THURSDAY, JULY 7, 1870

PASTEUR'S RESEARCHES ON THE DISEASES OF SILKWORMS

I HAVE recently received from M. Pasteur a copy of his new work, "Sur la Maladie des vers à soie," a notice of which, however brief and incomplete, will, I am persuaded, interest a large class of the readers of NATURE. The book is the record of a very remarkable piece of scientific work, which has been attended with very remarkable practical results. For fifteen years a plague had raged among the silkworms of France. They had sickened and died in multitudes, while those that succeeded in spinning their cocoons furnished only a fraction of the normal quantity of silk. In 1853 the silk culture of France produced a revenue of one hundred and thirty millions of francs. During the twenty previous years the revenue had doubled itself, and no doubt was entertained as to its future augmentation. "Unhappily, at the moment when the plantations were most flourishing, the prosperity was annihilated by a terrible scourge." The weight of the cocoons produced in France in 1853 was twenty-six millions of kilogrammes; in 1865 it had fallen to four millions, the fall entailing in the single year last mentioned a loss of one hundred millions of francs.

The country chiefly smitten by this calamity happened to be that of the celebrated chemist Dumas, now perpetual secretary of the French Academy of Sciences. He turned to his friend, colleague, and pupil, Pasteur, and besought him with an earnestness which the circumstances rendered almost personal, to undertake the investigation of the malady. Pasteur at this time had never seen a silkworm, and he urged his inexperience in reply to his friend. But Dumas knew too well the qualities needed for such an inquiry to accept Pasteur's reason for declining it. "Je met," said he, "un prix extrème à voir votre attention fixée sur la question qui interesse mon pauvre pays; la misère surpasse tout ce que vous pouvez imaginer." Pamphlets about the plague had been showered upon the public, the monotony of waste paper being broken at rare intervals by a more or less useful publication. "The Pharmacopœia of the Silkworm," wrote M. Cornalia in 1860, "is now as complicated as that of man. Gases, liquids, and solids have been laid under contribution. From chlorine to sulphurous acid, from nitric acid to rum, from sugar to sulphate of quinine,—all has been invoked in behalf of this unhappy insect." The helpless cultivators, moreover, welcomed with ready trustfulness every new remedy, if only pressed upon them with sufficient hardihood. It seemed impossible to diminish their blind confidence in their blind guides. In 1863 the French Minister of Agriculture himself signed an agreement to pay 500,000 francs for the use of a remedy which its promoter declared to be infallible. It was tried in twelve different departments of France and found perfectly useless. In no single instance was it successful. It was under these circumstances that M. Pasteur, yielding to the entreaties of his friend, betook himself to Alais in the beginning of June 1865. As regards silk husbandry, this was the most important department in France, and it was also that which had been most sorely smitten by the epidemic.

The silkworm had been previously attacked by *muscardine*; a disease proved by Bassi to be caused by a vegetable parasite. Muscardine, though not hereditary, was propagated annually by the parasitic spores, which, wafted by winds, often sowed the disease in places far removed from the centre of infection. According to Pasteur, muscardine is now very rare; but for the last fifteen or twenty years a deadlier malady has taken its place. A frequent outward sign of this disease are the black spots which cover the silkworms, hence the name pébrine, first applied to the plague by M. de Quatrefages, and adopted by Pasteur. Pébrine also declares itself in the stunted and unequal growth of the worms, in the languor of their movements, in their fastidiousness as regards food, and in their premature death. The discovery of the inner workings of the epidemic may be thus traced. In 1849 Guerin Méneville noticed in the blood of certain silkworms vibratory corpuscles which he supposed to be endowed with independent life, and to which he gave a distinctive name. As regards the motion of the particles, Filippi proved him wrong; their motion was the well-known Brownian motion. But Filippi himself committed the error of supposing the corpuscles to be normal to the life of the insect. They are really the cause of its mortality—the form and substance of its disease. This was studied and well described by Cornalia; while Lebert and Frey subsequently found the corpuscles not only in the blood, but in all the tissues of the silkworm. Osimo, in 1857, discovered the corpuscles in the eggs, and on this observation Vittadiani founded, in 1859, a practical method of distinguishing healthy from diseased eggs. The test often proved fallacious, and it was never extensively applied.

The number of these corpuscles is sometimes enormous. They take possession of the intestinal canal, and spread thence throughout the body of the worm. They fill the silk cavities, the stricken insect often going through the motions of spinning without any material to answer to the act. Its organs, instead of being filled with the clear viscous liquid of the silk, are packed to distension by these corpuscles. On this feature of the plague Pasteur fixed his attention. He pursued it with the skill which appertains to his genius, and with the thoroughness that belongs to his character. The cycle of the silkworm's life is briefly this:—From the fertile egg comes the little worm, which grows, and after some time casts its skin. This process of moulting is repeated two or three times at subsequent intervals during the life of the insect. After the last moulting the worm climbs the brambles placed to receive it, and spins among them its cocoon. It passes thus into a chrysalis; the chrysalis becomes a moth, and the moth when liberated lays the eggs which form the starting-point of a new cycle. Now Pasteur proved that the plague-corpuscles might be incipient in the egg, and escape detection; they might also be germinal in the worm, and still baffle the microscope. But as the worm grows, the corpuscles grow also, becoming larger and more defined. In the aged chrysalis they are more pronounced than in the worm; while in the moth, if either the egg or the worm from which it comes should have been at all stricken, the corpuscles infallibly appear, offering no difficulty of detection. This was the first great point made out in 1865 by Pasteur. The Italian naturalists, as aforesaid, recommended the examination of the eggs before risking their incubation. Pasteur showed that both eggs and worms might be smitten and still pass muster, the culture of such eggs or such worms being sure to entail disaster. He made the moth his starting-point in seeking to regenerate the race.

And here is to be noted a point of immense practical importance. The worms issuing from the eggs of perfectly healthy moths may afterwards become themselves infected through contact with diseased worms, or through germs mixed with the dust of the rooms in which the worms are fed. But though the moths derived from the worms thus infected may be so charged with corpuscles as to be totally unable to produce eggs fit for incubation, still Pasteur shows that the worms themselves, in which the disease is not hereditary, never perish before spinning their cocoons. This, as I have said, is a point of capital importance; because it shows that the moth-test, if acted upon, even though the worms during their "education" should contract infection, secures, at all events, the next subsequent crop.

Pasteur made his first communication on this subject to the Academy of Sciences in September 1865. It raised a cloud of criticism. Here forsooth was a chemist rashly quitting his proper *métier* and presuming to lay down the law for the physician and biologist on a subject which was eminently theirs. "On trouva étrange que je fusse si peu au courant de la question; on m' opposa des travaux qui avaient paru depuis longtemps en Italie, dont les resultats montraient l'inutilité de mes efforts, et l'impossibilité d'arriver à un resultat pratique dans la direction que je m'étais engagé. Que mon ignorance fut grande au sujet des recherches sans nombre qui avaient paru depuis quinze années." Pasteur heard the buzz, but he continued his work. In choosing the eggs intended for incubation, the cultivators selected those produced in the successful "educations" of the year. But they could not understand the frequent and often disastrous failures of their selected eggs; for they did not know, and nobody prior to Pasteur was competent to tell them, that the finest cocoons may envelop doomed corpusculous moths. It was not, however, easy to make the cultivators accept new guidance. To strike their imagination and if possible determine their practice, Pasteur hit upon the expedient of prophecy. In 1866 he inspected at St. Hippolyte-du-Fort fourteen different parcels of eggs intended for incubation. Having examined a sufficient number of the moths which produced these eggs, he wrote out the prediction of what would occur in 1867, and placed the prophecy as a sealed letter in the hands of the Mayor of St. Hippolyte.

In 1867 the cultivators communicated to the mayor their results. The letter of Pasteur was then opened and read, and it was found that in twelve out of fourteen cases, there was absolute conformity between his prediction and the observed facts. Many of the educations had perished totally; the others had perished almost totally; and this was the prediction of Pasteur. In two out of the fourteen cases, instead of the prophesied destruction, half an average crop was obtained. Now, the parcels of eggs here referred to were considered healthy by their owners. They had been hatched and tended in the firm hope that the labour expended on them would prove remunerative. The application of the moth-test for a few minutes in 1866 would have saved the labour and averted the disappointment. Two additional parcels of eggs were at the same time submitted to Pasteur. He pronounced them healthy; and his words were verified by the production of an excellent crop. Other cases of prophecy still more remarkable, because more circumstantial, are recorded in the work before us.

These deadly corpuscles were found by Leydig in other insects than the silkworm moth. He considers them to belong to the class of psososperms founded by J. Müller. "This," says Pasteur, "is to regard the corpuscular organism as a kind of parasite, which propagates itself after the manner of parasites of its class." Pasteur subjected the development of the corpuscles to a searching examination. With admirable skill and completeness he also examined the various modes by which the plague is propagated. He obtained perfectly healthy worms from moths perfectly free from corpuscles, and selecting from them 10, 20, 30, 50, as the case might be, he introduced into the worms the corpusculous matter. It was first permitted to accompany the food. Let us take a single example out of many. Rubbing up a small corpusculous worm in water, he smeared the mixture over the mulberry leaves. Assuring himself that the leaves had been eaten, he watched the consequences from day to day. Side by side with the infected worms he reared their fellows, keeping them as much as possible out of the way of infection. These constituted his "lot temoign," his standard of comparison. On the 16th of April, 1868, he thus infected thirty worms. Up to the 23rd they remained quite well. On the 25th they seemed well, but on that day corpuscles were found in the intestines of two of the worms subjected to microscopic examination. The corpuscles begin to be formed in the tunic of the intestine. On the 27th, or eleven days after the infected repast, two fresh worms were examined, and not only was the intestinal canal found in each case invaded, but the silk organ itself was found charged with the corpuscles. On the 28th the twenty-six remaining worms were covered by the black spots of pébrine. On the 30th the difference of size between the infected and non-infected worms was very striking, the sick worms being not more than two-thirds of the size of the healthy ones. On the 2nd of May a worm which had just finished its fourth moulting was examined. Its whole body was so filled with corpuscles as to excite astonishment that it could live. The disease advanced, the worms died and were examined, and on the 11th of May only six out of the thirty remained. They were the strongest of the lot, but on being searched they also were found charged with corpuscles. Not one of the thirty worms had escaped; a single corpusculous meal had poisoned them all. The standard lot, on the contrary, spun their fine cocoons, and two only of their moths were found to contain any trace of corpuscles. These had doubtless been introduced during the rearing of the worms.

As his acquaintance with the subject increased, Pasteur's desire for precision augmented, and he finally gives the growing number of corpuscles seen in the field of his microscope from day to day. After a contagious repast the number of worms containing the parasite gradually augmented until finally it became cent. per cent. The number of corpuscles would at the same time rise from 0 to 1, to 10, to 100, and sometimes even to 1,000 or 1,500 for a single field of his microscope. He then varied the mode of infection. He inoculated healthy worms with the corpusculous matter, and watched the consequent growth of the disease. He showed how the worms inoculate each other by the infliction of visible wounds with their "crochets." In various cases he washed the "crochets," and found corpuscles in the water. He demonstrated the spread of infection by the simple association of healthy and diseased worms. In fact, the diseased worms sullied the leaves by their dejections, they also used their crochets, and spread infection in both ways. It was no hypothetical infected medium that killed the worms, but a definitely-organised and isolated thing. He examined the question of contagion at a distance, and demonstrated its existence. In fact, as might be expected from Pasteur's antecedents, the investigation was exhaustive, the skill and beauty of his manipulation finding fitting correlatives in the strength and clearness of his thought.

Pébrine was an enigma prior to the experiments of Pasteur. "Place," he says, "the most skilful educator, even the most expert microscopist, in presence of large educations which present the symptoms described in our experiments; his judgment will necessarily be erroneous if he confines himself to the knowledge which preceded my researches. The worms will not present to him the slightest spot of pébrine; the microscope will not reveal the existence of corpuscles; the mortality of the worms will be null or insignificant; and the cocoons leave nothing to be desired. Our observer would, therefore, conclude without hesitation that the eggs produced will be good for incubation. The truth is, on the contrary, that all the worms of these fine crops have been poisoned; that from the beginning they carried in them the germ of the malady; ready to multiply itself beyond measure in the chrysalides and the moths, thence to pass into the eggs and smite with sterility the next generation. And what is the first cause of the evil concealed under so deceitful an exterior? In our experiments we can, so to speak, touch it with our fingers. It is entirely the effect of a single corpusculous repast; an effect more or less prompt according to the epoch of life of the worm that has eaten the poisoned food."

It was work like this that I had in view when, in a lecture which has brought me much well-meant chastisement from a certain class of medical men, and much gratifying encouragement from a different class, I dwelt on the necessity of experiments of physical exactitude in testing medical theories. It is work like this which might be offered as a model to the physicians of England, many indeed of whom are pursuing with characteristic skill and energy the course marked out for them by this distinguished master. Prior to Pasteur, the most diverse and contradictory opinions were entertained as to the contagious character of pébrine; some stoutly affirmed it, others as stoutly denied it. But on one point all were agreed. "They believed in the existence of a deleterious medium, rendered epidemic by some occult and mysterious influence, to which was attributed the cause of the malady." Between such notions and the work of Pasteur, no physically-minded man will, I apprehend, hesitate in his choice.

Pasteur describes in detail his method of securing healthy eggs, which is nothing less than a mode of restoring to France her ancient prosperity in silk husbandry. And the justification of his work is to be found in the reports which reached him of the application, and the unparalleled success of his method, at the time he was putting his researches together for final publication. In France and Italy his method has been pursued with the most surprising results. It was an up-hill fight which led to this triumph, but it is consoling to think that even the stupidities of men may be converted into elements of growth and progress. Opposition stimulated Pasteur, and thus, without meaning it, did good service. "Ever," he says, "since the commencement of these researches, I have been exposed to the most obstinate and unjust contradictions; but I have made it a duty to leave no trace of these contests in this book." I have met with only a single allusion to the question of spontaneous generation in M. Pasteur's work. In reference to the advantage of rearing worms in an isolated island like Corsica, he says:—"Rien ne serait plus facile que d'éloigner, pour ainsi dire, d'une manière absolue la maladie des corpuscles. Il est au pouvoir de l'homme de faire disparaître de la surface du globe les maladies parasitaires, si, comme c'est ma conviction, la doctrine des générations spontanées est une chimère." It is much to be desired that some really competent person in England should rescue the public mind from the confusion now prevalent regarding this question.

M. Pasteur has investigated a second disease, called in France *flacherie*, which has co-existed with pébrine, but which is quite distinct from it. Enough, I trust, has been said to send the reader interested in these questions to the original volumes for further information. I report with deep regret the serious illness of M. Pasteur; an illness brought on by the labours of which I have tried to give some account. The letter which accompanied his volumes ends thus:—"Permettez-moi de terminer ces quelques lignes que je dois dicter, vaincu que je suis par la maladie, en vous faisant observer que vous rendiez service aux Colonies de la Grande Bretagne en repandant la connaissance de ce livre, et des principes que j'établis touchant la maladie des vers à soie. Beaucoup de ces colonies pourraient cultiver le mûrier avec succès, et en jetant les yeux sur mon ouvrage vous vous convaincrez aisement qu'il est facile aujourdhui, non seulement d'éloigner la maladie régnante, mais en outre de donner aux récoltes de la soie une prospérité qu'elles n'ont jamais eue."

Royal Institution, 30th June JOHN TYNDALL

WHAT IS ENERGY?

III.

THE CONSERVATION OF ENERGY

IT is well-known that certain organisms of the animal world do not confine themselves to one state of being or to one order of existence, and the most familiar instance of this roving habit of life is the caterpillar, which passes first into the chrysalis state, and after that into the butterfly. This habit is not, however, peculiar to the organic world, for energy delights in similar transmutations, and we have just seen how the eminently silent and invisible electrical current may occasionally be transmuted into the vivid, instantaneous, awe-inspiring flash of lightning. Nor is this element of change confined to our peculiar corner of the universe, but it extends itself to remote starry systems, in some of which there is a total extinction of luminosity for a while, to be succeeded by a most brilliant luminous outburst, presenting all the appearance of a world on fire.

We shall not enter here into great detail regarding the various changes of energy from one form into another; suffice it to say, that amid all these changes of form, and sometimes of quality, the element of *quantity* remains the same. Those of our readers who are mathematicians know what is meant by variable quantities, for instance, in the equation $x+y+z=A$, if x, y, & z are variable and A constant, you may change x into y and into z, and y into x and into z, and in fact perform any changes you choose upon the left hand side of your equation, *provided that* you keep their sum always constant and equal to A. It is precisely thus in the world of energy; and the invariability of the sum of all the energies of the universe forms the doctrine known as the "conservation of energy." This doctrine is nothing else than an intelligent and scientific denial of the chimera of perpetual motion.

Recognising the great importance of work, it was natural enough at an early stage of our knowledge that enthusiasts should endeavour to create energy or the power of doing work, that is to say, endeavour to construct a machine that should go on working for ever without needing to be supplied with fuel in any way, and accordingly inventors became possessed with the idea that some elaborate system of machinery would, no doubt, give us this grand desideratum, and men of science have been continually annoyed with these projects, until in a moment of inspiration they conceived the doctrine of the conservation of energy!

It flows from this doctrine that a machine is merely an instrument which is supplied with energy in one form, and which converts it into another and more convenient form according to the law of the machine.

We shall now proceed to trace the progress of energy through some of its most important transformations. To begin with that one to which we have already alluded, what becomes of the energy of a falling body after it strikes the earth? This question may be varied in a great number of ways. We may ask, for instance, what becomes of the energy of a railway train when it is stopped? what becomes of the energy of a hammer after it has struck the anvil? of a cannon ball after it has struck the target? and so on.

In all these varieties we see that either percussion or friction is at work; thus it is friction that stops a railway-train, and it is percussion that stops the motion of a falling stone or of a falling hammer, so that our question is in reality, what becomes of the energy of visible motion when it has been stopped by percussion or friction?

Rumford and Davy were the pioneers in replying to this important question. Rumford found that in the process of boring cannon the heat generated was sometimes so great as to boil water, and he supposed that work was changed into heat in the process of boring. Davy again melted two pieces of ice by causing them to rub against each other, and he likewise concluded that the work spent on this process had been converted into heat.

We see now why by hammering a coin on an anvil we can heat it very greatly, or why on a dark night the sparks are seen to fly out from the break-wheel which stops the motion of the railway train, or why by rubbing a metal button violently backwards and forwards against a piece of wood we can render it so hot as to scorch our hand, for in all these cases it is the energy of visible motion which is being converted into heat.

But although this was known nearly a century ago, it was reserved for Joule, an English philosopher of the present day, to point out the numerical relation subsisting between that species of energy which we call visible motion and that which we call heat.

The result of his numerous and laborious experiments was, that if a pound of water be dropped from a height of 772 feet under the influence of gravity, and if the velocity which it attains be suddenly stopped and converted into heat, this heat will be sufficient to raise the whole mass 1° Fahr. in temperature.

From this he concluded that when a pound of water is heated 1° Fahr. in temperature, an amount of molecular energy enters into the water which is equivalent to 772 foot-pounds, that is to say, to one pound raised 772 feet high against the influence of gravity, or allowed to fall 772 feet under the same influence.

He found again that if a pound of water were to fall twice this distance, or 1,544 feet under gravity, the velocity if stopped would raise its temperature 2° Fahr., and in fact that the rise of temperature under such circumstances is proportional to the height from which the pound of water is supposed to fall. By this means an exact relation is established between heat and work. Grove was the first to point out the probability of a connection between the various species of molecular energy; and the researches of Joule, Thomson, and others, have established these relations with numerical accuracy. No better example of the correlation of the various kinds of energy can be given than what takes place in a galvanic battery. Let us suppose that zinc is the metal used. Here the source of energy is the burning or chemical combination of the zinc with oxygen, &c., in order to form a salt of zinc. The source of energy is in fact much the same as when coal is burned; it is the energy produced by chemical combination. Now, as we have said, the zinc combines with the oxygen, and sulphate of zinc is produced, but the result of this combination does not at first exhibit itself in the form of heat, but rather in that of an electric current. No doubt a great portion of the energy of this electric current is ultimately spent in heat, but we may, if we choose, spend part in promoting chemical decomposition; for instance, we may decompose water. In this case part of the energy of the battery, derived as has been stated from the burning of the zinc, is spent in heat and part in decomposing the water, and hence we shall have less heat than if there were no water to decompose. But if when we have decomposed the water, we mix together the two gases hydrogen and oxygen which are the results of this decomposition, and explode them, we shall recover the precise deficiency of heat. Without the decomposition, let us say that the burning in the battery of a certain weight of zinc gives us heat equal to 100, but with the decomposition only 80, twenty units of energy have therefore become spent in the decomposition, but if we explode the mixture of gases procured from the decomposition we shall get back heat equal to 20, and thus make the whole result of the burning of the zinc 100 units of energy as before.

