

The fungoid origin of disease, and spontaneous generation / by Jabez Hogg.

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Hogg, Jabez, 1817-1899.
Royal College of Surgeons of England

Publication/Creation

[London] : [Robert Hardwicke], [1871]

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THE MONTHLY

MICROSCOPICAL JOURNAL:

TRANSACTIONS

OF

The Royal Microscopical Society,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

All Microscopists who care to know what is going on at home or abroad should take in this Journal.

THE
MONTHLY MICROSCOPICAL JOURNAL

TRANSACTIONS OF

The Royal Microscopical Society,

AND RECORD OF HISTOLOGICAL RESEARCH.

Edited by HENRY LAWSON, M.D., F.R.M.S.,

ASSISTANT PHYSICIAN TO, AND LECTURER ON HISTOLOGY IN, ST. MARY'S HOSPITAL.

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ESTABLISHED JANUARY, 1869.

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LONDON: ROBERT HARDWICKE, 192, PICCADILLY, W.

VII.—*The Fungoid Origin of Disease, and Spontaneous Generation.*

By JABEZ HOGG, Hon. Sec. R.M.S.

IN the report of the medical officer of the Privy Council just issued, the origin and pathology of contagion is ably discussed, and the crude hypothesis of Hallier bearing upon this point, who, it will be remembered, sought to prove that the microzymes and sporules of fungi which he found in the fluids of persons affected with cholera caused the disease and explained its contagious nature, is finally disposed of. This vexed question, one of no small importance to the public, and of great interest for the medical profession, receives at the hands of Dr. Sanderson, the writer of this part of the report, all the care and attention it really deserves. His experiments and investigations fully bear out all I have stated on this subject, and conclusively show that neither bacteria nor microzymes are concerned in the production of any specific form of disease in the living animal body, and therefore when found must be looked upon as an indication of a putrefactive process occurring after death. A drop of water, a glass slide, or even a finger coming into contact with a fluid or tissue under examination, is quite sufficient to cause the development of either bacteria or microzymes, in an incredibly short space of time. In this way a disturbing element is introduced which mars and mystifies the most carefully made investigations of the histologist.

Admitting that the spores of fungi are always present in the atmosphere, although at some periods not in very great multitudes, it by no means follows, nor can it be shown that they are the cause of any specific form of disease. And, if it be true that so slight a contamination as that spoken of by Dr. Sanderson when brought into contact with a fluid is sufficient to change its character and start organic germs into life, then experiments said to prove that living matter can begin *de novo* in solutions subjected to long boiling must be accepted with extreme caution. For who can undertake to say with any degree of certainty that the breaking of a becker, in which a vacuum has been produced, can be conducted with sufficient care to prevent the possibility of a rush of air, carrying with it some organic particles, which shall contaminate or impregnate the whole? This, a point of the utmost importance, has not received much attention, although it is sufficient to embarrass and confound the results arrived at in the investigations of Dr. Bastian.

The ingenious way in which it is sought to explain experiments made by submitting a solution to a temperature of 160° F., alleged to be sufficient to destroy all evidence of life, while in another subjected to a much greater heat, ranging from 260° to 302° F.,

living creatures have reappeared, is by no means satisfactory. This admits of a different explanation, which will at once suggest itself to those who have thought over the phenomenon. Neither does it prove that because the elements of non-living matter are known to group themselves anew, so as to produce living matter under the influence of those physical forces which are concerned in bringing about the growth of a plant; that the same forces can be made to combine by long boiling to reproduce life or reconstruct the disintegrated particles of dead matter, and convert them into higher organisms than had previously existed. It seems to me impossible to attempt in this manner the settlement of a point of so much importance as that of the origin of life. And since we cannot undertake to say with anything like certainty that we have succeeded in destroying every living germ in any experiment we may institute, then, I fear, the spontaneous generation hypothesis is hardly worthy of further serious consideration. But with regard to Dr. Sanderson's investigations of certain contagious forms of disease, he produces positive evidence that nothing like bacteria or microzymes can be discovered in the blood of persons affected with scarlatina. This is an important and interesting fact, one very suggestive as to the cause of particular forms of disease, and seeming to lead to the conclusion that contagious affections are produced by a putrefactive change, a contamination introduced from without into the circulation.

From whatever stand-point we view the important question of contagion, or the origin of life, I am quite sure it will ultimately end in a gain to our scientific knowledge; and as every additional contribution will I am sure be acceptable, I shall offer no apology for introducing a very interesting letter, written by Henry J. Carter, F.R.S., some four or five years ago, as a criticism on a paper of mine which appeared in the '*Intellectual Observer*,' "*On Phases in the Developmental History of Infusorial Life*," a great portion of which is quite pertinent to the question under review at this moment.

BUDLEIGH SALTERTON, DEVONSHIRE, *March 14th, 1867.*

MY DEAR SIR,—I do not yet believe in spontaneous generation, nor will the theory, if ever substantiated, be so until a knowledge of the ultimate forms of the phenomena called "life" is obtained; while it seems to me that we are as far from this as from the ultimate atoms of matter.

