OBSERVATIONS ON SOME OF THE WAYS IN WHICH DRINKING-WATER MAY BECOME POLLUTED WITH THE CONTAGIUM OF ENTERIC FEVER *

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PERMIT me to commence this paper with a confession of faith. In a recent report which I had occasion to make to the Local Government Board upon an outbreak of enteric fever, I thus formulated my creed. "Enteric fever contagium, as we now meet with it, has an ancestry; and, however long and widely and through whatever media it may have travelled about prior to finding a lodgment suitable for its development in a human system, it or its ancestor at one time issued with excremental matter from some individual affected with the disease. I accept this doctrine provisionally, as most in accord with my own experience, and also, so far as I have been able to judge, with that of the most competent and, let me add, most patient observers. With such as dissent from it, I can agree in admitting that there is very much in the natural history of contagium in general, and of enteric fever contagium in particular, wrapt in an obscurity yet unpenetrated; but I am hopeful enough to look forward to the time when the advance of scientific inquiry will render that clear which is now dark. But, however this may be, the doctrine is one which furnishes a salutary rule of practice, inasmuch as it is calculated to foster a minuteness of investigation which has, at any rate, the chance of being fruitful."

In this expression of belief, it is encouraging to me to be in accord with my distinguished colleague Dr. George Buchanan, who four years ago wrote as follows. "In an out-of-the-way house, looking as if isolated from all chance of infection, where there has been no previous sickness for months or years, but where a number of common nasty conditions exist, a case of fever occurs, and the impulse of the doctor who investigates it is to say that the disease has had a *de novo* origin from the ordinary excrement-filth of the place. Now, whether or not this has been so is a question of the highest moment to science, but is by no means to be settled out of hand by the apparent absence of specific cause in a few instances. Repeated observations and very complete record of all the circumstances are wanted, before any deduction can be properly drawn from such an occurrence as I am supposing. Often room has been left in the records of such cases for doubt as to the nature of the disease in question; or an oversight on a point so demonstrably important as the period of incubation has led observers to hasty and untenable conclusions; or the possibility of dangerous material passing a long distance through the soil or along a stream may not have been held in view; or some vehicle for the conveyance of specific contagium may have been overlooked." Referring then to a piece of inductive work which it was my privilege to be able to do in 1870, he goes on to say: "If we consider how, a dozen years ago,..... those many wonderful outbreaks of enteric fever that we now trace to the agency of milk were unintelligible, and how all of them, instead of being merely regarded as unexplained, were liable to be regarded as demonstrating *de novo* origin of the disease, we shall the better apprehend the caution with which it is necessary still to proceed before we can affirm the adequacy of any common filth-condition to originate enteric fever..... Obviously, we have to be incessantly on the look out, not only for these, but for other and even obscurer vehicles for the specific poison; and there are most serious difficulties in arriving at a definite conclusion that no antecedent case has been concerned in the production of a given fever occurrence." (Dr. Buchanan's Inaugural Presidential Address to the Society of Medical Officers of Health, 1875.)

And here, since the Association has under consideration the spread of enteric fever specially through the agency of drinking-water, let us not be forgetful to do honour to the memory of the dead. I allude to my old friend Dr. John Snow, whose indomitable industry and patience of research, undaunted by ridicule, established, a quarter of a century ago, for us and for the world, in respect of cholera, the doctrine of the transmission of bowel-contagium through this medium. Had it not been for his laborious inquiry, it is not likely that our knowledge of the etiology of enteric fever would have been what it is now. He opened the gate and showed us the road. Not only so; he showed us how to tread it in safety.

The object I have in writing this paper is to recall to the memory of the Association some of the results of work done in this direction, demonstrating the difficulties which surround endeavours to trace the

ancestry of the enteric fever contagium, indicating some previously unsuspected ways in which water may come to contain it, and drawing the moral of untiring patience of research and caution in deduction. I will be brief (for the time allotted to me is short); and I will draw my illustrations almost wholly from my own experience or from that of my colleagues in the department which I have the honour to serve.