In like manner, if our electric battery is made to do work, thus forming a kind of engine, we shall have the heat produced by the current diminished by the exact equivalent of the mechanical effect which we have obtained from this engine.

There is nothing for nothing in the universe of energy.

B. STEWART

ROUMEGUERE ON FUNGI

Cryptogamie Illustrée, ou Histoire des Familles naturelles des Plantes Acotyledonées d'Europe. Famille des Champignons. Par Casimir Roumeguère. (Paris : J. B. Baillière. 1870.) 4to., pp. 164, figures 1700.

THE numerous introductions to the study of fungi, whether as articles of food, objects of physiological and botanical interest, or as the cause or aggravator of disease both in the animal and vegetable world, which are constantly issuing from the press, or whose speedy appearance is announced, are a certain proof of the daily increasing appreciation of the importance of a tribe which has often been considered as the mere offscourings of the earth, and worthy only of the title of "abominations." These publications of course are of very different value, and the glowing terms in which they are announced sometimes lead only to disappointment after an inconvenient outlay. As a striking instance, Valenti-Serini's work on doubtful or poisonous fungi of the neighbourhood of Turin may be mentioned, which was characterised in the "Annals of Natural History" as "this important work," its true characters being admirably exposed by Mr. Worthington Smith in "Seemann's Journal of Botany ;" and unsparing as the remarks are, I consider that they are completely justified. It is simply a disgrace to the Academy under whose auspices it is published.*

This is not, however, the case with the publication whose title is given above ; for though it is far from being free from faults, and the illustrations, though selected with considerable skill, are in some cases so coarse as almost to render them useless ; still there is such a mass of information as may make it acceptable even to those who are well versed in the subject ; and though unfortunately the several matters which come under review are seldom thoroughly worked out, yet they indicate the proper line of research and the best sources of information, in such a manner as to ensure it a hearty welcome. Every possible nook and corner of the mycological library seems to have been thoroughly ransacked, and that without any national prepossession such as occasionally detracts from the credit even of highly approved authorities. Indeed I was not a little surprised to find how diligently English works on the subject had been sifted, and not the less to recognise an allusion even to a sectional address at Norwich, though the remarks of its author have not been quite correctly interpreted.

It is not likely that there should be much novelty in so unpretentious a work, and perhaps it may be as well that no new views should be propounded, founded on imperfect data. It is a great matter to find no glaring errors likely to mislead ; though here and there the drift of what has been written may have been misunderstood.

It is scarcely possible to overrate the importance of the study of fungi in any of the points of view which were enumerated. The Society of Arts and the Horticultural Society of London have very properly called attention to the great importance of fungi as articles of food, by encouraging inquiry or offering rewards for the best collections of esculent, doubtful, and poisonous species. The South Kensington Museum has also done its part. The very faithful set of drawings by Mr. W. G. Smith, exhibited on its walls, and the admirably prepared specimens by Mr. English—which retain their form perfectly, and, to a great extent, their proper colours—must eventually facilitate the due discrimination, which, as in the case of other vegetable esculents, must be matter of experience. It is quite lamentable to reflect what a vast quantity of wholesome food, and food which, from its chemical composition, may profitably replace the consumption of meat in the labourer's family, is utterly neglected, either from ignorance or prejudice.

In the second point of view as regards their physiological and botanical interest, it need only be mentioned with respect to the former, that, with the exception of the true algæ, the phenomena of impregnation cannot be studied more profitably than in those wonderful plants which occur on dead animals or decaying vegetables in water, and which are, undoubtedly, aquatic forms of various moulds, though in some respects they approach the algæ. Then as respects a biological point of view, the question of the origin of atmospheric germs, one of the most difficult of solution which can engage the attention of the microscopist, and which, in my opinion, has never been carried out so as to trace *accurately*, and free from all doubt, the development of the minute bodies which occur in fluids, whether of organic or inorganic composition, into higher forms ; while the botanist will find a variety of form and structure which is scarcely surpassed in the higher branches of the science.

As regards the third point. If we consider fungi as the causes or aggravators of disease, it may be remarked, that, notwithstanding all that has been written on the subject, a great deal still remains to be discovered. The dreadful forms of Erysipelas and Hospital Gangrene, which occur so fatally in London hospitals, are, in all probability, dependent somehow on fungi, though the matter has not, hitherto, been found capable of proof, and whatever may be thought of Dr. Tyndall's views, the medical world cannot be too thankful to him for bringing the matter so prominently before the public.* The same also may be said with respect to Dr. Hallier's speculations, though, as I believe, they have been justly challenged both here and on the Continent.

A great deal is known about the influence of fungi in the production of disease in plants, but much more remains to be discovered. It may, eventually, prove that

* I need only refer to Tab. 30 to justify this remark ; and this instance is not a solitary one.

* That the reproductive bodies of the larger fungi and moulds are widely carried about by the air, will be very evident to any one who has seen the clouds of spores which, in some cases, arise like smoke on the least agitation. Some years ago two flakes of snow were sent to me from Hampstead, prepared as microscopic objects, with the intention of exhibiting the organic matters which they might carry down with them in their course, and both, undoubtedly, contained perfect spores of fungi. Much more then may we expect that organisms which do not exceed a thirtieth or a fiftieth part of their diameter, and which are quite invisible except under very high magnifying powers, should be present everywhere to perform their functions as putrefactive ferments.

some of the more obscure cases of vegetable pathology depend on the minute fermentative bodies which, it should seem, play such an important part in animals. Certain it is that yeast globules and bacteria occur in vegetables where there is, apparently, no immediate communication with the atmosphere, or where, at least, it is as obscure as in some cases which engage the attention of the students of animal biology. Matters which have been long since ascertained to be facts, are still challenged by incompetent and uninstructed observers, and every one who can remove any portion of the prejudices which so materially retard the progress of science, will be doing a good work.

It is to be regretted that the quarto form of the work before us makes it very inconvenient for students, and it is to be hoped that a revised edition in octavo will secure a wider circulation. M. J. BERKELEY

AMERICAN NATURAL HISTORY

The American Naturalist. A popular illustrated Magazine of Natural History. Vols. 1, 2, and 3, from March 1867, to February 1870. (Salem, Mass. Peabody Academy of Sciences. London: Trübner.)

WE have several distinct reasons for bringing this useful periodical before the notice of our readers. In the first place, American Naturalists and writers on science generally complain, and not altogether without reason, that many of the most important works that issue from the Transatlantic press are much less known in this country than their merits deserve. Our personal experience leads us to believe that this complaint is well founded. We lately applied in vain to the Libraries of the Royal Society, of the University of Cambridge, and of a Scottish University, for the *American Naturalist* and for Clark's "Mind in Nature;" and we suspect that very few copies of such books as the following are to be found in English libraries, namely, a complete set of the works of Agassiz since he went to America, Binney's "Terrestrial Mollusks of the United States," Gould and Binney's "Report on the Invertebrata of Massachusetts," Tooney and Holmes's "Fossils of South Carolina," Samuel's "Birds of New England," Dekay's "Fishes and Reptiles of New York," the reports on the Pacific and other Railway Surveys, and the numerous contributions to science published during the last few years, by Marsh, Lea, Leidy, Hall, Wynam Baird, Coues, Packard, Scudder, Le Conte, Stimpson, Verrill, and a host of other writers.

Our second and chief reason for noticing the *American Naturalist*, is on account of its intrinsic value. The three volumes now completed contain a series of important original contributions, by Professors Bailey, Cope, Edwards, Hayden, Henrichs, Orton, and Verrill, Drs. Brewer, Cooper, Coues, Hunt, Joseph Jones, Le Conte, Lincecum, Norton, A. S. Packard, Perkins, Wood, &c. Messrs. Brigham, Dall, Hartt, Hyatt, Lockwood, Morse, Russell, Scudder, and many others. Botany, Geology, Physical Geography, and Zoology, receive their due shares of attention, and in a scientific point of view, the articles occupy an intermediate position between those which we find in "The Annals of Natural History" and Hardwicke's "Science Gossip." Each number contains (1) Original Articles; (2) Reviews of Works on Natural History; (3) Natural History Miscellany, including recent discoveries in Geology, Botany, Zoology, and Microscopy, and terminating with answers to correspondents; (4) Proceedings of Scientific Societies; the whole concluding with a List of Books Received, and a glossary of all the scientific terms occurring in the current number.

If our readers require any further evidence of the value of this periodical, we may add that after one year's independent existence, it has been issued as a publication of the Peabody Academy of Sciences. The trustees "consider it one of the legitimate objects of their trust to assist in the publication of the *Naturalist* by advancing funds sufficient to enable the editors to continue its publication in an improved condition."

Our third and last reason for now urging the claims of this periodical is, that being informed by Messrs. Trubner and Co., to whom we are indebted for the loan of these volumes, that new subscribers can for a time obtain the parts already published at a reduced rate; this seems to be an excellent opportunity for natural history clubs and libraries, both to obtain the back volumes and to order the future numbers. G. E. D.

OUR BOOK SHELF

The Science and Art of Arithmetic, for the Use of Schools. Part I., Integral. By A. Sonnenschein and H. A. Nesbitt, M.A., University College, London. (London: Whittaker and Co.)

FORTY years have elapsed since the appearance of Prof. De Morgan's "Elements of Arithmetic," at a time when perhaps few teachers, as they submitted the rules of the science to their pupils, cared to establish them upon reason and demonstration. The effect of this work was that a rational arithmetic began to be taught generally, and the mere committing of rules to memory took its due subordinate position in the course of instruction. Such a method of treatment will go far to develop and exercise the reasoning powers, and in the case of many pupils, there is hardly any other subject which can so well be made a groundwork for the exercise of the reasoning faculty. The book before us is avowedly drawn up in agreement with the principles of Mr. De Morgan's work, and the aim of the authors is to lead the student "to the discovery of the several rules by some path such as an original discoverer might have travelled." In this first part, which treats of Integral Arithmetic, we consider that they have carried out their principles successfully, and hope they will succeed as well with the remaining two parts, which are to embrace respectively Vulgar Fractions and Approximate Calculations. The rules enunciated are few and tersely given; there is a great store of illustration; elementary difficulties are well stated and honestly grappled with, and cleared up in a way that brings the subject to the level of the capacities of junior students; at the same time advanced as well as young teachers may gather much that is useful from the book. A reader who has carefully gone through the work, can hardly fail to master the early details of the science; if he fail, it will not be the fault of the authors. The subjects treated of are numeration, modes of computation, the so-called first four rules, contracted operations, scales of notation, and properties of numbers. Under this last division we have much valuable matter grouped under the several heads of Divisibility of Numbers, Casting out Nines, Resolution into Prime Factors, Greatest Common Measure, and Least Common Multiple. Throughout and at the end of the work occur numerous examples, very varied, all of which are carefully arranged, and many fully worked out in two or more ways. With this short analysis of the contents, we heartily commend the work to teachers generally, assuming, of course, that they will regulate their use of it in proportion to the requirements of age and ability of their pupils. The work is neatly got up, and we have detected hardly any errata. On page 51, ex. 2, we have "How many petals are there in 376 forget-me-nots?" Here there is an omission and a slight technical error. In botanical language the "forget-me-not" (*Myosotis*) is monopetalous, the number of *lobes* of the corolla being *five*. R. T.

Transactions of the Woolhope Naturalists' Field Club for 1869.

NO slight service has been rendered to the cause of natural science by the numerous naturalists' field clubs scattered here and there through the country, not only in the exploration of the natural products of their respective districts during their summer excursions, but in infusing a love of such pursuits among dwellers in the country. When the transactions of the year are published in so attractive a form as the volume before us, an additional benefit is conferred. The Woolhope Club is one which has been for some years favourably known, chiefly through the labours of one or two genuine naturalists among its members, as having furnished some real contributions to science by its researches among the pleasant woodland county of Herefordshire. The volume consists mainly of lively accounts of the various excursions made by the club during the summer of 1869, with lists of the rarities, zoological and botanical, met with, and reports of the papers read by its members. Among the more important of the latter we may mention Dr. Bull's history of "The Ancient Forest of Deerfold;" and papers on the occurrence and identification of rare birds in Herefordshire and Radnorshire, by Mr. Armitage, Rev. Clement Ley, and Mr. James W. Lloyd, including the peregrine falcon (*Falco peregrinus*), the hobby (*F. subbuteo*), the little merlin (*F. æsalon*), the grasshopper warbler (*Sylvia locustella*), the fire-crested wren (*Regulus ignicapillus*), and the great and little bittern (*Ardea stellaris* and *minuta*). Dr. Bull has given a celebrity to the Woolhope Club for its enthusiasm in favour of edible fungi; there are several papers on the subject, to which is appended Mr. W. G. Smith's *Clavis Agaricinorum*. Several very pretty illustrations ornament the book, among which may be mentioned photographs of some of the remarkable trees of Herefordshire, and a drawing of the famous Deerfold mistletoe-oak.

Contributions to Botany, Iconographic and Descriptive. By John Miers, F.R.S., F.L.S. Vol. II. (Williams and Norgate, 1869.)

THIS volume will be welcomed as an addition to Mr. Miers' contributions to systematic and structural botany, all of which possess the value of the labours of a careful and accurate observer, and one especially conversant practically with South American botany. We find in this volume carefully worked papers on the *Calyceraceæ*, a small order closely allied to *Compositæ*, on the carpological structure of *Bignoniaceæ*, on the history of the maté plant, and the different species of ilex used in the preparation of Paraguay tea, a monograph of the *Tricuspidarieæ*, an essay on the genus *Goupia*, one on the structure of *Heliotropiaceæ*, and a paper on the South American forms of *Ehretiaceæ*. But the most important article is one on the genus *Ephedra*, which Mr. Miers considers has been improperly placed among gymnosperms, maintaining that it has neither naked ovules nor naked seeds, and believing that it is more allied to *Urticaceæ* than to *Cycadaceæ* or *Coniferæ*, presenting a far higher order of structure than these latter orders. The third volume, devoted entirely to *Menispermaceæ*, is promised shortly. A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expr.ssed by his Correspondents. No notice is tak n of anonymous communications.*]

Life in the Deep Sea

THE interest which attaches to every fact which bears upon the phenomena of life at great depths in the ocean, will, I hope, excuse me for especially directing the attention of the readers of NATURE to the "Beiträge zur Plastiden Theorie" (published in the fifth volume of the *Jenaische Zeitschrift*), with a separate copy of which my friend Prof. Haeckel has just favoured me.

The longest of the papers which constitute the "Beiträge," is devoted to a careful study of *Bathybius*, and the associated Coccoliths and Coccospheres; and it is a matter of great satisfaction to me that Prof. Haeckel has arrived at conclusions which, in all the main points, agree with my own respecting these remarkable organisms.

In a second paper Prof. Haeckel describes a wonderful Radiolarian, *Myxobrachia*, observed during his stay at the Canary Islands, the further study of which promises to throw a new light upon the nature of the Coccoliths and Coccospheres; inasmuch as bodies of the same character were found accumulated, and apparently developed, in masses at the extremities of certain prolongations of the protoplasm of *Myxobrachia*. As *Myxobrachia* attains a length of half an inch, and seems to be abundant in the harbour of Lanzerote, it is to be hoped that Prof. Haeckel, and other naturalists, will not long remain deprived of the opportunity of submitting it to re-examination.

Another important discovery made public in the "Beiträge," is the existence of starch in the well-known "yellow cells" of the *Radiolaria*. In connection with this fact, it is interesting to remark that all the *Radiolaria* are floating organisms, and, consequently, that they are fully exposed to the light of the sun.

T. H. HUXLEY

Jermyn Street, June 23

The "English Cyclopædia"

YOUR issue of June 2 contains a long letter from "Nemo," to which a short reply seems desirable. Most of his statements are incorrect, and, as an illustration of the trustworthiness of his facts, or supposed facts, allusion may be made to his remark that all he can find in the Cyclopædia about *Arvicolæ*, *Crocidurœ*, *Crossopi*, *Hypudæi*, and *Sorices* is that *Hypudæus* is sometimes spelt *Hipudæus*; whereas all the species mentioned in the Close Time Report to which he refers are described or noticed in the Cyclopædia. The species of the sub-genera *Crocidura* and *Crossopus* are referred to under their generic heading *Sorex* in the article *Sorecidæ*, E. C. Some of the terms which he says are omitted properly belong to another division of the Cyclopædia. Thus *Acclimatisation* is noticed in the Arts and Sciences division, and something additional will probably be given in the supplement to that division. Again, *Deep Sea Dredging* had scarcely become a subject of general interest when the Natural History Supplement was being written, while the character of the principal results, and the probability of great additions to the subject, rendered it advisable, as was thought, to postpone its consideration until the Arts and Sciences division was supplemented. Some of the results are, however, given under *Alcyonaria* and elsewhere in the Natural History Supplement. As regards the other subjects said to be omitted, most of them do occur. *Darwinism* is noticed under *Species*, E. C. S., and also under *Palæontology*, *Crustacea*, &c. *Dimorphism in Animals* will be found under *Annelida*, *Hydrozoa*, *Generations* (*Alternation of*), *Crustacea*, &c., in E. C. S. *Eophyton* is noticed; and *Eozoon* is repeatedly mentioned, while its systematic position is described under *Foraminifera*. The article *Entophyta* in E. C. is devoted to the fungi connected with skin diseases, while those which are associated with ague and other diseases would be most appropriately noticed in connection with those diseases, which do not belong to the Natural History division. A whole column is given to *Hyalonema* under *Alcyonaria*, E. C. S., in which the contradictory views of Drs. Bowerbank, Gray, Wright, and others, are distinctly referred to. Something is said about *Hybridity* under *Primula*. *Ornithoscelida* is not in; the term was first proposed in a paper read Nov. 24, 1869, which paper was not published in the printed form until after the Supplement had been finished. The general views on *Protoplasm* are given under *Cells*, *Amœba*, and so far as it is identical with Sarcode under *Actinophrys*, *Sarcode*, and other headings in E.C.S. *Rhizocrinus* is referred to under *London Clay*, and its occurrence in the living state mentioned. *Aerolites:* The latest reference is said to be 1861, implying, as it seems, that none of the information is of later date. Falls subsequent to 1861 are mentioned, and many of the facts are of later date; as, for example, those relating to the Alais and Orgeuil aerolites; Sorby's conclusions published in 1865; and Daubrée's experiments, of which accounts were given in 1866 and 1868. The article itself appeared early in 1869. As to the bibliography, the principal authors are mentioned, and a list of the works consulted was written, but was inadvertently omitted. It is also said that the latest reference under *Alca* is 1861, but this again is not correct. The writer of the article *Annelida* was not aware of Claparède's strictures at the time he wrote it; but, after all, they do not seem to affect materially the general statements given in the supplementary volume. Prof. Huxley's views respecting the systematic position of *Archæopteryx* are given under *Birds*, E.C.S. No reference is made to *Protagon* under *Blood*, E.C.S., nor is mention made of Day's colour tests, nor Dr. Richardson's renunciation. Of the last, all that was found in the Reports of the British Association is the title of his paper, which runs thus, "On Coagulation of the Blood; a correction of the Ammonia theory," and of which nothing more is said. Hence it was thought best to say nothing about the matter. Of the long string of terms which "Nemo" has culled from Prof. Huxley's last address to the Geological Society, and which are said to be omitted, the majority are given in the Supplement. For instance, to cite one or two cases: *Anthracosaurus* occurs under *Carboniferous* system, E.C.S.; *Evolution* under *Palæontology*; *Microlestes* under *Rhætic Beds*; and so on. As to the other remarks which have not been specially alluded to, it may be admitted that some of the articles might have been improved. *Foraminifera* would have been all the better if Haeckel's volume had been consulted, only Haeckel's work could not be got. It would have been very desirable if subjects which have been omitted had been inserted, and if cross references had been more numerous; but there were restrictions as to space which rendered it necessary to make a selection. Thus, *Meloe* was inserted and *Sphegidæ* rejected, because there was no room for both. What a Cyclopædia ought or ought not to contain is an open question. It cannot give information upon everything; and probably very few persons not specially interested in the subject want to know about *Hyænictis* or *Ictitherium*. If regard was had to the theoretical view of the matter, and not to the cost and other practical drawbacks, a full account of all that has been done in the last sixteen years would fill several volumes as large as the Supplement to the "Natural History Division of the English Cyclopædia." I beg to sign myself

THE EDITOR

Cuckows' Eggs

WHAT is the drift of this discussion on the eggs of the cuckow? Is it "natural selection," "mimetic analogy," or what? Are we to understand that by some process of "natural selection" the European cuckow can change the appearance of her egg to that of the selected foster parent? or that one set of cuckows lays eggs like titlarks, let us say, another like hedge-sparrows, and so on; and always select each its particular nest in which to deposit its particular coloured egg?

