Observations

ON

TYPHOID OR INTESTINAL FEVER: THE PYTHOGENIC THEORY.

BY

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"So the ordinary face and view of experience is many times satisfied by several theories and philosophies; whereas, to find the real truth, requireth another manner of severity and attention." (*Advancement of Learning.*)

PART III. (Continued.)

DOES INTESTINAL (TYPHOID) FEVER EVER ARISE SPONTANEOUSLY?

Summary of Conclusions established in preceding Papers. Their general Effect to show Typhoid Fever to be exclusively contagious. The only Evidence to the contrary Negative Evidence. If Typhoid Fever originate spontaneously, by the same Rule Small-pox and Plants and Animals originate in the same Way. First Origin of all these Things wrapped in impenetrable Obscurity. Small-pox. Itch. Syphilis. Résumé of the whole Argument.

The facts as applied to the disease of which these papers treat, stand, therefore, thus:—Cases of small-pox, like cases of fever, are constantly arising which cannot be traced to contagion. There is the strongest conceivable evidence, notwithstanding, that whatever its first origin, small-pox does not spring up *de novo* now. The occurrence of such cases is, therefore, no warrant for supposing that either of these diseases does so spring up.

Unassailable as this argument is, it may be well, on other grounds, to cap it with another, which covers a still larger issue. I have said that, if we are to believe in the spontaneous origin of typhoid fever, we must hold the same of small-pox also. But it is obvious that, by the same rule, we must believe in the spontaneous origin of plants and animals. For in these, too, in countless instances, there is the same impossibility of tracing a linear descent. The chain, we know, is unbroken; but the connecting links escape us. And what is especially worthy of remark is, that this holds, not only of a species here and there, but of the immense majority of the living types which make up organic nature. All the minuter plants and animals, the great tribe of fungi, the entozoa, and that countless brood of infusoria which Owen has aptly named the "milky way" of organisation, sow themselves over the earth in this invisible and untraceable way. And here, no doubt, it is again impossible to prove a negative.

It is open to the partisan of spontaneous generation to argue, if he will, that the mildew and the tapeworm may, for aught we know, and in spite of all the evidence to the contrary, still spring up anew. But the naturalist, having discovered in both these types, a mode of reproduction containing ample provision for the maintenance of the species, in the very way in which it is observed; and having further found that the hypothesis of spontaneous generation breaks down when brought to the test of experiment in any given case, has rightly declined, in the absence of all proof, even to entertain such a suppo-

sition.* And so the physician, having discovered in the contagious fever, also, a mode of reproduction fully as adequate to the same ends (including a perfect explanation of the actual phenomena); and having further found the hypothesis of spontaneous origin, in regard to a typical member of the family, opposed, not, indeed, by experimental evidence, but by evidence incomparably stronger than any which, in such a question, experiment can bring, is equally justified, in the similar absence of all proof, in taking the same course.

Spontaneous generation is, in fact, in either case, not only a pure hypothesis, but of all hypotheses the most gratuitous. And if, in reply to this, it were argued that it is much easier to conceive of a contagious fever originating anew, than of a plant or an animal—a position which must, however, by no means be taken for granted—the answer is that this is not at all the question at issue; that question being simply what is the value of the evidence which has been supposed to render such a supposition necessary in either case.

But we have already seen that this evidence is negative only, and consists solely in our inability to trace with the eye the continuity of a chain whose connecting links are known to be invisible. To conclude from this that no chain exists, is palpably absurd.†

But nothing, I repeat, can show more forcibly how absurd it is, than the fact that the same evidence, were it worth anything at all, would prove the spontaneous generation of plants and animals.‡

* In one of his able and interesting papers on the Entozoa, in the *Annales des Sciences Naturelles*, Blanchard says:—"Pour ceux qui ont pu apprécier cette fécondité du tenia, combien ne doit-il pas paraître surprenant, que de nos jours l'on ait pu penser encore à citer ce type comme un exemple à l'appui de l'idée des générations spontanées, et qu'une telle doctrine puisse encore être professée même dans nos Facultés de Médecine."

