handled a scientific subject."

MR. W. CROOKES has reprinted for private circulation his article in the current number of the *Quarterly Journal of Science* on "Spiritualism viewed by the Light of Modern Science." While admitting that phenomena have come under his notice which seem inexplicable on any known physical laws, the main part of Mr. Crookes's paper is occupied by a statement of the tests to which "Spiritualists" should subject their manifestations, which they have at present failed to do.

"DER rationelle Wiesenbau, dessen Theorie und Praxis," by L. Vincent, an exhaustive treatise on arable agriculture, has now reached its third enlarged edition.

*FACTS AND REASONINGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS**

III.

THE results at which we have arrived now require to be looked at from two or three different points of view.

In the first place, with regard to these latter experiments, in which, with the help of Dr. Frankland, a perfect vacuum was procured in the experimental flasks previous to their being hermetically sealed, and before the exposure of them and their contained fluids to the temperature of 146 to 153°C. for four hours, it is desirable to know what the influence of such a temperature would be upon fungus-spores and filaments purposely exposed thereto. It is certain that, so far as all experimental observations have gone at present, no fungus-spore has been known to germinate after it has been exposed in a fluid to a temperature of 100°C. for even a few seconds.† What then would be the effect of a temperature of 150°C. for four hours? Is it possible that a fungus-spore or a fungus-filament at all similar to those which were met with in the preceding experiments could remain as such—could retain its morphological characters, in fact, after an exposure in fluid to a temperature of 150°C. for four hours? With the view of answering this question, I placed a quantity of a small fungus, consisting of mycelial filaments and multitudes of spores, closely resembling although not quite so delicate as those which were met with in the saline mixtures, into a solution (of the same strength as that which had been previously employed) of tartrate of ammonia and phosphate of soda in distilled water, and then handed it over to Dr. Frankland with the request that he would kindly treat this in the same way as he had done the other four solutions. Accordingly, on May 11, a vacuum having been produced within the flask before it was hermetically sealed, the solution was submitted in the same digester to a temperature of 146° to 153°C. for four hours When taken out from the digester, the previously whitish mass of fungoid filaments and spores had assumed a decidedly brownish colour, and it was in great part converted into mere *débris*. On the following morning the flask was broken, and some of the remains of the fungus and its spores were examined microscopically. *The plant was completely disorganised: not a single entire spore could be found;* they were all broken up into small more or less irregular particles, and the filaments were more or less empty, containing no definite contents, and being only represented by torn tubular fragments of various sizes. This utter disorganisation was in striking contrast with other specimens of the fungus, as it existed before exposure in the digester, which I had mounted in order to retain for purposes of comparison. And from the amount of destructive influence which was exercised upon the microscopic fungus in question, we may fairly imagine that the destructive influence of a similar temperature for four hours upon the still more delicate fungus represented in Fig. 17 would have been by no means less in extent. It would seem, at all events, well-nigh impossible that such a fungus could have pre-existed in the solution before its exposure in the digester, and could afterwards have retained all its morphological characters unimpaired, as they may be seen in the specimen now in my possession, from which the above-mentioned drawing was made. The plant must have been developed, therefore, within the flask itself subsequently to its exposure in the digester. What then could its origin have been? No fungus-spore has hitherto been known to germinate—no previously Living thing has been known to live—after the fluid containing it has been raised to a temperature of 100° C. for a few seconds. The fluid in Experiment 19 had however been raised to a temperature of 146° to 153° C. for four hours. We have even seen, in addition, that such a temperature completely disorganises certain closely allied fungus-spores, so that there is good reason for presuming that it would be similarly destructive to such spores as are represented in Fig. 17 if they had pre-existed in the solution. All that I have just said applies equally to the fungus-spores found in Experiments 18 and 20, and to the Ciliated Monad found in the turnip solution.

For the present, therefore, all presumptions, based upon the best available scientific evidence, are strongly in favour of the *de novo* evolution of these organisms within their respective flasks.

Whilst it seems, however, that the Living things which were found in these four experiments must have been evolved *de novo*, it does not follow necessarily that they were evolved in Experiments 19 and 20 out of the re-arranged elements of the saline substances themselves, because no proof has been offered that these substances were chemically pure. That such *may* have been the origin of these Living things seems, however, to be possible from what I have already said, and will, I think, appear even probable, after a due consideration of some of the facts which are now about to be related.

"Germs" are supposed by many to be universally diffused, more especially in the air and within organic substances. It seemed possible, however, and only reasonable, to suppose that they might exist much less abundantly in saline materials than within organic substances, and this was one reason why such materials were made use of in my later experiments. In order to ascertain whether any visible organisms or spores were to be found in the saline materials employed, portions of these have been repeatedly dissolved by distilled water in a watch-glass, and the fluid has afterwards been submitted to the most careful microscopical examination. Moreover, after sufficient time has been allowed for subsidence, the bottom of the watch-glass has then been most carefully scrutinised by a powerful immersion lens. The saline materials employed in the preceding experiments have been potash-and-ammonia-alum, tartar emetic, phosphate of soda, phosphate of ammonia, oxalate of ammonia, acetate of ammonia, carbonate of ammonia, and tartrate of ammonia. The result of repeated examinations of these substances in the manner above stated, has been that not a trace of anything like an organism—no fungus-spore, germ, or egg of any kind—has been found in solutions of any of the substances employed, except in one. This one in which such bodies have been found is that which I have named last—the neutral tartrate of ammonia.

Several of these salts—the oxalate, the acetate, the carbonate, and the tartrate of ammonia—contain within themselves all the ele-

* (Concluded from p. 201.)

† Such a temperature, also, very frequently suffices to produce a considerable amount of disintegration in fungus filaments which are submitted to its influence. It is almost impossible that a perfect organism with a mass of loose spores around it could have braved such a temperature for fifteen minutes, and could then have presented an appearance such as is represented in Fig. 14, the original of which is still in my possession. If not the result of a new evolution, therefore, this fungus must have been developed from a spore which was able to germinate after having been boiled for fifteen minutes; and if so it would be an exception to a rule which has hitherto been found to be general.

ments necessary for the building up of organic substances. Nitrogen, carbon, hydrogen, and oxygen are there, and only require to fall into other modes of collocation in order to give birth to an organisable material. The crystals of the oxalate are very small, those of the acetate are very deliquescent, and carbonate of ammonia exists generally in the form of non-crystalline cakes.* The neutral tartrate, however, exists in the form of large distinct prismatic crystals. Solutions of the first three substances showed no trace of Living things; though organisms were frequently discovered when crystals of tartrate of ammonia were examined.

Before describing these organisms more particularly, it will be well to glance for a moment at the origin or mode of preparation of this salt. The tartaric acid entering into its composition is obtained from *argol*—the crude bitartrate of potash derived from the grape. And although this latter salt is derived from the tissues of a Living plant, the processes to which it is submitted, in order to obtain the tartaric acid in an uncombined state, would most certainly destroy all living "germs" that might have been contained therein. After a solution of the bitartrate of potash has been boiled for a time, tartrate of lime is gradually precipitated by the addition of chalk and chloride of calcium. The insoluble tartrate of lime, after having been washed several times, is then brought into contact with *strong sulphuric acid, diluted with only about four times its bulk of water*, and this mixture is boiled for half an hour.† All this is necessary before a filtrate can be obtained from which the first crystals of tartaric acid are procurable. Ammonia, the other constituent of the neutral tartrate, being a product of the destructive distillation of coal tar, and itself exercising such a destructive influence upon organic matter when existing in the form of strong *liquor ammoniæ*, would not seem to be a very promising nidus for organic "germs." The neutral tartrate of ammonia is, however, prepared by mixing a solution of tartaric acid, procured as above mentioned, with an adequate quantity of liquor ammoniæ, and then evaporating the mixture at a gentle heat. Thus prepared, the crystals contain a notable quantity of water of crystallisation, and are not specially liable to contain organic impurities.

In the stock of crystals that I obtained from Messrs. Hopkin and Williams,‡ and which had been made about six months previously, some were well formed, and almost perfectly transparent, whilst others were less regular in shape, and presented an opaque appearance with more or less of striation within. When a crystal of moderate size was taken, about $\frac{1}{8}''$ in diameter, or a portion of a larger one, and was placed in a large watch-glass with some distilled water, it was frequently found that at first a certain number of opaque-white scales, having a granular aspect under a high magnifying power, dropped from the surface of the crystal to the bottom of the watch-glass. This material, which seemed to have been produced by some superficial alteration of the substance of the salt, dissolved with much more difficulty than the unaltered matter of the crystal. It remained for a long time at the bottom of the glass, and only very slowly disappeared. As the substance of the crystal slowly dissolved away, a number of large and small gaseous bubbles gradually escaped from it. When the crystal was examined with a one-inch object-glass whilst solution was taking place, these air bubbles could be seen at first within cavities, from which they were afterwards liberated by a solution of their walls. Occasionally, from the very centre of a crystal, from which bubbles of gas had been escaping, there floated out a very small and almost invisible filamentary mass, more or less thickly studded with minute air bubbles. Such masses were just visible with an ordinary pocket-lens, and when transferred on the point of a needle to a slip of glass, and examined with a magnifying power of about 600 diameters, they were found to contain more or less of the following constituents:—(1) a minute fragment of cotton or paper fibre; (2) a variable quantity of an almost transparent, insoluble plate-like substance, homogeneous, though broken up in all directions by intersecting cracks; (3) more rarely a small quantity of a tenacious mucoid matter, containing refractive protein-looking granules of various sizes; (4) a quantity of a colourless, confervoid-looking mass, some of whose smaller filaments, $\frac{1}{20000}''$ in diameter, looked like a mere linear aggregation of irregular masses of protoplasm, though in certain larger filaments continuous with these it became obvious that the irregular protoplasm masses were contained within a delicate hyaline cylinder across which dissepiments were sometimes to be seen, as in very minute fungus-filaments; (5) and lastly, certain fungus-spores in almost all respects similar to those which have been met with in so many of the saline experimental fluids. Although four or five of these were frequently interspersed amongst the confervoid-looking filaments, they did not seem to be in organic connection with them. The confervoid-looking, though really abortive fungus-filaments, were also almost precisely similar to the filaments containing irregular masses of protoplasm which were met with (in Experiments 12 and 13), in solutions containing tartrate of ammonia.

