

similar institutions, and in devising the most suitable means of diminishing the preventable causes of disease, which the present advanced state of medicine, as a science, enables us to anticipate with confidence.

This is the merest sketch of the principle which should guide improvements in the Indian army. It would be easy to work out such a scheme in detail, were it advisable in the present instance to do so.

With regard to honours, if it be deemed not desirable to bestow an occasional baronetcy or knighthood on any officer of very superior and decided merit, upon the recommendation of the Governor-General of India, the honours of the Bath should be opened to the military surgeon for service in the field, as freely as to other branches of the army.

For distinguished services in the civil ranks of the profession, it would not be difficult to devise a suitable badge of distinction, upon the principle of the corresponding branch of the Legion of Honour in France. Ribbons and crosses are the cheap rewards of governments. "The reward of individual services," says a recent writer on this subject, "is an important part of the debt due by the State to its officers; but its importance does not end here. For one individual marked out for reward and distinction, we make a hundred others emulous of similar rewards, and eager, through the exercise of their talents, and the amount of their services, to deserve similar recompences."

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#### REVIEW XIII.

*Mode of Communication of Cholera.* By JOHN SNOW, M.D.  
Second Edition.—London, 1855. pp. 162.

NONE of our readers can be ignorant of the opinions of Dr. Snow on the communication of cholera by means of drinking water, nor of the perseverance and energy with which he has sought for facts to corroborate this view. The present work is a recapitulation of all the evidence he has hitherto published, with the addition of certain facts lately acquired.

We have read this work carefully, and shall endeavour, in the following critique upon it, to do full justice to Dr. Snow, while we shall strictly examine, as it is our duty to do, if there is anything hollow or unsound in the facts brought forward, or in the arguments founded upon them.

Dr. Snow believes not only that cholera is propagated by means of water, but that it is solely and exclusively so propagated. He is therefore obliged, at the very outset of his inquiry, to assume that cholera only spreads where human intercourse is possible. Thus he writes,

"It travels along the great tracks of human intercourse, never going faster than people travel, and generally much more slowly. In extending to a fresh island or continent, it always appears first at a sea-port. It never attacks the crews of ships going from a country free from cholera, to one where the disease is prevailing, till they have entered a port, or had intercourse with the shore. Its exact progress from town to town cannot always be traced; but it has never appeared except where there has been ample opportunity for it to be conveyed by human intercourse." (p. 2.)

We do not wish to argue the several clauses of this paragraph, but we do most decidedly protest against its reception, as a complete and final expression of the mode of spread of cholera. While we admit at once

that there are now several cases which show human intercourse to be occasionally influential in some way in transmitting cholera, we deny altogether that the phenomena, either of its rise or decline, can be always, or even frequently, so explained.

But our object now is not to discuss the general question of the contagion of cholera, but to see the strength of Dr. Snow's evidence on one particular presumed mode of propagation.

At page 10, Dr. Snow informs us that he was led to his view of the spread of cholera by a consideration of its pathology. He believes that the symptoms commence in the intestinal canal, and (apparently) that the disease is entirely local, and that neither the blood nor the nervous system are primarily affected. Having arrived at this opinion on grounds which appear to us insufficient to warrant so grave a conclusion, he writes as follows:\*

"As cholera commences with an affection of the alimentary canal, and as we have seen that the blood is not under the influence of any poison in the early stages of this disease, *it follows that the morbid material producing cholera must be introduced into the alimentary canal*—must, in fact, be swallowed accidentally, for persons would not take it intentionally; and the increase of the morbid material, or cholera poison, must take place in the interior of the stomach and bowels." (p. 15.)

We cannot admit the cogency of the *must* in this quotation; since we do not see that it is satisfactorily made out that the blood is "not under the influence of a poison."

Dr. Snow then goes on with the following sentence, for which we can find no warrant in anything which has gone before; it appears to us to be a mere hypothesis.

"It would seem that the cholera poison, when reproduced in sufficient quantity, acts as an irritant on the surface of the stomach and intestines, or, what is still more probable, it withdraws fluid from the blood circulating in the capillaries, by a power analogous to that by which the epithelial cells of the various organs abstract the different secretions in the healthy body." (p. 15.)

Dr. Snow afterwards says, that there is sufficient "to prove the communication of cholera here explained, independently of the pathology of the disease." (p. 16.) We do not hesitate to say that the argument from pathology seems of little value, and it somewhat lessens our faith in Dr. Snow, to find that so doubtful a view, to say the least of it, should have suggested a special opinion of the propagation of the disease.

However, this is of little moment, and we proceed to the grand object of the work, and of this review—viz., the evidence for the propagation by water, and an examination of it.

From page 16 to page 22, Dr. Snow makes general reflections on the habits of the persons among whom cholera chiefly prevails, in order to show that their want of cleanliness would render it likely that portions of cholera evacuations might get mixed with their food or drinking water. We pass over this as not of great weight, and arrive, at page 22, at the first case in which direct evidence is sought to be given in favour of the hypothesis.

1. In two small adjacent courts in Horsleydown (Surrey-buildings and

\* In this and in subsequent extracts we have italicized some passages which contain the pith of the sentence, or which appear to us illogical.

