NOTES

ON THE

ORIGIN AND DIFFUSION OF ENTERIC
FEVER AND OF DIPHTHERIA.*

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OF all hindrances to the establishment of a scientific basis for medical practice, the greatest, I take leave to say, is the too prevalent custom of carrying deductions from a limited number of facts up to an extreme degree of accuracy. So captivating is the result of this process, that one is immediately led to believe that the question at issue is solved; and the reader, unless he be more than usually on his guard, is very apt to believe the question solved for him also. And in no department of the wide field of medicine is this error so often committed as in that of etiology, which treats of the origin and diffusion of diseases.

Now enteric fever has long been the battlefield for opposite parties to tilt at each other; and the cause is not far to seek. It is no other than the use of the *deductive* in place of the *inductive* method of reasoning, which is so frequently adopted when a question of causation comes under discussion. Each combatant has observed a limited number of facts, and verified them as facts. The conclusions deduced from them become his theory—his idolon. And a nice well-rounded theory looks so well, that few of us are able to withstand its blandishments. Facts which oppose this theory are either boldly denied, or, if that may not be, they are so manipulated by the mind that, instead of opposing, they come to support the favourite dogma. Big names, we know, carry weight; whilst more modest and less aspiring, yet equally trustworthy observers, are apt to keep their observations for their own use, and so error is perpetuated.

The time will not allow of my quoting authorities, or citing times and instances, either of observation or publication, in regard of the matters now under discussion. I am familiar with most of the authorities on enteric fever, from the first distinct proof of its specific distinction from typhus fever made by our valued associate Dr. A. P. Stewart, forty years ago, down to the present day. I will merely recall to your recollection the names of Brétonneau, Gendron de Leure, William Budd, Corfield, and others, as advocating one theory of the causation of enteric fever; and Louis, Andral, Murchison, Alfred Carpenter, and others, who have chiefly supported the other. To avoid circumlocution, let me define these two theories at the outset.

To take the later and more aggressive theory first: it states categorically that enteric fever is a specific febrile disease of the order of the exanthemata, just as much so as are small-pox and measles, with which two species it is supposed to have many analogies. Like them, it is originated and propagated by specific disease-germs which pass out of the intestine of one sufferer into the system of another person by the ordinary channels used by other diseases; viz., by the atmosphere, by imbibition by means of polluted water or food, or perhaps by immediate contact of the healthy with the sick. The late Dr. W. Budd of Bristol is taken to be pre-eminently the apostle of this theory in this country, from the great care and minuteness with which he investigated his facts, the clearness with which he stated them, and the seeming cogency of the deductions which he drew from them.

The other theory, called by Murchison and his followers the pythogenic theory, although it does not deny the existence of disease-germs, which certainly are sometimes cast off by the intestinal canal of the sufferer to reproduce themselves in the intestines of others, teaches that enteric fever originates in some unknown *materies morbi* (microzymes, or fungi, or chemical product), which is generated in sewage, in putrefying human excrement, or in other animal matters, at uncertain times, and under conditions not yet accurately defined; and that this *materies morbi* may be conveyed in the atmosphere, in water or food, or by fomites, or, possibly, very occasionally, by direct contact of the healthy with the sick.

A third or intermediate theory, held by some, makes it necessary for the specific germs of theory No. 1 to be developed or rendered virulent

by admixture with sewage or other foul or decaying organic matters in which they grow, or at least undergo some change by which they become more active and capable of diffusing the poison to an extraordinary extent.

Now, in starting a discussion upon the origin of enteric fever, I do not think it necessary that I should pin my faith to the correctness of either theory, or to try to persuade you that such and such a view is the correct one. Rather, as it seems to me, I ought to lay before you the salient points of correspondence or of antagonism of facts to either theory, and, by eliciting the opinions of many observers upon these facts, corroborated by their own experience, to induce some one to gather up the scattered threads of the argument, and by *inductive*, and not by *deductive* reasoning, to construct from them an unassailable theory.

