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.

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

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

* 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.