If this is it, *cui bono?* Of course to deceive the foster parent. Is this needed? I doubt it. I do not think the foster parent cares what coloured eggs she sits on, so long as they are about the size of her own, so as not to inconvenience her.

Let us see what cuckows do in other countries, and let me select Africa as my field. If deception is necessary in one country, why not in another? Le Vaillant is so inaccurate that one must take all his statements *cum grano*, but he is right in some things, where, I suppose, he had no temptation to go wrong. He says of *Cuculus gularis* "that its egg is olive grey, dotted with red" (gris olivacé, piqueté de rouge), and that it is laid in the nests of—1, the Jean Frédérick (*Bessonornis phœnicurus*); 2, Coryphée (*Bradypterus coriphœus*); 3, Traquet-patre (*Pratincola pastor*); 4, Pie-grièche fiscal (*Lanias collaris*); and 5, Bacbakiri (*Telophonus bacbakiri*). Now the eggs of No. 1 are of a dirty white or buff ground, more or less spotted with pale rufous; 2, a lovely verditer, irregularly blotched with brown; 3, also verditer, indistinctly clouded with brown; 4, pale grey, blotched at the obtuse end with greenish and reddish spots; 5, light blue, profusely spotted with brown.

Cuculus solitarius, he says, lays its pink egg, dotted with clear brown spots, in the nests of—1, *Bessonornis phœnicurus;* 2, *Bradypterus coriphœus;* 3, Le Capocier (*Drymoica capensis*); 4, Le Réclameur (*Bessonornis vociferans*); and 5, Le fauvette à tête rousse. The eggs of the last two I do not know; those of the first two are described above; those of No. 3 are blue, with brown blotches.

Oxylophus edolius and *O. melanoleucus* he confounds together, but it matters little, as the eggs are alike—pure white—and deposited in nests of—1, Bergeronette brun (*Motacilla capensis*); 2, *B. coryphœus;* 3, Gobemouche mantelé (*Tchitrea cyanomelas*); and others, whose eggs I do not know. Of 1, the eggs are greyish white, or rather nankin, minutely freckled with brown; of 3, they are cream-coloured, profusely spotted with red, brown, and purple spots, in a band at the obtuse end. One of my correspondents finds eggs of *O. edolius* in the nests of *Pycnonotus capensis*, whose eggs are rather deep lake, profusely spotted with dark markings! They also, I know, lay in the nests of *Pycnonotus nigricans*—eggs as of the last. I found Mud-birds (*Malacircus bengalensis*) in Ceylon, feeding a young *O. melanoleucos*, and their eggs are of a uniform deep verditer.

Chalcites auratus lays white eggs also, and some of my correspondents have sent what I *believe* to be their eggs taken from the nests of *Hyphantornis capitalis*, whose eggs are green, profusely speckled with brown, and dark salmon-colour profusely speckled and spotted with dark brown and black.

Now, will any one say, after comparing these different cuckows' eggs with those of the nests in which they have been found, that there is any attempt at imitation, and if not in so many cases, why in that of *C. canorus?*

I used to think and so I wrote ("Birds of South Africa," p. 252) that the eggs of parasitic birds "usually resembled those of the foster parent." This was my idea founded on statements concerning the European cuckow taken from books; but a valued correspondent, taking exception to my position, set me to investigate the subject for myself, and to collect together and analyse my own observations and those of my collectors in this country. She writes as follows:—"The eggs of all the cuckows that I have met with in this country (South Africa) are white, and moreover they are nearly always larger than the eggs of the bird in whose nest they are deposited. With regard to distinguishing eggs, birds of all kinds are exceedingly short-sighted. We used to amuse ourselves by changing the eggs in all the birds' nests we knew of. The owners seldom left them, but took to the strange eggs; and unless their habits were remarkably different, they would blindly rear each other's young, just as they do the young cuckows. It is not necessary, therefore, for nature to make this provision. My second son once filled a Cape canary's nest with so many eggs that when the young were hatched they were more than the poor birds could manage to provide for, and having repented of his mischief, he was obliged to help them bring up their young." (Cf. *Ibis*, 1868, p. 247.)

Since this was written, I have had the advantage of visiting my correspondent, who is well known throughout this colony for her talents, love of natural history, and powers of observation. We often discussed this subject. She and her sons assured me they never cared to select eggs like those of the foster-parent, but simply eggs of those whose food they knew to be similar. They said the confusion they caused was most amusing, but only after the young were hatched. The eggs were incubated without any demur on the part of the foster-mother. After this, surely I may ask *cui bono* the *C. canorus* imitation?

E. L. LAYARD

Cape Town, Cape of Good Hope, May 3

The Chromatic Octave

I HAVE to thank "M. A." for his letter in NATURE of June 9th, suggesting that the wave-frequency to which the complementary of any tint is due, may be, not the geometric mean between that tint and its octave, as I suggested in a letter in NATURE of 28th April, but the harmonic mean. I can scarcely doubt that there must be some simple arithmetical relation between the wave frequencies of any tint and its complementary, but I see no *à priori* reason for expecting to find one such law rather than another; we must try which assumed law will most nearly coincide with fact, and the hypothesis of a harmonic mean does so coincide pretty nearly. The following table (see my previous letter) gives the ratios of the wave-frequencies of red, orange, and yellow as observed, of their complementaries as observed, and of the same as calculated on the hypothesis of the harmonic mean:—

		Observed.	*Calculated.*
Red . . .	36·40	Bluish Green 48·30	48·53
Orange .	39·80	Blue . . . . . 51·80	53·07
Yellow .	41·40	Indigo . . . . 54·70	55·20

The discrepancies between the observed and the calculated outlines are much less on this hypothesis than in that of the geometric mean; but they are on the same side, and, as I explained in my former letter, I think it likely they may be due to the solar spectrum not being of a truly white colour, owing to the absorption lines toward the violet end. They are on the side which this way of accounting for the fact requires. It would be desirable to make a set of comparative experiments with solar light and the electric light, as I suggested before, in order to clear up this question.

Old Forge, Dunmurry, Co. Antrim, June 13 JOSEPH JOHN MURPHY

On the reported Current in the Suez Canal

IT is stated on excellent authority that a constant current runs through the central portion of the Suez Canal, from the side of the Mediterranean to that of the Red Sea, and a good deal of surprise has been excited by this apparently anomalous phenomenon. A little consideration will, however, suffice to establish a theory, that constant currents are almost necessary conditions of inter-oceanic canals, and that their absence, not their presence, would be contrary to just expectation. My reason is based on the improbability that a long canal, A B, could be constructed across strata that are almost necessarily inclined in one direction more than another, which should not resist the flow of tidal water from, say, A towards B, more than from B towards A. Wherever this differential aspect is established, a quasi-valvular action is called into existence, and a current along the middle of the canal, in a constant direction, is the necessary consequence.

Let A B be the canal, and *a b* the extreme limits of tidal influence. After each successive rise and fall of the tide on either side, more water will have passed from A towards *a*, than

A *a* *b* B

will have returned from the side of *a* to A, and more water will be able to travel from the side of *b* to B, than can get up the canal from B towards *b*. Consequently there will be a constant current in the ultra-tidal portion, *a b*, of the canal, from the side of A to that of B.

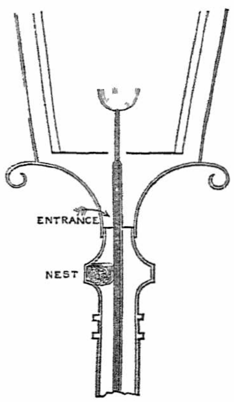
I have made some inquiries, but am unable to learn what notchings, indentations, or sweeps of the sides of a canal, would exercise the greatest differential effect, at low velocities, of the kind of which I am speaking. However, I hear it is a fact well known to sailors, that a spar cannot be towed behind a boat, unless with the greatest difficulty, if its small end be foremost, whereas, it is moved easily enough if its thick end be in front. I argue from this that if a number of spars were moored against the sides of the canal, with their large ends towards A, much less strain would be exerted on the ropes by which they were secured when the current ran from A to B, than when it ran from B to A, and consequently that the current itself would be much less resisted in the former than in the latter case. A succession of very long notches in the sides of the canal would produce identically the same effect, and might call into existence a considerable aggregate of differential resistances. I constructed a model for the purpose of experiment, but found it much too small to give satisfactory results; nevertheless, I will describe it, in hopes it may save trouble to others in designing a suitable arrangement, for the same purpose, on a larger scale. A notched trough was cut, running up and down in long zigzags, and its two ends were brought together into the same reservoir. By alternately allowing water to run into the reservoir, and then drawing it off, the effect of the rise and fall of the tide was simulated. I scattered lycopodium on the water, in the middle part of the channel, to show the direction of the current.

I venture to suggest to those engineers who are connected with inter-oceanic canals, the importance of making experiments on this problem, because it may prove to be quite within their means to produce and to regulate a current within such canals, in the direction and of a velocity most convenient to keep its bed clean and serviceable. FRANCIS GALTON

Birds' Nests

BIRDS, though almost always adapting themselves to circumstances in the use of materials, are frequently, even in the country, very eccentric in their choice of a place for their nests. I have seen a blackcap's nest built of the ordinary materials, in an open flower-pot standing on the top of a garden wall. Apparently there was no possible reason for this, there being plenty of hedges and banks hard by. But in the neighbourhood of London birds may be allowed an excuse for their eccentricities.

In a quiet street in one of the southern suburbs there is now a pair of tom-tits who have taken possession of a cast-iron lamp pillar, wherein they have built their nest and reared their young for two or three years past. It is curious to think what business they could have had there, to have found out that it was a suitable residence. The nest is placed in the bulb or swelling out of the column, just below the lamp, and the birds creep through



the space between the gas-pipe and the iron rim at the top of the column. This space is not three-quarters of an inch in width. The nest is on one side of the pipe, and cannot be more than two inches across. The lamp is lighted every evening, and on one occasion the pillar was actually taken down for some repairs with the nest inside, containing seven or eight eggs, which were, I believe, destroyed; but the birds, concluding I suppose that this was not done with *malice prepense*, but that it was only a necessary domestic difficulty, wisely returned to their home, and continued to occupy the lamp pillar for the remainder of the season, rearing another brood that same year. The accompanying sketch shows the position of the nest. Under the eaves of the adjacent house, two pairs of house martins have built this year. They came flitting about on the 1st June, and the weather being very dry, and no mud to be got at, the "gudeman" of the house kept a little spot in the road well watered, from whence the birds obtained all their necessary mud. The sparrows would plump down after the martins, thinking there was food there, and stand watching the martins at this little wet spot, and wondering apparently why they kept on flying down here, where there was no grub to be got. They tried hard to obtain possession of the martin's nests when half built, but were constantly driven away by the gentleman of the house, and now the nests are finished, and the entrance too small for the sparrows to get in; so that they dwell in comparative security. Two of these martins seem to be the sole occupants of their nest, but the other nest appears to be visited, at least, if not owned by, more than one pair of birds, three or four birds being often seen there at one time. I have often noticed this in the country, but never saw any remarks about it recorded by any one.

C. W. W.

"Other Worlds than Ours"

MR. PRITCHARD, in reviewing my book on the plurality of worlds, says that I represent Mr. Lockyer as impeding the progress of science; on the contrary, I regard Mr. Lockyer as one who has, in a most marked and important manner, advanced the cause of science, and I confidently anticipate admirable work from him in the future. It is surely not wrong in me to express openly my opinion that Mr. Lockyer's theory of the corona* is erroneous, or that (precisely *because* an expedition will set forth next December to observe the corona) the arguments against it cannot advantageously be neglected. But to assert, in the face of the fact that I give *reasons* for objecting to it, that I object "simply because" Mr. Lockyer's "opinions do not square with mine," is to make a misstatement which one can scarcely imagine to result from mere negligence.

Mr. Pritchard quotes my words, "I have very little doubt that Uranus has at least eight satellites," and asks how I venture to set my *opinion* in antagonism with Mr. Lassell's *observations*. How strange he should not have quoted the next sentence also, which would have shown that, as a matter of fact, I set the *observations* of Sir W. Herschel against the *opinion* of the esteemed and eminent astronomer who is President of the Astronomical Society. One can scarcely imagine this omission to result from mere negligence.

Mr. Pritchard makes me say, in the face of Sir William Thomson's abandonment of the theory, that the sun's heat is derived from a battery of meteors, "*I am quite certain* . . . that at least an important proportion of the sun's heat" is so supplied. And he adds, "We may fairly ask whence has Mr. Proctor this *certain* knowledge?" How strange that he should have omitted the remainder of the sentence! What I actually wrote was, "I am quite certain there is no flaw in the evidence I have adduced from the laws of probability, and that we are bound to accept as a legitimate conclusion from that evidence the theory that at least an important proportion," &c. This reference to the evidence, and to the laws of probability, would have spoiled Professor Pritchard's reasoning. Here, again, we can scarcely imagine that the omission results from mere negligence.

There are other points of the same kind in Mr. Pritchard's review, which space prevents my dwelling on. Suffice it to say, that *every criticism it contains* is vitiated by misstatements or omissions, which one can scarcely imagine to result from mere negligence.

RICHARD A. PROCTOR

Pinkish Colour of the Sun

IN reference to the "pinkish colour of the sun," noticed by several of your correspondents, it may interest them to learn that in one of the last numbers of *Cosmos* an account is given of this very same appearance, observed on the 23rd of May, at Rohrbach, on the Moselle, by a M. Hamant. He states, "that up to about two o'clock the day had been very warm, without a breath of wind. At twenty minutes past two the horizon became charged with mist, and a storm seemed imminent. About three the sun lost its brilliancy, assumed a pale yellow hue, and might have been taken for the moon had it not been for its diameter. The mist now began to rise, a north-west wind began to blow very hard; at half-past four the sun became rose-coloured, and at a quarter-past five it turned scarlet."

The exact coincidence to be observed between this account and that given by Mr. A. S. Herschel (NATURE, June 16), is worthy of notice.

Mr. Herschel similarly observed this "very unusual pinkish colour," between five o'clock on the 23rd of May, at Hawkhurst in Kent. He notices the "thick haze of apparently low cirrostratus or, perhaps, rain cloud." This phenomenon is so rare that it is mentioned in old chronicles as a sign of Divine wrath. Of late years the most remarkable case was that observed in South America by M. Emdiais, alluded to in *Cosmos*.

It is, however, especially to be noticed that whereas the two accounts referred to above state distinctly that the phenomenon occurred on "the 23rd of May," your other corespondents state that it also occurred "on Sunday, the 22nd," at about the same time, five o'clock. (See NATURE, May 26, June 2).

It is most remarkable that such a rare phenomenon should have occurred on two consecutive days; visible on the first day at Dunmurry and Dublin and Tynemouth, and on the second in Kent and Gloucestershire and on the Moselle. The hazy nature of the atmosphere on both days seems to have been permanent, and is, without doubt, the cause of the phenomenon.

JOHN P. EARWAKER

Merton College, Oxford, June 28

Monographs of M. Michel Chasles

A FEW years ago I read ten or a dozen papers of a masterly history of geometry by M. Chasles. It was in French, in some quarto transactions of a learned society.

I am desirous of recovering the title and reference, and ask for assistance in the columns of NATURE.

No such a paper as that I refer to is in the Royal Society's admirable catalogue.

Was Chasles' *Aperçu historique* contributed to a learned society? It was published at Brussels, in 1837, but it is scarce, and I have not seen a copy.

C. M. INGLEBY

Ilford, E., June 11

Geographical Prizes

IN reading the report in a recent number on the Prize Medals of the Royal Geographical Society, doubtless many of your readers will have thought very reasonable the wish of Sir Roderick Murchison, "that Eton, Harrow, and Rugby, and other great schools might in future years send candidates for these medals."

It may be well therefore if I explain very briefly the grounds on which the masters of Rugby were almost unanimous in wishing to decline the invitation of the Royal Geographical Society.

The examination is in fact a competition between schools in a subordinate branch of education. Hence the advantage lies not with the best school, but with the one which allows the greatest liberty of choice of special studies. A school like Rugby, whose curriculum, though not narrow, is strongly defined, is at a positive disadvantage in such a competition with a school whose general curriculum is narrower, but its organisation looser; one which allows free specialisation, and prepares for particular examinations. We cannot feel that the school that wins is likely to be the best school.

Further, we agreed that the proposal would not really encourage the study of geography in the school, but would attract only a few individuals. There are in every school certain accumulative prize-acquisitive boys who would learn Chinese or Crystallography or Indian Finance if a prize were offered for such subjects; and it would be these boys who would compete for the geographical medals—such boys would gain little by learning, and the school would gain nothing.

On these and some other grounds the proposal was declined by the Rugby masters; and when it is recollected that it was when Dr. Temple was head-master, most of your readers will be sure that it was not from indifference to real progress, nor from stupidity, nor from fear of novelty, nor to avoid honourable competition with other schools, that we did so decline it.

Rugby, June 13 J. M. WILSON

* For an accurate though incomplete statement of Dr. Frankland's and Mr. Lockyer's theory of the Corona, we refer our readers to the first number of NATURE. Many of them will not be surprised to find that it is *not* what Mr. Proctor states it to be. Dr. Frankland and Mr. Lockyer, from their laboratory experiments, have shown that the pressure at the base of the chromosphere is small, and they have therefore stated that it is scarcely possible that a very extensive atmosphere lies outside the chromosphere. Mr. Lockyer has shown, moreover, that the height of the chromosphere as seen by the new method probably falls far short of its real height as seen during an eclipse as it was seen by Dr. Gould. A reference to the same number of this journal will also show that Mr. Proctor has misrepresented Dr. Gould's statements, which endorse the idea put forward by Dr. Frankland and Mr. Lockyer. Dr. Gould has expressly stated "that there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon." This is an opinion held by Faye and other distinguished astronomers, and Mr. Lockyer has simply shown that should this turn out to be the case, the continuous spectrum observed may be explained. Astronomers did not require Mr. Proctor to tell them what he has recently been enforcing; but, more modest than he, they have been waiting for facts, and Mr. Proctor surely is old enough to see that by attempting to evolve the secrets o the universe, about which the workers speak doubtfully, out of the depths of his moral consciousness, he simply makes himself ridiculous, and spoil much of the good work he is doing in popularising the science.—ED.

NOTES

It is with very great pleasure we state that an English friend has just received a letter from Baron Liebig, in his own handwriting, dated Munich, the 1st inst. Although still very weak, he is now able to get into his garden for some little time daily. If he continues to progress as he is now doing he proposes going to Switzerland, to the Engadine, in three weeks, where it is hoped he may soon become quite strong.

