

## CHAPTER 7

# The Evolution Of Snow's Thinking On Cholera, 1854–1858

The five-year period from 1849 through 1853 was fruitful for Snow's work on cholera in two respects. First, the nature and means of transmission of communicable diseases, particularly cholera, became clearer to him as he drew the threads of his concept of these diseases together. This is evident on his monograph *On Continuous Molecular Changes*.<sup>1</sup> Second, he published the monograph that is more important and for which he is most widely known — *On the Mode of Communication of Cholera*.<sup>2</sup> In this he strengthened his concept that cholera was communicated not by an atmospheric agency or "miasma" (the majority view) but by simple forms of human actions — through ingestion of polluted water or food or through contact with fomites. This concept, as was recognized by a few of Snow's contemporaries, notably William Budd,<sup>3</sup> was original with Snow, deserving of priority as a scientific idea.

The following five-year period, from the beginning of 1854 to his death in 1858, but especially from 1854 through 1856, was equally productive. Snow consolidated his ideas on cholera; he gathered more information about its spread; he tested his relevant hypothesis (first formulated in 1849) by performing a "crucial" experiment; he published the results of this convincing experiment, with an equally convincing argument supporting his views on the spread of cholera, in the second and much larger edition of his monograph;<sup>4</sup> and he continued to lecture and write about cholera.

In his lifetime, few people accepted Snow's ideas. Later, however, the logic of his ideas convinced more and more people, and the old ideas, such as the belief that cholera spread through

the air as a miasma, gradually faded. Even for some knowledgeable and intelligent persons, however, this process took many years. It was not until 1890, for example, that Sir John Simon, who had held the influential posts of Medical Officer to the General Board of Health and later to the Privy Council,<sup>5</sup> finally came to agree that Snow had been right. By that time Simon, who had long believed in the miasmatic theory of cholera, was forced to conclude, but in memorable and gracious words, that Snow's proven means of the transmission of cholera was "the most important truth yet acquired by medical science for the prevention of epidemics of cholera."<sup>6</sup> Regrettably, Snow was not alive to read these words; but, as one who always sought the "truth," he would have derived intense satisfaction from them had he still been alive. His work was indeed important in advancing the understanding of the causation and the spread of cholera.

This second quinquennium in Snow's thinking on cholera was characterized by three elements: the consolidation of his concept of the cellular nature of the cholera agent; his demonstration of the varying incidence of cholera in south London in the summer of 1854; and his study of the outbreak of cholera in and around Broad Street in September 1854.

### **The Cholera Organism As A Cell**

On January 21, 1854, Snow gave a paper on the principles of treatment of cholera to the Medical Society of London.<sup>7</sup> Then President-elect of the Society, he was becoming well known both for his work on anesthesia and for his opinion as to how cholera is caused and communicated. His fellow physicians still held a variety of opinions as to how cholera occurred and should be treated, and this diversity of opinion is evident from the report of the discussion<sup>8</sup> of Snow's lecture. Doctors differed so much in their opinions about cholera that "if a layman had been present at the meeting, he might have witnessed a remarkable illustration of the manner in which 'doctors differ', and surely his faith in physic would have been scarcely increased in strength." The barrenness of most peoples' ideas about cholera was evident in the discussion, and the evening terminated "without fresh light being thrown upon the nature or treatment of that formidable disease."

In replying to comments on his paper and on cholera, Snow pointed out that the supposed presence of the cholera agent in the atmosphere just would not explain several obvious facts. For example, it would not account for isolated cases of cholera. If one believed in the atmospheric theory, one would have to assume the existence of a predisposition of some individuals to the disease — but, said Snow, the idea of predisposition was only “a metaphysical abstraction.”