Let us examine, first, the first-named theory. And here let me say that I have carefully re-read every line of Dr. Budd's classical work; and I do not hesitate to say it of one whose memory I hold in affectionate admiration, that anything more misleading than the deductions in that book, truly drawn as they are from the facts before him, is scarcely anywhere to be found in the whole range of medical literature. Dr. Budd, as you are aware, collected a mass of facts, all going more or less to prove that, in the remarkable outbreaks of enteric fever inquired into by him, he was able to trace the introduction of the disease to the affected locality by some person who was either at the time suffering from the disease, or who had lately been in contact with some person so suffering; or, at all events, had lately arrived from an infected locality. The celebrated case of North Tawton is a typical instance of this. On the arrival of the presumed infected person, the disease spreads in the house, to the adjoining houses, then to others in the district, and then to a distance; but always, when at a distance from the original focus, a carrier of the infection was traced. This was so at Chaffcombe, and at the convent near Bristol. Observe, however, what Dr. Budd does not omit to point out, that all these places, at the time of the outbreak, were the abode of sanitary abominations quite sufficient, according to the pythogenic theory, to breed the fever of themselves, or, as we say, *de novo*. Thus, at St. Arno's Court, where there was undoubted evidence of a person introduced from an infected district, Dr. Budd relates that the main drain of the building had long been blocked up; and that, at the very time of the fever, a perfect cess-pool of sewage was found under the very building! But, then, these abominations (says Dr. Budd) had existed for months—yea, for years—in the same condition, yet no fever makes its appearance until the germs had been introduced by an infected person. If any suggestions are made that, after all, these insanitary conditions might of themselves have given origin to the disease, under some unknown influence, the suggestions are put aside, with pity for those who make them, as "too preposterous and absurd". Now, the deductions from these cases, as I have said, are so well drawn, and were so convincing to the minds of the author and of those who were associated with him in the inquiry, that it appears as though nothing could be said against them. So far as they go, they are unassailable. But, mark what a fatal error runs all through the book! No mention at all is made of the hundreds of cases recorded by other observers, in which the most careful inquiry failed to discover any probable—aye, even possible—introducer of the contagion. You are, in fact, led to suppose that what Dr. Budd has seen himself is all that *can* be seen of the disease. He found no outbreak unless, to already filthy conditions of dwellings, etc., the specific poison was introduced *ab extra*. Yet, you know that Dr. Murchison and others have related how the introduction of enteric cases into fever hospitals scarcely ever conveyed the infection to the nurses and attendants; and all hospital physicians can tell how seldom it spreads to the neighbouring patients when introduced to our general hospitals, notwithstanding some laxity in dealing with the discharges. This depends on one condition only: namely, that the sanitary arrangements of such hospitals are good, and that proper means are adopted to keep them good whilst such cases remain in the hospital. This important proviso will come into prominence presently.

What, then, is the summary of Dr. Budd's own cases and of those of similar observers, and the deductions to be drawn from them? Only this. For a long period—sometimes, it would appear, even for years—very insanitary conditions of dwelling-houses, of their surroundings, filthy privies and piggeries, foul drinking-water, etc., may exist in a given locality, especially in country districts, without giving rise to an outbreak of enteric fever. We can all verify this proposition. But when, to these conditions, the propagating germ, brought from a distance, is added, then the yeast is set to work, and the germs multiply in a medium suited to their growth: namely, in the intestinal canal of those attacked by the disease. The germs are cast out thence upon the earth, into the privy, into the drain, the water-course, or elsewhere.

* Read in opening a discussion in the Section of Public Medicine at the Annual Meeting of the British Medical Association at Ryde, August 1881.

The people of the house to which the introducer has arrived are the first to be attacked, then others at a greater distance, both up and down the district. Note, however, that in every case where these disease germs have been carried by persons going from an infected locality into an uninfected one, the same insanitary conditions have existed in the latter as in the former neighbourhood. The hot-bed was already prepared, so to speak, awaiting only the spawn to bring forth an abundant crop. The deduction as to the necessity of the previous introduction of the special disease germ, from the facts narrated, you will see is perfect.