Death has been busy lately among the more eminent members of the medical profession. We have this week to record the decease of Sir James Clark, Bart., M.D., F.R.S., chief physician to Her Majesty, which took place on the 29th ult., in the 82nd year of his age. Sir James had held the appointment of physician to several members of the Royal Family, was the author of several works on climate and on consumption, and was a member of the Senate of the University of London. He attended the poet, John Keats, during his last illness at Rome in 1821.

The Radcliffe Observer at Oxford, the Rev. R. Main, has just issued a second Radcliffe catalogue, containing 2,386 stars deduced from observations extending from 1854 to 1861. The well-known care taken by the observer in the reduction of observations, and the admirable instruments in the observatory under his charge, render this volume a very valuable addition to our astronomical libraries.

We have received from Mr. Van Voorst a most beautiful book, which every scientific man in this and other lands will be glad to possess. Its contents are sixteen portraits of eminent scientific men, photographed by Dr. Wallich, the well-known naturalist, and with a skill quite unsurpassed, so far as we know, in any previous attempts. Dr. Wallich states that his aim has been to supply likenesses that shall be both characteristic, free from some errors in taste but too frequently manifest in photographic portraiture, and trustworthy as records of individual feature and expression; and in all these points the likenesses are admirable. We could hope that the success of this volume would be such as to induce Dr. Wallich to enlarge his scientific series. We may add that the present portraits are those of General Sir E. Sabine, Sir R. I. Murchison, Profs. Owen, Bentham, Huxley, Tyndall, Stokes, Ramsay, and Williamson, Dr. Hooker, Sir C. Lyell, Sir W. Logan, Viscount Walden, the Rev. G. B. Reade, and Messrs. Lassell and Prestwich.

At the meeting of the French Academy of Sciences on the 27th ult. the section of Anatomy and Zoology presented the following list of candidates for the place of correspondent vacant by the death of Prof. Carus. In the first rank, Prof. Brandt of St. Petersburg; in the second rank and in alphabetical order MM. Bischoff (Munich), Darwin, Huxley, Hyrtl (Vienna), Leuckart (Leipzig), Lovén (Stockholm), Steenstrup (Copenhagen), and Vogt (Geneva). The election was to take place on Monday last.

A Committee of the House of Commons having reported in favour of the establishment of the national natural history museums on the Thames Embankment, Mr. Beresford Hope inquired, on Monday evening last, whether it was the intention of the Government to carry out this recommendation. The Chancellor of the Exchequer replied that such an appropriation would deprive the ratepayers of a large portion of the land which was to be laid out as public gardens, and therefore Her Majesty's Government were not prepared to ask Parliament to carry out the recommendations of the committee. We are very glad to hear that the Government does intend to insist on so large a portion of the Embankment being rescued from the builders. But have the interests of the ratepayers been so jealously guarded when other claims than those of science were placed in competition with them?

At the D.Sc. examination of the University of London, just held, candidates passed in the following branches:—Branch I.—Mathematics, John Hopkinson, Trinity College, Cambridge, and Owens. Branch IV.—Inorganic and Organic Chemistry or Mineralogy: James Bottomley, B.A., Owens College; David Watson, Royal School of Mines; John Watts, private study. Branch V.—Organic and Inorganic Chemistry: James Campbell Brown, Royal College of Chemistry and private study; Charles Romley Alder Wright, Owens College. Branch VIII.—Physical Optics; Heat; Acoustics: John Hopkinson, Trinity College, Cambridge, and Owens. The D.Sc. examination is intended by the University to be the highest possible test of proficiency in some one particular branch of science, and can be taken in any one of sixteen different branches. It is encouraging to find that not only have a larger number passed the examination than in any previous year, but that it has been taken in branches in which there has hitherto been no candidate; and moreover, that one gentleman has passed in two distinct branches. Owens College may well be proud of the number of successful candidates it has sent up.

At a meeting of the Council of the Royal School of Mines, held on Saturday, July 2nd, the following awards were made:—Two Royal Scholarships of 15*l.* each for first year's students, to W. H. Greenwood and F. C. Milford; H.R.H. the Duke of Cornwall's Scholarship to P. C. Gillchrist; the Royal Scholarship of 25*l.* to R. R. Atkinson; the De la Beche medal and prize of books to W. Gowland; and the Director's medal and prize of books to P. C. Gillchrist. The Edward Forbes medal and prize of books were not competed for this year. The title of Associate of the Royal School of Mines was conferred upon the undermentioned gentlemen:—In the Division of Mining—Messrs. William Gowland, Archibald Liversidge, and H. J. Renwicke. In the Division of Metallurgy—William Gowland, Dillon, Liversidge, A. W. Bickerton, F. W. Bayley, and T. Jones. In the Division of Geology—W. Johnson Sollas.

The examination for women above eighteen years of age, conducted by the University of Cambridge, has been proceeding during the present week, at three centres, London, Manchester, and Rugby. The number of entries last year was thirty-six, this year they have increased to eighty-four, and we are glad to observe that there are candidates in several branches of natural science which were not touched last year, among them Zoology, Botany, and some of the higher branches of Mathematics.

The *Athenæum* states that the University of Vienna has decided to admit women to all the advantages of its medical school, and that two French students have already availed themselves of the privilege.

A telegram from Athens speaks of an earthquake which has taken place in the island of Santorin. The town is a mass of ruins, and several small islets have been submerged.

Dr. Petermann announces in the *Cologne Gazette* that the *Warag*, one of the best steamers of the Russian fleet, left Helsingfors on the 7th of June, under the command of Captain Krämer, on a scientific expedition to Nova Zembla and Spitzbergen. The *Warag* is to proceed *viâ* Bergen, Hammerfest, and Vardö. It will be accompanied by two or three steamers from Archangel, with the Grand-duke Alexis and several eminent scientific men on board. This expedition is to be the precursor to a more important one which is being prepared by the Russian Geographical Society, and will proceed next year to the North Pole.

THE *Moniteur Scientifique* for June 15th, edited by Dr. Quesneville, contains three articles taken from our columns, the source of two only of which are acknowledged—"The Science of Explosives applied to the Art of War," and "Scientific Experimental Research," by Mr. George Gore (or, as the French rendering has it, Mr. George Core). The third, Dr. Lankester's article on "The Extract of Meat," appears as an original paper.

THE annual meeting of the Royal Archæological Institute of Great Britain and Ireland will be held at Leicester at the end of the present month. The meeting is under the patronage of Her Majesty and the Prince of Wales, and Lord Talbot de Malahide, F.S.A., is the president of the year. Leicester itself possesses many features of archæological interest. These include its mediæval churches, the Norman hall of its ancient castle, the "newarke" of the castle, numerous Roman pavements, one being *in situ*, and the noted mass of Roman masonry called the "Jewry Wall," the ancient hall of Corpus Christi Guild, the Chapel of Trinity Hospital, and the now forsaken hospital of William de Wyggelston. With these may be mentioned the site of Leicester Abbey, surrounded by its original walls, where Cardinal Wolsey breathed his last. Among the features of interest which will be included in the proceedings of the week will be the inspection of Kirby, Muxloe Castle, Ashby-de-la-Zouch Castle, the ruins in Bradgate Park, so intimately connected with Lady Jane Grey, the remains of Ulverscroft Priory, the curious stained glass of Woodhouse Chapel, the house known as Latimer's at Thurcaston, the noble church at Melton Mowbray, the fine Norman hall at Oakham, the interesting hall and church at Exton, and the remarkable encampment at Barrow-on-the-Hill. A temporary museum will be established as usual during the week, and will include a collection of portraits of worthies connected with the counties of Leicester and Rutland.

THE general monthly meeting of the Royal Institution of Great Britain was held on Monday, July 4, Sir R. I. Murchison, K.C.B., F.R.S., in the chair. The secretary announced the receipt of 2,000*l.*, a legacy from the late Mr. Alfred Davis, M.R.I., for the promotion of experimental researches.

BABOO RADANAUTH SICKDAR, for many years chief computer to the Trigonometrical Survey of India, at one time in charge of the Calcutta Observatory, and a mathematician of some attainments, died in May last at Calcutta.

THE Governor of Madras has presented to the Horticultural Gardens there four young date palms, which he has procured from Egypt.

THERE has been for some time in India a discussion on the subject of pearl oyster banks on the Tinnevelly coast. Those who have maintained the existence of the oysters are now fully confirmed by the discovery of young oysters in abundance. One bank is four miles in length and about two in width, and in another two years will yield a good harvest of pearls.

A MINERAL spring has been discovered in the Gheiveh hills, near Broussa, in Asia Minor.

THE foundation-stone of a new Observatory was laid at Port Louis, Mauritius, on the 30th of May, by H.R.H. the Duke of Edinburgh, to be called the Royal Alfred Observatory. In addition to astronomical observations, it is intended to make the Observatory a centre for researches for the advancement of meteorology and terrestrial magnetism. Full particulars of the ceremony will be found in another column. In Sir Henry Barkly, who is about to leave Mauritius for the Cape, the colony will lose a Governor who has always had at heart the promotion of everything connected with physical and natural science.

THE temperature of the Cranial Cavity has lately been investigated by Mendel, of Pankow near Berlin. He states that Fick had already found the normal temperature of the cranial cavity to be lower than that of the body generally. Jacobson and Bernhardt had similarly noticed the inferior temperature of the blood arriving at the heart by the superior vena cava, and the depression produced by it in the right cavities. M. Mendel corroborates these results, and finds constantly that in health there is a difference of from seven-tenths to one degree centigrade between the temperature of the cranial cavity and the rectum in the rabbit, and that in the dog the difference is almost as well marked. Duméril and Demarquay have shown that the temperature of the body is lowered by the action of chloroform. Bouisson arrived at the same results, as have also Sulzynski and Scheinnesson; the latter experimenting upon man. The difference observed by Mendel between the cranial and rectal temperature is much more pronounced when the animal is under the influence of chloroform than when in health. Chloroform lowers the temperature generally, but especially that of the cranial cavity. The effects produced by chloral on the general temperature have been already studied by Demarquay. This author has found that the temperature of the body falls several tenths of a degree. Mendel arrives at the same results in regard to the temperature of the cranial cavity, except that it falls to a still greater degree than the general temperature. Dequix, Dupuy, Leuret, and Gscheidlen have found that after a medicinal dose of morphia the temperature of the body rises, though when given in a poisonous dose it falls. Mendel again arrives at a similar conclusion, viz., that the depression of temperature is more rapid and more marked in the cranial cavity than in the rest of the body. In poisoning by alcohol the temperature of the cranial cavity rises to such a point that it surpasses the temperature of the rectum.

THE hygrometrical balance compensating for the severe drought under which Western Europe has been suffering for the past three months, appears to have been maintained by an unusually heavy rainfall, reported at the Paris Observatory as having been experienced in Egypt, Lybia, Asia Minor, and Russia. The Paris correspondent of the *Daily News* states that two theories to account for the drought have been broached among French *savants*. One of these attributes to the cutting of the Suez Canal a displacement of the atmospheric currents which regulate the weather of Europe and North Africa! The other theory is grounded on the statement that the spring rains in France are caused by atmospheric commotions resulting from the breaking up of the ice fields to the east of Newfoundland, and that for this reason a long and severe winter in the Arctic regions is followed by a dry spring and severe and destructive summer storms in Central and Western Europe. The author of this theory suggests that science should be employed in breaking up the ice-fields at the end of a severe winter by nitro-glycerine and picrate of potassium. This scheme is advocated in the pages of *Cosmos*, by a civil engineer of the name of V. Prou, who proposes forming an "Insurance Company against Drought," for the purpose of assisting nature in the disintegration of the Polar ice, and thus forming a more equable temperature.

WE are glad to see that the *savants* of Haarlem continue to bring out the "Archives du Musée Teyler." The new fasciculus just received in London contains the following papers:—"Sur les insectes fossiles du calcaire lithographique de la Bavière," by H. Weyenbergh, jun.; "Description d'un crinoide et d'un poisson du système heersien," by Dr. T. C. Winkler, and a further instalment "Sur la Dispersion," by Van der Willigen. The plates representing the fossil insects and the crinoid are beautifully executed, and for purposes of study are almost as good as the originals. By way of explanation we mention that the "Système Heersien" takes its name from a district in the province of Limburg, in which two villages, Upper and Lower Heers, are situate.

FACTS AND REASONNIGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS *

II.

A.—*Experiments in which the fluids employed were raised to a temperature of* 100° *C. for from* 10 *to* 20 *minutes before the flask was hermetically sealed.*

a. Fluids employed being filtered infusions containing organic matter in solution.

1. *Infusions either slightly alkaline or neutral.*

Experiment 1.—A flask containing filtered beef-juice *in vacuo*, which had been hermetically sealed twelve days previously, after the fluid had been boiled for 15 minutes, was opened on January 7, 1870.

The solution itself was clear, and there was no pellicle on its surface, although there was a slight flocculent deposit at the bottom of the flask. The end of the flask having been broken off and its contents shaken, the first drop which was examined with a magnifying power of about 600 diameters † showed the most unmistakeable living organisms. There were mere moving

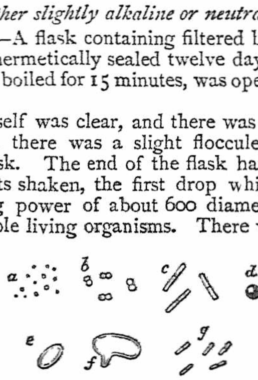


FIG 6.—*Monads, Bacteria*, and other organisms met with in first experiment.

specks or monads (*a*); several active organisms made up of two minute sphericles (*b*); a few medium sized, actively moving bacteria (*c*); an actively moving spheroidal body $\frac{1}{10000}$" in diameter (*d*); and several simple cellular bodies with a moderately thick cell wall, but apparently containing only fluid contents. These latter bodies varied in size from $\frac{1}{7500}$" to $\frac{1}{3500}$" in diameter. Most of them were simply ovoidal (*e*), one or two of them were seen to be more irregular in shape, either having projections (*f*), or even, as in one case, presenting projections something like cilia.

In the second drop of fluid examined, with the exception of a little granular matter, there were only seen several small but very active bacteria (*g*).

Experiment 2.—A flask, containing a mixed infusion (rather weak) of beef, carrot, and turnip, *in vacuo*, which had been hermetically sealed fourteen days previously, after the fluid had been boiled, was opened on December 23, 1869. The first two or three drops showed a large number of actively moving monads; a number of moving particles having an altogether irregular shape; bacteria distinct and moving, though less numerous; a number of spherical *Torula*-like cells of different sizes, containing a central dot (*a*); and a number of bodies mostly in the form of

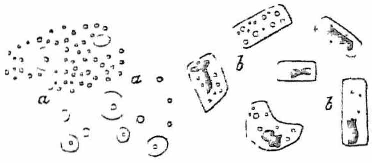


FIG. 7—*Torulæ* and other organisms met with in second experiment.

parallelopipeds with rounded angles (*b*), though some were quite irregular in shape—these containing granules of various sizes, with sometimes large and altogether irregular protoplasmic-looking masses.

Experiment 3.—A flask containing a weak decoction of beef and carrot *in vacuo*, which had been hermetically sealed thirty-nine days before, after the fluid had been boiled, was opened on January 18, 1870.

This flask had been allowed to remain so long before it was opened because the fluid within showed no noticeable signs of change. There was no pellicle, and only a very small amount of granular material at the bottom of the flask.

On microscopical examination no distinct living things were found. Only a small quantity of motionless granular matter was seen.

Experiment 4.—A flask containing an infusion of hay, together with a few grains of phosphate of soda, *in vacuo*, which had been hermetically sealed seventeen days previously, after the fluid had been boiled, was opened on January 25, 1870.

The fluid itself was not turbid or cloudy, though it had become darker in colour. The bottom of the flask was irregularly lined with granular and slightly flocculent material.

On microscopical examination of two or three drops, there were seen many actively-moving monads; some bacteria of medium size; many quite irregularly-shaped particles in active movement; many flattened bits of protoplasmic-looking material with irregular and slightly curled edges, slowly moving, and ranging in size from $\frac{1}{10000}$" to $\frac{1}{5000}$" in diameter (other masses of this kind were distinctly hollow though mostly irregular in shape); and lastly there were several large irregular masses of fibres, the nature of which could not be determined.

2. *Infusions having an acid reaction.*

Experiment 5.—A flask containing a filtered infusion of turnip *in vacuo*, which had been hermetically sealed only *five* days previously, after the fluid had been boiled, was opened on December 15, 1869.

On the second day after the flask had been sealed, the previously clear solution began to exhibit a cloudy appearance. The next day a reticulated scum was seen on the surface of the fluid, which gradually became more manifest on the two following days. When the neck of the flask was opened, its contents were found to emit a most fœtid, sickly odour.

Microscopical examination revealed a very large number of bacteria and vibrionic-looking rods, some straight and others bent, some motionless and others exhibiting languid movements. These, mixed up with a thickly interlaced network

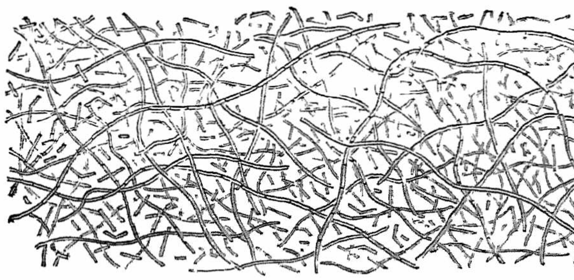


FIG. 8.—*Bacteria, Vibrios*, and *Leptothrix* filaments, met with in a Turnip-infusion which had been only five days *in vacuo*.

of *Leptothrix* filaments, constituted the reticulated pellicle which was seen on the surface. The *Leptothrix* fibres were partly plain, and partly segmented; they had a precisely similar appearance to the vibrionic-looking filaments, with which in thickness they also closely agreed. The long filaments seemed, in fact, to be only developed forms of the shorter rod-like filaments. A large nucleated ovoid cell was also seen, $\frac{1}{1425}$" in its longest diameter, as well as a smaller vesicle, enclosing a rod-like body.

Experiment 6.—A flask containing an infusion of turnip *in vacuo*, which had been hermetically sealed seventeen days previously, after the fluid had been boiled for 20 minutes, was opened on January 25, 1870. The fluid never exhibited any distinct turbidity, and no pellicle formed on the surface; there was, however, an irregular covering of the bottom of the flask by fine granular matter, with here and there a small patch of filamentous-looking substance. No bad odour was perceived on opening the flask.

Unfortunately just as I was proceeding to examine the contents microscopically, nearly all the fluid was lost, including the filamentous-looking masses. Examination of a few drops of the fluid which remained showed a very large number of monads and bacteria; only a few of them were isolated and displayed movements; they were for the most part aggregated, either into spherical masses or else in the form of portions of a broken layer which presented traces of a *secondary organisation* similar to that represented in Fig. 1.

Experiment 7.—A flask containing an infusion of turnip *in vacuo*, which had been hermetically sealed seven days previously, after the fluid had been boiled for 20 minutes, was opened on February 4, 1870.

* Continued from p. 177.

† An objective of this power was used throughout these investigations; it was an "immersion" glass (No. 9) of Nachet's make. All the objects were drawn, therefore, as seen with a magnifying power of about 600 diameters.

The solution itself was much clouded, and its surface was covered by a thick gelatinous pellicle.

On microscopical examination of the fluid it was found to contain a multitude of very active monads and bacteria. The thick gelatinous pellicle was also made up of an aggregation of these in the usual transparent mucoid material. In very many situations this uniform pellicle was undergoing a process of *secondary organisation*, such as I have already fully described as leading to the production of unicellular organisms, and such as was seen to a less extent in the last experiment.

Experiment 8.—A flask containing a very strong infusion of turnip *in vacuo*, which had been hermetically sealed fifteen days previously, after the fluid had been boiled for 15 minutes, was opened on February 28, 1870.

The solution itself was very cloudy, and there was on its surface a thick coriaceous sort of pellicle distinguished by closely set aggregations or islets of denser growth.

On microscopical examination the fluid was found to contain a multitude of very active monads and bacteria. The bacteria were almost more active than any I had before seen, and there were many different kinds. Some exhibited rapid serpentine movements, accompanied by flexions of the two segments of which they were composed; whilst the movements of others were rapidly progressive in straight or curved lines.

