

ADDRESS delivered by the Rev. M. J. BERKELEY, *President of the Biological Section of the BRITISH ASSOCIATION, at the Meeting held in Norwich, September, 1868.*

FEW points are of greater significance than those which touch upon the intimate connection of animal and vegetable life. Fresh matter is constantly turning up, most clearly indicating that there are organisms in the vegetable kingdom which cannot be distinguished from animals. The curious observations which showed that the protoplasm of the spores of *Botrytis infestans* (the potato mould) is at times differentiated, and ultimately resolved into active flagelliferous zoospores, quite undistinguishable from certain infusoria, have met their parallel in a memoir lately published by MM. Famintzin and Boranetzky, respecting a similar differentiation in the gonidia of lichens belonging to the genera *Physcia* and *Cladonia*. It is, however, only certain of the gonidia which are so circumstanced; the contents of others simply divide into motionless globules.

A still more curious fact, if true, is that described by De Bary, after Cienkowsky, in the division of fungi known under the name of *Myxogastres* or false puff-balls. Their spores, when germinating, in certain cases give rise to a body not distinguishable from *Amœba*, though in others the more ordinary mode of germination prevails. In the first instance De Bary pronounced these productions to belong to the animal kingdom, so striking was the resemblance; but in our judgment he exercised a wise discretion in comprising them amongst vegetables in a late volume of Hofmeister's 'Handbuch.'

The point, however, to which I wish to draw your attention, and one of great interest if ultimately confirmed, is that the gelatinous mass produced either independently, or by the blending of these amœboid bodies, is increased, after the manner of true *Amœbæ*, by deriving nourishment from different organisms involved by accident from the extension of the pseudopodia. These foreign bodies, according to our author, behave themselves precisely after the same manner as those enclosed accidentally in undoubted animals. If this be true, it shows a still more intimate connection, or even identity of animals and vegetables than any other fact with which I am acquainted.

You are all doubtless aware of the important part which minute fungi bear in the process of fermentation. A very

curious contribution to our information on cognate matters has lately been published by Van Tieghem, in which he shows that tannin is converted into gallic acid by the agency of the mycelium of a species of *Aspergillus*, to which he has given the name of *Aspergillus niger*. The paper will be found in a late number of the 'Annales des Sciences Naturelles,' and is well worth reading.

We now come to a subject which is at present of much importance, viz. the theory of Hallier respecting the origin of certain diseases. His observations were at first confined to Asiatic cholera, but he has since made a communication to the authorities of the medical department of the Privy Council office to the effect that, in six other diseases—typhus, typhoid, and measles (in the blood), variola, variola ovina, and vaccinia (in the exanthemes)—he has found certain minute particles which he calls micrococci, which under culture experiments give, for each of the above-mentioned diseases, a constant and characteristic fungus. He states that in variola he gets the hitherto unknown pycnidia of *Eurotium herbariorum*; in vaccinia, *Aspergillus glaucus*, Lk.; in measles, the true *Mucor mucedo* of Fresenius; in typhus, *Rhizopus nigricans*, Ehrenberg; and in typhoid, *Penicillium crustaceum*, Fries. He adds that the culture experiments, especially with the variola diseases, have been so very numerous as to exclude from the results all supposition of accident—that different districts, different epidemics, and different times have given identical results. I am anxious to say a few words about the subject, because most of the reports which have been published in our medical journals give too much weight, in my opinion, to his observations, as though the matter had been brought to a logical conclusion, which is far from being the case. I am happy to say that it has been taken up by De Bary, who is so well calculated to give something like a conclusive answer to the question, and also that it has been taken in hand by the medical authorities of our army, who are about to send out two of their most promising young officers, perfectly unprejudiced, who will be in close communication, both with De Bary and Hallier, so as to make themselves perfect masters of their views, and to investigate afterwards the subject for themselves.

The fault, as I conceive, of Hallier's treatise is that, while his mode of investigation is unsatisfactory, he jumps far too rapidly to his conclusions. It is quite possible that certain fungi may occur constantly in substances of a certain chemical or molecular constitution, but this may be merely a case of effect instead of cause. Besides, as I conceive, the only

safe way of ascertaining what really originates from such bodies as those which he terms micrococci, or the larger ones commonly called yeast globules, is to isolate one or two in a closed cell so constructed that a pellicle of air, if I may so term it, surrounds the globule of fluid containing the bodies in question, into which they may send out their proper fruit—a method which was successful in the case of yeast, which consists of more than one fungus, and of the little *Sclerotium*, like grains of gunpowder, which is so common on onions. Any one who follows the growth of moulds on moist substances, and at different depths, as paste of wheat or rice flour, will see that numberless different modifications are assumed in different parts of the matrix, without, however, a perfect identification with fungi of other genera. Some of these will be seen in the figures I have given in the 'Intellectual Observer,' Nov., 1862, and 'Journal of Linnean Society,' vol. viii, No. 31, of different forms assumed by the moulds to which that formidable disease, the fungus foot of India, owes its origin. This is quite a different order of facts, from the several conditions assumed by the conidiiferous state of some of the vesiculiferous moulds. As, for example, *Botrytis Jonesii*, which has been ascertained to be a conidiiferous state of *Mucor mucedo*, while two forms of fruit occur of the same mould in what is called *Ascophora elegans*, or the still more marvellous modification which some of the *Mucors* undergo when grown in water, as evinced by some of the *Saprolegniæ*, the connection of which was indicated by Carus some fifty years ago, but which has never been fully investigated.