Truscott's-court), cholera prevailed (in 1849) with very unequal severity, 11 fatal cases occurring in Surrey-buildings, and 2 cases (1 fatal) in Truscott's-court. In all other respects similar (according to Dr. Snow), the only difference was in the water supply; the well supplying Surrey-court was contaminated by the slops and dirty water in which the clothes were washed being poured by the inhabitants into drains which communicated with the well; while the water of Truscott's-court was free from this impregnation. A curious fact was, that all the fatal cases were in women and children.

Admitting Dr. Snow's hypothesis would explain all the other cases, we must account in some different way for the first case. The mode in which this is done shows Dr. Snow's perfect faith in his creed; instead of leaving the origin of the first case uncertain, he says the "two first cases were probably caused by the cholera evacuations contained in the Thames water, as it came from the waterworks." (p. 24.) Considering that this is the very point to be proved, we think the probability of this alleged mode is rather too easily admitted.

We visited these courts in 1849, not long after Dr. Snow's visit, and found that the number of the population of Truscott's-court was very much below that of Surrey-buildings. We have not been able to find the note made on the point, but we distinctly remember that the disproportionate mortality in the two courts was in this way partly, though perhaps not altogether, explained.

We again visited these courts a few weeks ago, and found the differences between them to be as great as we had conceived them to be. Thus, Surrey-buildings consists of fifteen houses with four rooms each, while Truscott's-court contains only ten houses with two rooms each. There are, therefore, sixty rooms in the first-named court to twenty in the second; and if we suppose the density of population to be two to each room, there would be 120 persons in Surrey-buildings to 40 in Truscott's-court; and if the cholera had been equally severe in the two courts, it would of course have affected three persons in one, to one in the other. The disproportion is thus partly, but not altogether, removed. Truscott's-court, however, with its small, low houses, is much more airy than its neighbour; it is close to an unoccupied piece of ground, of the benefit of which Surrey-buildings, facing the other way, and having no through ventilation, is deprived. We do not think that any one seeing these two courts, and knowing how much accumulation of persons in the same house tends to aggravate cholera, and how ventilation checks it, but would see in these circumstances an explanation, as satisfactory as that of contaminated water, of the difference in the mortality which remains unexplained by the difference in population.

2. The next case is that of Albion-terrace, Wandsworth, where, in 1849, cholera prevailed in a single row with considerable severity. Dr. Snow informs us that

"The water got contaminated by the contents of the house drains and cesspools. The cholera extended to nearly all the houses in which the water was thus tainted, and to no others.

"These houses were numbered from 1 to 17, in Albion-terrace, and were supplied with water from a copious spring in the road in front of the terrace, the

water of which was conducted, by a brick barrel-drain between Nos. 7 and 8, to the back of the houses, and then flowed right and left, to supply tanks in the ground behind each house, the tanks being made of brickwork and cement, covered with a flat stone, and connected with each other by stoneware pipes six inches in diameter. A leaden pipe conveyed water from each tank to a pump situated in the back kitchen. There was a cesspool behind each house, under the privy, and situated four feet from the water-tank. The ground was opened behind the houses No. 1 and No. 7, and the drains examined under the superintendence of Mr. Grant, the assistant-surveyor to the Commissioners of Sewers. The cesspools at both these places were quite full, and the overflow-drain from that at No. 1 choked up. At this house the respective levels of the cesspool and the water-tank were measured, and the top of the overflow-drain from the cesspool was found to be fifteen inches above the top of the tank, and the intervening ground was very wet. The overflow-drain mentioned above had no bottom, or one so soft that it could be penetrated with a stick; and it crossed, at right angles, above the earthenware pipe of the water-tank, the joints of which were leaky, and allowed the water to escape. Behind No. 7, Mr. Grant found a pipe for bringing surplus water from the tanks, communicating with a drain from the cesspool; and he found a flat brick drain laid over the barrel-drain before mentioned, which brought the water from the spring. It appears, from a plan of the property, that this drain, which was continued in a direction towards the sewer in Battersca-fields, brought surface-drainage from the road, and received the drains from the cesspools, the house-drains from the sinks in the back kitchens, and the surplus water, or some of it, from the tanks. There was every reason to believe that this drain was stopped up, but that was not ascertained: at all events, it was unable to convey the water flowing into it during the storm on July 26th, as it burst near the house No. 8, and inundated the lower premises of that and the adjoining house, No. 9, with fetid water; and it was from this time that the water, which had occasionally been complained of before, was found by most of the people in these seventeen houses to be more or less impure or disagreeable." (pp. 25-7.)

And he gives a description of the drains and water pipes, to explain how this took place.

The first person was attacked on July 28th:

"There are no data for showing how the disease was communicated to the first patient, at No. 13, on July 28th; but it was two or three days afterwards, when the evacuations from this patient must have entered the drains having a communication with the water supplied to all the houses, that other persons were attacked, and in two days more the disease prevailed to an alarming extent." (p. 28.)

The premises were examined by Dr. Milroy, who attributed the outbreak to an open sewer, to effluvia from sinks, and to an accumulation of offensive rubbish, in the house in which the first case occurred. Dr. Snow considers, and perhaps correctly, this explanation to be incorrect.