When we see, under the microscope, insect forms almost as small as the smallest animalcules, and know, from inference, how complicated their structure must be; when we find their limbs as transparent as glass, and thus, apparently, as structureless, yet know that there is structure even in glass.

When we find that there is no extent to the slowness of change of form and movement in organized matter, that with the highest magni-

fying power possessed we can limit; that even unmelted iron is said to flow: when, on the other hand, the power of determining the velocity of bodies diminishes with the magnifying power, so that distance and magnitude itself are required to make us sensible of the rate at which comets travel, even if not of the presence of the atoms of matter *en masse* which form their nebulosities, so that neither one nor the other could be seen if close to us, any more than electricity or uncondensed steam.

When, I repeat, our perceptions in these respects remain so finite, how can anyone come forward with the assertion that there is such a thing as "spontaneous generation," based upon the presence of animalcules which, produced under any circumstances, may be, and probably are, far more complicated in their structure, and therefore higher in the scale of organic development, than a host of living beings with whose forms even we have as yet no means of becoming cognizant?

Progressive knowledge may lead the human mind to the beginning of vitality, to the quickening power of matter and its processes, but until this is reached, it seems to me premature to assume as a fact that there is such a power as spontaneous generation.

With reference to the next point in your paper, the transformation of the protoplasm of the vegetable cell into amœboid forms, who shall limit the extent to which such forms may not penetrate into and live passively in the protoplasm of both animal and vegetable cells, until a favourable opportunity arrives for their further development? I, of course, include in the amœboid forms, the *Myxogostres*, now called by Du Bary "*Myxozoa*." Just before leaving Bombay, I found the brown stains in some cotton which was submitted to me for microscopical examination, to arise from the development of a mycelium originating in cells or germs of a mycetozoon, which were probably introduced into the cell of the cotton fibre when fresh, and which, on the moisture of the cotton during exposure to the rainy monsoon finding the vitality of its host extinct, naturally appropriated its protoplasm, and produced, while growing, the stains mentioned and consequent injury to the staple.

I am glad that we are at one accord as to the origin of protozoa in the cells of organized beings.

It appears to me that Dr. Hicks is in the same zone (so to write) of investigation in this respect as I was before I renounced my opinion of the "fancied" transformation of the vegetable protoplasm into amœboid animalcules. It was only after studying the *mycelizoa* fungi that I began to see the unlimitable extent to which such beings commencing their existence and even feeding themselves up to maturity, might enter into, and develop themselves upon the remains of their dead or dying host.

The contents of the root-like extremities, filamentous mycelium, and pin-head-like capsules of the *Mucorideæ* may issue, when their cellulose covering is ruptured, in the form of amœboid cells (that is, of course, as regards the *sporidia* before they are capsuled), and so creep away. Then the *Mucorideæ* are closely allied to the *Myxogostres* or *Mycetozoa*; and here no doubt the protoplasm of the *Mucor*-cell or fila-

ment, &c., issues in amœboid forms from its cellulose investment, which seems to be, as in many other instances, secreted by, and common to, a congeries of amœboid bodies, thus assuming the specific form of *Mucor*.

But there is no "transformation" here of the protoplasm, no perishing. The amœboid cells come forth at once and do not bore holes through the cell-wall as the *Mycetozoa* family when developed in the vegetable cell.

Hence, unless the protoplasm issues at once in an amœboid form, or forms, as a whole, as from the cell of *Edogonium*, &c., or in plurality, as from the filaments and pin-head-like capsule of unmaturing sporidia in *Mucor*, &c., I should still be inclined to view the product as not of the same, but of a different organism.

No doubt you saw the statement I last made of the probable reproductive process by impregnation in the Rhizopoda in the xvth vol. of the 'Annals,' p. 172.

I found in a pair of *Diffugia urceolata* (Carter) in zygosis, when crushed under the microscope, a number of monad-like monociliated, polymorphic bodies in active movement; the usual nucleated cells much larger; and apparently some of the latter which had become polymorphic or amœbiform.

The origin of the nucleated cells I had not been able to ascertain—that is, from what part of the *Diffugia* they come. That of the monad-like bodies I knew to have come from the nucleus, which frequently (under reproductive circumstances?) breaks up into these bodies, and therefore, in this case, was not present—had thus disappeared; while the amœboid bodies *without* cilium seemed to be but a more advanced state of the passive nucleated or ovi-like cells.

Thus I inferred that the small monociliated bodies coming from the nucleus were the male elements, and the larger nucleated cells the female elements, which, meeting together in the body of the parent, were, in their plastic state, thus brought together at the most favourable moment for impregnation, *i.e.* for the blending of the two elements. While the active amœboid bodies *without* cilium might have been the product after impregnation, thus prepared for independent existence when the parent might choose to throw them off, or might become effete and thus by dissolution allow them to escape into the water.

I have much more that I could state to you on the subject, but neither my leisure nor your patience, I fear, admits of the extension. Sufficient however has been written to show you the amount of interest I still take in these matters, and thus to prove to you how acceptable was the copy of your paper.

I am, my dear Sir,

Yours very truly,

HENRY CARTER.

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January, 1871.

LONDON: ROBERT HARDWICKE, 192, PICCADILLY, W.

ESTABLISHED 1861.

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