Repeated examination of crystals during their dissolution convinced me that such organic bodies invariably came from the interior of the crystal, often from its very centre, and that they were not to be met with on its surface. Seeing, however, that minute shreds of cotton or paper fibre also as frequently came from the interior of the crystal,* it was obviously possible that the organisms met with might have been engaged mechanically during the process of crystallisation, just as it must have happened with the shreds above mentioned. From what has previously been stated concerning the mode of preparation of the neutral tartrate of ammonia and the origin of its constituents, it may be considered almost certain that these organisms could not have pre-existed in the strong *liquor ammoniæ*, and that all living organisms which might by chance have been associated with the bitartrate of potash must have been hopelessly destroyed by the boiling with sulphuric acid, which occurred at one stage in the process employed for the separation of the tartaric acid from its base. During the subsequent process of crystallisation of the tartaric acid from its mother liquor, it is of course possible that any spores existing in the adjacent atmosphere might have dropped into the fluid, and have then become mechanically enclosed within the crystals; and the same chance of such a contamination with spores would exist during the process of crystallisation of the tartrate of ammonia itself. If this, however, had been the real source of the fungus-spores and masses of confervoid-looking filaments, such bodies *might* be found in freshly prepared crystals just as well as in those which had existed for six months.† I therefore asked Messrs. Hopkin and Williams to prepare for me a fresh batch of crystals of neutral tartrate of ammonia. This they were kind enough to do; obtaining them in the same place by the same process, and exposing the mother liquor in a precisely similar way.

An examination of some of these crystals, whilst they were being dissolved by distilled water in a watch-glass, showed that (unlike the older crystals) they were not at all coated on the surface by the comparatively insoluble granular plates; and that only a few very small air bubbles emerged from their interior. And at the bottom of the watch-glass, neither during dissolution nor afterwards, was there seen any trace of the confervoid-looking filaments or of the fungus-spores, though minute shreds of cotton and paper fibres were seen similar to those which were found in the older crystals. An examination of a large number of the new crystals was attended with similar results to those just mentioned.

This absence of the *confervoid-looking filaments* and of the *large fungus-spores* from the recently prepared crystals may be accounted for by either one of two suppositions:—

First. It may be supposed that in the case of the older crystals, the spores and filaments had dropped as such into the solutions in which the tartaric acid alone, or the tartrate of ammonia was crystallising; that they were mechanically engaged in the crystals, and were subsequently liberated unchanged (without having undergone any growth or development) on the dissolution of the crystal.‡ Whilst, on the other hand, in the case of the recent crystals, it may have happened that no such filaments or spores were floating in the atmosphere at the time of *their* formation, and that, consequently, none could have dropped into the solutions. Hence none of these could have been enclosed within the crystals.

* Obtained by a process of sublimation at high temperatures.

† The boiling point of such a solution would be several degrees above 100° C. Heat and acid combined exercise a most powerfully destructive influence upon organic matter, though even *very* dilute sulphuric acid, at *ordinary* temperatures, has been found to be peculiarly destructive to all Living things.

‡ Of New Cavendish Street.

* I had often been surprised at finding such shreds when I submitted some of my experimental fluids to microscopical examination, knowing that I had frequently used freshly prepared distilled water, and had taken every precaution thoroughly to cleanse the flasks which were employed

† I have been unable to obtain crystals of the neutral tartrate of ammonia of an older date than this, and I should feel much obliged to any one who could send me such specimens, or who could furnish me with a few *crystals* of carbonate of ammonia.

‡ If they had been engaged within the crystals of tartaric acid, they must have been liberated from these during the preparation of the neutral tartrate, only to be re-entangled whilst the crystals of this salt were forming.

This supposition is, I think, unlikely to be the real explanation of the difference between the two sets of crystals. My reasons for so thinking will, however, appear more fully during the discussion of the other supposition.

Second. It may be supposed, on the other hand, that the confervoid looking *filaments* and the *spores* are organisms which have assumed their existing forms and dimensions by a process of growth and development within the crystal, and that *the starting-point of each alike was a mere speck of Living Matter.*

By this supposition we give the panspermatists the full benefit of our microscopical researches, and so narrow their real requirements in the matter of pre-existing spores. It becomes a much simpler case for them, if instead of being compelled to calculate upon the pre-existence of fully formed fungus-spores, and of confervoid-looking filaments, they need only presume upon the pre-existence of a mere speck of Living matter less than $\frac{1}{50000}''$ in diameter. I most candidly confess, however, that the pre-existence of such specks of living matter is all that is really necessary for them.* Most of those who have worked much at the microscopic investigation of the organisms met with in organic infusions, must have come to the conclusion that there is no break in the continuity of that developmental series which commences with the mere speck of living matter—the primordial *Monad*—and thence proceeds through such forms as the *Bacterium*, the *Vibrio*, the *Leptothrix* filament, and the mycelial filament of a microscopic fungus. I do not mean to say that this is a necessary order of development, which invariably occurs—far from it, but rather that, as *Bacteria* commence their visible existence in the form of Monads, so *Vibrios* are but the developed representatives of certain *Bacteria*, just as the various kinds of *Leptothrix* filaments grow from certain pre-existing *Vibrios*, and just as certain of these *Leptothrix* filaments themselves may perchance become modified into larger segmented fungus-filaments, which, under favourable conditions, may fructify and produce spores, each of which is capable of developing into a plant like its parent in its latest phase of evolution. Originating, then, in the form of the minutest visible Living speck, we may find an organism passing more or less rapidly through the *Bacterium* and the *Vibrio* phase in order to grow into a *Leptothrix* thread, which, in its turn, by further growth and development, may give rise to a microscopic fungus producing large and definite spores. These fungus-spores, under similar influences, are capable of developing at once into a mycelium similar to that from which they have been produced. They do not again go through the lower terms of the series, but are veritable spores, serving only immediately to reproduce a fungus. It is an undoubted fact, on the other hand, which although often stated, is not generally known or admitted, that *Torula* cells and other fungus-spores may also originate as minutest visible *Living specks, which grow and develope at once into fungus-spores*, instead of passing through the intermediate stages of *Bacterium*, *Vibrio*, *Leptothrix*, and fungus-mycelium.

* Although this supposition is so far favourable to the views of the panspermatists, since it makes their real requirements so much more simple, still I am afraid they will find it a most troublesome and unorthodox supposition, unless they are disposed at the same time to become out-and-out developmentalists. Their position would be a much more easy one than it is at present if they chose to maintain that such specks of living matter—whatever their precise origin may have been—are practically mere specks of indifferent living matter, having no inherent tendencies, but plastic to the full, and capable of growing into such forms as their environing conditions may determine. (But having thus "swallowed a camel," why should they "strain at a gnat"? Why should they not also believe that the speck of indifferent living matter itself was formable by concurrence of necessary matter and conditions?) Unless the panspermatists were to adopt some such thorough-going developmental views as that which I have just indicated, they will gain comparatively little from the concessions which science compels us to make to them. They will better be able to reconcile their position with the comparative paucity of definite spores and germs which are actually detectable in the atmosphere; but they will find it as difficult as ever to account for the fact *that the right spores or germs should always be in the right place at the right time.* Very little short of a belief that each cubic inch of air contains the germs of myriads of organisms which are known, or which may hereafter be found under previously unknown sets of conditions, would be adequate to account for all the known and observable correspondences between the organisms found, and the precise nature of the fluids employed. And although the wildness and extreme improbability of this supposition must seem patent to all who have a knowledge of such subjects, strange to say, there are very many scientific men who would rather harbour such a belief—who would even, in spite of all laws of evidence, think it more probable—than another supposition, which is, on the contrary, in thorough harmony with all the main principles of their scientific creed. That a "vitalist" should reject this other supposition I can understand; but that all those scientific men—and they are happily numerous—who have discarded the notion of a special "vital principle," should still reject the notion that Living matter is capable of being evolved under suitable conditions and yet should accept this Panspermic hypothesis seeing the nature of the evidence which is respectively adducible in favour of the two views—seems to me almost inexplicable.