We must observe, however, that in addition to the impregnation of the water, it appears that there must have been, immediately before the attack of cholera, contamination of the air also, as on the 26th of July, two days before the first case, the lower premises of two houses were flooded with fetid water from the blockage of the drain; and doubtless the same effect was produced, in a less degree, in the other houses. Although we are not informed how long this stagnant water remained, it would probably, for some days, be sufficient to render the air impure. We are led to believe, from Dr. Snow's account, that the water had been previously contaminated by faulty arrangement of the drain and water-

tanks, and yet no decided cholera seems to have occurred till this overflow, two days after which the first case (the origin of which Dr. Snow leaves doubtful) took place.

One fact mentioned by Dr. Snow strengthens our suspicion that the air may have had as much to do with it as the water, for it is mentioned, that

“There were two or three persons attacked with cholera amongst those who came to nurse the patients after the water was condemned, and who, consequently, did not drink it; but these persons were liable, in waiting on the patient, to get a small portion of the evacuations into the stomach in the way first pointed out; and there might be food in the houses previously prepared with the tainted water.” (p. 29.)

These conjectures as to how the cholera matter got into the stomach of the nurses who did not drink the water, appear to us to be rather out of place. The point is to prove the fact of water being the agent, and not to assume it, and then to seek for some other explanation of those cases for which the presumed contamination cannot account.

3. Dr. Snow then quotes from Dr. Lloyd two instances in which cholera prevailed in limited districts, where water contaminated with sewage matter was used. The first instance is very inconclusive; the second is the following:

“Charlotte-place, in Rotherhithe, consists of seven houses, the inhabitants of which, excepting those of one house, obtained their water from a ditch communicating with the Thames, and receiving the contents of the privies of all the seven houses. In these houses there were twenty-five cases of cholera, and fourteen deaths; one of the houses had a pump railed off, to which the inhabitants of the other houses had no access, and there was but one case in that house.” (p. 31.)

In this example, as in almost all the other cases adduced by Dr. Snow, we miss the very necessary information as to the number of persons resident in each house; their ages, occupations, and habits; the kind of house in which they lived, &c. In six houses there were altogether twenty-four cases of cholera, in the seventh house only one case. For anything we are told to the contrary, however, there may have been only a single case in one of the six houses, and a greater number than the average in some of the others. If this were so, the point and force of the argument at once disappears.

4. Another case, quoted from the Board of Health, is stronger:

“In Manchester, a sudden and violent outbreak of cholera occurred in Hope-street, Salford. The inhabitants used water from a particular pump-well. This well had been repaired, and a sewer which passes within nine inches of the edge of it became accidentally stopped up, and leaked into the well. The inhabitants of thirty houses used the water from this well; among them there occurred nineteen cases of diarrhœa, twenty-six cases of cholera, and twenty-five deaths. The inhabitants of sixty houses in the same immediate neighbourhood used other water; among these there occurred eleven cases of diarrhœa, but not a single case of cholera, nor one death.” (p. 31.)

5. A fifth instance is mentioned, on the authority of Dr. T. K. Chambers:

“Dr. Thomas King Chambers informed me, that at Ilford, in Essex, in the summer of 1849, the cholera prevailed very severely in a row of houses a little way from the main part of the town. It had visited every house in the row but one. The refuse which overflowed from the privies and a pigstye could be seen

running into the well over the surface of the ground, and the water was very fetid; yet it was used by the people in all the houses except that which had escaped cholera. That house was inhabited by a woman who took linen to wash, and she, finding that the water gave the linen an offensive smell, paid a person to fetch water for her from the pump in the town, and this water she used for culinary purposes, as well as for washing." (p. 32.)

We think we ought to be careful how far we attach great weight to an argument of this sort; we are not informed how many persons lived in the house with this woman, how many houses there were in the row, and other circumstances of the like kind. If the woman was the only inhabitant of the house, as we are led to conclude from the mode in which the sentence is worded, her escape would have nothing wonderful about it, as only the minority are attacked with cholera.

6. The next case is one mentioned to Dr. Snow by a friend. Cholera prevailed at Locksbrook, near Bath, and the owner of some houses was informed that the water was bad:

"He sent a surveyor, who reported that nothing was the matter. The tenants still complaining, the owner went himself, and on looking at the water and smelling it, he said that he could perceive nothing the matter with it. He was asked if he would taste it, and he drank a glass of it. This occurred on a Wednesday; he went home, was taken ill with the cholera, and died on the Saturday following, there being no cholera in his own neighbourhood at the time." (p. 32.)

Thus the surveyor finds nothing wrong, and the hapless owner is so convinced that the water is good, that he drinks a glass of it, and dies three days afterwards; therefore it was the water.

Nothing could be very much looser than this statement; we might just as well affirm that the man having been in the very place, and in the very houses, where cholera was, received it, as we believe many persons do receive it, through the medium of the air. Dr. Snow will say that the other evidence in proof of the propagation by water gives weight to his view of the case, but we may quite as well say that the evidence by transmission through the air is much more stringent, and gives weight to our hypothesis.

7. The next case appears to us to be quite worthless as evidence. The village of Newburn, near Newcastle-on-Tyne, suffered frightfully from cholera in 1832, at which time the sources of water-supply were the same as at present. Dr. Snow applied to Mr. Davison, surgeon, of Newburn, who informed him that—

"It was considered that the evacuations of the people could not get into any of the wells; but the vicar thought that the water of a little brook which runs past the village, and falls into the Tyne immediately afterwards, might find its way into that well which is chiefly resorted to." (p. 33.)