But now, with respect to those other cases, quite as numerous and quite as carefully observed, in which, the same filthy conditions existing, no trace of the introduction of disease germs, *ab extra*, can be found on the most careful inquiry; and in which the discharges from the intestines of the sick have *not* spread the disease. Surely, in investigating the origin and diffusion of such a disease as enteric fever, all the observed facts must be taken into the account, and have their proper value assigned to them? And if known facts are not sufficient to enable us to construct a workable theory of the causation of the disease, we must wait for more facts and look out for more light.

Now let us look at the second theory, the so-called pythogenic origin of enteric fever and also of diphtheria. After lying in peaceful repose for years, the stench from its filthy drains and cesspits only offending the noses of the passers-by, some village is suddenly wakened up by a tithe of its inhabitants being attacked by enteric fever or diphtheria. What has happened? Has some person been discovered who has brought the germs on his person or clothing from an infected district? And have these germs got into the waters and drains, where they immediately gave birth to a swarm of infant germs, which speedily diffused themselves all over the infected area? This is what ought to have happened if Dr. Budd's theory be true—true at all times and in all places.

Now, I have seen a great deal of the origin of enteric fever in my time, and have met with a great number of intelligent country practitioners, and I believe there is scarcely one of the latter who has not said, or who is not ready to say, that in his experience, after most careful inquiry, in the great majority of cases, no introduction of the poison, *ab extra*, can be made out. I might mention scores of cases from my own note-book, but similar instances must be present to the minds of you all.

Now, it may be asked, why introduce the question of the origin of diphtheria to complicate that of enteric fever? Because of the proved, and, I believe, sufficiently acknowledged, double origin of diphtheria. Here is Sir W. Jenner's statement as to the etiology of this disease, published in 1861. 1. "That diphtheria is contagious"—admitted. 2. "That the infecting element does not require for its development any of the ordinarily considered anti-hygienic conditions"—granted also, if we say not always required. 3. "That it is very doubtful, even, if any of the anti-hygienic conditions favour its development, or give to it a more untoward course when it occurs." This proposition is more than doubtful, and is entirely opposed to the experience of those who have seen much of the disease amongst the poor, especially in country districts, where it can be observed with greater accuracy than in large towns. For valuable facts, and arguments based upon them, in reference to this point, see a paper by Mr. Fosbrooke in the *Sanitary Record* for March last.

It cannot be, and I think is not, denied, that cases of diphtheria may have a double origin: first, from filth and foul or damp air, which engender sporadic cases, and these may sometimes so act as to give rise to an epidemic of the disorder. Secondly, from direct contagion, more especially when the disease prevails epidemically. The *de novo* origin of diphtheria, then, is not disputed. The contagious origin is not disputed, and it does not require anti-hygienic conditions for the contagion to act.

Now, the characteristic features of enteric fever and diphtheria have much in common. In each case we have to deal with a poison in the blood, introduced *ab extra*, multiplied in the bodies of the sick, and cast out thence from the surface especially affected; viz., the intestinal canal in the one case, or the mucous membrane, of the throat and pharynx, in the other. The poison, whatever be its nature, produces in each case its specific symptoms, and the contagion, when conveyed to another subject, always produces the same disease. The two poisons, therefore, are specifically distinct, although arising apparently in the same media, and entering the body by the same channels. Filth of some kind, chiefly, if not entirely, of animal origin, seems to be the initial medium whence the poison is derived, and it is not in our power to say which of these two very distinct diseases will be produced by contact with the same filthy medium.

