Observations

ON

TYPHOID OR INTESTINAL FEVER: THE PYTHOGENIC THEORY.

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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 I.

Pythogenic Theory Irreconcilable with the Prolonged Absence of Typhoid Fever in Villages where Pythogenic Compounds are rife; also, with the Low State of Fever-Sickness and Mortality in the Population exposed to the Foul Sewage Emanations of the Thames in 1858-59. The Phenomena really those of a Contagious Fever disseminated (chiefly) by Liquid Discharges.

In studying the march of scientific discovery, there are few lessons more strongly impressed upon us than this: that a theory which merely embodies an inference drawn from a single order of facts is, for that reason, almost sure to be wrong, however clear and inevitable the inference itself may at first sight seem to be.

Philosophy teaches us why this should be so; experience illustrates the fact by a long list of signal examples of it.

There are, no doubt, cases, and that of contagion may be named as one of them, in which the relations between events really are, in general terms at least, what from the first they appear. But this can only happen when the fact on which we chance to light is one of a high order.

From the infinite complexity of things, it necessarily follows that such cases are few in comparison with those in which the direct contrary holds. It is not often that nature "wears her heart on her sleeve," or delivers up her secret at the first summons. Quite as often it seems to be her mood, so to speak, to mislead by deceiving shows, as if in counterpart of the principle by which man is said to have invented language for the concealment of thought.

It should be especially borne in mind in connection with the question about to be discussed, that *primâ facie* evidence, which to the common eye seems irresistible, offers no guarantee that the conclusion on whose behalf it is cited may not be utterly false. Illustrations of this must be familiar to all who are acquainted with the history of science. The conviction held so undoubtingly, for so many ages, that the earth is immovable; that it is the centre of the universe, and that the sun and stars revolve round it, and were made for its use, is a very striking one. Sir J. Herschel has made the pertinent remark that, in astronomy, nearly all the facts are, in reality, exactly the opposite to what they at first appear with such certainty to be. The notion held as undoubtingly, and for a still longer time, that mildew

and kindred organisms are the actual offspring of damp and decay, is another case in point. Founded, like these discarded figments, on an induction from which the most capital facts were excluded, the theory that typhoid fever is actually caused by the chemical compounds which result from the decomposition of organic matters, is evidently destined to share in their fate.

The recent adoption of the principal conclusions which I have endeavoured to establish in these papers, by the Central Board of Health, so long the great authority for directly opposite views, will, no doubt, hasten this result. (See *Third Report of the Medical Officer of the Privy Council*, pages 2 to 12.)

On the other hand, it is characteristic of theories which, like these, are the expression of simple and overt relations, to obtain a strong hold on the popular mind. Suggested in the first instance by an obvious fitness, which, although superficial, is not the less striking, they soon gain general acceptance, and pass without challenge. From the nature of the case, a large number of phenomena fit the formula, and the convenient class of "anomalies" and "exceptions" is there to receive the rest. When a theory of this sort has once become widely current, no rival theory (unless it be within the circle of the exact sciences) has, for a time, at least, any chance of a fair hearing.

"Acclinis falsis, animus meliora recusat."

The Copernican theory of the heavenly bodies had a hard battle to fight before it finally drove the Ptolemaic view from the field; and the ancient doctrine would, no doubt, have survived still longer, had it not been that the new one, from being founded on mathematical proof, was of a nature to compel belief. Even to this day, the Chinese, who, whatever else they may be, are a cultivated and intellectual people, treat as an insult to the understanding a notion which, in their eyes, is in such flagrant contradiction with the plainest evidence of the senses.

The belief in spontaneous generation, long given up for alligators, toads, and fishes, and, even in these latter days, for the entozoa also, still lingers among us in its application to microscopic types; to those beings—that is to say, in which, from their minuteness, the act of propagation cannot be directly traced.