On further inquiry, it was found that the brook received "the refuse of a small village," and also passed "through a privy used by the workmen of a steel factory." The drain conveying water from an old coal-pit to the well chiefly used in Newburn passed underneath the brook, and then ran alongside it for about three hundred yards:

"Mr. Davison said that it was disputed whether there was any communication between the drain and the brook, but that it was highly probable that there might be; and that an occurrence which took place a few months previously seemed to prove that there was. Some gas-water from the steel manufactory mentioned

above got by accident into the brook, and *some of the people affirmed that the water in the well was strongly impregnated with it.*" (p. 34.)

Thus the water of the brook, it is thought, *might* find its way into the drain leading to the well. Although no communication can be traced between the two, the affirmation of *some* of the people, that the well had been impregnated with gas-water some months before, is taken as conclusive evidence that there was such communication during the time of the cholera: that conclusion arrived at, the first case of cholera occurs; it is left unaccounted for, but from it, it is easy to deduce all the rest.

"As several days elapsed between the first case of cholera and the great outbreak, it is probable that the water in which the soiled linen must have been washed, and which would necessarily run into the brook, was the means of communicating the disease to the thirteen persons taken ill on the night between the 9th and 10th of January." (p. 35.)

This surely cannot be considered as sound evidence. The mere *possibility* of the well being contaminated cannot be held sufficient in an inquiry demanding such accuracy and care in the collection of the facts.

8, 9. After an allusion to a very obscure Indian case, of which nothing can be made, Dr. Snow refers to the late attack of cholera in the Black Sea fleet. He quotes from the 'Medical Times and Gazette,' September 30th, the following passage from the letter of a naval medical officer:

"A week after the return of the fleet to Baljik, on the 7th of August, about four thousand French troops encamped on the heights abreast our anchorage. These were part of the first division of the army that had marched to Kostenje, about ten days before. By it the first blood had been drawn on the part of the allied army. The loss in battle was small, but they had encountered an enemy more terrible than the Russians. The cholera had broken out among them, and attacking four hundred on the first night, had destroyed sixty. The total loss had been something incredible. It was said, that out of eleven thousand men, not less than five thousand had perished in a few days. This dreadful calamity was attributed to drinking water from wells that had been poisoned by throwing in putrid carcases.

"Putting aside the question of intentional poisoning, which always presents itself as the most ready way of accounting for such destruction, perhaps some support to the theory, that water is the medium by which cholera poison is conveyed, may be found in this circumstance, and in another of which I was witness. These soldiers, wearied by marching from a focus of cholera infection, were seen, many of them, washing their persons and clothing in the stream from which all the French ships of war, and the majority of the English fleet, obtained their supply of water. This was going on on the 7th and 8th, and, on the nights of the 9th and tenth, the disease burst out with great violence among the crews of several ships." (pp. 36, 37.)

We do not find this point alluded to in the 'Report' on this attack of cholera, just published by the Admiralty. It would appear, from that able document, that the main outbreak of cholera appeared in the fleet at Varna and at Baljik nearly at the same time. At the latter place, the intense outbreak was certainly from four to six days after the French troops had marched down; but it would appear that a solitary case had occurred on board the *Diamond* on the 16th of July, and that bowel complaints, gradually assuming a more severe form, and at last with "decided choleraic character," were common on board the fleet between the 1st and the 7th of August—i.e., the day that the French troops arrived to whom the outbreak is attributed—and on the 7th, a fatal case

occurred on board the *London*. The evidence, therefore, that connects these two circumstances—the arrival of French troops and the outbreak on board the ships—is not so stringent as it seems. But even if the French troops brought the cholera with them, the evidence of its being communicated by the water is most imperfect.

10. Dr. Snow makes no remark on the suggestion contained in the letter quoted in the 'Medical Times,' but proceeds to consider the terrible outbreak of cholera in a limited district near Golden-square, London, which occurred in 1854:

"There were a few cases of cholera in the neighbourhood of Broad-street, Golden-square, in the latter part of August; and the so-called outbreak, which commenced in the night between the 31st August and the 1st September, was, as in all similar instances, only a violent increase of the malady. As soon as I became acquainted with the situation and extent of this irruption of cholera, I suspected some contamination of the water of the much-frequented street-pump in Broad-street, near the end of Cambridge-street; but on examining the water, on the evening of the 3rd September, I found so little impurity in it of an organic nature, that I hesitated to come to a conclusion. Further inquiry, however, showed me that there was no other circumstance or agent common to the circumscribed locality in which this sudden increase of cholera occurred, and not extending beyond it, except the water of the above-mentioned pump. I found, moreover, that the water varied, during the next two days, in the amount of organic impurity, visible to the naked eye, on close inspection, in the form of small white, flocculent particles; and I concluded that, at the commencement of the outbreak, it might possibly have been still more impure. I requested permission, therefore, to take a list, at the General Register Office, of the deaths from cholera, registered during the week ending 2nd September, in the sub-districts of Golden-square, Berwick-street, and St. Ann's, Soho, which was kindly granted. Eighty-nine deaths from cholera were registered, during the week, in the three sub-districts. . . .

"On proceeding to the spot, I found that nearly all the deaths had taken place within a short distance of the pump. There were only ten deaths in houses situated decidedly nearer to another street-pump. . . .