The pellicle was made up mainly of simple *Leptothrix* filaments (mostly without joints or evidences of segmentation); and the thicker islets were found to be produced by a more luxuriant growth in these situations of densely interwoven filaments.

The pellicle was found to be so tough and elastic that some of it could only be mounted as a microscopical specimen after it had been compressed for an hour or two, by placing a small weight on the covering glass.

On the same day that this solution was hermetically sealed *in vacuo*, two other portions of the same infusion were treated in a different manner for the sake of comparison. One (A) was enclosed, and sealed, in a flask with ordinary air and without the infusion having been boiled; another portion (B) was boiled for fifteen minutes, and when the solution was cool, so that the flask was filled with ordinary air as before, its neck was hermetically closed by the blow-pipe flame. The third portion (C), as above mentioned, after it had been boiled for fifteen minutes was sealed up *in vacuo*.

The changes which took place were as follows. Towards the close of the *second* day, solution A became cloudy, and twenty-four hours later that in B was in a similar condition. It was not till two days later that solution C became cloudy. Afterwards, till the fifteenth day, when they were all opened, solutions A and B underwent comparatively little change, only becoming rather more opaque, though no distinct pellicle was formed on either; in solution C, however, the pellicle continued to grow thicker and more distinct throughout the whole period. When opened the reaction of all three was still found to be slightly acid. In other respects their characters were as follows:

Odour.		*Nature of Contents.*
Somewhat sickly, with smell of baked turnip.	Solution A	A multitude of monads and bacteria of medium size, all very active.
Fragrant, like that of fresh turnip.	,, B	Small monad-like granules most numerous; bacteria scarce, and movements not very active.
Decidedly fœtid...	,, C	Monads and bacteria of many kinds, whose movements were extremely active. An enormous quantity of *Leptothrix* filaments.

The results of these comparative experiments are most interesting. The changes commenced first in the unboiled solution, as might have been imagined; then, in the two boiled solutions. We find them commencing in the solution in contact with air before they did in that which was contained *in vacuo*—the reason of this being not quite so obvious. But the changes in A and B were only able to advance to a certain extent, because, apparently, the space above the fluid being already filled with air, these changes, necessitating the evolution of gases as residual products, were only able to go on so long as the increased tension occasioned did not reach such a stage as to stop these molecular re-arrangements altogether. As might have been expected, the changes which took place in A were different from those which occurred in B. Those of A seemed to have been more thorough, giving rise to a much larger quantity of bacteria, and the somewhat sickly odour of this was also intermediate between the comparatively unchanged odour in A and the decidedly fœtid odour of B, in which such an enormous development of organisms had taken place. Here, then, the advantages of the vacuum seem to be most clearly shown.

After having read M. Pasteur's account, concerning the growth and development of fungoid organisms which had been placed in saline solutions,* it occurred to me that it would be a subject of much interest to determine whether any evidence could be obtained tending to show that organisms might even be evolved *de novo* in certain saline solutions. This, in fact, seemed to be a problem of very great importance; for, if otherwise suitable, the employment of such saline solutions would be attended by certain advantages. In the first place it was likely that the saline materials in solution would be far less injured by the high temperature of 100° C. than organic substances. And since, in working with a vacuum, we are able to get rid of the air altogether as a possible "germ" containing medium, so, with the view of simplifying our experiments, it becomes desirable that everything in our power should be done to diminish the number of possible pre-existing germs in the solutions employed. But this end, also, seemed likely to be best carried out by the employment of certain simple saline solutions. We should thus be able to get rid, as it were, of Buffon's "molecules organiques," which he supposed to pervade all organic matter that had been fashioned in a Living organism—and also of those ordinary organic molecules whose presence is supposed, even by M. Pouchet and other heterogenists,† to be absolutely necessary in order that "spontaneous generation" may occur. We should thus, indeed, best emerge from the circle of the organic, thus as far as possible elude the company of all those embarrassing molecules which the vitalist may choose gratuitously to endow with "vital" properties, and to regard as the chosen seat of a special "vital force," uncorrelatable with the ordinary physical forces. We may thus be best brought face to face with the problem—Whether the pre-existence of organic matter, which has been elaborated in pre-existing organisms, is, at present, absolutely necessary for the *de novo* origination of Living things; or whether, in fact, these may arise, more or less directly, by changes taking place in an aggregation of new formed molecules of an organic type, which have been themselves produced by the combination of some of the dissociated elements of the saline substances employed.

Just as nitrate of ammonia, carbonate of ammonia, free carbonic acid, and water, with a few saline substances, constitute the materials which—under the influence of the modified physical forces operating in the Living plant—are convertible into similar living vegetable protoplasm, so the problem which now presented itself was, whether under the influence of purely physical forces—acting upon solutions containing some such ingredients—re-arrangements might not be brought about amongst the elements of the substances dissolved and of the aqueous medium itself, resulting primarily in the formation of certain new complex molecules, which, secondarily, under the continued influence of similar physical forces‡, are capable of permitting the occurrence of new modes of collocation resulting in the evolution of the minutest specks of Living Matter §.

It resolved itself, in fact, into an inquiry whether Living things can now originate on the surface of our globe in that fashion after which alone (in accordance with the evolution hypothesis) they could have originated in those far remote geologic ages when Life first began to dawn upon the still heated surface of that rotating and revolving spheroid which now constitutes our planet. Before organic matter of the ordinary kind could exist organisms must have been present to produce it. Organisible compounds of a certain kind must, nevertheless, have preceded organisms. And just as chemists are now able to build up a great number of organic compounds in their laboratories, so it seems quite possible that some such mobile compounds may have been evolved by the agency of natural forces alone acting on the heated surface of the earth, at a period anterior to the

* Annales de Chimie et de Physique, 1862, p. 106

† Who at the same time that they are heterogenists, are firm believers in a special "vital" force.

‡ Those who wish to understand how incident physical forces are capable of bringing about such re-arrangement, should consult vol. i. chap. 2, of Mr. Herbert Spencer's "Principles of Biology."

§ For the genesis of Life, we need only look after the origin of such a speck of Living Matter. This is all that would be required by the greater number of the most advanced physiologists of the day. Given a mere speck of living matter less than $\frac{1}{80000}$" in diameter, and a Living thing may soon appear in the form of a *Vibrio*, a *Torula* cell, or a *Leptothrix* filament. By all such biologists the "vital" forces are held to be *molecular* forces, and such molecular forces are nought else than the resultant attributes of the particular advent of Living things.* We at present may, therefore, well wish to know, whether what is presumed to have taken place then may still take place now, since the affirmative solution of this problem would suffice to throw a halo of reflected light back through the ages, and would thus make that which is now a mere hypothesis, approximate as much as possible to the rank of one of the best established probabilities concerning the life-history of the globe on which we live.

It was with a feeble hope of throwing light upon the above-mentioned subject that I commenced the series of experiments which are now to be detailed. These were at first somewhat tentative, but the success obtained at each step emboldened me to proceed in my endeavours to obtain Living things under more and more arduous conditions. In all cases the saline substances were carefully selected; one of the first requisites being that they should, at all events, contain the four fundamental ingredients of Living things: nitrogen, carbon, hydrogen, and oxygen. Therefore it was that in almost all cases an ammoniacal salt was one of the substances used, on account of its capability of supplying nitrogen. And, with the view of keeping up a sort of uniformity, phosphate of soda was employed as the second saline ingredient in as many of the solutions as possible. This was considered quite as suitable as any other salt, and as a phosphate it presented certain advantages.† It was deemed necessary, moreover, that a certain mixture of substances should exist, in order that there might be sufficient diversity amongst the elements coexisting in the solutions. These elements being then affected differently by the play of incident physical forces upon them, under their new modes of vibration thus excited, new and altogether different mutual affinities might become dominant, after the fashion so clearly indicated by Mr. Herbert Spencer ‡ in reference to other molecular re-arrangements. And these affinities *might* also be such as would tend to a colloidal rather than to a crystalloidal mode of aggregation. Concerning such probabilities, we doubtless have much to learn. Amongst saline substances, colloidal modes of aggregation seem to be favoured under certain sets of conditions, and crystalloidal modes of aggregation under certain other sets of conditions. Prof. Graham showed that this was the case with such mineral compounds as silica, the sesqui-oxides of iron, chromium, and other bodies. Then, again, this possibility of an isomeric modification is admirably exemplified by the now well-known tendency of cyanate of ammonia to become converted into the organic compound called urea. We are told in Watts's *Dictionary of Chemistry* § that after an aqueous solution of cyanate of ammonia has been prepared, the "liquid exhibits the reactions of a cyanate, but when *heated* or left to evaporate spontaneously, it is converted into urea." This would seem to show that the passage from the crystalloid to the colloid mode of molecular collocation is by no means a difficult one—that it may be brought about, in fact, by very slight determining causes.

That transitions of a reverse order may be effected from the one to the other state or mode of aggregation—and this, too, even with more complex substances—seems indicated by the fact that certain protein compounds may exist at one time in their usual colloidal state, and at another time as statical crystalline aggregates. The possibility of the assumption of the crystalline form by a certain number of these protein or colloidal substances is, indeed, now placed beyond all doubt.‖ As the most familiar instance of this I may mention the now well-known oblique rhombic *hematoidin* crystals and other crystalline forms obtainable from blood. And amongst these latter are to be included certain tetrahedral crystals discovered¶ by Reichert, in connection with the placenta of the guinea-pig, the behaviour of which to reagents rendered it certain that they were of an albuminous or protein nature.* Chlorophyll, also, has been observed in a crystalline state by M. Trécul.† In these and in many other cases which might be cited, we have, apparently, to do with some mere alteration in the arrangement of molecules, and the crystalline form is to be regarded as possible when certain isomeric modifications are brought about in substances which ordinarily are non-crystallisable. No less an authority than M. Trécul, also, tells ‡ us that he has actually seen and watched a tetrahedral crystalline mass of this kind, which had been produced within the cells of the bark of the common elder (*Sambucus nigra*), gradually undergoing a modification in form at a certain part of a most startling nature. Whilst altering in shape, the part so altering was seen to become converted into a short fungoid filament which grew at the expense of the crystal. This change in form, then, could only be taken as the external sign of a much more profound molecular rearrangement taking place within the mass, whereby it was changed from a non-living albuminoid crystal into a Living and growing organism.§ In this remarkable case there must have been not only a relapse into the colloidal mode of molecular aggregation, but a secondary assumption of a still more unstable mode of collocation, by means of which the mass was gradually converted into a living embryo fungus.

It is well, then, for us to bear these facts and statements in mind, whilst we enter into the kindred though somewhat different inquiry, whether, under the influence of suitable conditions, there is any disposition for the ultimate constituents of different saline substances, existing intermixed in a state of aqueous solution, to fall into new groupings or modes of collocation of a non-saline or colloidal nature. If this can take place, we should then have a new kind of decomposition with an almost simultaneous recomposition—a re-arrangement, in fact—giving birth to substances allied to those of the protein group. And it would be only rational for us to suppose that such new-formed protein substances would be as prone to undergo change as these substances are generally found to be. If ordinary protein substances, therefore, which have been built up as parts of Living things, are capable of going through certain Life-giving changes, it would be quite possible that the differently evolved protein—that which comes into existence "spontaneously," or without the influence of pre-existing living things—may go through similar changes. The molecular constitution of these two kinds of matter may be closely allied, and wherever Life-giving changes occur, we are entitled to look upon these as actions resulting from the influence of physical forces upon material collocations whose molecular constitution is of such a nature as to render them prone to undergo current re-arrangements. A series of actions and reactions occur between such material collocations and their environment, and as a result Living things appear and grow. This tendency to undergo change is inherent in colloidal substances. As Prof. Graham told us:—"Their existence is a continual metastatis. . . . The Colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition. The colloid possesses ENERGIA. *It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.*"

B.—Fluids employed being Solutions of Saline Substances in Distilled Water.

Experiment 9.—A flask containing a solution (neutral) of crystallised white sugar, tartrate of ammonia, phosphate of ammonia, and phosphate of soda‖ *in vacuo*, which had been hermetically sealed nine days previously, after the fluid had been boiled for 20 minutes, was opened on January 4, 1870.

atomic collocation which displays them. The speck of living matter is held to contain no newly created force—just as no one believes it to contain newly created material units. Such a germ or embryo Living thing, is only a new mode of collocation of pre-existing matter, and of pre-existing force or motion. Its properties—the "vital" phenomena which it manifests—are the results of the particular material collocation which exists, and of the "conditions" by which this is surrounded. A Living thing is a dynamic aggregate, and between such aggregate and its environment, there is continuous action and reaction so long as Life exists.

I have given the reasons for such views a little more fully in an article entitled "Protoplasm" (NATURE, Feb. 24, 1870), and I would again refer the reader to the views announced by Prof. Huxley and Prof. Tyndall (Note, p. 175).

* See *Appendix* to Mr. Herbert Spencer's "Principles of Biology."

† The presence of phosphates was found by M. Pasteur greatly to favour the growth of fungi in certain saline solutions. (Annales de Chimie et de Physique, 1862, p. 108.)

‡ Principles of Biology, Vol. I. chap. ii., entitled, "The Action of Forces on Organic Matter." § Vol. ii. p. 193.

‖ See an article on "Albuminous Crystallisation," in *British and Foreign Medical and Chirurgical Review*, Oct. 1853.

¶ Beobachtungen über eine eiweissartige Substanz in Crystal-form:—Müller's Archives, 1849, p. 197.

* For an account of these reactions see *Brit. and For. Rev.*, loc. cit., p. 354.

† Comptes Rendus, t. lxi., p. 436. ‡ *Ibid.*, t. lxi. (1865), p. 435.

§ M. Trécul's own words are as follows:—"Lors de mes observations en 1860, j'avais reconnu que des corpuscules colorables en violet par l'iode remplacent frequemment les tétraèdres après la putréfaction, mais je ne vis pas à cette époque la transition des uns aux autres. Je fus plus heureux cette année; j'ai vu les tétraèdres s'allonger par un de leurs angles, et passe graduellement à nos singulières plantules en produisant une tigelle cylindrique. Dans ce cas le tétraèdre, arrondi ou encore anguleux, représente la bulbe. Le tétraèdre peut même s'effacer complétement, et ne laisser après lui qu'une plantule fusiforme ou cylindrique."

‖ The several ingredients were contained in this solution in the following proportions:—To 80 parts of water there were added 16 parts of sugar, 1 part of tartrate of ammonia, and ½ a part each of phosphate of ammonia and phosphate of soda. This solution, therefore, did not contain saline substances alone; the presence of sugar made it form a connecting link, as it were, between the solutions containing organic matter only, and those in which saline substances alone were contained.

Before the flask was opened the solution itself was clear and without the least trace of a pellicle on its surface, though for the last three or four days a very fine deposit was seen on certain parts of the bottom and sides of the flask.

When examined microscopically, a very few monads and bacteria were found in the first few drops of the fluid, which had been poured out before the whole was shaken. The remainder was then poured into a conical glass, and after having been allowed to stand for a time, the supernatant fluid was removed, and the last few drops containing the sediment were examined. In this were seen many bacteroid particles (*a*) and monads of different sizes, exhibiting the most active movements, as well as some irregular-

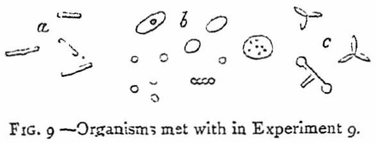


FIG. 9.—Organisms met with in Experiment 9.

shaped particles, also active. Many of the mere monad-like particles had the appearance of being very small *Torula* globules (*b*), and one cell of this kind was seen $\frac{1}{1500}''$ in diameter, and containing a nuclear particle in its interior. Many groups made up of three ovoid particles (*c*) were also seen, and also a dumb-bell-like body, all exhibiting slow movements. There were, moreover, motionless protoplasmic-looking masses—some cuboidal or more or less spherical and hollow, and others altogether irregular in shape, though appearing like masses of formless protoplasm; also peculiar branched fibres with knob-like swellings at the termination of the branchlets, and lastly a small mass made up of a spirally-twisted fibre, similar to that (represented in Fig. 13) which was found more abundantly in subsequent experiments* where tartrate of ammonia and phosphate of soda were employed.

Experiment 10.—A flask containing a solution† of acetate of ammonia and phosphate of soda, *in vacuo*, which had been hermetically sealed forty-two days previously, after the fluid had been boiled for 20 minutes, was opened on March 10, 1870.

The solution during this time had shown no signs of deposit, turbidity, or pellicle, and on microscopical examination of the fluid, no organisms of any kind were discovered.

Experiment 11.—A flask containing a solution‡ of oxalate of ammonia and phosphate of soda, *in vacuo*, which had been hermetically sealed sixty-one days previously, after the fluid had been boiled for 20 minutes, was opened on March 27, 1870. Its reaction was then found to be slightly acid.

The solution itself continued clear throughout, and presented no scum on its surface; but, after a time, a very small amount of deposit made its appearance at the bottom of the flask, and latterly this had appeared as a small, irregular, and opaque whitish aggregation, with branch-like ramifications. It was very friable and easily broke up into a white granular material. Almost immediately after the neck of the flask had been broken, small acicular crystals appeared in abundance on and in the solution, varying from about ½ to 1 line in length.§

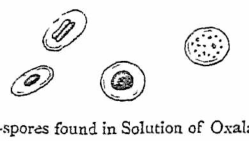


FIG. 10.—Fungus-spores found in Solution of Oxalate of Ammonia.

On microscopical examination, monads and altogether irregularly-shaped particles, showing tolerably active movements, were abundant. A few hyaline spherical vesicles were seen having no solid contents, and some of them were flattened at one pole. Three distinct fungus-spores were seen, and also another otherwise similar body in which there were small granules rather than a single nucleus. There was also a nucleated organism slightly larger, one of whose extremities was rectangular, whilst the other was rounded. In addition, there was a considerable quantity of white granular material which had resulted from the crumbling of large, though opaque white, masses of the same substance; and also some semi-crystalline, opaque rhomboid plates.

Experiment 12.—A flask containing a solution* of tartrate of ammonia and phosphate of soda, *in vacuo*, which had been hermetically sealed eleven days previously, after the fluid had been boiled for 20 minutes, was opened on January 21, 1870. The reaction was then found to be still slightly acid.

No turbidity of the fluid was observed, and no scum formed on its surface, though a very slight deposit gradually collected at the sides and bottom of the vessel, and amongst this there was seen, during the last six or seven days, a small white, flocculent mass, which gradually increased in size.

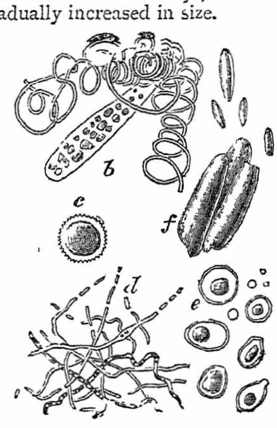


FIG. 11.—Spiral and Confervoid-looking Filaments, with Fungus-spores and Crystals, from a Tartrate of Ammonia Solution.

On microscopical examination of the white mass, this was found to be made up for the most part of a densely interlaced and spirally-twisted fibre, almost precisely similar to what was alluded to in Experiment 9 and to what is represented in Fig. 13. This was colourless, highly refractive, quite homogeneous, and apparently solid. In one place there was seen emerging from amongst the spiral fibres an elongated chamber (*b*) resembling a spore case, and containing many separate protoplasmic-looking masses. At one border of this spirally-twisted fibre-organism, there were a number of other filaments (*d*) about $\frac{1}{20000}''$ in diameter, and containing irregular masses of protoplasm in their interior. These were colourless confervoid-looking filaments.

A few moving monad-like particles were seen, and in the sediment there were many motionless aggregations of such particles imbedded in an almost invisible and scanty jelly-like matrix. In these masses rounded or ovoid fungus-spores (mostly with large nuclei in their interior, in various stages of formation) existed pretty abundantly (*e*). Some of the spores showed a slight neck-like projection and flattening at one extremity, as though they were about to germinate. In addition another much larger spore (*c*) was found. Some of the rounded crystals which were met with in this solution are also represented (*f*).