1. I need not go far back for my first illustration; it is an instance of one of the most admirable pieces of work yet done in the department. In the early part of the current year, my colleague Dr. Thorne Thorne was instructed to inquire into an outbreak of enteric fever at Caterham in Surrey and in its neighbourhood, which was known, in the course of about six weeks, to have attacked three hundred and fifty-two individuals, rich and poor. Following the *via exclusionis*, the favourite method in the department, and indeed the only one applicable to such difficult inquiries, Dr. Thorne spotted the water supplied by the Caterham Waterworks Company as the medium through which the disease had been communicated; and the question then to be solved was, how this water itself came to be polluted with the specific poison of the fever. Here was the difficulty. False scents presented themselves, leading in various directions; it was long before the true one was found. In an early stage of the inquiry, Dr. Thorne's curiosity led him to descend one of the wells which furnished the water; and, having his eyes wide open, he noticed that any article let down by a rope, by its oscillation to and fro, came into constant and somewhat violent contact with the walls of the well. When he made this apparently trivial observation, he certainly could have had no notion of its importance or of what it was ultimately to lead to. The well was under repair; a new adit was being constructed to a new bore which was being sunk; and a number of men were employed, some on the surface, and others in the well below. This fact afforded a new direction for the inquiry, and the investigation pushed in this direction proved fruitful. One of the men who worked below, it was believed, had been ill; but no one knew anything about his illness. With much trouble, this man was sought out and discovered; and then the following strange history became disclosed. The man had had opportunities of contracting fever elsewhere; and on January 5th, on which day he was at work, the first symptoms of fever, and among them diarrhoea, appeared. Notwithstanding his illness and persistent frequent diarrhoea (sometimes his stools ran from him), he continued to work in the well for fifteen days, using for the relief of his bowels the buckets by which the excavated chalk was drawn by the rope to the surface. According to the man's statement, the bucket was used by him as a closet when it was empty, when half-full, and when full; and the man admitted that some of the contents of the bucket frequently fell over into the water below. On further inquiry, the man added that some portions of his evacuations probably did so also; and he further stated that the looseness of his bowels was such that the bucket itself must almost of necessity have been stained with them. "Here, then, were the stools of an enteric fever patient, from about January 5th onwards, getting into the company's water, and distributed with that water to the district served by the company." The first case of fever from this cause occurred on January 19th, just fourteen days from the commencement of the workman's illness.

2. In November 1873, an outbreak of enteric fever occurred among the students at Caius College, Cambridge, with special incidence on a portion of the buildings which had only been erected about four years. Caius College is a peculiarly medical college, and not only were the sanitary arrangements excellent generally in the establishment, but "such an amount of care and thought was bestowed upon the structural arrangements in the new buildings at the time of their erection, that it appeared almost impossible for any injury to health to occur from mischances of sewers, drains, or water-pipes". Yet out of sixteen cases of fever among the one hundred and twelve residents in the College (for none residing out of College suffered), no fewer than twelve were among the residents in Tree Court, the new buildings referred to. Fever had been more than usually prevalent in the town for about four months; but, although this fact might have led to an explanation of an indiscriminate distribution of cases among the students residing within and outside the College, it would not, of course, explain the special incidence referred to. All the more usual ways of spread of enteric fever, including that through milk, were severally passed under review, closely scrutinised and dismissed one after another, as inadequate to explain the intensity of incidence of the fever upon Caius College, or its remarkable incidence on the new buildings in Tree Court. The puzzle remained still unsolved, when it occurred to Dr. Buchanan that, notwithstanding the good character of the general supply of water given by the water-company, there might still be some local contamination in the local service. A surrounding five-inch main supplied water to the College on the constant system at six different places, but one of the

* Read in the Section of Medicine at the Annual Meeting of the British Medical Association in Cork, August 1879.