"With regard to the deaths occurring in the locality belonging to the pump, there were sixty-one instances in which I was informed that the deceased persons used to drink the pump-water from Broad-street, either constantly or occasionally. In six instances I could get no information, owing to the death or departure of every one connected with the deceased individuals; and in six cases I was informed that the deceased persons did not drink the pump-water before their illness." (pp. 38—40.)

Dr. Snow then refers to workhouses and large establishments in the neighbourhood, which did not use the pump water, and the inmates of which escaped cholera. He then quotes the case of a gentleman who went to Poland-street, where his brother had just died of cholera, drank a small tumbler of water mixed with brandy, left the neighbourhood, and was seized with cholera on the following day. This, however, is inconclusive, as the person was in an infected neighbourhood.

Finally, Dr. Snow relates the following most extraordinary case, which, if there is not some fallacy, is certainly unanswerable:

"In the 'Weekly Return of Births and Deaths' of September 9th, the following death is recorded as occurring in the Hampstead district: 'At West-end, on 2nd September, the widow of a percussion-cap maker, aged 59 years, diarrhoea two hours, cholera epidemica sixteen hours.'

"I was informed by this lady's son that she had not been in the neighbourhood of Broad-street for many months. A cart went from Broad-street to West-end

every day, and it was the custom to take out a large bottle of the water from the pump in Broad-street, as she preferred it. The water was taken on Thursday, 31st August, and she drank of it in the evening, and also on Friday. She was seized with cholera on the evening of the latter day, and died on Saturday, as the above quotation from the register shows. A niece, who was on a visit to this lady, also drank of the water; she returned to her residence, in a high and healthy part of Islington, was attacked with cholera, and died also. There was no cholera at the time, either at West-end or in the neighbourhood where the niece died. Besides these two persons, only one servant partook of the water at Hampstead West-end, and she did not suffer, or, at least, not severely." (pp. 44—45.)

It will have been observed, that the contamination of the pump water with drains, or by any other method, is not even attempted to be proved, and the disease had commenced to decline before the supply of the suspected water was stopped.

"The greatest number of attacks in any one day occurred on the 1st of September, immediately after the outbreak commenced. The following day the attacks fell from one hundred and forty-three to one hundred and sixteen, and the day afterwards to fifty-four. A glance at the table will show that the fresh attacks continued to become less numerous every day. On September the 8th—the day when the handle of the pump was removed—there were twelve attacks; on the 9th, eleven; on the 10th, five; on the 11th, five; on the 12th, only one; and after this time, there were never more than four attacks on one day. . . .

"There is no doubt that the mortality was much diminished, as I said before, by the flight of the population, which commenced soon after the outbreak; but the attacks had so far diminished before the use of the water was stopped, that it is impossible to decide whether the well still contained the cholera poison in an active state, or whether, from some cause, the water had become free from it. The pump-well has been opened, and I was informed by Mr. Farrell, the superintendent of the works, that there was no hole or crevice in the brickwork of the well, by which any impurity might enter; consequently in this respect the contamination of the water is not made out by the kind of physical evidence detailed in some of the instances previously related. I understand that the well is from twenty-eight to thirty feet in depth, and goes through the gravel to the surface of the clay beneath. The sewer, which passes within a few yards of the well, is twenty-two feet below the surface. The water at the time of the cholera contained impurities of an organic nature, in the form of minute whitish flocculi, visible on close inspection to the naked eye, as I before stated. Dr. Hassall, who was good enough to examine some of this water with the microscope, informed me that these particles had no organized structure, and that he thought they probably resulted from decomposition of other matter. . . .

"A very important point in respect to this pump-well is *that the water passed with almost everybody as being perfectly pure, and it did in fact contain a less quantity of impurity than the water of some other pumps in the same parish, which had no share in the propagation of cholera.* We must conclude from this outbreak that the *quantity of morbid matter which is sufficient to produce cholera is inconceivably small.*" (pp. 51—54.)

Dr. Snow also observes, that he inquired of many persons if any change in the character of the water had been observed, and was answered in the negative. Mr. Gould, the eminent ornithologist, however, noticed that on the 2nd September "it had an offensive smell," although it was perfectly transparent.

The chief circumstances which Dr. Snow appears to rely upon are, that the great mortality took place in the district supplied with this pump-water, and that persons in the district who did not drink the

water escaped. He has not been able to prove that all were attacked who drank this water, and that none were attacked who did not drink; but such precision of evidence could not fairly be demanded from him.

The weak points in this array of evidence are, 1st, the want of proof of contamination of water, or, rather, the evidence in favour of its purity; 2ndly, the deficiency in negative evidence, that there was no other local cause which produced this partial outbreak; and, 3rdly, the fact that the disease ran rapidly to its acme, and then declined, while the water supply remained the same.

On examining a map given by Dr. Snow, it would clearly appear that the centre of the outburst was a spot in Broad-street, close to which is the accused pump; and that cases were scattered all round this nearly in a circle, becoming less numerous as the exterior of the circle is approached. This certainly looks more like the effect of an atmospheric cause than any other; if it were owing to the water, why should not the cholera have prevailed equally everywhere where the water was drunk? Dr. Snow anticipates this by supposing that those nearest the pump made most use of it; but persons who lived at a greater distance, though they came farther for the water, would still take as much of it. We observe also that there are several other pumps in the neighbourhood, and in one of these the water was notoriously offensive; yet comparatively little cholera took place thereabout. There are, indeed, so many pumps in this district, that wherever the outbreak had taken place, it would most probably have had one pump or other in its vicinity.