There is, indeed, strong reason for believing that the spores and confervoid-looking filaments in question have not dropped as such from the atmosphere, but that they are, rather, organisms which have developed within the crystal. It is almost impossible not to be struck with the improbability of the former of these alternatives, on account of the number of such large spores and filaments which this supposition would require to have been present in the atmosphere over the pans containing the crystallising materials, as compared with the extremely limited number of such large organisms which have ever been obtainable when experimental observations have been made upon the nature of the solid particles existing in the air of all ordinary localities.* The best evidence, however, in proof of the view that they are products of a development which has taken place within the crystal would be, if it could be shown that in a given batch of recently prepared crystals no such organisms were to be found, whilst in many other crystals belonging to the same batch, after an interval of weeks or months, the spores and filaments were to be discovered. Sufficient time has not yet elapsed to enable me to speak definitely on this subject. This much, however, I can say. Certain of the crystals of the batch prepared for me by Messrs. Hopkin and Williams, when examined two days after preparation, were found to contain scarcely a trace of air within. Now, however, after an interval of three weeks, through which they have been kept during the day-time at a temperature of about 80° Fahr., certain other of these crystals do, when dissolved, give exit to a notable quantity of air bubbles. This seems to indicate pretty clearly that a change of some kind has been taking place in the material of the crystal, which has led to the liberation of some of its constituents in a gaseous condition, and also, perhaps, to a liberation of some of its water of crystallisation. Whilst this has been taking place, its other elements may have been grouping themselves anew. Although, at present, there is still no certain trace of the spores or filaments, I am strongly disposed to expect that such organisms will manifest themselves in the course of a few weeks more.

[Two weeks after writing the above paragraph, and whilst these proofs were going through the press, on June 9 I examined three more specimens from the recent batch of crystals which had been set aside for observation. The quantity of gaseous bubbles which escaped from within the crystal seemed almost equal to those which had been met with within the older crystals. One or two small fragments of cotton also emerged, and in addition several very small masses of a transparent mucoid material, containing refractive protein-looking granules of various sizes and shapes. These were almost precisely similar to masses which had been met with in the older crystal. Here and there an early stage, or short portion, of a filament was seen amongst the granules, though none of these were sufficiently long to make me certain as to their nature and affinities. Although nothing else was found, the occurrence of the very small masses of mucoid material seemed to represent a stage in advance of what was met with at the last examination. One of these small mucoid masses I saw within an elongated cavity (near the surface of a half dissolved crystal), two-thirds of which was occupied by a large bubble of gas. Whilst the crystal was still under the microscope, I saw the bubble and the small mucoid mass emerge from the cavity.]

Assuming, then, the view which seems most probable, that the spores and filaments have grown within the crystal—that they are the developed representatives of certain specks of Living matter—two views may still be taken as to the origin of such Living specks. Either (1) these are some of the pre-existing "germs" of the panspermatists which have become mechanically enclosed within the crystal, or (2) these Living specks have been therein evolved by virtue of certain changes and re-arrangements which have taken place amongst the non-living constituents of the crystalline matter.

Of these two alternative views I am, after reflection on the following considerations and evidence, strongly inclined to believe that the latter is most probably the true one:—

(*a.*) It must be remembered that however strange and unlikely a situation the interior of a crystal may appear for the evolution of organisms, there is the strongest reason for believing that cavities are formed within crystals of tartrate of ammonia,†

* In all my investigations I have never met with spores *similar* to these except in one or other of the ammoniacal solutions.

† The gases which appear in bubbles increase in quantity with the age of the crystal, and these gases have been seen to be lodged in cavities within the crystal. These cavities are, perhaps, more especially liable to

and there is almost as much reason for believing that the confervoid-looking filaments and the fungus-spores have undergone a process of *growth* and *development* within such cavities. Other facts, which seem to lend an increased probability to this supposition, will shortly be detailed. But if "the conditions" are favourable enough to permit, or even to stimulate the molecular activity of certain Living particles, and if such molecular activity, whereby the Living speck grows and developes, is but the modified manifestation of the physical forces acting thereupon, I see no theoretical reason why the self-same physical forces acting upon the self-same materials should not have been able, in the same place, to *initiate* a molecular collocation similar to that which they now help to build up from moment to moment. We have been, perhaps, only too much in the habit of looking upon this as impossible. But let us sweep away this habit of mind for the moment, let us look at the facts as they are, and will it be at all easier for us, who believe in no special "vital principle," to understand how from moment to moment non-living matter is converted into matter which lives? However little we may understand it, this process is continually taking place in all growing representatives of the vegetable kingdom, and no one ever thinks of doubting that it does take place because he is unable to understand *how* it occurs. If it were once conceded that a *de novo* evolution of specks of Living matter were possible, then I think most physiologists would at once admit that where specks of Living matter are able to grow and develope, there also they may be quite capable of originating.

(*b.*) The matter of the crystals of tartrate of ammonia is, by a re-arrangement of its atoms, quite capable of giving origin to organisable compounds. If a small quantity of tartrate of ammonia is dissolved in a watch-glass with distilled water, and is protected as much as possible from dust and evaporation by being covered with a wine-glass from which the stem has been broken, and then again with a tumbler, it will be found during warm weather, that in the course of two or three days the bottom of the watch-glass is covered by a number of minute microscopic crystals, interspersed amongst a mixed layer composed of monads, bacteria, and minute *Torula* cells.* These organisms form, in fact, almost as freely (though more slowly) in the ammoniacal solution, as they do in an ordinary infusion containing organic matter. There can be little doubt that the amount of ammonia and of tartaric acid actually diminishes, and that the elements of these enter into new combinations.†

It may be said, however, that such changes do not take place by the mere action of physical forces upon the unstable molecules of the dissolved tartrate of ammonia, and that *Living ferments* are necessary for the initiation of such molecular re-arrangements. In answer to this I can only call attention to the fact that similar changes must have taken place in the fluids within the experimental tubes which were submitted by Dr. Frankland to a temperature varying from 146° to 153° C. for four hours, and that there is not one tittle of evidence at present existing to show that any Living thing could live through such an exposure, whilst there are very strong reasons indeed which should incline us to believe that no Living thing could be subjected to such a temperature without being hopelessly destroyed. Therefore in these cases it would appear that such molecular re-arrangements must have been initiated without the intervention of Living ferments, and thus, too, they would appear to be comparable with those that are known to take place in a solution of cyanate of ammonia. Here "spontaneously," or with the aid of a little heat only, a molecular re-arrangement occurs, and the saline cyanate of ammonia is replaced by a colloidal compound, urea. In order to effect this transformation, no Living ferments are necessary—none have been even supposed to exist, and there is, really, no more reason why we should imagine their presence to be necessary in order that tartrate of ammonia may undergo a more or less similar isomeric transformation.

A careful examination of the mode in which bacteria and *Torula* cells appear at the bottom of a watch-glass containing tartrate of ammonia in solution is also rather valuable on account of its bearing upon this question. What is true of the *Torula* cells is also true concerning the mode of origin of bacteria; the facts, however, can be ascertained rather more satisfactorily concerning the *Torula* cells, and for the sake of brevity I shall now speak only of them. These *Torula* cells, like the bacteria in their earlier stages, are motionless; although, therefore, they increase rapidly after one or more have been formed by a process of pullulation and growth, the numerous quite distinct patches which may be seen scattered over the bottom of the watch-glass, often at well marked distances from one another, represent so many distinct centres of origin. In these several patches there may be seen delicate ovoid *Torula* cells of almost any size beneath $\frac{1}{6000}''$ in diameter. The larger cells exist united in little groups of twos and threes, and budding from them may be seen pullulating projections of different sizes. Separate cells, also, may be seen, smaller and smaller in size, till at last they cease to be cellular in form, and we see only peculiarly refractive dots or specks less than $\frac{1}{50000}''$ in diameter. In other places a colony of *Torula* cells seems to be about to grow up. Here there may be seen merely one or two of the smallest bodies which distinctly display the cellular form interspersed amongst a variable number of the refractive specks of all sizes down to the *minimum visible* stage.* Beyond this, of course, all is darkness. We must be guided by other evidence in forming an opinion as to the probable source or mode of origination of these specks of Living matter, which are so extremely minute that they only just come within the range of our aided vision.