supplies from it was for Tree Court, and Tree Court only. Dr. Buchanan was also early "struck with the circumstance that, while every water-closet in the old part of the building was provided with a cistern proper to itself, the closets in the Tree Court buildings were supplied with water direct from the high pressure constant service pipes. Here was the clue, the importance of which quickly became evident." Soon, Dr. Buchanan learned that from time to time air had come mixed with water from the taps, notably from those of Tree Court, which, notwithstanding the presumed constant service, could only have occurred from some real intermission of supply; and this intermission must have gone to the extent of allowing the whole horizontal main service of Tree Court to become emptied of water, if any hypothesis of mischief resulting from this air in the water-pipes was to be upheld. Inquiring further, complete intermission was found to have actually occurred on two occasions, the dates of which were determinable, viz., on October 25th, and again on November 1st. Now, a fortnight is about the incubation time of enteric fever. A fortnight after October 25th is the date of the first attack in Tree Court. A fortnight after November 1st is the date of the second, third, and fourth attacks. The prospect was already brightening. The closets of Tree Court alone were unprovided with special cisterns, and hence this part of the College alone ran the risk incidental to the omission of this arrangement; but, of these closets two only were directly connected with the service that furnished drinking-water for every staircase in the court. The interest chiefly centred upon one of these two, which was situated on the first floor; the other was in the basement. The scope of the inquiry was narrowing. What was the fault here? A strict investigation of the arrangements disclosed it, and cleared up the mystery. When the pressure of water was intermitted, a supposed automatic valve-tap at the foot of the staircase on which these closets were situated clearly failed to fulfil its intention of preventing air from entering the horizontal main. On such an occasion, it only needed the water-valve of the upper closet to have been open or leaking, while in the basement the water-valve of the closet, or the tap over the adjoining sink, was also open or leaking, in order to cause the entire horizontal main of Tree Court to have become filled with air containing water-closet vapours, afterwards to be delivered in drinking-water. Dr. Buchanan further showed that, from the construction of the closet, any such water-closet air "must have been essentially sewer-air". This would have been enough for some people, but not for Dr. Buchanan, who succeeded in ascertaining that the particular sewer into which the closets discharged themselves and from which the air mingled with the drinking-water was derived, was actually contaminated by having received, in the month of October, the excreta from known cases of enteric fever in a neighbouring part of the town. Diligence and patience met their reward.

Passing now from town to country supplies of water, I will offer a few illustrations from my own experience.

3. The first shall be an instance of pollution of a river water-supply to a village. In the summer of 1872, an epidemic of enteric fever prevailed in the little village of Nunney, near Frome, the inhabitants of which mostly took their drinking-water from a brook, which traversed the village after passing through a steep gorge just above it. This stream was habitually foul from receiving the sewage of the place and of a hamlet above the gorge; yet for twenty-eight years no epidemic fever had occurred. The general filthiness of the stream was no greater this year than in former years. The facts elicited left no doubt that the river-water used for drinking was the medium of spread of the disease. The interest of the inquiry lay, therefore, in the differentiating of the circumstances of this and former years. It turned out on inquiry that, in the month of June, a man ill with fever had come from a distance and had taken up his residence in a cottage on the summit of the gorge above the village, and that his evacuations had been deposited on an ash-heap at the very edge of the gorge. On July 6th, a heavy rainfall, capable of washing down some of this excremental matter into the brook below, occurred, and it was the first such rainfall that happened after it is certain that the excrement was so deposited. On July 20th, a singing-master at Frome, who had taken a meal in the village on the 10th (four days after the heavy rain), was attacked with fever. But for a knowledge of the history of this man's proceedings, and of what had happened on the gorge, his case might easily have been taken to have been one of *de novo* origin. In the meantime, other people had been attacked with fever in the cottage on the gorge, and their evacuations had been similarly dealt with. On July 25th, a thunderstorm occurred, which must certainly have carried more of the specific matter into the brook, and probably much more than previous more gentle rains. Twelve days thereafter, the first burst of fever occurred in the village below, in five families who drew their water from the brook at nearly the same spot. Before the epidemic was over, there had been seventy-six cases of the disease in a population of about eight hundred persons.