11. The last case noticed by Dr. Snow is that of a limited outbreak in Deptford, which seems to us as deficient in weight of evidence as these which have preceded it.

"Deptford is supplied with very good water from the river Ravensbourne by the Kent Water Works, and until this outbreak there was but little cholera in the town. . . . On going to the spot on September 12th and making inquiry, I found that the houses in which the deaths had occurred were supplied by the Kent Water Works, and the inhabitants never used any other water. The people informed me, however, that for some few weeks the water had been extremely offensive when first turned on; they said it smelt like a cesspool, and frothed like soap suds. They were in the habit of throwing away a few pailsful of that which first came in, and collecting some for use after it became clear. On inquiring in the surrounding streets, to which this outbreak of cholera did not extend, viz., Wellington-street, Old King-street, and Hughes's-fields, I found that there had been no alteration in the water. I concluded, therefore, that a leakage had taken place into the pipes supplying the places where the outbreak occurred, during the intervals when the water was not turned on." (pp. 55—56.)

We have now given, as far as possible, in Dr. Snow's own words, an abstract of his evidence. In estimating its weight we must bear in mind the object for which it is adduced. It is not to prove that bad water acts as a predisposing cause, but that the water contains itself the cause of cholera. To prove so weighty a fact, we require not only positive, but negative evidence. If the cause of cholera cannot be absolutely discovered in the water, we must at least have proof that the water is contaminated, and we must also have negative evidence that no other circumstance existed which could explain the attack except the contaminated water. Now, certainly in no less than seven of the eleven cases

(Nos. 3, 5, 6, 7, 8, 9, and 11), the evidence to prove the effect of the water is so loosely stated, and the accessory circumstances of the outbreaks are so utterly disregarded, that we do not think any one can feel that even a tolerable case is made out in favour of Dr. Snow's opinion.

In the four remaining examples, the evidence is stronger. The case of the two courts at Horsleydown is the best, though it is not conclusive; and next to this is the instance at Salford quoted from the Board of Health, although even here the account is so meagre that we scarcely know whether to accept it. The outbreak at Albion-terrace, singularly localized as it was, is yet susceptible of another explanation, viz., that the air was contaminated by the bursting of the drain; and the attack in Broad-street wants entirely one material item of evidence, viz., proof that the water was contaminated; indeed, we have seen that Dr. Snow is here absolutely obliged to admit, that the water may be apparently pure, and that the quantity in it "of morbid matter sufficient to produce cholera is inconceivably small." If we accept this opinion, we can never exclude the agency of water in any case.

Considering, therefore, the imperfection of both the positive and negative evidence, and the want of explanation of the earliest cases, we conclude that Dr. Snow has not yet proved that cholera is *always* communicated by means of water; and that he has not even *proved* that it has been so communicated in a single case. Yet, in the face of the evidence furnished by the attacks at Horsleydown and Salford, we cannot entirely reject Dr. Snow's views. We have made no reference to the other phenomena of the spread of cholera which Dr. Snow's hypothesis cannot explain; but have simply taken the facts given to us by Dr. Snow.

We must now, however, turn to the second part of Dr. Snow's book, in which a fresh argument for the influence of water is brought forward. Almost half of the work is taken up with a most elaborate inquiry into the water supply, as compared with the number of deaths from cholera. The object is to show, that when the water was supplied from an impure source, and was therefore probably contaminated with sewage matters, cholera was most prevalent.

It is impossible for us to go minutely into the mass of evidence so laboriously collected and arranged by Dr. Snow. In this part of his work, as in the former half, Dr. Snow does not sufficiently discuss the other conditions under which the people living in various districts of London were placed, besides those of varying water supply. He alludes, indeed, to, but speedily dismisses, the important law of the influence of elevation, demonstrated by the Registrar-General; and refuses, indeed, to admit the effect of elevation, and refers the difference of prevalence entirely to the water supply. He does not, in our opinion, pay sufficient attention to the density of the population, nor to the other causes of impurity of the air.

We shall now shortly advert to some part of the evidence.

In 1849, two water companies—the Southwark and Vauxhall, and the Chelsea—took their water from the same part of the Thames; the district supplied by the former company suffered severely, that supplied by the latter, very little. Dr. Snow explains this as follows:

"The Chelsea Company, which supplies some of the most fashionable parts of

London, took great pains to filter the water before its distribution, and in so doing no doubt separated, amongst other matters, the greater portion of that which causes cholera. On the other hand, although the Southwark and Vauxhall and the Lambeth Water Companies professed to filter the water, they supplied it in a most impure condition." (p. 64.)

We doubt the correctness of this answer. Both companies filter, says Dr. Snow, but one filters more thoroughly than the other, and therefore gets rid of that which causes cholera. But what evidence is there, that at the time when cholera prevailed, the one company did filter so carefully, and the others so carelessly? There appears to have been no examination made of the water at the time, and certainly we are not inclined to believe, without definite testimony, that one company could purify their water completely, and the other not at all.