Now, can anyone at this point answer this question: Why, admitting

diphtheria to have this double origin, enteric fever may not have it also? Are there not other diseases of analogous nature which exhibit an origin in two sets of conditions? Erysipelas breaks out in hospitals and in private houses also, without any introduction *ab extra*. From attacking cases under the care of the surgeon, it makes its way to the adjoining beds under the care of the physician. If you doubt the fact of contagion here, and assign the outbreak to anti-hygienic conditions affecting all alike, we will follow the patient to the private house into which he carries the disease, although the place may be in possession of the most perfect wholesomeness. It appears to me that the philosophical physician ought to endeavour to reconcile all these conflicting observations by availing himself of all analogies by which one disease can throw light upon another, and not by forcing them all into one category, willy nilly, for the sake of backing up some foregone conclusion, or of building up some specious theory. He will also see if he cannot lay his hand upon some *third* factor, some *tertium quid*, by the aid of which the discrepancies of evidence may happily be adjusted; or, if he cannot, try to discover some hitherto unsuspected analogies by which the conflict may be ended, and the diverse facts made to lie down together in peaceful agreement.

Returning once more, only for a moment, to the first theory, that of specific germs necessary to generate and reproduce enteric fever. The difficulty of accounting on this theory for the isolated cases which arise apparently *de novo* is immense, if not insurmountable. It is proverbially difficult to prove a negative; and, no doubt, it may be conceded to Dr. Budd and his followers that the introduction of these poison germs to sewage, to foul air, even to the earth, may give rise to a most rapid and extended multiplication of them, and to their diffusion by various means over a considerable area. But if every case which arises, whether in the remote farmhouse or rural cottage or in the dense rows of the crowded city, is to be accounted for only on this theory, these germs must be almost universally diffused through earth, air, and water—must be, in fact, ubiquitous.

Now what do we actually find to be the case? This. Where no fever cases has been before, whenever the house-drain is choked up, when the cesspit is more than usually filthy and stinking, when the water-closet is out of order; when sewage, even from a simple drain in the country village, enters a well and pollutes the drinking water, *there and then* enteric fever occurs in cases too numerous to be ignored. If all these cases are caused by some sanitary fault simply giving exit to the specific germs of the disease, supposed to have been imprisoned there from some long ago previous case, how universally diffused they must be! Again, we find that, whenever sewage matters laden with the excreta of human beings do gain entrance into a well, those who partake of the water are almost certainly immediately attacked. On the other hand, I have never been able to trace with certainty a single case of this disease to the drinking of our river-water, which, although filtered through gravel and sand, could scarcely escape conveying some of these minute specific disease-germs, if they were so universally diffused as, to accord with the one theory, they must necessarily be. These germs ought to find their way into the river, which is the main sewer of the district, in countless millions, were Dr. Budd's theory absolutely the only correct one; for they would enter it from every sewer and drain along its whole course. Are these things so? I trust and believe not.

Another objection to this theory: If you confine the emanations, say from a badly ventilated water-closet, or the effluvia from a foul drain, you will probably very soon originate enteric fever or diphtheria. But let in plenty of air, cut off the closet by a cross-current, and the house is safe. Now, if the *materies morbi* were of the nature of living organisms, would they be likely to be so easily killed or rendered harmless by a little fresh air? But if, on the other hand, the *materies morbi* be of pythogenic origin, that is, probably, a chemical product generated by some process of putrefaction, then we can well understand its destruction by chemical action, or its oxydation in pure air; and can explain how it happened that the specific germs in Dr. Budd's cases only operated when the sanitary conditions surrounding the patient were such as to promote these putrefactive changes.

Not to weary you by any further discussion of antagonistic theories, let me briefly point out the direction in which I think we should work in order to add to our stock of real knowledge bearing upon the etiology of these two diseases.

1. I would suggest that all available facts as to the origin, spread, and true nature of enteric fever and diphtheria, should be marshalled side by side, without prejudice or favour to any theory.

2. I would collate the facts derivable from the history of cognate diseases, such as cholera, yellow fever, erysipelas, and place them alongside the above; and also collate the phenomena of certain diseases of animals—chicken cholera, for example, with those observed as above.

3. Compare the conditions under which the diseases in these two last heads prevail with those observed of enteric fever and diphtheria.