Another remarkable observation made upon a simple solution of carbonate of ammonia, in a watch-glass, makes still clearer the fact of the disseminated origin of organisms in such solutions. It throws light also upon the previous question as to whether the fungus-spores were developed within the crystals of tartrate of ammonia from specks of Living matter, or whether they were mechanically enclosed in their developed form; and it is sufficiently suggestive as to the possible influence of electrical conditions in promoting evolutional changes. Referring to notes made at the time, I extract the following particulars. About eleven P.M. on the 14th of the present month (June) a small quantity of ordinary sesquicarbonate of ammonia was dissolved in some apparently pure (though not distilled) water, in a watch-glass. After solution, and in about an hour's time, the fluid was carefully examined with different microscopic powers, and lastly the bottom of the watch-glass was scrutinised in very many situations with an immersion $\frac{1}{12}''$ object-glass. No Living thing of any kind was seen, though scattered over the bottom of the glass were a large number of tiny crystals, some larger and some smaller than $\frac{1}{3000}''$ in diameter. Under the polariscope they gave the most beautiful and varied colour reactions. The watch-glass was then placed on a mantel-piece with a soft surface (covered with velvet), a wine-glass, with its stem broken off, was inverted over it, and this again was covered by a tumbler, in order, as much as possible, to prevent evaporation and keep out dust. After twenty-four hours the bottom of the watch-glass was again carefully examined, with the $\frac{1}{12}''$ object-glass, and no change was observable. There were the same minute crystals, perhaps rather more numerous than before, but no recognisable specks of protoplasm or other trace of living things. The watch-glass was then replaced as before. The next day (June 16) the weather was hot and extremely sultry. The temperature was about 85° F. in the shade, and the thunder-storm, which seemed imminent during the whole of the day, began about 7 P.M., and continued till the early hours of the morning of the following day. At about 11.30 P.M. of this 16th of June, I again examined the solution in the watch-glass—forty-eight hours after it had been prepared. Then, scattered over the whole of the bottom of the glass, fungus-spores were seen in all stages of development intermixed with the small crystals. They were quite motionless, and mostly separate, rather than in distinct groups. They varied in size from the minutest visible speck up to a spherical nucleated body $\frac{1}{3300}''$ in diameter. No moving particles or bacteria were seen. Probably more than a thousand of these bodies were developing in the one watch-glass—each growing in its own place, and showing no evidence of multiplication by division or pullulation. Until they attained the

form in those crystals which are not perfect in shape, and which present a more or less opaque appearance in their interior. These less perfect types are probably for that reason more prone to undergo molecular changes under the influence of incident forces, especially in the neighbourhood of and around some fibre-fragment which has been enclosed.

* In saline solutions I have generally seen the organisms first, and have found them accumulated principally at the *bottom* of the watch-glass or other vessel in which the solution may have been contained.

† Saline solutions in which spores of fungi were placed, having been analysed previously by M. Pasteur, were again analysed by him after the plants had grown for a time. The proportion of ammonia and of other ingredients was found to have undergone a diminution correlative with the growth of the plants.

* When such a patch is marked, and watched at different intervals, a crop of perfect *Torula* cells is soon seen to occupy this same situation. And it may be well to state here that *Sarcina* also makes its appearance after a fashion which is essentially similar.

size of about $\frac{1}{100000}''$ in diameter no nucleus was visible, though they had by this time assumed a distinctly vesicular appearance. As the spores increased in size, the thick wall gradually became more manifest—though it had a rather rough granular appearance—and a nucleus gradually showed itself within, which was also granular.* The next morning, after twelve hours, the spores seemed to be much in the same condition, though numerous small colonies (30 to 50 in each) of motionless bacteria were now visible. During the day the air was clear, and the temperature lower (76° F.); and after twelve hours more (in

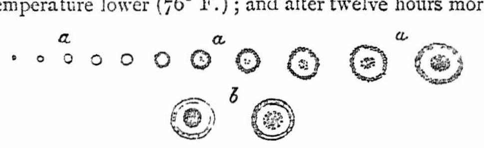

FIG 19.—Representing different stages in the development of Fungus-spores in a solution of Carbonate of Ammonia.

the evening) the bacteria were found to have considerably increased in number, and several of the fungus-spores were seen in a more devoloped condition—their thick walls being wholly or partially consolidated, and the nucleus was also more distinctly defined. In this condition they perfectly resembled the spores which were found in *Experiment* 20, and very closely resembled those which are to be met with in some of the old tartrate of ammonia crystals. The great majority of the spores were, however, still in the granular condition, and they seemed to have made no advance whatever. On the following day these spores were not quite so distinct—some of them seemed to be disintegrating, whilst none of them had undergone any further development. The bacteria, on the contrary, had decidedly increased in quantity. After two days more, minute *Torula* cells began to appear. These did not rapidly multiply, but soon began to develope into mycelial filaments.

The thick-walled spores had possibly come into existence under the influence of the high temperature and the disturbed electrical condition of the atmosphere †; and they seemed to be so much the creatures of these conditions that they were unable to survive under others which were different.

The mode, then, in which fungus-spores make their appearance in a solution of carbonate or tartrate of ammonia, seems to show that they must have originated in all parts of the solution, either by a coalescence and re-arrangement of the invisible molecules of a *pre-existing* colloidal compound,‡ or else through the development of innumerable but invisible "germs," which were disseminated through the liquid. That such invisible "germs" may have existed in the form of colloidal molecules, I am quite disposed to believe—though I am as strongly inclined to disbelieve that these fluids were saturated with "germs" of veritable fungus-spores, which had emanated from some pre-existing fungus of the same kind. We may grant that germs were there *in posse*, though not *in esse*. What warrant have we, indeed, for talking of actual though *invisible* fungus "germs"? No one can know more concerning their existence or formation than I know concerning the coalescence of colloidal molecules into minutest specks of Living matter. The necessity for the postulation of such "germs" must, therefore, seem different to different people, in accordance with the particular views which they may hold concerning Life. Those who believe in a special "vital principle" may naturally enough cling to the notion of a pre-existing germ, which may be the direct recipient of this peculiar power from some pre-existing organism; whilst those who are believers, rather, in the physical doctrines of Life will, I think, gradually find themselves contented with the pre-existence of potential "germs" in the form of colloidal molecules.

* This appearance I had not unfrequently seen before, where such spores had been developing in saline solutions, and it had always strongly suggested the notion to me that these spores were formed by a coalescence of granular particles. Here, however, there were no granules or moving particles present, the spores themselves were the only Living things, and it seemed quite certain that they could not have originated after this fashion. They obviously commenced as minute specks, and the granular appearance manifested itself so long as they were still increasing in size. When growth stopped consolidation began to take place, and an even double-contoured wall soon replaced that which was before irregular and granular.

† We may, perhaps, connect this possibility with the well-known fact that milk, beer, and other fluids are so very prone to turn sour during a thunder-storm, or whilst it is threatening.

‡ One which had existed before the organisms made their appearance, but which was the product of an isomeric modification of the carbonate of ammonia itself.

(*c*.) We find, also, associated with different sets of conditions, different kinds of Living things. In none of the crystals of tartrate of ammonia have I ever found a single distinct bacterium, and there has been the same complete absence of organisms of this kind in all my experimental fluids containing tartrate of ammonia and phosphate of soda, which have been sealed up *in vacuo*. This agreement is very striking, seeing that whenever a similar fluid, or a solution of tartrate of ammonia alone, is exposed to the air, then bacteria appear in abundance.* There is a marked accordance then between the organisms which are produced in the experimental tubes *in vacuo*, and those which come from the cavities within the crystals. There is the strongest reason for believing that the organisms which were met with within one of these experimental tubes must have been evolved *de novo*, since the existing state of our knowledge does not entitle us to believe that any such pre-existing Living thing could continue to live after it had been exposed to a temperature of from to 146° to 153°C. for four hours; and so we derive an additional presumption in favour of the *de novo* origination within the crystals, of those minutest specks of Living matter, which, as we have seen, are capable of developing into such fungus-spores as are there to be found.

In the face of this much more severe test (*Experiment* 19) it is needless to insist upon the results of other experiments in which the solutions were merely exposed to a temperature of 100°C. The fungus-spores which exist within the crystals of tartrate of ammonia do not differ, however, from all other fungus-spores that have been made the objects of experimentation. They too will not germinate after they have been exposed for one minute to a temperature of 100°C. I have taken spores and filaments from a crystal, and one half of them I have boiled for about a minute whilst the others have not been heated at all. The two patches have then been placed, at some little distance from one another, in the same growing box, with a few drops of a solution of tartrate of ammonia. The spores which had been boiled did not germinate, but those which had not been heated soon began to develope filaments. The pre-existing confervoid-looking organism, also, in the one case underwent no change, whilst in the other it grew into a distinct fungus—its filaments widening out till they became about four times as broad as they were originally. These unmistakeable fungus-filaments showed dissepiments at intervals dividing them into chambers, within which were contained large irregular blocks of protoplasm. Occasionally a filament larger than the others, might be seen terminating with a broad convex extremity, and afterwards there gradually appeared on the surface of this the minutest dot-like projections, which slowly increased in size and number. The larger of them soon became vesicular, and after a time within the vesicle granules began to cluster so as to constitute a nucleus. Thus were watched the early stages of the development of a head of fructification similar to, although much smaller than that which is represented in Fig. 17. The rate of growth was generally very slow, and after a time development ceased in my growing box, apparently because the conditions were not suitable for the evolution of such an organism as did grow luxuriantly enough within my experimental flasks. These observations were, however, extremely interesting, because I was thus able to trace all the stages in development, on one and the same plant, from mere granular abortive-looking *Leptothrix* threads, only $\frac{1}{20000}''$ in diameter, which gradually grew into a distinct confervoid-looking tube, having broken masses of protoplasm within, into slowly widening and dissepimented fungus-filaments, that were capable

