MICROSCOPICAL INVESTIGATIONS IN CHOLERA.

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THE researches of Messrs. Brittan and Swayne will doubtless guide many more observers towards the same channels of investigation; and whatever may be the nature of the bodies so readily seen in the "rice-water" evacuations of cholera, this far is certain—the discoveries of those gentlemen are of the highest interest.

But the greatest caution is needful in determining the nature of cryptogamic organisms developed in fluid media, even when the materials are in practised hands, and more especially so when these investigations are attempted, perhaps, for the first time.

It is well to know that mycetoid forms are so very generally and abundantly, and also rapidly, propagated in any fluids containing organic matters, and have been exposed to the atmosphere, that mycologists lay little stress upon this circumstance. At this present time, mycology is but little studied without a small circle of botanists, and to most of the profession it is indeed an unknown land.

Were this not generally the case, toruloid filaments in organic fluids, and bacillariæ in water, would not be regarded as unusual phenomena.

It is from this cause, when an investigator finds in certain fluids fungoid or algoid structures new to him, he naturally is tempted to conclude such to be novelties to science likewise. I am inclined to think Mr. Grove has been led astray by this error. That gentleman considers the mucinous filaments and germinating cells found in the urine of a cholera patient, as identical with the bodies of the "rice-water" evacuations, and which bodies I will venture to denominate Brittanian corpuscles.

The figures given by Mr. Grove, although good and distinct, cannot be admitted as demonstrating anything of the kind; and the statement that they prove the vitality of the so-called cholera fungus by its germination, as far as published accounts go, is certainly not yet made manifest.

Many months since,—I think during the last winter,—my colleague, Dr. W. B. Herapath, gave me samples of albuminous urine, containing toruloid filaments in abundance, very like some of Mr. Grove's figures. I have seen similar in saccharine urine, and am strongly inclined to believe that all kinds of urine, healthy or otherwise, when decomposition has commenced, will be found abounding with these mycelia. I have under examination, at this present time, a phial of "rice-water," which contains tufted septate mycelia in multitudes. The fluid is several weeks old, and is now horribly foetid.

Mr. Grove's experiment of transplanting his fungus into healthy urine, and the readiness with which it is propagated by this plan, seems clearly to indicate there is nothing specific, so far as cholera is concerned, in the nature of these organisms. No measurement is given, but from the figures alone the submersed form of some kind of mucor seems to be indicated; yet mucinous filaments, propagated in fluids, are generally much larger in size than Mr. Grove's plants probably are; indeed, the aggregate of submersed mucor mycelia are usually visible to the naked eye.

Figures 2 and 3 are evidently mycelian. Figure 4 seems to indicate an attempt towards the production of fructification. This last figure is indeed very like, as far as structure goes, the submersed state of *Mucor clavatus* of Link, to which, if it were not for the probable difference of size, I should be inclined to refer Mr. Grove's plant.

Will that gentleman allow me to direct his attention to a highly interesting and important paper by the Rev. M. J. Berkeley, "On a Confervoid State of *Mucor Clavatus* Link," *Annals of Natural History*, vol. ii. October, 1837; he will there see how closely his own drawings, in outline, resemble those of Mr. Berkeley.

I would suggest the following experiment:—A little flour paste should be moistened with a portion of urine containing these filaments, and placed under a glass. Possibly the aerial or normal state of the plant might be developed, and all doubts concerning the nature of it removed.

In the long and learned paper of Dr. Stuart Wilkinson, a case is detailed, in which mycoderms were produced abundantly within the uterine cavity, certainly an odd locality.

I think it probable these productions are a masked form of some penicillium or monilia, originally inhabiting the dis-

charges collected about the labia and orifice of the vagina; and then propagated upwards along the vagina and through the os uteri.

We here find the circumstances known as favourable to the growth of such fungi—viz., organic matters in a decomposing state, moisture and atmospheric air; for the flaccid vulva of an aged woman, in many positions of the body, will readily allow the ingress of air into the vagina.

These circumstances considered, the singularity of the habitat of Dr. Wilkinson's mould is not so inexplicable as at first sight it appears. I have myself seen spore-like cells, in the greatest abundance, in the fluid contents of an ovarian cyst. Nor are mycelia rare in the digestive and respiratory cavities of various animals. See "*Des Vegetaux qui Croissent sur l'Homme et sur les Animaux Vivants*," Par M. C. H. Robin,"—a work which appears to be little known to English medical men.

A word on Mr. Busk's assertion, that the Brittanian corpuscles are identical with the spores of an uredo. It is just but scarcely possible, Mr. Busk may have found the spores of *tilletia* caries in cholera evacuations, inasmuch as these are often abundant in common sorts of bread, and have mistaken these for the bodies described by Messrs. Brittan and Swayne. The only fungus spores I am acquainted with, at all resembling the Brittanian bodies, are found in the hypogæous tribe, among the tuberacæ.

Thus far was written several weeks since. In the interim, another paper by Mr. Grove has been published, with a reference to a paradoxical growth of a fungus within the yolk of an egg, detailed in the before-mentioned monograph of Dr. Robin.

Even in this case, it is not absolutely necessary, in order to account for the introduction of the dactylum oogenum, that we assume the productive matter of the fungus to have entered the ovum through the general circulation of the fowl; but rather take this to be a case analogous to Dr. Wilkinson's, in which the plant passed through the apertures of the external organs. It is quite conceivable the spore of the dactylum might have been introduced into the cloaca of the bird, and during coitus carried through the oviducts, and then have gained entry into the ovum in its early state. There are difficulties of the same nature in the way in which entophthal fungi occur: doubtless all these apparent difficulties really consist in our imperfect knowledge and incomplete powers of investigation.

Bristol, December, 1849.

ON SECTION OF THE TENDO-ACHILLIS AS A REMEDY FOR SPASM OF THE MUSCLES OF THE LEG.

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FACTS in surgery and medicine are of two kinds. The first would illustrate occurrences exactly as they present themselves, the second would mould them to suit a theory. The first would be the beautiful delineations of an honest and observant mind, the second the *ad captandum* argument for personal advantage.

Surgery, like medicine, has much about it that is strictly empirical, not to say mechanical, with but a tinge of science. When we say "surgery," we but personify the subject, for we mean strictly some of its professors.

Muscles are excited to spasmodic contraction by the nervous power supplied to them, or they are not. If the former, the mechanical act of cutting through a tendon, to which the contracted muscles are inserted, is but a clumsy attempt to put a stop to a diseased action without interfering with the condition by which it was produced. If muscles be not excited to abnormal action by the power described, then must we unlearn all that we have been taught and seen, and Marshall Hall and his predecessors are but learned visionaries, whose eyes have been dimmed, and have seen nothing.

Those of my professional brethren who have seen practice in those districts where accidents are frequent and severe, must often have met with cases of fracture, and seen the usual accompaniment of violent spasm for the first night or two. But I will venture to assert, those surgeons, unless there have been something peculiarly severe and extraordinary in the nature of the accident, have not only cured the spasm without cutting through tendons, but have sent their patients abroad with more perfect and more immediately useful limbs than those who have so hastily resorted to section.