† Rasori puts the whole argument very well in the following passage. "Gli osservatori medici vogliono dunque incominciare a diffidare, per lo meno, dell'attribuita origine della febbre petecchiale alle varie cause tratte dai cibi, dalle località, dalle vicende atmosferiche, da tutto in somma, fuorché dal contagio. Considerino che questo dottrina non ha in ultima analisi, altro fondamento se non quello dei fatti negativi, cioè a dire, del non aver visto donde provenne il fomite contagioso depositato nel luogo dello sviluppo. Che i fatti negativi portano seco, se non hanno altro appoggio, una grave presunzione di colpa dell'osservatore; e che questo avvenimento, il quale si è verificato tante volte nelle scienze sperimentali, si è poi verificato nel modo il più luminoso nella fisica animale, sulla pretesa generazione spontanea, che fu per tanti secoli un errore delle scuole, e di cui non hanno il più analogo a quello de contagi spontanei." (*Delle Febbre Petecchiali*, pp. 231-2.)

‡ It will be seen that I have not thought it needful to refer to M. Pouchet's recent attempt to reopen the question of spontaneous generation before the French Academy of Sciences, in a series of experiments which, according to his own view of them, answer that question in the affirmative. I quite agree with Quatrefages in thinking it only remarkable that these experiments should have imposed for a moment on men of real eminence. All M. Pouchet's results have been upset by those since obtained by M. Pasteur.

The last named physiologist has not only completely confirmed the well known experiments by which Schwann and Henle showed that if the air admitted to organic infusions be first subjected to a high temperature or to the action of strong mineral acids, no living beings ever appear in them, but has further shown that the same result may be obtained by merely giving a form to the apparatus, which mechanically prevents the access of organic germs to the liquid.

It had been objected to Henle's experiment, that it prevented the appearance of infusoria only, because air so treated was rendered unfit to sustain life. M. Pasteur has completely refuted this objection, by showing that if the infusion supplied with air thus acted on be artificially sown with organic germs, it soon swarms with the most flourishing living organisms. M. Balbiani, on the other hand, in demonstrating that the infusoria are endowed with reproduction by sexes, has shown that these beings fall as to their mode of propagation under the common law. With these results, the only remaining pretext for the hypothesis of spontaneous generation falls to the ground; and we have probably now heard the last of this figment.

Stated in the form of syllogism, the position is this:—Animals and plants are not generated spontaneously, but propagate by the law of continuous succession. It is impossible, nevertheless, in countless instances, to trace their lineage. An impossibility of tracing lineage is, therefore, of no force whatever as an argument against propagation by continuous succession.

This reasoning is as simple as it is conclusive. The steps are few; every step is sure; and the conclusion to which they lead cannot be evaded.

The analogy of living types may, however, be employed to illustrate the phenomena of contagious fevers, not only in this negative sense, but, as showing, by *positive* evidence, what are the invariable characteristics which attach to the dissemination of entities *known to propagate by the law of continuous succession*, when this propagation is effected by invisible germs.

To spring up in unexpected places, and in places where their parentage, not only cannot be traced, but is, at first sight, sometimes even difficult to conceive of; to occur now in a sporadic way, and now over large areas at once; to be powerfully affected by season and climate, but in a manner to show that the influence of season and climate is secondary only, is the law that runs through them all. I need scarcely say what a counterpart this history is to that of the dissemination of the great group of epidemic and contagious disorders.

It would be easy to carry this parallel further, if need were. Sir H. Holland, in his interesting essay *On the Hypothesis of Insect Life as a Cause of Disease*, has, however, dealt with it in a way which enables me to dispense with any larger development of it here. I will only remark that the time is not so very remote, when, from the impossibility of tracing their descent from actual progenitors, animals and plants of well known species, and known to be endowed with enormous reproductive powers, were also universally believed to be constantly generated spontaneously.*

Under the pressure of advancing science, this figment has disappeared, and the law of specific propagation by continuous succession has been finally established in its place. And I think we may plainly discern, not only from the perfect analogy in all essential points of the relations which bear on the question in the two cases, but from the course which opinion has already taken in both, that the time is not far distant when the same conclusion will be extended also to the diseases which propagate by a specific contagion.