* There is another difference also which deserves to be pointed out. The crystals of tartrate of ammonia or of phosphate of soda have never shown a trace of the Spiral-fibre organisms or of *Sarcina* (Fig. 13 *d*), and yet when the two have been mixed, in several of the fluids which have been kept *in vacuo*, the Spiral-fibre organism has appeared, and, similarly, on two out of three occasions when this mixture has been exposed to the air *Sarcina* has made its appearance. In one of the solutions *in vacuo* containing carbonate of ammonia and phosphate of soda, a somewhat similar Spiral-fibre has been found, and in the other *Sarcina* was met with. Both these organisms therefore seem dependent upon the presence of phosphates, and it is worthy of note that hitherto *Sarcina* has, so far as I am aware, never been known to exist except in one of the fluids of the animal body where phosphates naturally or unnaturally are present. At first *Sarcina* was discovered by Goodsir in the contents of the stomach, then it was found in the urine, and afterwards within the ventricles of the brain by Sir Wm. Jenner. And now I meet wth it in solutions containing an ammoniacal salt and a phosphate. M. Pasteur has (Ann. de Chim. et de Phys., 1862, Pl. II., fig. 27, K., and p. 80) figured, and alludes to an "Algue formée de cellules quaternaires, déposée sous forme de prêcipitaté," upon the walls of a flask which had contained "l'eau de levûre non sucrée," and which certainly, if not *Sarcina*, must be very closely allied thereto.

of producing a head of fructification of the *Penicillium* type. Thus, in fact, there appeared to be a strong tendency in the *Leptothrix* filaments and in the loose spores found within the crystal, to develope into the same kind of organism when either of these was placed under the influence of other and more suitable conditions. In the crystal itself, apparently, just as the conditions were not suitable for the germination of the spores, so they were not favourable for the developmental conversion of the confervoid-looking filaments into a fungus.

Whether there was any genetic relationship existing between these confervoid-looking filaments which commenced life as *Leptothrix* threads, and the few scattered spores which were frequently found with them within the crystal, is not quite certain. If any relationship did exist, however, it could only have been of one kind : the spores may have been descendants from the matter of the filaments, but the filaments were most certainly not developments from the spores. The spores existed singly or in groups of twos and threes. They were never seen in organic connection with the filaments, so that I am inclined to believe they were not even formed by a process of budding. They must, then, either have derived their origin from a minute speck of the matter of the filament which subsequently grew into a spore,* or they must have been evolved *de novo* where they were found, just as we are compelled to imagine that the similar spores must have been evolved *de novo* within the flask used in *Experiment* 19, at first by a coalescence and re-arrangement of colloidal molecules, and subsequently by a process of development similar to what is represented in Fig. 19. And, if the fungus-spore and the confervoid-looking filament both tend towards the same ultimate developmental form, we can only attribute this to the fact of the existence of a harmony between the "conditions" and such an organism. The confervoid filament and the fungus-spore are both produced within the same crystal : they seem to be but different products of what appears to us to be the same matter and the same "conditions," and if minute differences may have existed at first tending to make the initial modes of development different, the main intrinsic similarity manifests itself at last by leading them both along a line of development which terminates in a common organic form.†

For these various concurrent reasons, therefore, I deem it much more probable that the filaments and spores found within the crystals of tartrate of ammonia have been developed from specks of Living matter there evolved *de novo*, rather than that they have originated from germs of similar pre-existing organisms which had accidentally been enclosed within the crystals.

Before closing this paper, it will be necessary that I should refer more particularly to a certain part of M. Pasteur's researches, seeing that these have so strongly influenced the opinions of very many scientific men on the question of the truth or falsity of the doctrines of the heterogenists. As an experimental chemist, M. Pasteur takes a most honourable position in the foremost rank of workers, and all his investigations on this subject appear to have been conducted with the most scrupulous care. His reasonings, also, may seem at first sight to be all convincing, so that most people might be inclined to admit that he had "mathématiquement demontré," as he so frequently claims to have done, all that he had set himself to prove. The case may seem at first a poor one indeed for the heterogenists ; but as soon as one gets over the first impressions produced by the various experiments, and begins to inquire whether the reasonings concerning them have been in all cases fair and logical, then it may be seen that the evidence against the occurrence of heterogenesis is very far from being so strong as it, at first sight, appeared.

On two or three occasions, when it was very important that results should be looked at from different points of view, M. Pasteur has altogether failed to do this, and has wished to interpret them only in accordance with the views of the panspermatists, *quietly ignoring the equally legitimate interpretation of the same results which might have been given by the heterogenists.* At present I shall confine myself to one instance of this kind, because I think that on this particular point the reasonings of M. Pasteur are as mischievous as they are illogical. If others were to follow his example, then certainly we could never hope to get rid of the clouds of controversy which at present obscure this subject.

The experiments of Schwann were for some time erroneously believed by very many to have upset the doctrines of the heterogenists. No organisms, it was said, were ever developed in hermetically sealed vessels when the solutions containing the organic matter had been boiled, and when all the air which was allowed access to them had been previously calcined. Schwann's experiments did yield uniformly negative results when solutions of meat were employed ; though his experiments concerning alcoholic fermentation yielded results which were sometimes positive and sometimes negative. M. Pasteur also, for a time, obtained only negative results in repeating the experiments of Schwann. In these experiments, however, he had generally made use of "l'eau de levûre sucrée," of urine, or of some other fluid which was naturally unfitted to undergo evolutional changes of a high order, or even to produce lower organisms in great abundance.* But there came a time when M. Pasteur chanced to repeat his experiments, using precisely the same precautions as before, and yet the results were quite different—organisms were now found in his solutions. There was one important difference, it is true. In these latter experiments, M. Pasteur had made use of milk. Now the quantity of organic matter contained in milk is, of course, very great ; it is a highly nutritive and complex fluid. It might, therefore, and ought, perhaps, to have suggested itself to M. Pasteur that the different results of his later experiments were possibly explicable on the supposition that the restrictive conditions—the boiling of the solution and the closed vessel already containing air—were too potent to be overcome by the organic matter in the one solution, whilst they were not too potent and could not prevent evolutional changes taking place in that of the other. For if, in accordance with the belief of the evolutionists, different organic fluids have different initial tendencies to undergo the changes of evolution, it may be easily understood that as the conditions favourable to evolution are more and more restricted, certain of these fluids may altogether cease to undergo such changes, others may manifest them to a meagre extent, and others still, only a little more fully. Therefore, if under the conditions peculiar to Schwann's experiments, certain fluids with low evolutional tendencies have given rise to no organisms, there is nothing whatever contradictory in the fact if it is subsequently ascertained that other fluids, with greater inherent capacities of undergoing change, will, notwithstanding all the restrictive conditions, pass through certain Life-producing changes. When subjected to a pressure of one atmosphere, water boils at 212° F., alcohol at 173° F., and ether at 96° F. The restrictive condition, or atmospheric pressure, is here in each case the same, only, having to do with differently constituted fluids, it is natural enough to look for different results under the influence of like incident forces. Ether raised to a temperature of 100° F. would rapidly disappear in the form of vapour, though no such result would follow the heating of water to a similar extent. And similarly, whilst milk might be capable of yielding organisms in Schwann's apparatus, another fluid less rich in organic matter might fail to do so. It seems almost incredible that such considerations should not have suggested themselves to M. Pasteur; but yet we have no evidence that they did occur to him.† On

* A mode of origin of spores which is, I believe, quite familiar to fungologists.

† This form of fungus-spore seems to be most prone to occur where different ammoniacal salts are employed. It has been met with not only in the tartrate of ammonia solutions, but also in those containing oxalate of ammonia and carbonate of ammonia respectively. And it has been found in no other of my experimental fluids.

* In order to avoid circumlocution in this note, I speak from the evolutionist's point of view. And whether the organisms found in a given fluid have been actually produced therein, or have only there undergone development, we may, for the sake of argument, measure the evolutional capacity of a fluid by the amount and kinds of organisms which are produced in a given quantity of it, in a definite time, and at a given temperature. We must not, however, judge of the evolutional qualities of a fluid by its mere tendency to emit a bad odour in a short space of time. A certain fluid—urine for instance—judged by these qualities, may be disagreeably putrescible, though its evolutional tendencies may be quite low. By many experimenters this difference has not been appreciated, and they seem to imagine that in employing urine they make use of a fluid which is very favourable for such experiments. But they forget that urine is an effete product containing comparatively stable compounds, which have already done their work in the body. It may after a short time swarm with bacteria, and these may be followed by fungi ; but there is no comparison between the actual quantities even of these organisms, which will be developed in equal amounts of milk and urine respectively, when they are both exposed to the air for the same time in similarly-shaped vessels, and under the same bell-jar. The milk soon becomes actually solid with fungus growths. M Pasteur's "l'eau de levûre sucrée," by his own confession (*loc. cit.* note, p. 58) is never found to contain any of the higher ciliated infusoria, and in all probability, though it produces fungi, these are met with in much smaller quantity than they would have been in an equal bulk of milk under the same conditions.