4. This is an illustration of a similar course of events, but in a village which draws its supply of water from wells sunk in the oolite of the Cotswolds. The accident occurred in the village of Hawkesbury Upton, consisting of a long street running down the slope of a hill. The oolite rock here is deeply, and in many places much and widely, fissured, and into this rock are sunk, often in close proximity, the privy-cess-pools and also the wells from which the water used for all purposes is drawn. It is significant that the inhabitants rejoiced in the convenient fact that their privies very rarely, if ever, required to be emptied or cleaned. The estimated population of the village is about 660. In this village, and among children from the neighbouring country attending school there, at least 113 cases of enteric fever occurred in the spring and summer of 1872. The habitual excremental pollution of the well-water cannot be questioned, yet for ten years at least previously there had been no remarkable prevalence of enteric fever, that is to say, any cases that had occurred had not spread extensively. But in February 1872, a man ill with enteric fever arrived in the place from Bristol, and passed through his illness at a cottage at the upper end of the village, and his evacuations were thrown into a privy cesspool situated only fifteen yards from a well, from which several families at this end of the village drew their drinking-water. The first families subsequently attacked were such as used this water, and of these, a very large proportion were invaded. Children from these families went to the national school (also at the upper end of the village) during early periods of their illnesses, and no doubt infected the privy-cesspool, and from it the well-water there, since the next persons attacked were children attending the school, and notably those who came from a distance and took their midday meals at the school. These children infected by their evacuations other privies and wells, and in this way the fever spread through the village and into neighbouring villages and hamlets.

Let me, in this connection, allude to the irregular course which enteric fever is apt to follow in children, so irregular that the nature of the ailment or illness, acquired perhaps by a single casual draught of infected water or milk at school or away from home, may pass unrecognised; yet such a case may be the means of introducing the contagion of the disease into an isolated house, thus giving occasion to the suspicion of a *de novo* origin of an unquestionable case of fever in it.

5. The case I am now about to mention illustrates the readiness with which a *de novo* origin might have been assumed by an investigator who had not received the correct clue to the cause of the accident. In the country, about a mile from Moseley, a suburb of Birmingham, in an isolated row of five cottages, an outbreak of fever occurred in January and February 1873, and the fever invaded four out of the five cottages; the cottage which escaped invasion being occupied only by three labouring men, while the other cottages contained women and children. Suspicion fell upon a well in the back yard, but it was not at first clear how this could have become specifically infected. The first case that occurred was in a boy, whose exposure to the contagion away from home was, from circumstances, improbable. This boy was attacked on January 4th, so that he probably received the contagion from ten to fourteen days previously. He, and the others who were subsequently attacked, drank of the water from the well in the back yard, across the imperfect wooden cover of which ran a slop-channel constructed of pairs of bricks laid angularly, the intervening space of one and a half inches only being occupied with a mixture of clay and ashes quite permeable by water. Now it turned out that a laundress lived in one of the cottages, and that the washing-slops were habitually thrown down this channel, and ran away by it across the top of the well. Habitually slop-water, then, fouled by nastiness of all kinds, must have found access to the well-water, yet no fever had occurred previously to this outbreak. The source of the infection became accidentally discovered by my friend Mr. Ross Jordan of Moseley, who, in the month of December, had been in attendance upon a fatal case of fever in the person of a servant in a private residence in Moseley. The case was one of a series of cases from infected milk, occurring during an epidemic that I was at that very time investigating, and I had myself seen the servant during her illness. This girl's linen, fouled with her diarrhoeal discharges, had been sent to the laundress in question, and had been washed by her, and the slops from the wash-tub had been, as usual, thrown down the channel above described. The condition of the linen had been observed, but no particular thought had been bestowed upon it. But the specific matter that fouled it had furnished the one element needed to originate the fever at the cottages. As it happened, the source of the infection of the well was traced; but how would it have been, and what inference might have been drawn, under other circumstances less favourable to the inquirer?