Experiment 13.—A flask containing a solution† of tartrate of ammonia and phosphate of soda *in vacuo*, which had been hermetically sealed twenty days previously, after the fluid had been boiled for 20 minutes, was opened on February 15, 1870. The reaction of the fluid was then decidedly acid.

The fluid itself showed no signs of turbidity, and there was no trace of scum on its surface. Small whitish flocculent shreds had, however, been seen at the bottom of the flask for the last twelve or fourteen days, during which time they seemed very slowly to increase in size. Some smaller sedimentary particles were also seen at the bottom.

On microscopical examination some of the white shreds were found to be composed of comparatively large masses of the small algoid filaments which were met with in the last experiment; whilst others were made up of an aggregation of fungoid spores with an abundant mycelium which had been developed from them. The spores were precisely similar to those which

* This fibre growth was twisted around a paper-fibre, but there is little doubt that it had simply grown round this as a mere foreign body. Every part of the paper fibre showed the most distinct colour reactions with the polariscope, though the spirally-twisted fibre did not show a trace of such reaction.

† Reaction neutral. ‡ Reaction neutral.

§ Two days before this flask was opened, it was removed for a few hours into another room, where the temperature was lower. The agitation of the fluid, and perhaps in part the lower temperature, soon resulted in the formation throughout the fluid of a number of large acicular crystals from ½ to 1 inch in length. These disappeared in the evening as soon as the temperature of the solution was again raised to about 80° F. The greatly superior size of the crystals produced *in vacuo* is worthy of note.

* Reaction slightly acid. † Reaction slightly acid.

were found in the last solution. There they were about to germinate, and here they had germinated into a fungus of the *Penicillium* type. In one mass the mycelium had produced four or five much larger filaments, terminating in artichoke-like heads of different sizes, bearing naked spores. Though two of the organisms met with in the last experiment were here reproduced, this was not the case with the spirally-twisted fibre organism.

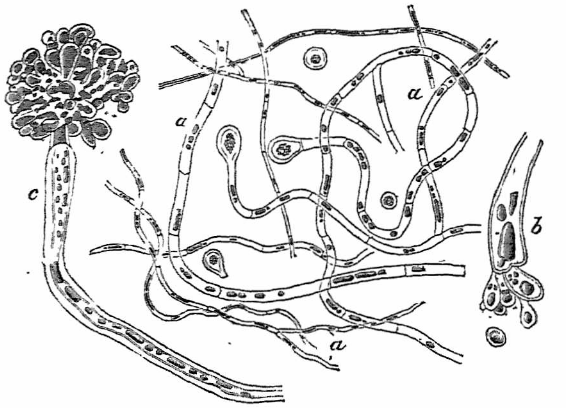


FIG. 12.—Fungus found in the Solution containing Tartrate of Ammonia and Phosphate of Soda.

A few actively moving granules and particles of various shapes were seen, though, as in the last solution, there was nothing resembling a bacterium. Spherules which seemed to represent different stages in the development of the fungus-spores were met with, varying in size from that of an almost inappreciable speck up to that of the perfect spore, which itself varied considerably in size at the time that it began to germinate. In one of these fungus-spores which was about half grown, the nuclear particle within was seen actively moving from end to end of the cell.*

Experiment 14.—A saturated solution of tartrate of ammonia and phosphate of soda *in vacuo*, prepared in same manner as the last solution and at the same time, though opened on the thirty-fifth day, yielded no organisms of any kind.

Experiment 14*a*.—(In this and in the following experiment the solutions were not contained *in vacuo*, but were intended rather to throw light upon the question as to whether the vacuum was favourable or prejudicial to the appearance and growth of organisms in these saline solutions.)

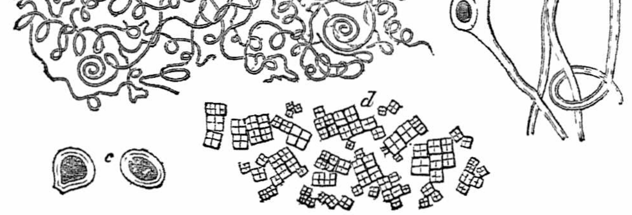


FIG. 13.—Spirally Twisted Fibre Organism.

A solution of tartrate of ammonia and phosphate of soda in distilled water was boiled for 20 minutes, and was then allowed to cool, so as to become filled with air at ordinary atmospheric pressure (which might have contained living germs, since no precaution was taken to destroy them). The neck of the flask was then hermetically sealed, and this was not opened till the thirty-eighth day, when the fluid was found to have a decidedly acid reaction.

The fluid never showed any signs of turbidity, and no pellicle formed on its surface, though a small white flocculent mass had been seen at the bottom of the flask for three weeks, which very slowly increased in size.

The white mass (Fig. 13, *a*) was picked out, and on microscopical examination it was found to be made up for the most part of a spirally-twisted fibre organism, very similar to that which was produced in a similar solution *in vacuo*. The spiral twisting was, however, even more marked, and the fibre in many places was somewhat thicker.*

In addition, two or three small fungus-spores were seen, very similar to those met with in Experiment 12; and there were also some confervoid-looking filaments with irregular masses of protoplasm in the interior, occurring sparingly here and there, similar to those which were met with in much larger quantity in the former experiment. Four or five larger cells were seen with very thick walls, closely resembling the sporangia of some fungi, and in one of them, measuring $\frac{1}{1000}''$ in diameter, many granules were present exhibiting the most active molecular movements. The contents of the other cells could not be made out, as they were so much obscured by the fibre organism in the midst of which they were imbedded.

Experiment 14*b*.—A solution of tartrate of ammonia and phosphate of soda in distilled water, without having been boiled, was exposed to the air † in a flask, and was then examined microscopically on the thirtieth day. The re-action of the fluid at this time was neutral.

There was no pellicle on, or opacity of the fluid, but a small whitish mass of matter had been seen at the bottom of the flask for the last fortnight, which slowly increased in size, till at last it formed a mass bigger than a mustard seed.

On microscopical examination this whitish flocculent matter was found to be made up principally of an enormous aggregation of *Sarcina*-like material, the divisions of which were rather more sharply defined though they were about equal in size to the *Sarcina* which is occasionally met with in urine.

Several fungus-spores were also seen (Fig. 13, *c*) larger than, though otherwise similar to, those met with in *Experiments* 12, 13, and 14*a*. There were none of the confervoid-looking filaments or of the spirally-twisted organisms.

* In Experiments 12 and 13, the actual strength of the saline solutions was not known, but in these as well as in Experiments 14*a* and 14*b*, there may have been about 15 grains of tartrate of ammonia, with about 5 grains of phosphate of soda to an ounce of water.

* It has been suggested to me by a friend whose opinion carries great weight with it, that this is not an organism which had grown in the solutions, but rather some non-living accidental product which had gained access to the solutions. This was a suggestion which deserved a most attentive consideration, more especially as in two other experiments, which will not be cited, foreign bodies were found in the solutions. Are these spiral-fibre masses, then, mere non-living *débris* of some kind? It was suggested that they were spiral ducts of some plant broken up and modified by the boiling process which they had undergone.

With reference to this question it seems most desirable to state, in the first place, that such a spiral-fibre mass has been met with four times in solutions containing a mixture of tartrate of ammonia and phosphate of soda, though never in solutions containing tartrate of ammonia alone, and in fact it has never been found in any other kind of solution except in one whose chemical constitution was almost precisely similar to the mixture above named. This seemed to indicate that it might have been contained within or upon the crystals of phosphate of soda. Successive solutions, however, of many of these in a watch-glass have shown no trace of such fibre-masses (and it may be well to add, perhaps, that a similar statement holds good for the crystals of tartrate of ammonia). These masses of spiral fibre, found only in solutions of a given chemical constitution, have been seen to increase in size from week to week. The Rev. M. J. Berkeley, who was kind enough to examine my specimens, could not identify them with any known cryptogamic organisms.

If they were really altered spiral ducts, then, seeing that they have only been met with in solutions of a certain chemical constitution, they ought to have been contained in the saline substances employed. But an examination of such saline substances as above stated, does not show a trace of these fibre-masses. All the spiral ducts which I have examined, moreover—even after boiling—polarise beautifully, whilst these spirally twisted fibre-masses do not give the least trace of colour reactions with polarised light. And lastly, in certain parts the apparently solid spiral fibre seems to become decidedly tubular, and in other places it widens out into flat expansions of a peculiar character. In one of these expansions (Fig. 13, *b*) differentiation had apparently taken place, which had led to the production of a spore-like body.

A somewhat similar twisted fibre-organism was met with in Experiment 16, though here it was evidently in an embryonic condition. (Fig. 15.) It seemed to be then arising by a process of differentiation taking place in a pellicular mass.

† A piece of paper merely, was loosely inserted into the mouth of the flask with the view of keeping out as much dirt as possible.

Experiment 15.—A flask containing a solution of a potash-and-ammonia alum, and of tartar emetic, *in vacuo*, which had been hermetically sealed twenty-eight days previously, after the fluid had been boiled for fifteen minutes, was opened on March 17, 1870. The fluid then had a decidedly acid reaction.*

The solution continued clear throughout; there was no trace of a pellicle and no deposit at the sides, though a whitish flocculent mass was seen at the bottom of the flask after the first fortnight, which gradually increased in amount, and at last formed a mass about $\frac{1}{8}''$ in diameter.

On microscopical examination the white mass was found to be made up of aggregations of variously sized and irregularly shaped protein-looking particles which were imbedded (*b*) in a most distinct hyaline jelly-like material. The granules were highly refractive, altogether irregular in shape, and they varied in size from $\frac{1}{3000}''$ up to $\frac{1}{5000}''$ in diameter. Though most of the particles

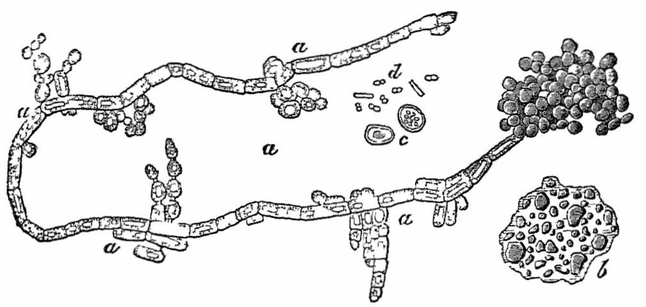


FIG. 14.—Fungus met with in a solution containing Potash-and-Ammonia Alum, with Tartar Emetic.

were motionless and imbedded in the jelly, very many were seen exhibiting active and independent movements; some of these were in the form of little double spherules (*d*), and a very few others resembled bacteria about $\frac{1}{8000}''$ in diameter, though they did not possess the accustomed joint.

Three fungus-spores with thick double walls were seen. Each these was about $\frac{1}{3300}''$ in diameter. Within one of them there were only a number of granular particles (*c*), but within each of the other two there was a large and somewhat irregular nuclear mass.

In addition there was found the complete fungus which is represented in the figure (*a*), with all its spores. In a portion of one of the granular aggregations, a mass of about thirty spores seemed to be undergoing evolution in a part of the mucoid material through which some fine granules were disseminated.

Experiment 16.—A flask containing a solution of carbonate of ammonia and phospate of soda *in vacuo*, which had been hermetically sealed thirty days previously after the fluid had been boiled for twenty minutes, was opened on March 1, 1870. The fluid was then found to have a very slightly alkaline reaction.

The fluid had continued clear and no pellicle had formed on its surface, though a light granular deposit had slowly collected in small quantities on the bottom and sides of the flask.

On microscopical examination, bright highly refractive moving particles, very similar in appearance to those of milk, were met with, of all sizes varying between $\frac{1}{30000}''$ and $\frac{1}{5000}''$ in diameter. There were also numerous crust-like aggregations of such particles. Small *Torula* cells, the smallest being about $\frac{1}{15000}''$ in diameter, were very abundant. They were either single or double, and each cell contained a distinct nuclear particle; some of the larger ones, indeed, contained two. All these cells exhibited very slight oscillating movements. Two cellular looking bodies, each about $\frac{1}{2000}''$ in diameter, and having granular contents, were also seen; and, in addition, there were two or three patches of a peculiar spirally twisted fibre-like organism, growing in portions of the granular crust, and apparently representing an embryonic condition of a spiral fibre, closely resem-

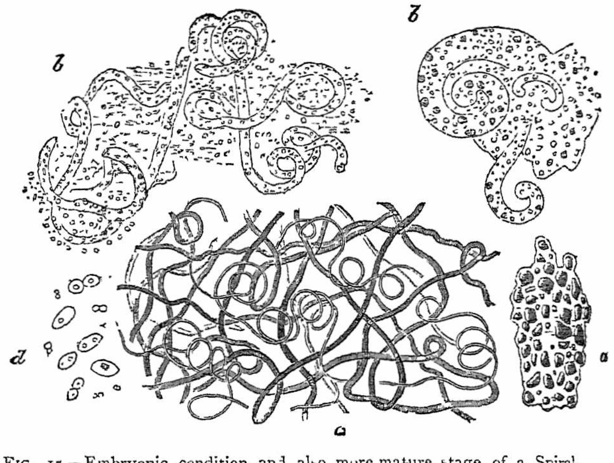


FIG. 15.—Embryonic condition, and also more mature stage, of a Spiral-fibre Organism met with in a solution of Carbonate of Ammonia and Phosphate of Soda.

bling those met within previous experiments. Neither of these gave the smallest trace of a colour reaction with the polariscope.

B.—*Experiments in which the fluids employed were raised to a temperature of from* 146° *to* 153° *C. for four hours, after all air had been exhausted and the flasks had been hermetically sealed.*

A temperature of 100° C. has been the degree of heat to which all the fluids in the experiments hitherto related have been subjected. It has been previously found that none of the lower organisms so treated, and which had been afterwards examined, were able to survive an exposure for a few seconds to such a degree of heat. They had nearly all been destroyed, in fact, at a temperature many degrees short of this. Many different kinds of organisms have been submitted to this test, and without the occurrence of any exceptions* such a degree of heat has always proved fatal to them. Looking, therefore, on the one hand, at the uniformity in the experimental evidence, which has itself extended over a wide basis, and, on the other, at the comparative uniformity in fundamental nature and property existing between all the lowest kinds of Living things, which are almost wholly made up of a more or less naked living matter or protoplasm, it is only reasonable for us to conclude, until direct evidence can be adduced to the contrary, that that which holds good for the many without exception may prove to be a rule of universal application. Therefore it was that the Societé de Biologie (and M. Pasteur himself for a long time) assumed that none of the lower kind of organism could survive in a fluid which was raised to a temperature of 100° C.

No evidence has as yet been adduced which is capable of shaking the validity of this conclusion, and the experiments just related are much stronger in favour of the view that the organisms found in my experimental fluids were there evolved *de novo* than were all the negative results in the experiments of Schwann (upon which so much stress was formerly laid) in proving the impossibility of such a mode of evolution. And yet Schwann's experiments were deemed by many so conclusive that they were thought to have upset the doctrine of heterogeny. The fluids with which he experimented were only exposed to a temperature of 100° C., and, working under a particular set of conditions which are considered to be adverse to the occurrence of evolutional changes, he found no organisms therein when his flasks were opened. I, on the contrary, subjecting my experimental fluids to the same temperature, though exposing them subsequently to quite different conditions, which I suppose to be more favourable for the occurrence of evolutional changes, do find organisms in the fluids when the flasks are opened. His negative results *may* be only applicable to the particular fluids and the particular conditions under which he worked; but my

* This solution was prepared one evening when I had been busy in devising several other mixtures with which I deemed it desirable to experiment. At the time I thought I possessed a pure potash alum, and by some strange oversight I had failed to recognise that, if this had been the case, I should have been employing a solution which contained no nitrogen. Having once planned what mixtures I would use, I did not further think of each of them analytically. It was not, therefore, till my attention was called by Dr. Sharpey to the assumed absence of nitrogen in the above solution, that I became aware of this. It seemed very incredible that an organism should have been produced in a solution containing no nitrogen. I then had some of my supposed potash alum carefully tested, and it was found to contain a considerable quantity of ammonia. I then also learned that as ammonia-alum is now the alum of commerce, it is very difficult to get a pure potash alum.

* No exceptions, that is, amongst such organisms as are met with in infusions. The only known exceptions to that rule being met with in the case of seeds naturally provided with a hard *testa*, and after these had undergone an extreme amount of desiccation.

positive results, and those of many other experimenters, may be considered to have a most important bearing upon the settlement of the general doctrine. It becomes simply a question as to whether the conditions which were formerly believed sufficiently stringent to destroy all pre-existing Living things within the experimental flasks are in reality adequate to effect this. Can the standard of vital resistance be raised? If the old landmarks cannot be shown to be false, then there is very far more evidence of all kinds in favour of, than there is against, the occurrence of "spontaneous generation."

But in order, still further, to put this question beyond the region of doubt, I gladly availed myself of Prof. Frankland's proffered aid. He very kindly offered to procure a perfect vacuum in my flasks, and then, after these had been hermetically sealed, to expose them and the fluids which they contained for some time to a temperature of about 150° C. This temperature for a fluid is so very far in advance of that which is at present believed to be destructive to all the lower organisms, that it is perfectly fair for us to presume, till evidence be adduced to the contrary, that Living things would be completely destroyed by an exposure to such a temperature even for a single minute.

It seemed desirable to try different kinds of solutions in this way, and I therefore asked Dr. Frankland to be kind enough to submit four flasks and their contents to this most stringent test. In order that they might be representative of the solutions with which I had already been working, one was to contain a freshly prepared and filtered infusion of turnip; another a solution of sugar, tartrate of ammonia, and phosphate of soda in distilled water; another a solution of tartrate of ammonia and phosphate of soda alone in distilled water; whilst the last was to contain carbonate of ammonia and phosphate of soda in distilled water.

The second of these solutions was submitted to these conditions by Dr. Frankland, on Feb. 11, 1870, together with two others, one containing turnip juice, and the other carbonate of ammonia and phosphate of soda. On the following day I received a letter from Dr. Frankland, in which he had written as follows:—"Yesterday, I exposed the three liquids to a temperature of 150° C. for four hours. On taking them out of the digester this morning two were broken; one had probably burst with the pressure of carbonate of ammonia vapour, the other had received some slight shock which had broken off the extreme point of glass where it was drawn off before the blowpipe. The third, containing the tartrate of ammonia, sugar, and phosphate of soda, I send along with this, but fear you will not consider its contents favourable for your operations, part of the sugar having apparently been converted into caramel."

A few days afterwards (on Feb. 15) Dr. Frankland submitted the three other solutions to a similar treatment, and he has kindly furnished me with the following statement of the conditions to which they were subjected:—

"Each liquid was placed in a glass tube about $\frac{3}{4}$ inch in diameter, 9 inches long, and closed at one end by fusion of the glass. The open end of the tube was then drawn out so as to form a thick capillary tube, which was afterwards connected with a Sprengel's mercurial pump. The action of the pump soon produced a tolerably good vacuum, when on gently warming the liquid, the latter began to boil, its vapour expelling the last traces of air from the apparatus. After the boiling had been continued for several minutes, the tube was hermetically sealed at the capillary part.

"Three tubes were prepared in this way, containing respectively—

"1. Turnip juice;

"2. Solution of ammonic tartrate and sodic phosphate;

"3. Solution of ammonic carbonate and sodic phosphate.

"The vacuum in tubes Nos. 1 and 2 was so perfect as to render them good water-hammers. In the third tube the vacuum was not so good, owing, doubtless, to the evolution of carbonic anhydride from the ammonic carbonate.

"The three tubes were now placed in the wrought iron digester, described by me in the Philosophical Transactions for 1854, page 260. It consists essentially of a cylindrical iron vessel, with a tightly fitting cover, which can be securely screwed on to it. Through the centre of the cover passes an iron tube, which descends half way down the centre of the cylinder. This tube is closed at bottom, and contains a column of mercury about an inch long, and a thermometer plunged into the mercury shows the temperature of the liquid inside the digester.

"Water being now poured into the digester until it covered the tubes, and the cover having been screwed on, heat was applied by means of a gas stove.