Although it is becoming clear to most thinking men that the days of the pythogenic theory of fever are numbered, there is every reason to believe that it will show a similar tenacity of life. Among other evidences of a still active vitality, may be mentioned the attempt recently made by a distinguished physician to replace the names by which typhoid or intestinal fever has hitherto been known, by the word *pythogenic* (i.e., born of putrescence); and, by thus embodying the theory in question in the very name of the disorder, to win for it the highest sanction which it is in the power of language to bestow.

In a series of papers published in the *Lancet* in 1858-59, I brought forward evidence to show that, while this theory is irreconcilable with what we know of the manner in which this fever is propagated, the facts on which it is founded may be readily and far more satisfactorily explained in another way. It is the purpose of the present communication to further show that this theory is untenable, even on its own grounds.

In the last of the series of papers addressed to the *Lancet*, it was argued that the power to propagate intestinal fever which sewers and other *cloacæ* undoubtedly possess, is due, not to the impurities which it is their office to receive, but to their frequent impregnation, in the way there described, with the actual fever-poison. Nothing can prove more clearly that this power depends on something much more specific than the compounds which mere chemical change breeds in these impurities, than the extreme contrast which is observed between the effects of exposure to emanations from sewage at different times and places. It has long been notorious—and the well known Windsor epidemic may be cited as a signal instance of the fact—that, under conditions which are of no infrequent occurrence, these emanations may propagate typhoid fever with intense power, and in a wholesale way. Experience the most ample has shown, on the other hand, that the disease may be absent for indefinite periods of time—often for many years together—from places where we have the evidence of a sense which cannot deceive us, that the foul products which emanate from sewage are in the highest degree rife.

The inference is inevitable that in the two cases the miasm must differ in some essential thing. The agency that is so intensely prolific of fever, and the agency that for long years together causes no fever at all, cannot be one and the same. It cannot be mere sewage, that is to say, that causes the fever; it must be sewage of a special kind.

When we consider the nature of the effect, who can doubt that it must be of that kind which is known to be so profusely supplied to sewers, by the diseased intestines of the hundred and fifty thousand persons who make up in England the annual tale of intestinal fever cases?

To see that this is the true interpretation of the facts, all that is necessary is to contrast the experience of large towns in regard to this fever with that of country places. In large towns, intestinal fever is being continually traced to sewer-emanations; because, from the constant presence of the disease in large communities, the sewers provided for them are as constantly being infected with the fever-virus. But, in villages and other places of small population, where, especially if remote from large towns, the disease is only an occasional visitor, the emanations to which it is commonly ascribed may continue in full force for years together without a single case arising.

I have already stated elsewhere, that before the great epidemic broke out which visited North Tawton so severely in 1839, there had been for ten years not more than one case of fever in the village. And, if it were argued in explanation of this, that the causes to which intestinal fever is supposed to be due, were, during that time, in less active operation, I can only say that, whether active or dormant, the hypothetical fever-demon of the sanitarian at least *stank as loudly* during that long period of entire exemption, as he did when nearly one-sixth of the whole population were stricken with fever. If he were not there in person, he surely had no business to smell so badly. Instances of the same prolonged absence of fever, in rural districts, in the presence of its reputed causes, might be multiplied to any extent, if there were need.

I may add further, that the fact which these in-

stances supply is true, not only of this or that particular place or people, but is true generally. It is as true of France as it is of England; it is as true of other parts of Europe as it is of France.

No better evidence of this could be given than the very large proportion in which, wherever the reckoning has hitherto been made, persons new from the country figure among the fever-patients of large cities. Louis has pointed out with his wonted clearness, that this peculiarity is mainly, if not wholly, due to the circumstance that in the city the disease is always present, but in the country only now and then. (*Recherches sur la Fièvre Typhoïde*, 1re edit., tome ii, p. 375.)

But, that the occurrence of these long intervals of rural exemption is not to be traced to any want or feebleness in country places of the fever-producing power is plain from this, that when the disease does break out in them, the ratio of persons attacked is incomparably greater than is ever seen in the city under the like circumstances.