† The experiments and reasonings to which I am now alluding are detailed in pp. 58—66 of M. Pasteur's Memoir (*Ann. de Chim. et de Phys.* 1862).

the contrary, he explains the discrepancy between his earlier and his later experiments by another supposition altogether. As on other occasions, he does not even suggest to the reader that any different explanation is possible from that which he adduces. He deliberately assumes that the bacteria and vibrios which were subsequently found in the milk used in these experiments had been derived from "germs" of such organisms which either pre-existed in or had obtained access to this fluid before it had been heated, and also (contrary to the general rule which had been previously admitted) he assumed that such supposed pre-existing germs were capable of resisting the influence of the boiling temperature in milk. No direct proof of the latter assumption was ever attempted, though M. Pasteur did afterwards endeavour to bring these exceptional cases under a general law by supposing that the results obtained were due to the absence of acidity in the fluids employed. Neutral or slightly alkaline fluids might, he thought, yield positive results in Schwann's experiments, because the germs of bacteria and vibrios were not destroyed by the boiling temperature in such fluids.

Such was the very definite statement made by M. Pasteur on the faith of a chain of evidence almost every link of which is ambiguous. The most direct observations, however, which can be made upon this subject (and to the desirability of making which he does not even allude) lend not the least support to his assumption. On the contrary, they go to confirm the rule which had hitherto been generally admitted as to the inability of any of these lower organisms to live after an exposure for even a few seconds in a fluid raised to a temperature of 100°C. I have again and again boiled neutral and alkaline infusions containing very active bacteria and vibrios, and the result has always been a more or less complete disruption of the vibrios, and the disappearance of all signs of life in the bacteria. All their peculiarly vital movements have at once ceased, and they have henceforth displayed nothing but mere Brownian movements.*

M. Pasteur approaches the solution of the discrepancy in this way. His attention was arrested by the fact that milk was an alkaline fluid, because he afterwards ascertained that other alkaline fluids also yielded positive results when submitted to the conditions involved in Schwann's experiments. Thus he himself helped to overturn the strongest evidence which had hitherto been brought to bear against the heterogenists. But, this being done, it was necessary for M. Pasteur to explain such an occurrence, if he was not prepared to yield his assent to the doctrine which he had formerly rejected. He now found, truly enough, that the mere alkalinity or acidity of the solution was a matter of great importance in these experiments; he found, for instance, that his "l'eau de levûre sucrée," naturally a faintly acid fluid, was always unproductive when submitted to Schwann's conditions unaltered, though it was, on the contrary, always productive if it had previously been rendered neutral or slightly alkaline by the addition of a little carbonate of lime. Facts of this kind were observed so frequently as to make him come to the conclusion that whilst acid solutions were never productive in Schwann's apparatus, any neutral or alkaline fluids might be, if it were otherwise suitable for such experiments. Then came the question as to how this was to be explained. It should be remembered that M. Pasteur was engaged in investigating the problem of the mode of origin of certain low organisms in organic fluids, concerning which so much controversy had taken place. In this controversy, hitherto, on the one hand, it had been contended that the Living things met with derived their origin from pre-existing "germs" that had survived all the destructive conditions to which the media supposed to contain them had been subjected; whilst, on the other hand, it was contended that if the media had been subjected to conditions which (by evidence the most direct and positive) had been shown to be destructive to the lowest Living things, then such Living things as were subsequently discovered in these fluids must have been evolved *de novo*. It was a question, therefore, on the one hand, as to the degree of vitality or capability of resisting adverse conditions peculiar to the lowest Living things; and, on the other, as to the strength of the tendency to undergo changes of an evolutional character in the organic matter existing in the solutions, and on the degree to which this molecular mobility could persist, in spite of the disruptive agency of the heat to which the organic matter might be subjected. When, therefore, after having been exposed to a given set of conditions, organisms are not subsequently found in the fluids employed, this is explicable in one of two ways—that is, in accordance with either of the two opposing views. Either the heat has proved destructive to all Living things in the solutions; or else the restrictive conditions to which the organic matter in the solutions has been exposed have been too severe to permit the occurrence of evolutional changes therein. Any person seriously wishing to ascertain the truth, and competent to argue, of course would not fail to see that he was bound to give equal attention to each of these possibilities. He had no right to *assume* that the probabilities were greater in favour of the one mode of explanation than they were in favour of the other; this was the very subject in dispute—this, it was, which had to be proved. When, therefore, it was definitely ascertained by M. Pasteur that acid solutions employed in Schwann's experiments yielded negative results as far as organisms were concerned, the establishment of this fact was in reality no more favourable to the one view than to the other. It is what the Panspermatists might have expected, it is true, because—regarding it only as a question of the destruction or non-destruction of germs—even they had convinced themselves that calcining the air and boiling the fluids were adequate to destroy all Living things contained in these media; but, on the other hand, it was equally open to the Evolutionists to say that—the restrictive conditions employed being so severe—they also were not surprised at the probable stoppage of evolutional changes and at the consequent non-appearance of organisms in the solutions. When positive results were obtained, however, the case became altogether different. The rule being absolute, so far as it had gone—and founded on good evidence, to which M. Pasteur and others had assented—with regard to the inability of Living things to survive in solutions after these had been raised to the boiling temperature for a few minutes; no one should have attempted to set aside this rule, except upon evidence equally direct and equally positive, though more extensive, than that upon which the rule had been originally founded. Certainly, no one should have attempted to set it aside on the strength of *indirect evidence, which, though equally capable of explanation in accordance with either one of the two opposing views, was tacitly represented to be explicable only in accordance with one of them.* Such, however, has been the conduct of M. Pasteur. It will, perhaps, scarcely be credited by many that the investigations of M. Pasteur, which have had so much influence, and which have been looked upon by many as models of scientific method, should really contain such fallacies. On other important occasions, however, his reasoning has been similarly defective, though he himself claimed and was believed by many to have "mathematically demonstrated" what he had so plausibly appeared to prove.*

In the present case, after his experiments with milk in Schwann's apparatus, M. Pasteur ascertained that in other alkaline or in neutral fluids, even when they had been subjected to all the conditions above mentioned, inferior organisms might be found more or less quickly. But he also discovered that even such solutions no longer yielded organisms if, instead of subjecting them to a heat of 100° C. they had been exposed for a few minutes to a temperature of 110° C. And it was on the strength of two or three other links of such evidence as this that M. Pasteur sought to upset the rule with regard to the inability of inferior organisms to resist the destructive influence of a moist temperature of 100° C. On such evidence as this he attempted to raise the possible limit of vital resistance by 10° C., and sought to establish the rule that Living organisms might survive in neutral or alkaline solutions if these had not been raised to a temperature of 110° C. He did not seem to see how utterly inconclusive his conclusions were, and that he had not so much right to assume that the organisms met with in his neutral or alkaline fluids had been derived from "germs" which had resisted the boiling temperature, as he or his opponents would have had at once to fall back upon the counter assumption that the evolutional tendencies of neutral or alkaline fluids exposed to high temperatures were greater than those of similar fluids when in an acid state—and that such neutral or alkaline fluids were, as was now seen, capable of overcoming the restrictive conditions in Schwann's experiments and of giving birth to organisms, by permitting the occurrence of Life-evolving changes amongst the colloidal molecules contained therein. He had less right to explain the facts as he did, than the evolutionist would have had to explain them as above mentioned, because he was thus attempting to upset

* See what has been previously said on this subject (p. 171).

* The space at my disposal does not permit of my alluding to these other occasions at present, though I shall do so in my forthcoming work.

previously admitted facts on insufficient evidence, whilst the reasonings of the evolutionist would have been in every way legitimate. And yet M. Pasteur left his readers to imagine that the explanation which he had adduced was that which was alone admissible; he did not refer to the existence of any other mode of explanation, but at once attempted to set aside the old rule. And similarly, when he ascertained that such alkaline or neutral fluids were no longer found to contain organisms if they had been previously submitted to a temperature of 110° C. he was entitled to draw no conclusion from such facts. Nevertheless, M. Pasteur did assume that such indirect evidence entitled him to come to the conclusion that the hypothetical "germs" contained in these solutions—those which were not killed, as he supposed by a temperature of 100° C, were destroyed by a temperature of 110° C. Such two-faced evidence is, however, worthless for raising the standard of vital resistance; and to ignore the possible differences which may exist, from the evolutionist's point of view, between acid and alkaline solutions, as M. Pasteur did, is about as reasonable as if he had imagined that because water does not boil at the temperature of 100° F. the same rule must necessarily hold good for ether.