"The temperature was allowed to rise to about 150° C., and was maintained between 146° and 153° C. for four hours, and it is almost needless to say that every part of the sealed tubes and their contents was exposed to this temperature during the whole time. The glass tubes, though of moderately thick glass only, ran no risk of fracture, because the pressure inside them was approximately counter-balanced by the pressure of steam outside.

"After cooling, the tubes were removed from the digester, when it was found that the turnip juice had become discoloured, and the liquid no longer acted as a water-hammer, showing that the vacuum in the tube had been impaired. The contents of the two remaining tubes were apparently unaltered, the vacuum in No 2. being as perfect as ever."

Experiment 17.—The tube containing the infusion of turnip was opened at the end of the twelfth day.

When received from Dr. Frankland the fluid had been changed to a decided, but light brown colour, and there was some quantity of a blackish brown granular sediment at the bottom, though the solution was free from all deposit when placed in the digester. After this tube was suspended in the warm place, as the others had been, it remained in the same position till it was taken down to be opened. A slight scum or pellicle was observed on the surface—covering this partially—on the sixth day. During the succeeding days it did not increase much in extent, though it became somewhat thicker. Although very great care was taken, still the slight movement of the flask, occasioned in knocking off its top, caused this pellicle to break up and sink to the bottom.*

The contents of the flask emitted a somewhat fragrant odour of baked turnip, and the reaction of the fluid was still slightly acid. On microscopical examination, there was found very much mere granular *débris* of a brownish colour, which probably represented the brownish sediment seen when the tube was removed from the digester. There were, also, a very large number of dark apparently homogeneous reddish brown spherules, mostly varying in size from $\frac{1}{7500}$" to $\frac{1}{20000}$" in diameter, partly single and partly variously grouped; the nature of these was doubtful though they were probably concretions of some kind. There were also other indeterminate flat and irregular masses, which seemed more to resemble protoplasmic substance in its microscopical characters.

In addition, many irregular and monad-like particles were seen in active movement, though there were no distinct bacteria.

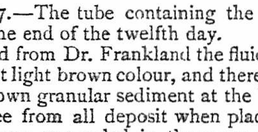


FIG. 16.—*Tailed Monad* and *Torula* Cell from a Turnip Solution which had been exposed to a temperature of 146°—153° C. for four hours.

Several rod-shaped bodies $\frac{1}{2000}$" in length (*d*) were seen, however, resembling ordinary bacteria, except that they were unjointed and motionless.† In one of the drops examined there was a delicate tailed monad in active movement—a specimen of *Monas lens*, in fact, $\frac{1}{7500}$" in diameter, having a distinct vacuole in the midst of the granular contents of the cell. Another ovoid body was seen, about the same size, without a tail and motionless, though it contained two nuclear particles within.‡

* It was owing to the appearance of the pellicle and the seeming likelihood of its breaking up and sinking to the bottom if allowed to remain, as I had known others do, that I was induced to open this tube so early. I thought it possible that nothing else might form afterwards. I felt anxious to be able to examine this pellicle before it got mixed with the granular matter at the bottom.

† The bacteria which appear in a simple solution of tartrate of ammonia, however, frequently remain motionless for a very long time, and the *bacteridia* of Davaine are both unjointed and motionless.

‡ Although these were the only bodies of this kind actually seen, it is worth noting that only five or six drops of about the whole ounce and a half of fluid were submitted to examination. This solution was undoubtedly examined rather prematurely. In another turnip solution which has been subsequently exposed in Dr. Frankland's digester to a temperatnre of 146° C. for a shorter time, and which was opened on the 67th day (the details of which I shall

Experiment 18.—The tube containing white sugar, tartrate of ammonia, and phosphate of soda in distilled water, was opened on the fortieth day after it had been heated in the digester.

When received from Dr. Frankland, this solution, instead of being colourless, was of a dark brownish-black colour, not unlike that of porter, this change of colour being apparently due to the conversion of some of the sugar into caramel; though it was free from any notable deposit. After about the fortieth day a thick iridescent scum gradually formed over the whole surface‡, and continued visible for more than a week. It afterwards disappeared, and then I noticed for the first time a large quantity of a brownish-black sediment at the bottom of the flask. No scum again formed on the surface, and no other change was seen to take place in the fluid.

When opened there was no very appreciable odour, though the reaction of the fluid was strongly acid.

On microscopical examination, the deposit was found to be made up in great part of dark, reddish-brown, or ruby-red coloured globules of various sizes, partly single, and partly in various kinds of aggregation. They varied in size from a minute speck $\frac{1}{20000}''$ in diameter up to large spherule $\frac{1}{1428}''$ in diameter. They presented no trace of structure, and were apparently quite homogeneous. Although larger, they resembled in other respects those which had been met with in the turnip solution. Other bodies, however, were seen, which presented much more obvious evidence of being concretions. They were mostly light brown in colour, some of the smaller somewhat resembling cells, with granular contents, whilst some of the larger, about $\frac{1}{2000}''$ in diameter, showed concentric markings, with or without the presence of a darker central nucleus of varying size. Some again were composite structures—each spheroidal as a whole, though made up of a close radiating aggregation of ovoidal bodies around such a central nucleus. None of these spherules showed any colour reactions with the polariscope. I regarded them all as non-living concretions of a doubtful nature

Two fresh looking fungus-spores were seen, however, of the most unmistakeable character. One, apparently just about to sprout, was $\frac{1}{7500}''$ in diameter; it had thick walls and a large central nucleus $\frac{1}{12000}''$ in diameter. The other was a smaller and more delicate-looking body, having more the appearance of a *Torula* cell. There were also some figure-of-eight-shaped bodies about $\frac{1}{20000}''$ in diameter, which were moving about most actively.

In addition, there were a multitude of particles altogether irregular in shape, in most active movement, having a protoplasmic appearance. Some were altogether irregular particles others were larger and more elaborate masses; whilst others —still exhibiting slow movements—were lozenge-shaped, or more or less cuboidal.

Experiment 19.—The tube containing tartrate of ammonia and phosphate of soda in distilled water was opened on the sixty-fifth day.

When received from Dr. Frankland, the solution in this tube was quite colourless, clear, and free from visible deposit. About the fifth or sixth day, however, after it had been suspended in a warm place, a number of small, pale, bluish-white flocculi made their appearance throughout the solution, and continued always in the same situation except when the fluid was shaken,—owing apparently to their specific weight being the same as that of the fluid itself. The contents of the flask were repeatedly scanned with the greatest care with the aid of a lens, though nothing else could be seen until about the expiration of a month. Then there was observed, attached to one of the flocculi about $\frac{1}{4}''$ from the bottom of the vessel, a small, opaque, whitish speck, little bigger than a pin's point. This increased very slowly in size for the next three or four weeks, and then another smaller mass also made its appearance. At the expiration of this time the larger mass was more than $\frac{1}{8}''$ in diameter, and both could be and were seen by several people with the naked eye. In the last three weeks previous to the opening of the flask it was often remarked that the mass did not appear to have at all increased in size.

Before the flask was opened it was found that it only acted as a water-hammer to a trifling extent, though when the narrow end of the flask was broken off there was a slight dull report, and a quantity of small particles of glass were swept by the in-rush of air into the fluid. There had still, then, been a partial vacuum in the flask. The reaction of the fluid was found to be slightly acid.

This flask was opened in Dr. Sharpey's presence. He had examined the white masses previously with a pocket-len , and when the flask was broken the larger white mass issued with some of the first portions of the fluid, which were poured into a large watch-glass. This was at once taken up on the point of a pen-knife and transferred to a clean glass slip, when it was immersed in a drop of the experimental fluid and then protected by a thin glass cover. On microscopical examination we at once saw that the whitish mass was composed of a number of rounded and ovoidal spores, with mycelial filaments issuing from them in all

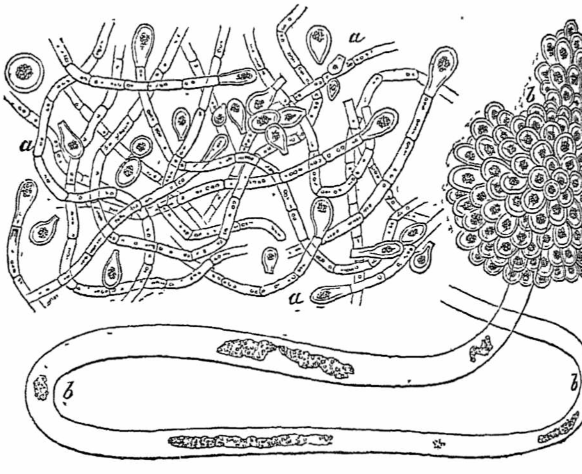


FIG 17.—Fungus obtained from a solution of Tartrate of Ammonia and Phosphate of Soda which had been exposed to a temperature varying between 146° and 153° C. for four hours.

stages of development. The spores varied much in shape and dimensions; the prevalent size being about $\frac{1}{3500}''$ in diameter, though one was seen as much as $\frac{1}{2000}''$ in diameter. They all possessed a single rather large nucleus, which was mostly made up of an aggregation of granular particles. Some were just beginning to develope mycelial filaments, and others had already given origin to such filaments about $\frac{1}{4500}''$ in diameter, in which there were scattered some colourless protoplasmic granules, but no vacuoles. Contiguous to these fresh and evidently Living portions of the plant, there were other parts in all stages of decay, in which the remains of the filaments were seen in the form of more or less irregular rows of brownish granules representing the altered protoplasmic contents of a previous filament whose walls were now often scarcely visible. Subsequently the smaller white mass was picked out, and this was found to contain some living mycelium and spores, and also a considerable patch of decaying filaments, in connection with which ther was a long and broader filament bearing at its distal extremity a large aggregation of more than 100 spores, quite naked, and very similar in character to those from which the mycelial threads arose. This plant was evidently a *Penicillium* quite similar to what had been obtained from a tartrate of ammonia and phosphate of soda solution on a previous occasion after twenty-one days (*Experiment* 13), and its spores were also similiar to those which had appeared in another solution (*Experiment* 11) of the same kind after eleven days.

The delicate flocculi which first made their appearance in the solution, and which persisted throughout, seemed to be made up of aggregations of the finest granules, which, however, became almost invisible when the flocculi were mounted in glycerine and carbolic acid.

Experiment 20.—The tube containing carbonate of ammonia and phosphate of soda in distilled water, was opened on the thirtieth day in the presence of Prof. Huxley.

When this flask was received from Dr. Frankland, the fluid was somewhat whitish and clouded. During the last ten days a thin pellicle had been seen gradually accumulating on its surface, and in the latter four or five days this increased much in thickness, and gradually assumed a distinct mucoid appearance. The fluid itself was tolerably clear, though an apparent turbidity was given by the presence of a fine whitish deposit on the sides of the glass.

afterwards publish), *Torula* cells, and fungus-spores with developing filaments, were formed in very large numbers, in company with other living organisms.

* This was much larger than that exposed in the other three vessels, owing to the fluid being contained in a small flask instead of in a tube.

When the flask was opened, the reaction of the fluid was found to be neutral.

Portions of the pellicle were at once transferred to a glass microscope slip, and then, protected by a covering glass, were submitted to a prolonged microscopical examination.* A number of little figure-of-eight particles, each half of which was $\frac{1}{20000}$" in diameter, were seen in active movement, even in situations where they could not have been influenced by currents. The portions of the pellicle were made up of large, irregular, and highly refractive protein-looking particles imbedded in a transparent jelly-like material. The particles were most varied in size and shape, being often variously branched and knobbed. There were also seen several very delicate, perfectly hyaline, vesicles about $\frac{1}{2000}$" in diameter, these being altogether free from solid contents.

A subsequent and most careful examination of a considerable quantity of the granular matter of the pellicle which had been mounted on two microscope-slips,† and at once protected by surrounding the covering glasses with cement, revealed five spherical or ovoid spores the average size of which was about $\frac{1}{3300}$" in diameter. They all possessed a more or less perfectly-formed

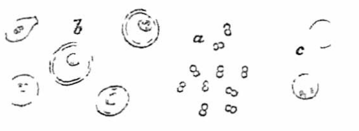


FIG. 18.—Fungus-spores found in a Carbonate of Ammonia and Phosphate of Soda Solution which had been exposed to a temperature varying from 146° to 153° C. for four hours.

nucleus, and all showed a most distinct doubly-contoured wall. One of the smaller of them showed that it had reached a stage where it was about to germinate. In addition, one small mass of *Sarcina* was seen, not very distinctly defined, owing to its being still in a somewhat embryonic stage.

H. CHARLTON BASTIAN

(*To be concluded.*)

* Professor Huxley examined portions of the pellicle as soon as they were removed. He at once admitted the general resemblance as regards microscopical characters between this and the pellicle which forms on solutions containing organic matter. The particles were, however, mostly indefinite in shape rather than definite, though they were obviously imbedded in a clear jelly-like material. They became deeply tinted, also, when tested with a weak magenta solution, in the same way that the monads and bacteria of an ordinary pellicle are tinted by such a fluid, whilst the jelly-like material only assumed the faintest tinge of colour.

† These were the two specimens which were mounted in Prof Huxley's presence, and which were examined by him. After he left I spent more than two hours in thoroughly searching over these specimens with a $\frac{1}{12}$" objective. And the result was that five fungus-spores were found in the two specimens as above stated. These are still in my possession.

NEW OBSERVATORY AT THE MAURITIUS

(*Communicated.*)

HIS Royal Highness the Duke of Edinburgh and his Excellency Sir Henry Barkly, attended by their respective suites, arrived at the site of the New Observatory at half past ten on Monday morning, May 30th, and were received by the President and Council of the Meteorological Society.

The architect of the building, Mr. Bowdler, of the Surveyor General's Department, and Mr. Horne, of the Royal Botanical Gardens, had elegantly decorated the grounds and the enclosed space around the massive stone.

The President, Captain Russell, R.N., read and presented the following address :—

"*To Captain His Royal Highness* ALFRED ERNEST ALBERT, *Duke of Edinburgh, Earl of Ulster, Earl of Kent, K.G., K.T., G.C.M.G., G.C.S.I., R.N., &c.*

"May it please your Royal Highness,

"We, the President and Council of the Meteorological Society of Mauritius, beg leave to approach your Royal Highness with the assurance of our loyalty to the Crown, and of our devoted attachment to Her Majesty the Queen and the Royal Family.

"We desire to offer to your Royal Highness a cordial welcome to these shores.

"Mauritius being admirably situated for meteorological and magnetical observation, it has long been the ardent wish of the Society that an Observatory, fully equipped wtth instruments of the most approved construction, should be permanently established for that purpose.

"Through the wise and liberal policy of the late Sir William Stevenson, and more immediately that of our present Governor and Patron, his Excellency Sir Henry Barkly, the moment for commencing the principal building has now arrived.

"We therefore hail with joy the auspicious occasion which allows us to request that your Royal Highness will be pleased to lay the Foundation Stone.

"Meteorological and magnetical researches, having for one of their main objects the safety and welfare of Navigation, touch closely the commercial prosperity of the world, and link together men of every nation by the cords of a common interest. Your Royal Highness, therefore, being not only an Officer in the Naval Service of a Queen whose ships traverse every sea, and whose Dominions extend to the remotest shores, but also the Son of an Illustrious Prince, who was ever foremost in encouraging Science, there could not, we venture to to think, be a more fitting Monument of the visit of your Royal Highness to Mauritius than a building to be specially devoted to objects with which your Royal Highness's experience cannot fail to prompt a generous sympathy.

"With our warmest wishes for your Royal Highness's welfare and happiness, and with renewed assurances of our devotion to our beloved Sovereign's Person and Family,—We have the honour to be, with the highest respect, your Royal Highness's most humble and most dutiful Servants,

"(Signed) James Tho. Russell, R.N., *President;* H. J. Jourdain, *Vice-President;* A. J. W. Arnott. *Treasurer;* J. C. Browne, C. Bruce, A. B. Commins, M. Connal, J. H. Finniss, Robt. Leech, George McIrvine, *Council;* C. Meldrum, *Secretary.*"

His Royal Highness replied in the following terms :—

"*To the President and Council of the Meteorological Society.*

"Gentlemen,

"I beg to thank you most heartily for your cordial welcome. It will afford me sincere pleasure to lay before Her Majesty the assurance of loyalty and devotion conveyed in your address.

"As an officer in the navy, I fully appreciate the advantageous position of this island for meteorological and magnetical observation, and it is with great satisfaction that I proceed to comply with your invitation of laying the foundation stone of a building to be specially devoted to objects of such importance.

"Your expressions of affectionate interest in all that concerns my future welfare and usefulness in the career which, by God's will, may be before me, are such as to demand a no less cordial response on my part.

"May this ceremony be auspicious to all concerned, and I sincerely trust that the Observatory, when completed, may realise all the expectations of its promoters, and continue throughout the future, not only a source of utility to the colony itself, but an aid to the cause of science, and a blessing to navigators throughout the world.

"(Signed) ALFRED."

The ceremony of laying the foundation stone was then proceeded with. His Royal Highness first deposited in a recess in the lower stone a bottle containing coins of the realm, a copy of the address, newspapers of the colony, and a paper on which was engrossed the following inscription :—

"This foundation stone of the Mauritius New Observatory was laid by Captain H.R.H. the Duke of Edinburgh, in presence of H. E. Sir Henry Barkly, K.C.B., F.R.S., Governor and Commander-in-Chief of Mauritius, at the request of the Meteorological Society, on Monday, the 30th of May, 1870."

His Royal Highness then spread the lower stone with mortar, and the upper stone was lowered into its bed. The Prince afterwards tried the stone with plumb-rule, level, and gavel in right masonic style, and declared it well and truly laid.

The trowel used by his Royal Highness, and presented to him by the society, was of solid silver, elegantly chased and of beautiful form. It was manufactured by Mr. G. Lewison, of Royal Street, and had engraved on it the following inscription :—

"Presented to His Royal Highness Alfred Ernest Albert, Duke of Edinburgh, K.G., K.T., G.C.S.I., R.N., by the Meteorological Society of Mauritius, at the laying of the Foundation Stone of a new Meteorological and Magnetical Observatory. Mauritius, 30th May, 1870."

The Secretary, Mr. Meldrum, then addressed his Royal Highness as follows :—

"May it please your Royal Highness.—On behalf of the Meteorological Society, and as Government Observer, I have the honour to thank your Royal Highness for having been graciously pleased to lay the Foundation Stone of the Mauritius New Observatory.

"I am sure I only give expression to the feelings with which the Society is animated, when I say that it will ever retain a most grateful sense of the generous sympathy and consideration which have induced your Royal Highness to come here, to-day, at personal inconvenience, to perform the interesting and important ceremony which the Society has had the extreme gratification to witness.

"If anything could enhance the pleasure which the Society now feels, it is the presence, on this auspicious occasion, of his Excellency, Sir Henry Barkly, who, during a long and an arduous administration, has not ceased to take a warm interest in the Society's objects, and to whom will be mainly due the merit of establishing in this distant Colony an Observatory which, I hope, will be second to none of the same nature in other portions of Her Majesty's dominions.

"Engaged for the most part in agricultural and commercial pursuits, but yet dependent for the necessaries of life on importations from other countries, and surrounded by a tempestuous ocean, the people of Mauritius, deeply interested in the progress of meteorological science, and many of them actively occupied in promoting it, will, I have no doubt, long preserve a fond recollection of the part which your Royal Highness has been kind enough to take in this day's proceedings.

"But the labours to be carried on here will be not merely of local utility. I trust they will also contribute to the advancement of meteorology and of terrestrial magnetism generally, as well as of certain branches of physical astronomy. In this respect their chief practical aim will be to render service to the noble profession of which your Royal Highness is so distinguished a member; and next to the pleasure of contemplating the works of the great Author of Nature, I know no stronger incentive to perseverance than the circumstance that the building about to be raised on this solitary spot, in the heart of the Indian Ocean, for the special object of conducting researches calculated to be of use to the maritime nations of the world, will in all future time be associated with the cherished name of England's sailor Prince."