Speaking of the fever which swept the parish of Great Horwood in 1857-58, Prof. Acland says:—"An endemic affecting a similar proportion of the population of London would attack, in nine months, more than a quarter of a million of persons, of all ages; above fifty thousand would die of the fever alone, while the deaths from other causes during the period would not diminish." (*Fever in Agricultural Districts*, by H. W. Acland, M.D., F.R.S., Regius Professor of Medicine in the University of Oxford.)

The North Tawton epidemic would lead to a similar calculation; while the two Caumont epidemics described by Gendron de l'Eure, and great numbers of the village outbreaks, recorded in the admirable *Rapports des Epidémies* of the Memoirs of the French Academy, would carry a proportionate metropolitan mortality to a still more enormous aggregate.

It will be seen, from the bare statement of them, that figures like these represent an amount of havoc, in proportion to population, to which no urban outbreaks offer the most distant approach. And, as if to remove the last doubt as to what is the inference to be drawn from this contrast, this immensely greater havoc occurs, *precisely because the village is worse off than the city in regard to those very conditions out of which, according to the commonly received theory, intestinal fever is supposed to spring.*

I have already shown that the rapid and wide dissemination of the pest at North Tawton—and the remark applies with the same strictness to all similar village outbreaks—was caused by the almost entire absence of sewerage; i. e., of means for preventing the discharges from the human intestine from polluting the soil and air of the inhabited area.

Nothing can well be more simple than the interpretation of these relations. Village life, not only in England, but in Europe generally, is constantly presenting to us the following conditions:—

1. Sewerage entirely wanting, or very defective; and pythogenic compounds rife in proportion.
2. Intestinal fever wholly absent for long periods, notwithstanding.
3. Great virulence and inordinate spread of the fever when imported, by reason of, and in proportion to, the defective sewerage.

The long continued and entire immunity from the disease amid the impurities to which it is commonly ascribed, especially when contrasted with its intense virulence when once introduced, shows, with a force of evidence which in medical problems is rare indeed, that these impurities have no power of themselves to cause fever, but only when, in the course of events, they happen to become charged with its specific poison.

One of the most striking testimonies to the truth of this theory consists, indeed, in the *perfect explanation* it gives of two facts *absolutely inexplicable* on any other grounds. I speak of the incomparably greater ravages which, as we have just seen, typhoid fever commits among rural communities when it attacks them, and its vastly greater apparent contagiousness in country than in town. Both these facts had been remarked over and over again by writers on the subject, but all had confessed their inability to explain them.

In referring to the evidence of the contagious nature of typhoid fever already recorded before the date of his own memoir on it, M. Piedvache says:—"All these observations (by Gendron de l'Eure, Leuret, Bretonneau, Ruefz, Putegnat, Patry, Jacquez, Ragaine) have been drawn from the country, or from towns of small population. The medical men of great cities, on the contrary, have scarcely observed any but facts negative of contagion. M. Gaultier de Claubry, who is an avowed partisan of contagion, could only muster, from his own practice, eight examples of it, for his paper on the subject, read to the Royal Academy of Medicine. In the discussion that followed, as well as in those which took place on the occasion of the reading of the other memoirs I have cited, the members of the Academy admitted, some to have seen one, and others, two, three, or, at most, four cases, in which they could recognise the operation of contagion; others denied it altogether, on the ground that they had never seen an instance of it!" (*Mémoires de l'Acad. de Méd.*, 1850, p. 246.)

In another passage of the same memoir, M. Piedvache adds:—"The observations which I have recorded as proofs of contagion were nearly all collected in the country. The third, fourth, fifth, sixth, and thirty-seventh, were the only exceptions; and in none of these did that extension of the disease to a whole family occur, of which I have related several examples, and which I have so often witnessed."