In contrast to the views of his audience, Snow's views were “clear and definite.” By now an experienced physician, Snow took a stand on the treatment of cholera that was similar to his stand on the management of anesthesia. Regarding cholera, he said that “without clear and definite ideas on pathology there can be no fixed principles of therapeutics;” concerning anesthesia, he said elsewhere that “it will be at once admitted that the medical practitioner ought to be acquainted with the strength of the various compounds which he applies, and that he ought, if possible, to be able to regulate their potency.”<sup>9</sup> Snow was always realistic and pragmatic. On cholera his ideas were based primarily on the clinical and pathological nature of the disease and not on speculation. So Snow reiterated his belief that cholera was a gastrointestinal disorder and should be treated as such; as he said in his lecture on January 21, 1854, “the symptoms which follow the affection of the alimentary canal in cholera are exactly those which this affection is adequate, and, indeed, could not fail to produce.” Snow's vision was crystal clear. He anticipated the modern rehydrating treatment of cholera: “we should give watery drinks, and be content to wait till they are absorbed, unless in desperate cases, when it might be desirable to inject into the blood-vessels a weak saline solution resembling ‘the portion of the blood which has been lost’.”

Snow's advanced views were evident in his lecture in January 1854. In particular, he restated his concept of the animate nature of the causative agent of cholera. He emphasized the view that any local form of treatment should be capable of destroying “low forms of organised beings.” He also reiterated his view that “the cholera poison having the property of propagating its kind, must be organised to some extent.” This belief he had discussed in some detail before the members of the Medical Society of London the previous year;<sup>10</sup> it was a concept that he would

develop later in the year, when he categorically stated that "the morbid matter of cholera... owes its properties, no doubt, to cells."<sup>11</sup>

At this time Snow was emphasizing an aspect of his concept of the spread of cholera that is still not fully appreciated. As the report of his January 1854 lecture observed, "his theory respecting the mode of communication of cholera was not merely a water theory, as he considered water only one of the vehicles which conveyed the poison";<sup>12</sup> it was, as Feachem has pointed out in explaining the relevance of Snow's concept to cholera in countries like Bangladesh today, much broader in scope.<sup>13</sup> Snow himself never claimed or suggested that cholera was exclusively or primarily a water-borne disease. While he often stressed waterborne transmission because he believed it had major role in the great urban epidemic of his time,<sup>14</sup> he stressed the role of food-borne transmission as well. He did so especially in the socioeconomic context of the day, when overcrowding, scarcity of water, poor understanding of the infectiousness of cholera, disregard for personal cleanliness, and unhygienic preparation and handling of food were factors in the spread of cholera. His own experience of cholera among the miners of Killingworth in 1832 supported, and was likely the seed of, this concept.

Snow's lecture on the treatment of cholera was a departure from his customary epidemiological approach, but he was always interested in the *pathology* of cholera; that is why he emphasized that "without clear and definite ideas on pathology there can be no fixed principles of therapeutics." But his lecture does illustrate Snow's familiarity with the disease as a practising physician — and his relative impotence as a doctor confronted with the need to give efficacious treatment. Like his colleagues, he had to admit that he had "no plan of treatment to offer which promises any great results in the more severe and fully-developed cases of cholera, and ... nothing in the way of treatment that is new." Even so, his classification of the principles on which the treatment of cholera should be based indicates how he, always the rational physician, thought patients should be treated, and it illustrates his logical thinking.

Snow, then, sought to relate the treatment of cholera to its pathology. Potentially the most rational therapy of his day was the intravenous infusion of saline, which had been advocated in