Much evidence, indeed, can be brought forward to show that even at ordinary temperatures, and under conditions in which there is a moderately free exposure to the air (and, therefore, with every facility for the entrance of "germs"), a neutral or slightly alkaline solution is not only found to contain organisms more quickly, but these are found to exist therein in much greater variety than in solutions in other respects similar, save for the fact of their being slightly acid rather than alkaline or neutral. Any of the higher forms of ciliated Infusoria *may* appear in different neutral or slightly alkaline solutions, though they never present themselves in those having an acid reaction, and *neither are their undeveloped ova or their dead bodies to be found therein.* The amount of difference capable of being produced by the mere acidity of a solution was well seen by me a few months ago. Having prepared * a mixture of white sugar and tartrate of ammonia, with small quantities of phosphate of ammonia and phosphate of soda in distilled water, whose reaction was found to be neutral, two similar wide-mouthed bottles of about three ounces capacity were filled with the fluid. Both were kept side by side in a tolerably warm place, the mouths of the bottles being merely covered in each case by a piece of glass, after glycerine had been smeared over the rim on which the cover rested. Although not hermetically sealed, these solutions were thus sufficiently protected to prevent the access of much dust from the neighbouring fire. The fluid in the one bottle was allowed to remain neutral, whilst to that of the other four or five drops of acetic acid were added, so as to make it yield a faintly acid reaction to test paper. The results were quite different in the two cases. Towards the end of the fourth day the originally unaltered neutral solution began to assume a cloudy appearance; this increased in amount during the next day, and at the close of the sixth day a thin pellicle was found on the surface, and beneath it there were some irregular, flocculent, whitish masses buoyed up by small air bubbles. Examined microscopically, the pellicles and also the flocculent masses beneath were found to be made up of medium-sized monads and bacteria, mixed with crystals of triple phosphate. There were also many scattered cells of a *Torula*, varying from $\frac{1}{5000}''$ to $\frac{1}{10000}''$ in diameter. By this time (close of the sixth day), however, the companion solution which had been slightly acidified, had undergone scarcely any appreciable change. It was still quite clear and transparent, and there was no pellicle on the surface, though there was a very slight whitish flocculent stratum at the bottom of the bottle. Even on the twenty-first day this solution continued in much the same condition—still showing no trace of a pellicle. The fluid itself was clear, and there had been only a very slight increase in the thickness of the white flocculent layer at the bottom of the bottle, which, on microscopical examination, was found to be made up mainly of a granular matter having no definite character—though mixed with this there were a small number of minute but well-formed bacteria. This acid solution had remained throughout in the same warm place, but the bottle containing the neutral fluid had not (after the examination on the sixth day) been replaced in its original place near the fire; it had continued since this time in a part of the room altogether away from the fire, and yet when this also was examined on the twenty-first day, it was found to present a very cloudy, whitish appearance throughout, there was a thick flocculent stratum at the bottom, and also a very consistent, well-marked pellicle on the surface of the fluid, made up almost entirely of large and well-formed *Torula* cells.

* Dec. 23, 1869. The weather being very cold and frosty. The mixture employed was another portion of the same solution as was used in *Experiment* 9.

Although the results here detailed, as occurring in the neutral and the acidified solutions respectively, are so strikingly different, still they are by no means singular or peculiar to the particular kind of solution which was employed in this experiment. Phenomena essentially similar in kind may be observed when almost any neutral or slightly alkaline organic infusion is employed. Thus, to quote one only out of many experiments bearing upon this point. A short time ago, having prepared a pretty strong infusion of mutton, about an ounce and a half was put, after filtration, into each of two similar flasks. The one portion of the infusion was allowed to remain neutral, whilst to the other were added three drops of strong acetic acid, so as to make the whole yield a faintly acid reaction to test paper. The two flasks were then exposed side by side to a temperature of 75° to 80° F. during the day. In twenty-four hours time the neutral solution was clouded and more or less opaque, whilst the portion which was acid appeared perfectly unchanged. It was as clear as ever; and so it continued even to the end of forty-eight hours, although by this time the neutral solution was quite opaque, muddy-looking, with a pellicle on its surface, and also some flocculent deposit at the bottom of the flask. A microscopical examination of two or three drops of this fluid showed that it was teeming with most actively moving monads, bacteria, and vibrios, whilst a similar examination of the acid fluid showed not a trace of these or of any other kinds of organisms.*

The difference between the results in these two sets of cases was thus extremely well marked, and the results themselves are well worth our serious attention. We had to do with equal bulks of fluid, placed under similar conditions and similarly constituted, with the exception that in each set a few drops of acid had been added to the one fluid, whilst the other was allowed to remain neutral. And it must be confessed that the difference encountered was very similar in kind to that which was observed by M. Pasteur when he made use of acid, or of neutral or alkaline solutions respectively, in repeating the experiments of Schwann. Only here we have had nothing to do with the destructive agency of heat, and germs were as free to enter into the one solution as they were into the other, so that the differences actually observed would seem now, at all events, due simply to the different qualities of the fluids themselves. Of course, such results cannot be adduced as evidence that the evolutional property of the neutral solution was higher than that of the acid solution. It may be not a case of evolution at all, but simply one of growth and development. The results, however, do show plainly enough that the neutral solution was the one most favourable to the growth and development of Living things. And if, starting from this fact, which cannot be denied, the evolutionists see reasons which induce them to assume the possibility that, in addition to mere growth and development, an actual origination of Living things may have taken place *de novo*, they would also be likely to suppose that the neutral fluid was more favourable to such evolution than that which had been acidified.† That solution which was found favourable for the processes of growth and development would also, in all probability, be favourable for evolution. A process would be most likely to be *initiated* where the conditions were suitable for its continuance. And surely the same factors would be at work in the initiation of a Living thing as would be called into play during its contiuance as a growing Living thing. The presumption, therefore, is a fair one, that solutions which are favourable to the growth and development of certain organisms would also be favourable to the evolutional changes which more especially lead to the initiation of such Living things. Seeing, then, that the question of the occurrence or non-occurrence of such initiations is the very matter in dispute, it is certainly most imperative that no one engaged in investigations bearing on the subject should fail to appreciate this that these are possibilities whose probability ought to be assumed as equal. We may well be amazed, then, at the utter one-sidedness of M.

* The reverse results, which may be produced by neutralising the acidity of a naturally acid fluid, will be exemplified further on.

† Taking it only for what it is worth, it is, at least, deserving of mention that no reason seems assignable for the presence of *Torulæ* in the one saline solution and not in the other. They were both equally exposed to the advent of "germs." It can scarcely be imagined that the *Torula* germs did obtain access to the both solutions, but that they perished in that which was faintly acid, for, as a matter of fact, *Torulæ* are much more frequently met with in acid solutions than in those which are alkaline.

Pasteur, when we find him completely ignoring one of these points of view, interpreting all his experiments by the light of a foregone conclusion, and looking upon the different solutions employed solely as fluids which are destructive or not destructive to hypothetical "germs" at a given temperature.

It should not be understood that I regard all acid solutions as having a low evolutional tendency. On the contrary, I believe I have helped to show in this paper that *some* acid solutions are most prone to undergo evolutional changes of a certain kind. These do not result in the production of Living things of a high type, but rather in an abundance of organisms of a comparatively low type. It seems to me, however, after careful observation and experiment, that a neutral or slightly alkaline solution to which a few drops of acid have been added is always found, after a given time, to contain a notably smaller number of organisms than an equal bulk of the unaltered solution. And conversely, having an acid solution whose productiveness is known, the number of organisms found in equal bulks under similar conditions, can almost always be notably increased in either one of them by the mere addition of a few drops of *liquor potassæ*, so as to render it neutral or slightly alkaline. This, as I previously pointed out, *may* be interpreted as an indication that alkalinity, or neutrality of the fluids, is more favourable than their acidity for the occurrence of evolutional changes. And thus the fact that organisms were never met with when an acid "eau de levûre sucrée" was used in repeating the experiments of Schwann, though they were met with, on the contrary, in other experiments where portions of this same fluid had been used which had been rendered slightly alkaline by the addition of chalk, might be explained without the aid of that supposition which alone seems to have occurred to M. Pasteur.

But, after reflection on this subject, it seemed to me quite within the range of probability, that the difference between acid and alkaline solutions in respect of the number of organisms which are to be found therein, when these have been simply exposed to ordinary atmospheric conditions, might be exaggerated after they had been exposed to the temperature at which water boils. It seemed quite possible that high temperatures might be more destructive to organic matter when this was contained in acid solutions than when it existed in alkaline solutions. Just as the acid seems to exercise a certain noxious influence even at ordinary temperatures, it may be conceived that this influence, whatever its nature, may be increased in intensity with the rise of temperature, and with the consequent greater facility for the display of chemical affinities. Hot acids will frequently dissolve metals which would remain unaffected by them at ordinary temperatures ; and chemical affinities generally are notably exalted by an increased amount of heat. Just as the addition of an acid, therefore, to a previously neutral or slightly alkaline fluid containing organic matter in solution, appears to alter its character in some mysterious way, so may we assume that its action upon the unstable organic molecules goes on increasing in intensity as the fluid becomes hotter. So that, when two portions of a solution containing organic matter —the one neutral and the other acid—have been raised to a temperature of 100° C., whilst the organic matter of the one has been injured only by the mere action of heat ; that of the other solution, which has been acidified, has not only had to submit to the deleterious influence of the high temperature, but also to the increased activity of the acid at this temperature. Thus the result would be that the amount of difference between the two solutions which existed before they had been heated, would be found more or less increased after they had been exposed to the high temperature, in direct proportion to the increase in intensity of the action of the acid produced by such high temperature. What we know concerning the precipitation of albumen in urine is quite in harmony with this view. When albumen is present, and the fluid has an alkaline reaction, mere boiling does not cause its precipitation, though, if the reaction had been acid,* the albumen present would have been precipitated, when, or even before, the fluid was raised to the boiling temperature. Or, the same result might have been brought about by the addition of a small quantity of acid to a portion of a neutral or alkaline albuminous specimen which had just been boiled without having brought about a precipitation of the albumen. Thus, the addition or presence of a small quantity of acid, in conjunction with an elevated temperature, is seen to be capable of producing results which cannot be produced by the mere elevated temperature alone. But the fact that an isomeric transformation of albumen can be brought about in this way—that albumen can be transformed so as to be no longer capable of remaining in solution—shows that a molecular change has been brought about by the influence of the acid working at high temperatures, which neither the acid nor the heat, working alone, are capable of effecting.