But in a later page we find Dr. Snow modifying his opinion, for he discovers a fact which shows that filtration is not all-powerful. Till lately, Milbank prison was supplied with Thames water, which was filtered most carefully through sand and charcoal, yet cholera prevailed in the prison. As filtration is not, then, sufficient, the purification of the Chelsea water, previously referred to filtration, is now ascribed to retention in the reservoirs causing the decomposition of the choleraic substance.

A second point of evidence on which Dr. Snow relies, requires rather a longer discussion.

Part of London, on the south side of the Thames, is supplied by the Southwark and Vauxhall Company, and part by the Lambeth Company; the former takes its water from Battersea, the latter from Thames Ditton. One portion of the town, however, is supplied by both companies; some houses being supplied by one, some by the other company. Now, it is evident, that if in this part of London it should be found that the inmates of the houses supplied with the Battersea water suffered greatly, and those supplied with Ditton water suffered little, then of course, as all other circumstances (locality, air, trades, &c.) in the district are the same, the influence of the water would be proved beyond a doubt. Dr. Snow, with most praiseworthy industry, proceeded to investigate the point:

"The inquiry was necessarily attended with a good deal of trouble. There were very few instances in which I could at once get the information I required. Even when the water-rates are paid by the residents, they can seldom remember the name of the water company till they have looked for the receipt. In the case of working people who pay weekly rents, the rates are invariably paid by the landlord or his agent, who often lives at a distance, and the residents know nothing about the matter. It would, indeed, have been almost impossible for me to complete the inquiry, if I had not found that I could distinguish the water of the two companies with perfect certainty by a chemical test. The test I employed was founded on the great difference in the quantity of chloride of sodium contained in the two kinds of water at the time I made the inquiry. On adding solution of nitrate of silver to a gallon of the water of the Lambeth Company, obtained at Thames Ditton, beyond the reach of the sewage of London, only 2.28 grains of chloride of silver were obtained, indicating the presence of .95 grains of chloride of sodium in the water. On treating the water of the Southwark and Vauxhall Company in the same manner, 91 grains of chloride of silver were obtained, showing the presence of 37.9 grains of common salt per gallon. Indeed, the difference in appearance on adding nitrate of silver to the two kinds of water was so great, that they could be at once distinguished without any further trouble.

Therefore, when the resident could not give clear and conclusive evidence about the water company, I obtained some of the water in a small phial, and wrote the address on the cover, when I could examine it after coming home. The mere appearance of the water generally afforded a very good indication of its source, especially if it was observed as it came in, before it had entered the water-butt or cistern; and the time of its coming in also afforded some evidence of the kind of water, after I had ascertained the hours when the turncocks of both companies visited any street. These points were, however, not relied on, except as corroborating more decisive proof, such as the chemical test, or the company's receipt for the rates." (p. 77, 78.)

We looked now anxiously for the result of this inquiry, and found it stated further on:

"There were three hundred and thirty-four deaths from cholera in these four weeks, in the districts to which the water supply of the Southwark and Vauxhall and the Lambeth Company extends. Of these it was ascertained, that in two hundred and eighty-six cases the house where the fatal attack of cholera took place was supplied with water by the Southwark and Vauxhall Company, and in only fourteen cases was the house supplied with the Lambeth Company's water; in twenty-two cases the water was obtained by dipping a pail directly into the Thames; in four instances it was obtained from pump-wells; in four instances from ditches; and in four cases the source of supply was not ascertained, owing to the person being taken ill whilst travelling, or from some similar cause." (pp. 79, 80.)

On first reading this paragraph, we thought that the deaths referred to, took place only in the district with the intermingled supply, and that this was the answer to the "experiment on a grand scale," so laboriously inquired into by Dr. Snow. But, on re-perusing the passage and its context, we found that these deaths had taken place in *all* the districts supplied by the two companies, separately or conjointly. If this reading be correct, we doubt if the comparison can be safely made, for the Lambeth Company supplies, to a considerable extent, a good neighbourhood on elevated ground (including the healthy districts of Streatham, Forest-hill, and Sydenham); while the Southwark and Vauxhall Company supplies the greater part of the poorest, lowest, and marshiest district in London.

If, however, the deaths referred to in the paragraph just quoted, represent those only of the subdistrict supplied conjointly by the two companies, we have still some grounds of objection. First, are we to rely on the chemical test referred to by Dr. Snow? Does the Ditton water never contain *more*, and the Battersea *less*, chloride of sodium? Dr. Snow himself mentions that:

"When the water (Battersea) of the Southwark and Vauxhall Company was examined by Messrs. Graham, Miller, and Hofmann, at the latter part of January, 1851, it contained only 1.99 grains of chloride of sodium, or about one-twentieth as much as it contained last September, and one fifteenth as much as on 21st November, 1854." (p. 97.)

May not this small amount have occurred on some of the days on which water was collected by Dr. Snow? Did he ascertain that the water in the reservoirs of the company contained always this large amount of chloride of sodium during the prevalence of cholera, or was the water he tested merely taken from the cisterns supplied by the Southwark and Vauxhall Company? We certainly do feel great doubt, on the evidence

before us, whether the source of supply could safely be inferred from a chemical test alone.

Another weak point in the argument seems to us to vitiate Dr. Snow's whole case. He has not told us how many houses in the district referred to were supplied respectively by the Battersea and Ditton waters. It may be that the Ditton water was furnished to so few houses, that the small number of deaths from cholera were merely proportionate to the smaller number of houses. Dr. Snow endeavours to meet this difficulty, by giving, from the Parliamentary return, the number of houses supplied respectively by the two companies. But this return applies to the entire districts, and not to the special district where the supplies are intermingled; so that really we are in doubt whether the Ditton water is supplied to half of this special district, or to a quarter or a tenth part of it.