England by O'Shaughnessy, Latta and others during the epidemic of 1831-1832.<sup>15</sup> (The value of this form of therapy, however, remained only potential in Snow's day because no one realized that cholera purges the patient of enormous quantities of body fluids, and that it is necessary to give many liters of saline.) Although there is no evidence that Snow himself treated patients in this manner, he recommended it in supporting the argument that cholera was an enteric, not a systemic, disease, and that the non-enteric symptoms were due to the dehydration of the blood.<sup>16</sup> As Snow knew, in the spring of 1849 Garrod and Parkes had confirmed the scientific basis of intravenous therapy; they found that the amount of water in the blood normally averaged 785 parts in 1,000, whereas in cholera it averaged only 733, with the solid constituents increasing from 215 to 267, and the "globules" in blood increasing from 208 to 256.<sup>17</sup> So Snow stressed that, "unless it is determined to inject the veins ... it is clear that the patient cannot recover from the state of collapse, except by the absorption of watery fluids from the alimentary canal."<sup>18</sup> He knew that after an infusion of saline the patient's appearance would change remarkably; as he said, "the shrunken skin becomes filled out, and loses its coldness and lividity; the countenance assumes a natural aspect, the patient is able to sit up, and for a time seems well." Snow used this response in corroborating his concept of the pathophysiology of cholera; "if the symptoms were caused by a poison circulating in the blood, and depressing the action of the heart, it is impossible that they should thus be suspended by the injection of warm water, holding a little carbonate of soda in solution."

In all, Snow enumerated five principles of treatment. None would be an anachronism today, especially the use of "a weak saline solution resembling 'the portion of the blood which has been lost'" — a principle that is the basis of the treatment of the patient with cholera.<sup>19</sup> Another principle was that "medicines should be chosen which have the effect of preventing the various kind of molecular change in organic matter" — the modern equivalent of which is an antibiotic, like tetracycline. A third principle was that "these remedies should be continued until there is no danger of a relapse" — again, most sensible advice that would not be out of place today, though in the 19th century failure of intravenous infusions was largely due to the failure to

persist in the treatment until the infected epithelial cells had been sloughed off and replaced by uninfected ones. A fourth principle: medicines "should be administered with a view to their action in the stomach and bowels, and not to their being absorbed," a principle of using locally acting drugs that is still sound, though not of crucial importance today. Finally, Snow suggested that "the medicines should be given in such quantities and in such form as to ensure as much as possible their application to the whole surface of the alimentary canal" – advice that would still be considered valid if the primary rather than the secondary object of treatment were to alleviate the vomiting and diarrhea of cholera. Snow's principles of treatment were far more rational than the unnecessarily aggressive and harmful steps of the traditional bleeding and purging.

What was Snow's concept of cholera in 1854? As he expressed it in his lecture of January 1854, cholera appeared to be a disease in which there is "a low form of organic action going on upon the interior surface of the stomach and intestines." The agent of cholera seemed to have biological characteristics. In addition to speaking of "the morbid poison ... multiplying or reproducing itself," he recommended medicines having "the property of destroying low forms of organised beings." While one could infer that Snow was thinking specifically of microorganisms organized as cells, at this time his ideas were couched in Liebigian terms:<sup>20</sup> he advised that the medicines should also be capable "of arresting fermentation, putrification, and other kinds of molecular change in organic matter."

In view of the thinking on biology that was current in Snow's day, it is, however, not unlikely that Snow was beginning to think of these "low forms of organised beings," as having a cellular structure. Theodore Schwann's theory that the primary and fundamental unit of matter was the cell, first stated in 1839, became familiar to many English-speaking physicians when Schwann's text was translated into English as *Microscopical Research into the Accordance in the Structure and Growth of Animals and Plants* by Henry Smith in 1847.<sup>21</sup> Schwann's theory was a "tremendous stimulus," in the dawn of the germ theory,<sup>22</sup> and Snow, who constantly showed his familiarity with the medical literature, would certainly have been familiar with the concept of the cell as a unit of biological matter. At all events, later

in 1854 Snow did state that "the morbid matter of cholera, like the pus of small-pox and other morbid poisons, owes its properties, no doubt, to cells."<sup>23</sup> This was on August 5, 1854, but he restated it on December 11, 1854, in a better known passage, when he concluded that "the morbid matter of cholera having the property of reproducing its own kind, must necessarily have some sort of structure, most likely that of a cell."<sup>24</sup> He was not disturbed by the fact that, to his knowledge, this cellular structure had not yet been recognized under the microscope, for he argued that "the matter of small-pox and of chancre can only be recognised by their effects, and not by their physical properties."<sup>25</sup> (The fact that, unknown to him, Pacini had recognised the cholera vibrio earlier in 1854,<sup>26</sup> serves only to emphasize Snow's vision and prescience.)