4. Inferences may now be drawn which will be available as landmarks, or provisional propositions, with which to go to work in the construction of a theory of the origin of these two diseases.

Allow me, in conclusion, to state what I look upon as correlated facts already known in the inquiry.

I think it is proved that enteric fever, however it may have commenced, is sometimes propagated from person to person by close propinquity, or actual contact, with the intestinal discharges. I had a clear case of this in my own house. And a *matrices morbi*, which may or may not be composed of living specific germs, is sometimes conveyed from the sick to the healthy at a distance, where it takes effect in producing exactly the same symptoms as in the initial case. But, in this event, in the great majority of cases at least, the sanitary surroundings of those who receive the disease are very faulty. This was the case with the epidemics of cholera, and it is still the case with the spread of diphtheria and erysipelas. These diseases seldom spread in a wholesome house or district.

It is also proved, as far as a negative proposition can be proved, that a *matrices morbi* of unknown nature, be it fungoid, or be it chemical in composition, is often bred in drains, sewers, privies, or on damp or foul surfaces, or in earth or water saturated with sewage matters; and that this *matrices* gives origin to enteric fever and to diphtheria without the action of any specific germ derived from the body of a previous patient. Unless we accept this proposition as founded in fact, the origin of thousands of cases of these two diseases is involved in impenetrable mystery. And if this proposition be accepted, then we must conclude that some specific or quasi-specific organism (or germ, if you please), or else a mere chemical product, is formed in such matters, *de novo*.

These substances, organisms, or chemical products, whatever they be, undoubtedly become capable of self-multiplication and intensification in the glands of the intestine on the one hand, and on the mucous membrane of the throat and pharynx on the other. They also now become capable of conveying their respective diseases by direct contact, as in the first-named proposition; and, to many minds, this is the usual order of events in the origin and diffusion of both enteric fever and diphtheria.

Finally, there are analogies observable between these two diseases and erysipelas and some well-known forms of septicæmia. Recent experiments, including those of Pasteur, Burdon-Sanderson, George Harley, and especially of Gravit, go to show the remarkable fact that certain deleterious organisms—fungi, etc.—may be so cultivated, by repeated inoculation into living tissues, that, from comparative harmlessness, they pass on to a condition of true infectiveness, their virulence increasing with each fresh inoculation. Gravit even believes, and in this he is supported by other German observers, that inoculation with germs of “low culture”, or with a very minute quantity of the *matrices*, will confer immunity from future attacks. Does not this lead up to the presumption, to say the least, that the poisons of typhoid and diphtheria, whatever may be their real essence, in passing through the bodies of patients, acquire increased virulence, and so give rise to those phenomena of direct contagion in some instances, which Dr. Budd and his followers so strenuously contend for in all?

Here, then, we come in sight of that *tertium quid*, that something *ab extra*, to which I have already alluded as apparently necessary to harmonise and reconcile the various conflicting phenomena of these diseases. And it will be in pushing investigations in this direction to their utmost attainable limits that I shall look for any real extension of our knowledge of them. Could the funds of our Association be better employed than in aiding skilled investigators to pursue these inquiries in a field which seems to promise the greatest results from scientific cultivation?

HALSTEAD RURAL DISTRICT.—The first division of this district, held by Mr. Hinds, comprises 17,480 acres, with a population in 1871 of 5,137 persons. The birth-rate for 1880, based upon this population, was 24.13, and the death-rate 16.35 per 1,000. Of the total number of 84 deaths, zymotic diseases were responsible for 13. Scarlet fever and diphtheria were somewhat prevalent; and Mr. Hinds records the extraordinary number of 10 cases of diphtheria (1 fatal) in a single family, the cause of the epidemic being somewhat unsatisfactorily ascribed to “a defective sink in the back kitchen”. Fourteen cases (1 fatal) of typhoid fever were traced to an impure water-supply, the outbreak terminating only on the closing of the well from which the water was obtained. The sanitary condition of the district seems on the whole to be fairly satisfactory, but the water-supply is defective in many places. It is satisfactory to note that provision for the isolation of infectious cases is likely to be made.