Nothing, in the history of this question, is more curious than to see how slow naturalists were to relinquish the idea of spontaneous generation, and with what tenacity they clung to it.

"It is probable," says Plutarch, "that the first generation has been made entire and complete of earth—that is to say, by spontaneous generation." He admitted, however, that in his time, *mice* only were formed in this manner.

* This was the case with the entozoa, especially up to a very recent date. Its utter untenability in regard to these types was shown by Eschricht about twenty years ago, in an essay in which the whole question of spontaneous generation is discussed in a masterly way. This essay, of which a translation appeared soon afterwards in *Lancet's Journal*, is still well worth reading.

"Aristotle limited spontaneous generation to *insects, to molluscs, and fishes*"—that is, says M. Flourens (from whom I quote these and the following two passages),* "*to animals of which he knew not the true mode of generation.*"

"A physiologist of the present day, M. Burdach, admits spontaneous generation for *fishes*, but he adds that it would be *too bold* to admit this for toads and frogs."

"M. de Lamarck finds that the *polypus* is too complicated to be produced by spontaneous generation; but he says the *monad* can be produced in that manner. Ehrenberg, who has dissected animals still smaller than the monad, and who has discovered in them a structure so marvellous, carefully avoids saying this."

It was but the other day, so to speak, that mildew was universally believed (as it still is by uneducated people) to be the direct offspring of damp, or to be generated by the decay of other organisms;† and so, in like manner, until quite lately, it was supposed by many that the *itch* was often actually bred by want of cleanliness. When, however, it was discovered that itch is the effect of a peculiar parasite, this notion, too, had to be given up. Fifty years ago, small-pox was very commonly imagined often to originate spontaneously; or, as the phrase was, "to be bred in the blood." Even Gregory went so far as to allow that this view might be supported by very plausible considerations. In the present day, intestinal fever, cholera, and yellow fever, are believed often to arise *de novo*; but most well-informed physicians are of opinion that it would be "*too bold to admit this for small-pox.*" When the true mode of propagation of intestinal fever, cholera, and yellow fever, come to be generally recognised, it will be seen at once that it is quite as bold to teach that these diseases spring up spontaneously, as to teach that small-pox does so.

* Flourens, *Human Longevity*.

† In arguing against this belief, which in his time was still commonly received, the late Mr. Knight says:—"And the best known and the most valuable species to mankind of this tribe of plants, the common mushroom, appears as obviously to spring from horse-dung, under favourable circumstances, as any species of the same tribe appears to spring from decomposing wood without the previous presence of seeds."

"Yet it can scarcely be contended that any vital powers capable of arranging the delicate organisation of a mushroom can exist in horse-dung; and the admission of any such power would surely lead to the most extravagant conclusions. For, if a mass of horse-dung can generate a mushroom, it can scarcely be denied that a mass of animal matter, an old cheese, may generate a mite; and if the organs of a mite can be thus formed, there could be little difficulty in believing that a larger mass of decomposing animal matter might generate an elephant or a man." (Knight's *Horticultural Papers*, p. 204.)

[To be continued.]

LIVING SPERMATOCYTES IN THE VAGINA EIGHT DAYS AND A HALF AFTER INTERCOURSE. In the *American Medical Times*, for March 9th, Dr. S. R. Percy reports a case in which, eight days and a half after the last sexual connection he found semen issuing from the os uteri. A microscopical examination revealed "living spermatozoa, and many dead ones." During this time the husband of the patient had been from home. Of the honour and veracity of the patient, Dr. Percy says: "I would stake my reputation on her honour." Dr. Percy thinks this fact may have a medico-legal bearing, and will necessarily widen the margin to the ordinary term of pregnancy, granted to exceptional cases.