From 1854 onwards, therefore, Snow believed that the causative agent of cholera had a cellular structure. However, unlike Budd, who in 1849 had erroneously postulated the presence of fungi as causative agents,<sup>27</sup> Snow was now on firm ground. Furthermore, since Robert Koch did not demonstrate, to public satisfaction, the existence of the cellular structure of the cholera vibrio until 1883,<sup>28</sup> Snow was considerably ahead of his time.

#### **Cholera in the South Districts of London in 1854**

For Snow, who always spent less time postulating than investigating, the summer of 1854 was a particularly active one. England had been relatively free of cholera from the latter part of 1849 until late in 1853. Then it attacked the north of England, killing some 1,500 persons in Newcastle-upon-Tyne and 400 in Gateshead before suddenly vanishing.<sup>29</sup> Cholera returned in the summer of 1854, when London was especially hard hit, some 11,000 persons succumbing.<sup>30</sup> Snow was personally involved this time, for he lived and practised medicine in the very area of London that was affected, as he himself wrote, by "the most terrible outbreak," that had ever occurred in Great Britain.<sup>31</sup> Snow lived in Soho, close to Broad Street, which was the "epicentre," of an outbreak of "upwards of five hundred fatal attacks in ten days." But he was also involved as a medical scientist who conducted a remarkable epidemiological investigation not only of the Broad Street outbreak but, more particularly, of outbreaks in many districts south of the river Thames.

Snow had traced the epidemic of cholera of 1848-1849 to the presence of the disease in a seaman who arrived in London on a ship from Hamburg in Germany.<sup>32</sup> About the epidemic of 1854 he took a somewhat similar view. Although he said that "we cannot trace the commencement of the epidemic in London to an individual case," he did wonder whether it could have originated in a like manner — this time by means of "intercourse" of ships between the British Baltic Fleet (at the time of the Crimean War) and the Thames.<sup>33</sup> He presented three pieces of evidence. One was the finding that "the first cases occurred in persons residing, or being employed on board the shipping in the river, and among others near the river side." The second was the fact that "the cholera also appeared about the same time in an emigrant-ship and a troop-ship, soon after leaving the Thames." More significant appeared to be the occurrence of cholera in "a chief-mate to a steam-vessel taking stores to and bringing home invalids from the Baltic fleet." According to this seaman's medical attendant, three weeks earlier "he brought home in his cabin the soiled linen of an officer who had been ill [and] the linen was washed at his house...." As Snow had suggested in 1849, the vehicle was the same — the bedlinen of an infected person; and in doing so Snow again emphasized that fomites, and not only water and food, might be responsible for the spread of cholera. Snow was careful not to go out on a limb and say that this case was the means of "actually introducing the cholera into London," but he did say it was "probable that a few simple regulations respecting this intercourse might have kept London free from the cholera." A confirmed contagionist, Snow took the opportunity once more to wage battle against the anticontagionists: he wrote that "unfortunately, the chief advisers of Government have had their minds occupied about drain-pipes and bad smells, and have neglected the specialties connected with the propagation of individual diseases."