6. Lastly, let me direct your thoughts to an epidemic of fever which prevailed at Ascot for the long period of four years and a half, namely, from July 1873 to November 1877, and which only came to an end

when certain alterations were made in respect of the drainage and pump of a dairy-farm, the supply of milk from which I clearly demonstrated to have been, for all that long period, the agent in the spread of the fever. In respect of duration, for a milk-caused epidemic, the case is unique, but I am not going to trouble you with this aspect of it. No case of fever had occurred at the dairy-farm, which was isolated about half a mile from the village of Ascot, until August 5th, 1876, that is to say, until the epidemic produced by the milk sent out from the farm had lasted for three years. From this time onwards, the water used for washing and rinsing the milk-cans had an opportunity of specific pollution, inasmuch as a careful investigation showed that, in consequence of defects in masonry and woodwork, drain-air had an opportunity of passing from the privy-cistern, which received the specific excreta, through drains, by a circuitous course into the pump-case, and also to a sink within the house, over which sink the cans were habitually inverted to drain. But this circumstance would not account for the infection of the milk from July 1873 to August 1876. I failed, on the closest inquiry, to discover that any person suffering from fever had, during these three years or previously, been upon the premises of the dairy-farm, nor could I discover how contagium from a distance could have reached the farm, except it had reached it in some way through the water that entered the well in the yard. So far as the method of exclusion was practically applicable, I seemed tied down to this agency, or to some untraceable accidental introduction of contagium, unless I could admit that the contagium had originated *de novo* in the filth which had accumulated in the drains, and in the heap of fermenting cow-dung and offensive matters which occupied much of the surface of the farmyard. There was filth everywhere, and slovenliness reigned supreme. Some medical men would probably have been satisfied that they had here fallen upon the *fons et origo mali*; I confess that I was not; the difficulties in admitting such an explanation appeared to me to be as great as, if not greater than, those attending the suggestion which I have hazarded. It is mainly based upon the geological conformation of that part of the country, and upon the fact that the first cases, those which initiated the epidemic, were imported cases, being offshoots of the great Marylebone milk epidemic of 1873. One of these cases occurred at the summit of the hill on which Ascot stands, and the evacuations were thrown carefully away, at a distance from the house, into a hole in the earth, and some of the earlier evacuations also probably found their way into the earth from some imperfect drains into which the water-closets discharged themselves. Now it is to the nature of this earth that I must call your attention. Ascot is situated on a hill formed of the lower Bagshot sand, beneath which lies the London clay. When the sand is dug into, it is found to become wetter and wetter, as the clay is approached, until at last it is found so supersaturated with water as to be no longer coherent, and, in fact, a water-layer is arrived at, into which all the wells of the district, including that of the dairy-farm in question, are sunk. The place where the case which I suggest to have been the origin of the milk outbreak occurred, is on the top of the hill, while the dairy-farm is half a mile off near the bottom of the hill to the northward; in the direct line, in fact, in which I am assured by the geological authority I consulted, the subsoil water most probably flows, especially when the quantity of water is augmented by the imbibition of rain. It is impossible, in a short account like this, to go into the details of my argument; but what I have suggested is, that the specific matter thrown with the evacuations into a hole in the very permeable sand, at a place not very far above the water-layer, would, with the help of rain also running into the hole, have scarcely failed to reach the water-layer beneath, and, having reached it, would have become more or less diffused in it, and, in the course of time, that the movement of the mass of subsoil-water might have brought it to the well at the farm. Theoretically, such an occurrence under the circumstances does not appear to be beyond the bounds even of probability; certainly it is quite possible. As a matter of fact, the dates at which the earlier cases of the milk outbreak occurred, coincide sufficiently in point of time with such an explanation, if we allow six weeks and upwards for the contagium to travel in the subsoil water a distance of half a mile.