THE PREVENTION OF ENTERIC FEVER.*

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I HAVE chosen the prevention of enteric fever as the subject of my short address, in the hope that we may impress upon ourselves at least one of the means of preventing this disease.

We must all have been struck with the fact that isolated cases of enteric fever are to be found north, south, east, and west. Why is this? My answer is, that the public have not grasped, and will not grasp, the fact, that private sanitary engineering is at fault, in contradistinction to public sanitation. In other words, people take houses, and seem perfectly indifferent as to sanitary condition, but nevertheless appear never to fail in giving vent to their feelings by expressing their horror and disgust of the smells emanating from the public sewers.

For the past six years, it has been my endeavour, when seeing a case of enteric fever, to make out whether the case could not have been prevented; and, in the majority of cases, I contend that it could, being caused either from faulty water arrangements, or defective drainage system. I find that where water-pipes are wrongly connected, there, as a rule, drainage is even still worse. It is common to find people living in houses where the water-pipes or the drainage are as wrongly constructed as in the two plans I now bring forward.

One plan shows a cistern situated over the slop-sink in a small housemaid's closet (with hot-water cistern attached to it), being 4 ft. 6 in. long, 3 ft. 6 in. wide, 4 ft. deep (a large one). It was the only supply to the house, with no history of its having ever been known to have been cleaned out. This cistern supplied immediately underneath it the housemaid's sink. The sink emptied itself into the upper part of the D-trap, belonging to the servants' water-closet. This D-trap also received the waste from the untrapped pipe from the butler's pantry; and the copper pan of the servants' water-closet being useless, there is no kind of ventilation to the soil-pipes; and the water-closets were of the foul pan order. There was no disconnection at the butler's sink, the housemaid's sink, or scullery sink; neither was there any main disconnection or ventilation between the cesspool and the ends of the drain. And yet, gentlemen, this was the residence of one who considered himself no ordinary sanitarian. I ask you, would you be surprised to hear that enteric fever broke out in this house? and is it not clear what could have prevented it?

The next plan shows the water-system in order; but, with regard to the drainage, there were four inlets into the main sewer untrapped, with the simple exception of the D-trap at each water-closet; the servants' water-closet being of the foulest pan order. Also the wash-basin in the library had no trap, and was in direct connection with the stable-drain. I have said that the inlets into the main sewer were only trapped by a D-trap. Now, what is a D-trap? Sir William Jenner calls it a double D-trap, for it will deal out disease and death; and yet how often do we see these used? Why is it? Because workmen have never been taught better; and, what is even worse, D-traps were to be seen in numbers at the International Medical and Sanitary Exhibition. In this state of drainage-system, is it difficult to understand what could have prevented enteric fever in this house?

And here I must say, it matters not what system of sanitation is carried out, unless skilled workmen be employed, I venture to assert, from experience, the last state will be worse than the first, and enteric fever will not be prevented.

If you wish to see a perfect system of sanitation, go to the Royal Isle of Wight Infirmary, where Banner's system is carried out (similar to Guy's Hospital). Again, if you would see “perfect ventilation”, go to the Milligan Convalescent Home, on the same grounds as the Infirmary. By “perfect ventilation”, I mean an inlet of fresh air, together with an outlet for foul air.

I contend that enteric fever can be prevented in the majority of cases by private systematic periodical sanitation, in contradistinction to public sanitation; because, as we are told, public health authorities have no duties until a nuisance is known or suspected. I say, systematic and periodical private sanitation, because there are no pipes or traps that will for any length of time continue in proper working order; hence, there must be regular periodical inspection. Then, and not till then, in my opinion, will enteric fever be, to a large extent, prevented.

The question to be considered by the Section is this: Is it, or is it not, the opinion of this meeting that the spread of enteric fever can be prevented by special attention to private systematic and periodical sanitation, in contradistinction to public sanitation?

* Read in the Section of Public Medicine in the course of a discussion on the Prevention of Enteric Fever.