Snow's actual investigation of cholera in London south of the river Thames in the summer of 1854 was based on three points. The first concerned the difference in origin of the water supplied by two companies. In accordance with an Act of Parliament,<sup>34</sup> one of the water companies supplying the south districts of London changed its source of water in 1852 from the Thames in London itself, near Hungerford Bridge, to the river well



outside the city, at Thames Ditton, and so "beyond the influence of the tide, and, therefore, out of reach of the sewage of the metropolis."<sup>35</sup> This company was the Lambeth Water Company. In contrast, the Southwark and Vauxhall Water Company, which supplied most of the houses in the districts also supplied by the Lambeth Company, was still drawing its water from Battersea Fields, a reach of the Thames near Vauxhall that was just as seriously polluted – indeed, probably more polluted – than it had been in 1849. In August 1854 Snow observed that the mortality from cholera was consistently higher in the areas supplied by this company.<sup>36</sup> In addition, in the previous autumn the mortality from cholera in those districts that had the new water supply had been lower than in those that were supplied exclusively by the Southwark and Vauxhall company. This difference had not existed in 1849 – that is, before the Lambeth Water Company changed to its new source of water. Snow confirmed this analysis at the beginning of October 1854. The mortality, up to to August 26, "was just nine times as great in the houses supplied by the former Company as in those supplied by the latter."<sup>37</sup>

Snow's second point was based on evidence given before the committee that prepared the Health of Towns Reports. An engineer, Mr. Quick, had stated that, in Snow's words, "throughout the greater part of Lambeth and Southwark, the whole of Newington, and a part of Camberwell ...the supply of the two companies above mentioned is actually intermingled, the pipes of both companies going down the same streets...."<sup>38</sup> This situation had arisen "in consequence of the active competition that once existed between three water companies, two of which [the Southwark and Vauxhall companies]...since ...amalgamated and [had] come to an agreement with the other – the Lambeth Company." Because of this intermixing of the two companies' waters, the effect of the Lambeth Company's new water supply on the progress of cholera was not at all obvious, though it did attract the attention of the Registrar-General. His office supplied Snow with information making his third point – the difference in mortality rates among the clients of the two water companies.



*Map of districts south of the River Thames showing "intermingling" of water supply from the Southwark and Vauxhall and the Lambeth water companies (darkest shade)*

The genius behind the Registrar-General was William Farr, the office's Compiler of Abstracts. Farr prepared the weekly returns of deaths from various diseases, cholera included. It was Farr who showed, in the return for November 26, 1854, that the mortality from cholera in those districts partly supplied by the Lambeth Water Company (61 per 100, 000 inhabitants) was lower than that for those districts supplied entirely by the Southwark and Vauxhall Company (94 per 100, 000).<sup>39</sup> Such data showed that the death rate from cholera for the former districts in 1849 was certainly of the same order then as for the latter districts.<sup>40</sup> Snow immediately understood the significance of these findings. He realized that "in order to ascertain clearly what this difference in mortality depended on, it was necessary to make an inquiry in detail."<sup>41</sup>

The results of Snow's investigation into these districts south of the Thames are fully described in the second edition of his monograph *On the Mode of Communication of Cholera*. He completed this on December 11, 1854. In it, besides describing his investigation south of the Thames, he described his inquiry into the better known Broad Street outbreak, summarized what he had learned about cholera outbreaks in many other places, and enumerated the principles of prevention of cholera.

Snow approached his investigation with his customary vigor and thoroughness, very much aware that he had the opportunity of conducting a unique epidemiologic experiment. It was an experiment "on the grandest scale:"

*No fewer than three hundred thousand people of both sexes, of every age and occupation, and of every rank and station, from gentle folks down to the very poor, were divided into two groups without their choice, and, in most cases, without their knowledge; one group being supplied with water containing the sewage of London, and, amongst it, whatever might have come from the cholera patients, the other group having water quite free from such impurity.*<sup>42</sup>

Snow realized that, in principle, the experiment was quite straightforward, since "to turn this grand experiment to account, all that was required was to learn the supply of water to each individual house where a fatal attack of cholera might occur."<sup>43</sup> To conduct it, however, was an enormous task, especially for one man, and he later asked a Dr. J.J. Whiting to help him in one part of his inquiry. Even so, Snow thought it essential that he be the principal and sole investigator:

*When the cholera returned to London in July [1854] ... I resolved to spare no exertion which might be necessary to ascertain the exact effect of the water supply on the progress of the epidemic, in the places where all the circumstances were so happily adapted for the inquiry. I was desirous of making the investigation myself, in order that I might have the most satisfactory proof of the truth or fallacy of the doctrine which I had been advocating for five years. I had no reason to doubt the correctness of the conclusions I had drawn from the great number of facts already in my possession, but I felt that the circumstance of the cholera-poison passing down the sewers into a great river, and being distributed through miles of pipes, yet producing its specific effects, was a fact of so startling a nature, and of so vast importance to the community, that it could not be too rigidly examined, or established on too firm a basis.*<sup>44</sup>

Snow began his investigation in mid-August 1854. He first asked the General Register Office for the addresses of persons who had died of cholera in those districts where the water supply of the two water companies was so intimately "intermingled." Initially he looked at two subdistricts of Lambeth, in Kennington. Up to August 12 there had been 44 deaths there. Of the houses in which these deaths had occurred, 38 had been supplied with water by the Southwark and Vauxhall Company, 4 by the Lambeth, and 2 by pump-wells on the premises and not by either company.

Snow told Farr about this. Impressed and curious, Farr asked the registrars in all the south districts of London to let him know which company supplied water for each of the houses where someone died from cholera. Since this could not be put into effect until August 26, Snow continued to work on his own. He visited houses in the subdistricts of Lambeth, Southwark and Newington, where the water supplies of the two water companies were "intermixed," and found much the same as he had for houses in Kennington. The investigation was arduous:

*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 ... [were] paid by the residents, they ... [could] seldom remember the name of the Water Company till they ... looked for the receipt. In the case of working people who pay weekly rents, the rates ... [were] invariably paid by the landlord or his agent, who often ... [lived] at a distance, and the residents ... [knew] 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 the 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.<sup>45</sup>*

This paragraph is indicative not only of the lengths Snow had to go to but also the imaginative approach he took in overcoming the problems he confronted. A bachelor, he was prepared to devote all his free time to solving problems, and he did so both in the "field" and in his home.

One problem was that, though a Parliamentary return gave the numbers of houses supplied by each of the water companies, it failed to give the number of houses so supplied in particular districts. Snow therefore had to pursue his inquiry in all the districts that were supplied by either of the two companies. This he did in order to show "the full bearing of the facts," brought out in those districts where the supply was "intermingled."<sup>46</sup> He himself followed up the details of every death from cholera in the districts supplied by the Lambeth Company, but it was to obtain similar information about the districts supplied only by the Southwark and Vauxhall Company that led Snow to ask Dr. Whiting to assist him. Dr. Whiting, who "took great pains with his part of the inquiry,"<sup>47</sup> had a heavy responsibility, for he had to ascertain whether the houses in which the fatal attacks had occurred were supplied with the Company's water, or from a

pump-well, or some other source. This he did for the first four weeks of the epidemic, from July 8 to August 5, working the streets of Bermondsey, Rotherhithe, Wandsworth and "certain other districts" that were supplied by the Southwark and Vauxhall Company.

In this four-week period, 334 people died from cholera in those districts that were supplied by the two water companies. In 286 cases the house where a death from cholera had occurred was supplied by the Southwark and Vauxhall Company, while in only 14 cases was the house supplied by its competitor. (In the other cases the water supply came from a pump-well or ditches, or could not be identified.) "As a guarantee that the water supply was inquired into, and to afford any person who wishes it an opportunity of verifying the result,"<sup>48</sup> Snow, always the honest scientist, listed the details of all of these 334 cases in a lengthy Appendix to his monograph.