I admit that the suggested explanation is somewhat startling; some may regard it as far-fetched, and be disposed at once to throw it over. I shall not quarrel with them if they do, although perhaps experience in etiology might suggest the wisdom of a different course. Let me commend to any such the consideration of an event that happened at Lausen in Switzerland in 1872, which has been recorded by Dr. Frankland for our instruction. Two parallel valleys, distinct from each other about one mile, are separated by a mountainous ridge. In one valley, through which a brook flows, several cases of fever occurred in an isolated farmhouse in the months of June, July, and August, and the brook-water, which at that time was made to irrigate the meadows, was

polluted with the infected dejections. It was known that the spring which supplied the drinking-water at Lausen, in the other valley, communicated with this brook through the intervening mountain; but whether by a fissure or by filtration through a loose soil, the experiments made to determine the point do not determine satisfactorily to my mind, although Dr. Frankland appears to believe it was by filtration. However this may be, about three weeks after the annual irrigation of the meadow, a sudden severe explosion of enteric fever occurred at Lausen, sparing, however, six houses which were supplied, not like the rest from the spring, but from private wells. Even assuming the Swiss case to have been one of communication by means of a fissure, the Ascot conditions are not altogether incomparable, inasmuch as the water at the depth of forty feet or more from the surface can scarcely be said to filter through sand, but must rather be regarded as a layer of water mixed with sand, so loosely aggregated as scarcely to be capable of exercising any filtering operation upon it at all. The well-sinkers call it a "running sand".

I must apologise for the manifold imperfections of this paper, on the ground of its having been written at intervals of pressing and absorbing official work.

THE ETIOLOGY OF ENTERIC FEVER.

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III.

THE objection usually urged against the proposition that enteric fever is contagious, arises from the fact that it rarely spreads to nurses in hospital. It has been compared with typhus, for which it was long mistaken; and because, unlike typhus, it comparatively rarely affects persons over the age of thirty, it has been concluded that it is not contagious. This is, in my opinion, a mistake. No one doubts that scarlatina is contagious, and that in a very high degree, even although it rarely spreads to nurses in hospital. Thus, whilst in the London Fever Hospital, during the years 1861-78, one hundred and ninety-five of the staff contracted typhus, only fifteen contracted scarlatina. To this it will be replied that the latter prevails in infancy, childhood, and adolescence, and that, therefore, the majority of adult persons are protected. It appears to have been forgotten that, for the same reason, enteric fever comparatively rarely spreads amongst nurses in hospital. It also is a disease of infancy, childhood, and adolescence, and by reason of this a very large proportion of adults, in particular of adults over thirty, are protected. It is for this reason that Dr. Wilks and others whom Dr. Murchison quotes have never seen the disease arise in hospital; and it might very well be that, with a nursing staff whose age ranged from thirty to fifty, and exposure to the few cases which find their way into the general hospitals, this were a rare occurrence, in the experience of many physicians, and yet might prove nothing against the view that enteric fever is contagious. That this experience is not universal we shall see as we proceed; but I would here direct attention to the importance of considering the element of age in etiological inquiry concerning enteric fever; for which purpose I have compiled the following statistics. (See Tables in next page.)

From these tables it appears that, whatever the cause, be it drains, stools recent or decomposed, or personal contagion, their effect is determined by the age of persons. For it may be fairly assumed that the various causes—if there were various causes—were in operation in the long period covered by the tables; so that, given a cause, whether continuously in operation or not, the extent of effect would depend upon the number of persons of the suitable or more liable age; in other words, of persons susceptible. Thus equal causative conditions would in different hospitals have different effects, and in the same hospitals at different times, varying in proportion to the varying number of the second factor—susceptible persons. Given, then, a cause of enteric fever, continuous or not, the answer to the question as to the probability that but a small number of any class of persons subjected to its influence shall be affected by it, is that the probability increases with the age of the persons of that class. In accordance with the law which the tables indicate, if the age of the persons were over thirty and verging upon forty, the probability of a small number would be great; whereas, if the age of the persons fell short of twenty-five, the probability of a small number would be very slight. This element of age Mr. Murphy