* Provided this was not due to the presence of a mere trace of nitric acid.

With the view of throwing further light on this subject, on March 27 of the present year I made the following experiments:—A tolerably strong infusion of white turnip was prepared and subsequently filtered.* This had a decidedly *acid* reaction. It was then divided into two portions, one of which was allowed to remain unaltered, whilst to the other a few drops of *liquor potassæ* were added, so as to give the fluid a very faintly alkaline reaction. This addition produced a slight alteration also in the naked eye appearance of the fluid ; the faintly whitish opalesence which formerly existed disappeared, and was replaced by an equally faint brownish tinge. About an ounce of each of the two fluids was then placed separately in two small flasks. The fluids were not heated at all, but a piece of paper having been placed loosely in the neck of each so as to exclude dirt, they were exposed side by side to a temperature varying from 75° to 85° F. After twenty-four hours,† the unaltered acid infusion merely showed a more decided opalesence approaching to cloudiness ; though that which had been rendered faintly alkaline, had a distinctly opaque whitish colour, and there was also a distinct pellicle covering more than one half of the surface of the fluid. In the three or four succeeding days the amount of opacity, of pellicle, and of deposit increased in both the fluids, though each of these continued to be more manifest in the alkaline than in the acid solution. After a week, however, the difference was scarcely appreciable, though on the whole, for about two weeks afterwards, the quantity of new matter seemed to be greater in the alkaline than in the acid solution.

But, on the same morning that these two portions of the acid and alkaline infusions had been set aside for observation, I had placed with them vessels containing two other specimens of the same fluids. These had been previously treated in the following manner : The acid fluid and the alkaline fluid, after they had been placed in their respective flasks, and the necks of these had been drawn out, were then boiled for ten minutes, and at the expiration of this time—whilst ebullition was still continuing— the drawn-out necks of the flasks were hermetically sealed in the blow-pipe flame. These vessels, therefore, were intended to show, by comparison with the other two, whether the difference produced by mere acidity or alkalinity of the solutions at low temperatures was or was not intensified by the action of heat. The flasks were all suspended in a group at the same time, and were, thenceforward, subjected to the same temperature. The results were as follows : After twenty-four hours the slightly alkaline fluid which had been boiled showed a slight though decided opalescence ; it was, in fact, very similar in appearance to the acid solution which had not been boiled. The boiled acid solution was, however, as clear as when the flask was first suspended, and so it remained, apparently quite unaltered, after it had been suspended a week, though the boiled alkaline solution had by this time become decidedly opaque, and also showed some flocculent matter lying at the bottom of the vessel. And now‡, after they have been suspended more than three weeks, the acid solution still remains almost transparent, presenting only the faintest cloudiness, though with no pellicle or deposit at the bottom.§ The boiled alkaline fluid, however, presents a totally different appearance ; it is whitish and quite opaque, there is a very thick pellicle covering part of its surface, and also some whitish sediment at the botton of the flask.

The difference which already exists between alkaline and acid solutions at ordinary temperatures is, then, seen to be most notably intensified after similar alkaline and acid solutions have been raised to a temperature of 100° C. And whilst these differences tend to substantiate the reality of the other mode of explanation (which I have suggested) of the discrepancies observed by M. Pasteur when he repeated Schwann's experiments with acid and with alkaline organic infusions respectively, they may also

* The turnip at this season of the year was however very poor and dry as compared with that which was employed in some of my earlier experiments (*Experiments* 4 to 9) during the winter months.

† During the whole of this time the heat only varied between the limits mentioned.

‡ April 19, 1870.

§ This solution was, therefore, much more backward in exhibiting signs of change than were the others which had been used in *Experiments* 4 to 8— a difference probably explicable by the poorer quality of the turnip used in this last experiment.

be considered to strengthen the probabilities in favour of my assumption that an acid fluid is less prone to undergo those molecular changes which lead to the evolution of Living things, than an otherwise similar fluid whose reaction is neutral or faintly alkaline. And yet this explanation was utterly ignored by M. Pasteur; he wrongly assumed that the before-mentioned discrepancies were explicable only in one way; and he moreover illogically attempted to set aside a rule to which he had previously assented, on the strength of evidence which was most ambiguous, and, therefore, inconclusive—*in nature*. M. *Pasteur engages himself* in a controversy concerning one of the most important questions in the whole range of biological science, and yet he assumes the attitude of a man who is so convinced beforehand of the error of those who are of the opposite opinion, that he will not abide by ordinary rules of fairness, he will not even, at first, assume the possibility of the truth of the opinions which are opposed to his own. Ambiguous evidence is explained as though it were not ambiguous; conclusions based upon good evidence are attempted to be set aside in favour of conclusions based upon evidence which is comparatively worthless; and, by such illogical methods, M. Pasteur proclaims that he has "mathematically demonstrated" the truth of his own views. Unfortunately for the cause of Truth, people have been so blinded by his skill and precision as a mere experimenter, that only too many have failed to discover his shortcomings as a reasoner.

But it will already have been perceived by the attentive reader, that it was not necessary for me—in my endeavour to establish as a Truth the great doctrine which M. Pasteur has striven to repudiate—to show the inconclusiveness of his reasonings on that branch of the subject to which I have just been alluding. I have striven rather to show in their true light the real nature of such modes of reasoning, which are I fear only too likely to be repeated by others. So long as people are unable readily to appreciate the worthlessness of arguments like these, they will never be likely to penetrate through the clouds of controversy which envelope this subject. Their mental vision will be blinded, and the truth will remain hidden from them. But, lured on by the success of reasonings such as these, others would have grown bolder still, and precisely as the exigencies of the case required, so would the standard of vital resistance to heat have been raised. *What object can there be in laboriously* ascertaining by direct experiment and observation at what temperature the lower kinds of organisms cease to live, if the information so obtained is to be studiously ignored just when it ought to be used as a kind of touchstone, or as a lamp to illumine phenomena whose explanation would otherwise be doubtful? It is a very easy process, certainly, first to start with the assumption that it is "impossible" for Living things to be evolved *de novo*, and then, every time that Living things are found under conditions where they ought not to occur (if the assumption were true, and if the generally received notions concerning vital resistance were correct), to assume that the very fact of their having been found under these conditions, of and by itself, shows that the previous notions concerning vital resistance were entirely wrong, and that the organisms which were formerly admitted to have been destroyed by a temperature of 100° C., must now be considered to be able to brave for four hours a temperature of 150° C., simply because they have been found in fluids which had been submitted to this temperature. The reasoning by which Truth is sought to be ascertained is, in fact, this:—No matter what the temperature to which the solutions and the hermetically sealed flasks have been exposed—be it even 500° C.—if Living organisms are subsequently found in the solutions, then they or their "germs" *must* have been able to resist the destructive influence of such a temperature, simply because Living things have been found, and because it is *assumed* that they cannot be evolved *de novo*. It is to be hoped that this is not the kind of reasoning which will find favour with those who are seeking for the advancement of Biological Science!

My principal objects in this paper have been to show:—

1. That there is a strong *a priori* probability in favour of the possibility of the occurrence of the heterogenous evolution of Living things, and that the most reliable scientific data which we possess do, in fact, fully entitle us to believe in this as a possibility.

2. That microscopical investigation, whilst it teaches us as much concerning the mode of origination of the lowest Organisms as it does concerning the mode of origin of Crystals, enables us to watch all the steps of various processes of heterogenous Evolution of slightly higher Organisms, such as may be seen taking place in a pellicle on a fluid containing organic matter in solution.

3. That the kinds of organisms which have been shown to be destroyed by a temperature of 100° C. may be obtained in organic fluids, either acid or alkaline, which, whilst enclosed within hermetically sealed and airless flasks, had been submitted not only to such a temperature but even to one varying between 146° and 153° C. for four hours.

4. That a new and direct evolution of organisable compounds *may, in all probability,** *be capable of arising, sometimes* by isomeric transformation of the atomic constituents of a single saline substance such as tartrate of ammonia, and sometimes by a re-arrangement of certain of the atomic constituents belonging to two or more saline substances existing together in solution. It is not only supposed that this may occur, but that even Living things may subsequently be evolved therefrom, when the solutions have been exposed, as before, in airless and hermetically sealed flasks to a temperature of 146° to 153° C. for four hours.

On account of this *a priori* probability, and in the face of this evidence, I am, therefore, content, and as I think justified, in believing that Living things may and do arise *de novo*. Such a belief necessarily carries with it a rejection of M. Pasteur's Theory of Putrefaction, and of the so-called "Germ Theory of Disease."

H. CHARLTON BASTIAN

* It is not pretended that this is proved. The aid of the chemist and physicist must be much more extensively resorted to before such a point could be proved. I hope soon, however, to be able to bring forward additional evidence bearing upon this part of the subject.

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