When Hallier intimates that he has raised from cholera evacuations such a parasite as *Urocystis occulta*, he should have been content with stating that a form of fructification occurred resembling, but not identical with, that fungus. Indeed, a comparison with authentic specimens of that species, published by Rabenhorst, under the generic name of *Ustilago*, shows that it is something very different, and yet the notion of cholera being derived from some parasite on the rice plant rests very much on the occurrence of this form. But even supposing that some *Urocystis* (or *Polycystis*, as the genus is more commonly named) was produced from cholera evacuations, there is not a particle of evidence to connect this with the rice plant. In the enormous collections transmitted by Dr. Curtis from the Southern United States, amounting to 7000 specimens, there is not a single specimen of rice with any endophytic fungus, and it is the same with collections from the East. Mr. Thwaites has made

very diligent search, and employed others in collecting any fungi which may occur on rice, and has found nothing more than a small superficial fungus nearly allied to *Cladosporium herbarum*, sullyng the glumes exactly as that cosmopolitan mould stains our cereals in damp weather. Rice is occasionally ergoted, but I can find no other trace of fungi on the grains. Again, when he talks of *Tilletia*, or the wheat bunt, being derived from the East—supposing wheat to be a plant of Eastern origin, there is no evidence to bear out the assertion, as it occurs on various European grasses; and there is a distinct species which preys on wheat in North Carolina, which is totally unknown in the Old World.

I might enter further into the matter, were it advisable to do so at the present moment. All I wish, however, is to give a caution against admitting his facts too implicitly, especially as somewhat similar views respecting disease have lately reached us from America, and have become familiar from gaining admittance into a journal of such wide circulation as 'All the Year Round,' where Hallier's views are noticed as if his deductions were perfectly logical.

The functions of spiral vessels, or of vascular tissue in general, have long been a subject of much controversy, and few matters are of more consequence as regards the real history of the distribution of sap in plants. A very able paper on the subject, to which allusion was made by Dr. Hooker in his address, has been published by Mr. Herbert Spencer (than whom few enter more profoundly into questions of physiology) in the 'Transactions of the Linnean Society.' By a line of close argument and observation he shows, from experiments with coloured fluids capable of entering the tissues without impairing vitality, and that not only in cuttings of plants, but in individuals in which the roots were uninjured, that the sap not only ascends by the vascular tissue, but that the same tissue acts in its turn as an absorbent, returning and distributing the sap which has been modified in the leaves. That this tissue acts some important part is clear from the constancy with which it is produced at a very early stage in adventitious buds, establishing a connection between the tissues of the old and new parts. This appears also from the manner in which in true parasites a connection is established between the vascular tissue of the matrix and its parasite, as shown by our president in his masterly treatise on *Balanophoræ*, and more recently by Solms-Laubach in an elaborate memoir in 'Pringsheim's Journal.' It is curious that in organs so closely analogous to the tracheæ of insects a similar connection

should long since have been pointed out by Mr. Newport, in the case of certain insect parasites.

A circumstance, again, which constantly occurs in the diseases of plants confirms the views of Mr. Herbert Spencer. In diseased turnips, grapes, potatoes, &c., it is especially the vascular tissue which is first gorged with the ulmates which are so characteristic of disease.

Monsieur Casimir de Candolle, in a clever memoir on the morphology of leaves, has come to the conclusion, after studying the arrangement of their vascular tissue, that they are branches in which the side towards the axis, which he calls the posterior, is atrophied. This subject has been followed out in those organs which are considered as modifications of leaves, as, for example, stamens, in which he finds sometimes the posterior side, sometimes the anterior, atrophied. If his theory is true, this would result from the way in which they originated, and the reference they bore to contiguous organs. The subject is well worth attention, and may eventually throw considerable light on those anomalous cases in teratology which will not accommodate themselves to the usual theory of metamorphosis. Some of these cases are so puzzling and complicated, that a very clever botanist once told me, "Monstrous flowers teach us nothing,"—not meaning to abjure all assistance from them, but simply to indicate that they may be deceptive. Such flowers as double primroses, and the strange developments on the corollas of some gloxinias, may possibly receive their explanation from a careful study of the course of the vascular tissue. As the colour on the anterior and posterior order in the latter case is reversed, the doctrine of "dedoublement" does not at all help us.