His Excellency the Governor briefly addressed the audience. His Excellency said that he had always, as Mr. Meldrum remarked, taken much interest in the Society, and had done all in his power to promote its objects. It gave him great pleasure that the foundation stone of a new observatory had been laid before his departure from the colony. The ceremony could not have been more appropriately or more gracefully performed than by his Royal Highness, who was not only the second son of Her Majesty the Queen, but also a distinguished naval officer. His Excellency concluded amid general applause, by heartily wishing every possible success to what he would propose to call the Royal Alfred Observatory. Thus terminated these interesting proceedings.

SCIENTIFIC SERIALS

Journal of the Chemical Society, June, 1870.—Messrs. T. Bolas and C. E. Groves give a description of the mode of preparation and the properties of tetrabromide of carbon, the discovery of which they had announced in the preceding number of the Journal. Bisulphide of carbon was digested with an excess of bromine in the presence of terbromide of antimony or of bromide of iodine in a sealed tube of 150° for 48 hours. The bromide can also be obtained from bromopicrin and bromoform by treatment with the same reagents. Tetrabromide of carbon crystallises in white lustrous plates, fusible at 91°, nearly insoluble in water, soluble in alcohol, ether, benzol, and bisulphide of carbon, decomposed by aqueous solutions of potassa and soda at 150°. With care it may be sublimed without decomposition. By reduction by nascent hydrogen it produces bromoform and dibromide of methylene. The authors intend to study the action of argentic oxalate, cyanide, &c., on this interesting compound. Prof. A. H. Church continues his researches on new and rare Cornish minerals, giving the analysis of restormelite, which appears to be kaolinite $Al_2O_3 2SiO_2$, 2Aq, in which some of the hydrogen is replaced by potassium and sodium and a portion of the aluminium by iron. The specific gravity is 2·58, and the hardness about 2. Chalcophillite contains 8 CuO, Al_2O_3, As_2O_5, 24 or 25 Aq., it loses 13·79 per cent. of water *in vacuo*, corresponding to 11 H_2O; the specific gravity is 2·44. This number also contains the commencement of a very long and elaborate paper on the combinations of carbonic anhydride with ammonia and water, by Dr. E. Divers. The author gives a history of the different compounds which he has examined, and describes no less than nine processes for the preparation of normal ammonium carbonate $CO_2 (OH_2)_2 (NH_2)_2$. Its properties and reactions are also fully given. In the second section, only a portion of which appears in this number, the preparations and properties of the half acid ammonium carbonate are detailed.

THE *Revue des Cours Scientifiques* for June 18, is almost entirely occupied with a translation of Prof. Huxley's address before the Geological Society on the course of palæontology during the last eight years. M. Bernard also proceeds with his course of lectures on suffocation by the fumes of charcoal, which is again continued in the following number, where we find also a paper read before the Congress of German Naturalists and Physicians at Innsbruck by Prof. Kékulé, on chemical work, and a review by Prof. Duclaux, of Pasteur's Researches on the Silkworm Disease. In the number for July 2, is a very important article by Prof. Agassiz on the Gulf Stream, being a report of the dredgings from the bottom of the Gulf Stream, made during the third expedition of the U.S. steamer *Bibb*. Prof. Agassiz believes that in the cretaceous period a current set in in the Atlantic from the north-east to the south-west, North and South America being then distinct continents, and that it was only at a subsequent period that communication between the Atlantic and Pacific became interrupted, and that the Gulf Stream set in in the opposite direction. In the same number is a continuation of M. Berthelot's paper on isomeric states of simple substances, treating especially of sulphur.

SOCIETIES AND ACADEMIES

LONDON

London Mathematical Society, June 9.—Prof. Cayley President, in the chair. The Hon. Sir James Cockle was elected a member. Mr. Spottiswoode, V.P., having taken the chair, the president communicated the following "Note on the Cartesian, with two imaginary axial foci." Let A, A′, B, B′, be a pair of points and antipoints; viz., A, A′, the two imaginary points, co-ords ($\pm \beta i$, 0); B, B′, the two real points, co-ords (0, $\pm \beta$), and write ρ, ρ', σ, σ', the distances of a point (x, y) from the four points respectively, say

$$\rho = \sqrt{(x + \beta i)^2 + y^2} \qquad \rho' = \sqrt{(x - \beta i)^2 + y^2}$$

$$\sigma = \sqrt{x^2 + (y + \beta)^2} \qquad \sigma' = \sqrt{x^2 + (y - \beta)^2}$$

we have

$$\rho^2 + \rho'^2 = 2(x^2 + y^2) - 2\beta^2$$
$$= \sigma^2 + \sigma'^2 - 4\beta^2$$

$$\rho\rho' = \sqrt{(x + \beta i + yi)(x + \beta i - yi)(x - \beta i + yi)(x - \beta i - yi)}$$
$$= \sigma\sigma'$$

and thence

$$(\rho + \rho')^2 = (\sigma \times \sigma')^2 - 4\beta^2$$
$$(\rho - \rho')^2 = (\sigma - \sigma')^2 - 4\beta^2$$

or say

$$\rho + \rho' = \sqrt{(\sigma + \sigma')^2 - 4\beta^2}$$
$$i(\rho - \rho') = \sqrt{4\beta^2 - (\sigma - \sigma')^2}$$

The equation of a Cartesian having the two imaginary axial foci, A, A′, is

$$(p + qi)\rho + (p - qi)\rho' + 2k^2 = 0$$

viz., this is,

$$p(\rho + \rho') + qi(\rho - \rho') + 2k^2 = 0$$

or what is the same thing, it is

$$p\sqrt{(\sigma + \sigma')^2 - 4\beta^2} + q\sqrt{4\beta^2 - (\sigma - \sigma')^2} + 2k^2 = 0$$

which is the equation expressed in terms of the distances σ, σ', from the non-axial real foci, B, B′. The radicals are to be taken with the signs $\pm$. This equation gives, however, the Cartesian in combination with an equal curve situate symmetrically therewith in regard to the axis of y.

The distance σ, σ' may conveniently be expressed in terms of a single variable parameter θ; in fact, we may write

$$\pm p\sqrt{(\sigma + \sigma')^2 - 4\beta^2} = -k^2 - k\theta$$
$$\pm q\sqrt{4\beta^2 - (\sigma - \sigma')^2} = -k^2 + k\theta$$

that is

$$\sigma + \sigma' = \sqrt{4\beta^2 + \frac{k^2}{p^2}(k+\theta)^2}$$

$$\sigma - \sigma' = \pm\sqrt{4\beta^2 - \frac{k^2}{q^2}(k-\theta)^2}$$

So that assigning to θ any given value, we have σ, σ', and thence the position of the point on the curve. We may draw the hyperbola $y^2 = 4\beta^2 + \frac{k^2}{p^2}x^2$ and the ellipse $y^2 = 4\beta^2 - \frac{k^2}{q^2}x^2$, and then measuring off in these two curves respectively the ordinates which belong to the abscissæ, $k+\theta$ for the hyperbola, $k-\theta$ for the ellipse, we have the values $\sigma + \sigma'$ and $\sigma - \sigma'$, which determine the point on the curve. Considering k, p, q, β as disposable quantities, the conics may be any ellipse and hyperbola whatever, having a pair of vertices in common. The author then proceeded to draw the curve and point out some of its peculiarities.

Mr. T. Cotterill then gave an abstract of his paper "On the intersection of curves, and a collinear correspondence in certain réseaus," going into the discussion of the following theorem :—If the assemblage of all the points of intersection of two plane curves be said to form an "intersect," and a series of groups of points, called the first, second, third, &c., be taken on a plane curve of an order > 2, such that the points in every two successive groups form an *Intersect*; then the points in any odd and any even group, or in any number of odd and an equal number of even groups, also form an *Intersect*. Amongst other applications of this theorem, certain exceptions were pointed out in the laws laid down concerning the intersection of curves. Thus a system of 13 points can be found such that a quartic through 12 of the points must pass through the 13th, whilst the quartics through the 13 points do not intersect again in three fixed points, but any such quartic is cut by any other quartic of the system in three points collinear with a fixed point on the first quartic. Hence arises a correspondence of three collinear points similar to Geiser's correspondence in cubics through seven given points. The details of the correspondence, including certain envelopes given in the paper, refer to curves of any order.

Syro-Egyptian Society, June 7.—Mr. W. H. Black in the chair. Mr. Robert Hay, of Limplum, exhibited a large and unusually fine collection of Egyptian Antiquities, the property of his late father, author of "Views in Cairo," by whom they were collected between the years 1828-33. They consist chiefly of five classes of objects. Bronzes, terra cottas, vases, funereal remains, and amulets in precious stones, and porcelain, besides six large mummies with their outer cases more or less complete. Mr. Bonomi, who gave an explanatory lecture on the various antiquities, pointed out the following objects as being noteworthy either from their rarity or beauty.

1. A large and fine bronze figure of Khonso, twenty-six inches high (No. 32), partly overlaid with antimony, and enamelled with a white opaque pigment, to represent the mysterious garment of that divinity. The head, hands, and collar had been gilt, and the eyes, staff, and mens inserted, but these latter had been lost before it was obtained by its late owner. The workmanship of the statue is of the best Theban art, and is an admirable illustration of the great chemical knowledge possessed by the ancient Egyptians.

2. A small terra cotta statue of a Royal Scribe (706), habited in the costume of the nineteenth dynasty. The seed bag hangs over the right shoulder instead of the left as usual, and the crossed hands clasp on his breast a figure of a hawk with human head and expanded wings. This bird symbolised the soul in Egyptian theroglyphy, and a like figure is used in Psalm xi. 1. Unfortunately this statue has been much injured. A second and more perfect example in limestone (No. 159) was also exhibited. Both rare specimens.

3. Four statues, one in bronze, one in stone, and two in porcelain (Nos. 12, 116, 846), of the Great Theban Divinity, Amun Ra. These figures are of rare occurrence in any material, as about the reign of Amenophis III., the worship of Amun Ra was overthrown, and his name sedulously erased from all inscriptions throughout Egypt, even upon the top of the loftiest obelisks. In a later period, that of Ramesis II., however, the worship of this divinity was resumed, and his name again appears on the monuments.

4. A porcelain amulet (894), representing a human-headed scorpion, having the tail turned up over the back. The head of this figure was originally surmounted by a crown or head-dress. Exquisite finish, probably unique.

5. A wooden tablet (539) of the kind described by some writers as astronomical, but more probably a votive stete dedicated to Homs, and intended as a charm or mystical talisman against accidents, the evil eye, and malign influences generally.

6. Several staves (572) in hard wood having a projecting spur or branch at the top. These staves were carried by the priests in funeral processions, and for some unknown reason it was necessary that they should be furnished with a short projection at a certain angle near the upper extremity, for if this were wanting in this natural stick it was supplied by art; such a specimen so furnished (573) was also exhibited.

7. A small bronze figure of the Goddess Pasht holding a shield in her right hand, with the left extended.—The preceding formed only a part of the objects successively noticed by the lecturer. Many domestic articles, models, lucernæ and personal trinkets remained, which from the lateness of the hour could not further be commented upon. The explanations of Mr. Bonomi were afterwards supplemented by various remarks from Messrs. Cooper, Cull, Hewlett Ross, Williams, Mills, Christie, Drach, W. H. Black, and other gentlemen, and it was further announced by the chairman (Mr. W. H. Black) that a special meeting would be convened for the first Tuesday in July, when the drawings, MSS., and papyri of the late Mr. Hay would be exhibited, and also a collection of photographs lately sent from Syria by Dr. Cull, in illustration of the same. It may be gratifying to our antiquarian friends to know that by special permission of the Council of the S.-E.S., the collection may be viewed during this present month at their rooms, No. 22, Hart Street, W.C., between the hours of eleven and four, where all particulars respecting the destination of the antiquities may be obtained.

DUBLIN

Royal Irish Academy, June 13.—Rev. Professor Jellett, F.T.C.D., president, in the chair. The minutes of the previous meeting were read and confirmed. Dr. Sigerson, F.L.S., read a paper entitled "Further Researches on the Atmosphere." He showed that his former discoveries had been confirmed by later investigations, and proceeded to detail the results of his examination of "special atmospheres." His lecture was illustrated with large diagrams representing some of the objects discovered. In "iron factory" air he found carbon, ash, and iron. The iron was discovered to be hollow balls averaging one two-thousandth part of an inch in diameter, their shell being only one thirty-thousandth, and the iron was then found to be translucid. In "shirt factory" air there were found to be filaments of linen and cotton with minute eggs. The air of thrashing and oat-mills had fibres, fragments, starch grains, and spores. Scutch mills, from the character and quantity of the spongy, spiky dust and its effects, he declared to be human slaughter-houses. By a suggested alteration, the works could be kept free of the dust, and many lives saved. In the air of printing offices antimony was expected, and Dr Sullivan's analysis confirmed the expectation. It is, probably, present to an injurious extent in type-foundries. Stable air was shown to contain scales and hairs, and hair-dressers had a similar atmosphere. Smoke being microscopically examined, the tobacco-smoker's air was shown to contain globules of nicotine. He concluded by stating conclusions arrived at with regard to lung-functions and contagion. The results of Dr. Sigerson's investigations proved, as might be expected, that the air in special workshops contained floating in it particles of the *débris* of the different materials operated on in these particular localities. Some discussion followed, at the conclusion of which, on the motion of Prof. Sullivan, the paper was referred to the Council for publication. John P. Keane, C.E., Calcutta, and Hugh Leonard, Kylemore, were elected members of the Academy.

PARIS

Academy of Sciences, June 20.—M. R. Clausius presented a note on a quantity analogous to the potential and on a theorem relating to it. M. Bertrand communicated a note by M. G. Darroux on the surface of the centres of curvature of an algebraic surface.—A memoir was read by M. Maurice Lévy on the general equations of the internal movements of ductile solid bodies beyond the limits at which elasticity can restore them to their original state.—The following papers on subjects connected with physics were presented to the meeting:—On the variations of temperature produced by the mixture of two liquids, by M. Jamin, upon which M. Bussey made some remarks.—On the electrical effects produced by the contact of inoxidisable metals with acids and neutral and saturated saline solutions, and on capillary affinities, by M. Becquerel. The author describes the results obtained with wires of pure platinum and gold, treated as described in a former communication, but immersed in acid or saline fluids.—Determination of the terrestrial magnetic intensity in absolute value, by MM. A. Cornu and J. Baille, presented by M. E. Becquerel, in which the authors describe a series of experiments made in accordance with the methods of Gauss and Weber.—Experimental researches upon the duration of the electric spark, by MM. Lucas and Cazin, also presented by M. E. Becquerel.—On the law of the points of congelation of saline solutions, by M. Guldberg.—A note by M. G. Rayet on the reversal of the two sodium lines in the spectrum of the light of a solar protuberance was presented by M. Delaunay.—M. Periseux communicated a note on the transit of Venus in 1874, containing a numerical correction of his former paper on this subject.—M. Delaunay presented a note by M. H. Tarry on the so-called showers of dust and blood. The author notices the storm phenomena preceding the showers of sand which fell in the south of Europe on the 10th and 24th March, 1869, and the 14th February, 1870, indicating that in each case a great barometric depression, accompanied by a violent wind storm, travelled from the north to the south of Europe, crossing into Africa, and returning thence laden with sand from the Sahara. He identifies the material deposited in these storms in all cases with the Sahara sand.—M. C. Dufour read a note relating to magnetic perturbations observed by Saussure at the Col du Géant before the great storm of 1788. M. H. Sainte Claire Deville communicated a note by M. C. Schlœsing on the precipitation of mud by very dilute alkaline solutions. The author stated that a very small quantity of a saline solution facilitates the deposition by water of earthy matter held in suspension by it, and that a deficiency of saline constituents is often the cause of water remaining muddy when standing in clearing tanks.—A note by M. Scheurer Kestner on the composition of crude soda and the loss of sodium caused by the adoption of Le Blanc's process was presented by M. Balard. The author noticed the chief compounds with which crude soda is contaminated, and stated as the result of his researches upon Le Blanc's method that in the fusion of crude soda there is no reduction of soda-salts into metallic sodium, and that the greater part of the loss experienced is due to the formation of insoluble compounds, and averages about 5 per cent.—M. Bertrand communicated a note by M. Rabuteau, on a new, simple, and rapid mode of quantitative determination of the ammoniacal salts, and on the reason why these salts can exist normally in the organism only in infinitesimal quantities. The author stated that chloride of soda prepared by pouring a solution of two parts of carbonate of soda into one part of chloride of lime contains an excess of carbonate and free soda, and that, by the aid of heat, this solution decomposes ammoniacal salts with evolution of nitrogen. From the amount of nitrogen evolved, the quantity of ammonia may be calculated. He considered that the alkalinity of the blood would enable it in like manner to decompose any ammoniacal salts contained in it.—A note on the tribromhydrines, by M. Berthelot, was presented by M. Bertrand, in which the author maintained the isomerism of those compounds in opposition to the opinion of M. Henry.—M. H. Sainte-Claire Deville communicated a note by M. Fontaine on the preparation of bibrominated ethylene, in which the author described a new method of obtaining that compound.—M. Wurtz presented a note on an aromatic glycol, by M. E. Grimaux.—An extract from a letter from M. Pasteur to Marshal Vaillant, describing the results obtained in breeding French races of silkworms, at Villa-Vicentina, was read.—M. C. Robin presented a note by M. Picot containing the results of some experimental researches upon suppurative inflammation and the passage of leucocytes through the walls of vessels.

June 27.—M. Serret presented a note by M. R. Hoppe on a corollary to a theorem of Mr. Crofton's; and a note by M. F. Lucas on some new properties of the potential function was communicated by M. Delaunay.—M. d'Abbadie presented a note by M. J. Hoüel on the selection of the angular unit, containing a further discussion of the question of the decimal division of the quadrant or the circle, and supporting the former as the unit. M. Yvon Villarceau remarked upon this paper, and maintained that the decimal division of the circle is preferable to that of the quadrant.—M. H. Sainte-Claire Deville read a paper entitled "Observations with regard to a Note by M. Jamin on the Variations of Temperature produced by the mixture of two Liquids."—A note by M. J. Chautard on the direction of the currents induced by means of electrical discharges was read.—A letter from Mr. C. K. Akin, claiming priority in the method of calorimetry employed by M. Jamin and ascribed to M. Pfaundler, was read; and M. Neyreneuf presented a note on the phenomena of electrical condensation.—A paper by M. Martin de Brettes on the determination of the thickness of iron casing that can be traversed by a projectile of which the weight, the calibre, and the striking velocity are known, was read, indicating the formulas to be employed, and giving a table of experimental results contrasted with those obtained by calculation.—A paper by Mr. J. N. Lockyer on the last eclipse of the sun observed in the United States was read.—The following papers on subjects connected with chemistry was communicated: —Investigations on some new derivations of triæthylphosphine, by MM. A. Cahours and H. Gal; letters by M. H. Bouilhet and Klein on the deposition of nickel by galvanism, presented by M. Dumas; on a new method of preparation of the chlorobrominated organic compounds by M. Henry; on silicopropionic acid by MM. Friedel and Ladenburg, upon which MM. Dumas and Thenard made some remarks; and a note on phospho-platinic compounds by M. Schützenberger.—A note by M. Montagna was read noticing the occurrence of organic remains in rocks regarded as of igneous origin; and M. de Vemeuil made some remarks on a memoir by M. Dieulafait on the zone of *Avicula contorta*, and the Infralias in the south and south-east of France. —M. de Clos communicated a note on the germination (or twin growth) of the whorls of floral axes in the Alismaceæ.—The remaining papers read need no notice, except one, of which unfortunately the title only is given us, in which M. Tremblay suggested *a means of terminating the present drought.*

BOOKS RECEIVED

ENGLISH.—The Revival of Philosophy at Cambridge: C. M. Ingleby (Hall and Son).

FOREIGN.—Annales de Chimie et de Physique, No. XX.—Annalen der Physique und Chemie, No. 5, Repertorium, Heft iv.—(Through Williams and Norgate)—Traite d' Histoire naturelle: M. A. T. Nogues.—Jahresbericht uber die Leistungen der chemischen Technologie fur 1869: J. A. Wagner.—Prodromus Floræ Hispanicæ, Vol. I.—Etudes sur les Diatomacées: Ch. Manourry.

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