"These facts," he continues, "which are in accordance with all hitherto published, lead to the conclusion that typhoid fever, while almost invariably contagious in country places, is only rarely so, and by exception, so to speak, in cities. It is a general fact, which it is impossible to deny; but it is, if I may so express myself, a crude fact (*un fait brut*), whose explanation is yet to seek." (*Ibid.*, page 308.)

To the common view, so great and so strange a contrast must seem in the highest degree perplexing. For, in town and country, as I have elsewhere observed, the fever itself is one and the same. It is not, in reality, less contagious in one place than in another. To multiply and throw off a specific poison constitutes, in town and country alike, its very essence. No one has yet suggested, in explanation of the seeming anomaly, that this pest has human sympathies, and is more merciful to the townsman

than to the rustic. And few, I imagine, would be disposed to follow the cautious Louis, who, in the act of admitting this fever to be contagious, thought it necessary to add—not by way of humour, but in the sober sense of the words—"at least, in the departments" ("*au moins dans les départemens*").

The fact that this fever spreads almost wholly by the discharges from the intestines gives a perfect solution of the enigma.

In the city, in a large proportion of cases, these discharges are no sooner cast off than they are swept out of harm's way, or are otherwise dealt with in a manner that prevents their taking effect on the group who watch the sick bed.

In country places, they accumulate day by day on the open soil which surrounds the patient's dwelling, until they envelope the whole household, and often the neighbours also, in a fever-miasm, which is incomparably more virulent than the atmosphere of the sick chamber itself.

In a paper which appeared in the *Lancet* on Dec. 6, 1856, on the Fever at the School of the Clergy Orphan Asylum, I pointed out the working of these conditions in terms to which I have nothing to add. Unfortunately, the area over which they operate is often much widened in country places by the dirty and neglectful habits of the people.

In the paper referred to, after observing that the case is, happily, one in which the natural instincts suggest what science prescribes, and that the rich, in using disinfectants to get rid of bad smells, often accomplish a higher purpose, it is added:—"Amongst the poor such refinements are never thought of; and when this fever breaks out in a poor family, the discharges from the bowels are thrown, without preparation, either into the common privy, or, as I have seen a hundred times in rural districts, are cast upon the dungheap, or into the open gutter. From this point, following the line of watershed, this pestilent stuff often makes its way to considerable distances, where, appearing now under the guise of an endemic miasm, which entirely masks its true origin, it may carry disease and death into many an unsuspecting household."*

[To be continued.]

* The paper continues:—"If the practice were universal of depriving this stuff of its infectious properties, by the method here indicated (that is, by disinfecting the stools, and everything imbued with them, including the privy and water-closet, by means of appropriate chemical agents), before it is let loose upon the world, I believe it is not too much to say that the lives of a large proportion of the many thousands who die annually of this fatal disease in the British Isles would be saved by the measure, and a corresponding amount of sickness prevented." (*Lancet*, Dec. 6th, 1856.)

This opinion was founded, not on theoretical considerations only, but on many years successful employment of the method in question. I proposed the same method for checking the spread of cholera as long ago as 1849, and have since published several instances of its successful application in cholera outbreaks. In one of my papers which appeared in the *ASSOCIATION MEDICAL JOURNAL* on October 13th, 1854, and in which the necessary measures were described in great detail, the following remarks occur:—"Looking at the subject as a whole, I feel sure that the mode of dealing with cholera here recommended will, in the end, be universally adopted; and I do not think I am assuming too much in believing that the time will come, and is not far distant, when every one will marvel that measures so obvious, supported by such wide analogies, and recommending themselves so plainly to common sense, should have been so long overlooked, and so slow in being carried out."

I am glad to see that the Privy Council (acting as the Board of Health) have taken the lead in this matter by making this method an integral part of their recently published "General Memorandum on Proceedings which are advisable to be taken in places attacked or threatened by Epidemic Disease." (*Third Report of the Medical Officer of the Privy Council*, p. 38, pp. 11.)