Hofmeister, in his 'Handbuch der Physiologischen Botanik,' has an important chapter on free-cell formation, which at the present moment is of great interest as connected with Mr. Darwin's doctrine of Pangenesis. Mr. Rainey has shown that the formation of false cells takes place in solutions of gum and other substances; and if this is the case where no vital agency is concerned, we may well be prepared for the formation of living cells in organizable lymph, or in other properly constituted matter. The curious cell-formation of gum tragacanth may be an intermediate case. Be this, however, as it may, we have examples of free-cell formation in the formation of nuclei, in the embryos of plants, and above all in the asci of ascomycetous fungi. In plants whose cells contain nuclei new cells are never formed without the

formation of new nuclei, the number of which exactly corresponds with that of the new cells.

It would be unpardonable to finish these somewhat desultory remarks without adverting to one of the most interesting subjects of the day,—the Darwinian doctrine of Pangenesis. After the lucid manner, however, in which this doctrine was explained by Dr. Hooker in his opening address, I should be inclined to admit it altogether had I not looked at it from a somewhat different point of view, so that I should not be trespassing upon your time in going over the same ground. Others, indeed, as Owen and Herbert Spencer, have broached something of the kind, but not to such an extent, for the Darwinian theory includes atavism, reversion, and inheritance, and embraces mental peculiarities as well as physical. The whole matter is at once so complicated, and the theory so startling, that the mind at first naturally shrinks from the reception of so bold a statement. Like everything, however, which comes from the pen of a writer whom I have no hesitation, so far as my own judgment goes, in considering by far the greatest observer of our age, whatever may be thought of his theories when carried out to their extreme results, the subject demands a careful and impartial consideration. Like the doctrine of natural selection, it is sure to modify, more or less, our modes of thought. Even supposing the theory unsound, it is to be observed, as Whewell remarks, as quoted by our author, "Hypotheses may often be of service to science when they involve a certain portion of incompleteness, and even of error." Mr. Darwin says himself that he has not made histology an especial branch of study, and I have therefore less hesitation, though "*impar congressus Achilli*," in expressing an individual opinion that he has laid too much stress on free-cell formation, which is rather the exception than the rule. Assuming the general truth of the theory, that molecules endowed with certain attributes are cast off by the component cells of such infinitesimal minuteness as to be capable of circulating with the fluids, and in the end to be present in the unimpregnated embryo cell and spermatozoid, capable of either lying dormant or inactive for a time, or, when present in sufficient potency, of producing certain definite effects, it seems to me far more probable that they should be capable under favorable circumstances of exercising an influence analogous to that which is exercised by the contents of the pollen tube or spermatozoid on the embryo sac or ovum, than that these particles should be themselves developed into cells; and under some such modification I conceive that the theory is

far more likely to meet with anything like a general acceptance. Be this, however, as it may, its comprehensiveness will still remain the same. We must still take it as a compendium of an enormous mass of facts, comprised in the most marvellous manner within an extremely narrow compass.

I shall venture to offer a very few words in conclusion, which, perhaps, may be thought to have too theological an aspect for the present occasion.

It is obvious how open such a theory is to the charge of materialism. It is an undoubted fact, however, that mental peculiarities and endowments, together with mere habits, are handed down and subject to the same laws of reversion, atavism, and inheritance, as mere structural accidents, and there must be some reason for one class of facts as well as the other; and whatever the explanation may be, the hand of God is equally visible and equally essential in all. We cannot now refer every indication of thought and reasoning beyond the pale of humanity to blind instinct, as was once the fashion, from a fear of the inferences which might be made. Should any one, however, be still afraid of any theory like that before us, I would suggest that man is represented in Scripture as differing from the other members of the animal world, by possessing a spirit as well as a reasoning mind. The distinction between *ψυχη* and *πνευμα*, which is recognised by the Germans in their familiar words *seele* and *geist*, but which we have no words in our language\* to express properly, or in other terms between mere mental powers which the rest of the creation possess in greater or less degree in common with ourselves, and an immortal spirit, if rightly weighed, will, perhaps, lead some to look upon the matter with less fear and prejudice. Nothing can be more unfair, and I may add unwise, than to stamp at once this and cognate speculations with the charge of irreligion. Of this, however, I feel assured, that the members of this Association will conclude with me in bidding this great and conscientious author God-speed, and join in expressing a hope that his health may be preserved to enrich science with the results of his great powers of mind and unwearied observation.

\* A proof of this poverty of language is visible in the words used in our translation for *ψυχικον* and *πνευματικον*—natural and spiritual, their proper meaning, taken in connection with *σωμα*, being a body with a soul, and a body with a spirit.