However, we learn from the Parliamentary return that the total number of houses supplied by the Southwark and Vauxhall Company is 40,046, and by the Lambeth Company 26,107, or as three to two, nearly. If this proportion be the same in the district common to both (of which, however, we have no certainty), then the deaths, of course, should properly be one-third more numerous in the houses supplied by the Southwark and Vauxhall than in those of the Lambeth Company. The difference in the mortality is, however, much more than this, if Dr. Snow's facts are to be received.

A better instance of the influence of water than either of these cases is given by the Lambeth Company. In 1849, it drew its supplies from Battersea; in 1854, from Thames Ditton. Was the mortality in the districts supplied by it diminished in the latter year? Of this there is proof, so far, that whereas the districts supplied solely by the Southwark and Vauxhall, show an equal mortality in the two years, those supplied partly by the Lambeth, as well as the Southwark and Vauxhall Company, show in several cases a remarkable diminution of mortality.

We shall not follow Dr. Snow into his account of the provincial towns, where we find little that is satisfactory. Here, as in all other cases, Dr. Snow refers only to the water-supply, and neglects all the other circumstances, as we could easily prove, had we space to refer to the cases of Birmingham, Leicester, or Exeter. The conclusions as to the attack at Newcastle do not appear to us to be borne out by the evidence.

Besides cholera, Dr. Snow thinks other diseases, yellow fever, intermittents, plague, and typhoid fever, may be also propagated by drinking water. The evidence for this is so slight, that we shall not discuss the point now.

We have already said, that from the positive evidence adduced by Dr. Snow, we were unable to do more than conclude that he had rendered the transmission of cholera by water an hypothesis worthy of inquiry; we cannot draw any other conclusion from his researches on water supply, than that the predisposing effects of impurity of water are also rendered highly probable. We may be mistaken in this, and the evidence which seems weak to us may not be so to others. If so, when additional evidence shall be given, we shall receive it with the greatest pleasure; for though we think

Dr. Snow's hypothesis, if proved, cannot explain all the phenomena of the spread of cholera,\* it would yet clear up some of the mysterious phenomena of its diffusion. Its establishment would therefore be an immense gain to science, and, we need not add, an important service to the State.

We cannot conclude without one remark. We have taken Dr. Snow's facts, and have not only criticised them as carefully as we could, but have expressed our opinions without reserve. In no other way could we treat this all-important question; and Dr. Snow, we are sure, would not have wished us to adopt any other course. But it is only simple justice to Dr. Snow to state, that no man could have pursued the inquiry with greater diligence. Although we think that he is biased by his creed,† and obstinately looks only in one direction, we close his book with the conviction that he is an honest and conscientious observer. If his discovery should be established, the prevention of cholera would be easy, and for this reason we think that the most careful inquiry should be made at once by the government into the subject, and proof or disproof given of Dr. Snow's opinions. In India, the point could be soon decided.

E. A. Parkes.

#### REVIEW XIV.

*Vertigo: a Paper read to the North London Medical Society, April 12th, 1854.* By J. RUSSELL REYNOLDS, M.D. Lond., University Medical Scholar.—London, 1854. 8vo, pp. 46.

WE are glad that Dr. Reynolds should have taken an opportunity of directing the attention of his professional brethren to the subject of vertigo, because we believe that the time is come at which a far more precise estimate can be formed respecting the nature of this condition, than was possible whilst the physiology of the sensori-motor apparatus was in a less advanced state; and because a careful appreciation of its phenomena will not only increase its scientific value, but will, in its

\* We would mention also here the experiments of Dr. Lauder Lindsay, who appears to have given dogs cholera by making them breathe a choleraic atmosphere, but who, like others before him, could not cause cholera by feeding the dogs on cholera dejections.

† As an instance of this we may cite Dr. Snow's mode of accounting for the outbreak of cholera in London in 1854. On the 25th of July, the mate of a merchant steamer which had returned three weeks previously from the Baltic, died of cholera in London. "This patient was the chief-mate to a steam-vessel taking stores to, and bringing home invalids from, the Baltic fleet. Three weeks ago he brought home in his cabin the soiled linen of an officer who had been ill. The linen was washed and returned." The time when this steam-vessel arrived in the Thames with the soiled linen on board, was a few days before the first cases of cholera appeared in London, and these first cases were chiefly amongst persons connected with the shipping in the river. It is not improbable, therefore, that a few simple precautions, with respect to the communications with the Baltic fleet, might have saved London from the cholera this year, or, at all events, greatly retarded its appearance." Was any deduction of so extraordinary a kind ever made on such grounds? We are not told that the officer whose clothes were brought home had had cholera; we are not told where the clothes were washed, by whom they were washed, or when they were washed. Supposing the mate to have sent them to be washed the day he arrived—three weeks before his death—as is most probable, can the most perverse ingenuity connect his death with these clothes? And then with regard to London itself, as many cases of cholera had occurred before July, how is it possible for one moment to adhere to the hypothesis that the soiled linen of a sick officer in the Baltic fleet being washed in the Thames was the cause of the epidemic?