To these findings Snow added information from the Parliamentary returns concerning the water supply for all the districts. In 1853 the Southwark and Vauxhall Company had supplied 40,046 houses; the Lambeth, 26,107. The ratio of fatal attacks for the two groups of houses in which these cases had occurred was, respectively, 71:5. Or, as Snow wrote, "the cholera was therefore fourteen times as fatal at this period, amongst persons having the impure water of the Southwark and Vauxhall Company, as amongst those having the purer water from Thames Ditton."<sup>49</sup>

Another ratio was significant. In all of London, the number of houses at the time of the 1851 census was 327,391. Deducting the 40,046 of them in which 286 persons had died and which were supplied by the Southwark and Vauxhall Company, left 287,345 other houses, and in them 277 deaths had occurred in the first four weeks of the epidemic. For this latter number of houses, there was a ratio of 9 deaths to each 10,000 houses. In those houses supplied by the Lambeth Company however, the mortality was only 5 in each 10,000 houses. Snow concluded that that the residents of these houses, although their dwellings were intimately mixed in terms of the water supply with those of the Southwark and Vauxhall Company (which was associated with so great a proportional mortality), did not suffer even so

much as the rest of London, "which was not so situated."<sup>50</sup> Snow showed, once again, that the quality of the water was clearly a factor in the spread of cholera.

As the epidemic continued Snow extended his analysis. The findings were similar for each period of that epidemic. For the seven weeks ending August 26, the effect of the purer water came out just as strongly. The ratio of deaths for each 10,000 houses supplied by the Southwark and Vauxhall and Lambeth Companies was, respectively, 315:37; that is, the mortality associated with the polluted water was between eight and nine times higher than that associated with the purer water. Snow's findings remained consistent. His own inquiry did not extend beyond August 26, after which the district registrars began to make returns relative to the water supply of each house where someone died from cholera. This combined inquiry provided a much larger data base for the time period, the number of houses and the population studied, yet the same general result held. For the 14-week period up to October 14, 1854, the findings were as follows: in the houses supplied by the Southwark and Vauxhall Water Company to a population of 266,516 (as obtained by the 1851 census), there were 4,093 deaths, while in those supplied by the Lambeth Water Company to a population of 173,748, there were 461 deaths. The ratio of deaths per 10,000 population for the water from the two companies was 152:26, or nearly 6:1.

Snow also compared the mortality rates for the two water supplies for 1849 and 1854. He analyzed the findings for 12 subdistricts supplied by the Southwark and Vauxhall Company, for 16 supplied by both companies, and for 4 by the Lambeth Company. For the Southwark and Vauxhall the rates were similar — 2,261 deaths in 1849 and 2,458 in 1854; for the two companies jointly the rate was lower in 1854 than in 1849 — 2,547 versus 3,905; and, to no surprise at all in Snow's mind, for the Lambeth alone the rate was very much lower in 1854 than in 1849 — 37 versus 162.

One final set of data contributed to what Snow called the "mischief"<sup>51</sup> that the Southwark and Vauxhall Company, in not complying with the law in changing its source of water, had been perpetrating on Londoners. He examined the statistics relating to four workhouses — public institutions that were controlled by

parishes to house the indigent and poor. Three were supplied by the Lambeth Company and one was supplied by the Southwark and Vauxhall Company. Among the former, Newington's 650 inmates lost only 2 from cholera up to September 21; the Lambeth Workhouse's nearly 1,000 paupers lost only 1 up to the first week in September; and St. Saviour's workhouse, none. In contrast, the workhouse run by the parish of St. George, Southwark, lost 6 of its 600 inmates before August 26, when the epidemic had run only one third of its course.

In commenting on the "mischief" that he laid at the door of the Southwark and Vauxhall Company, Snow was blaming not only the water company itself but also those who believed in the whole concept of the "sanitary idea," that was so prevalent in the middle years of the 19th century. In his letter to the *Medical Times and Gazette* of October 2, 1854,<sup>52</sup> Snow remarked that more than 50 per cent of the 563 Londoners who had died from cholera had been customers of the Southwark and Vauxhall Water Company and that many of the others were seamen or labourers working along the Thames who "almost invariably" drew their drinking water from the river. The Southwark and Vauxhall Company, however, like so many physicians and public health authorities in Snow's day, held that filth and atmospheric pollution were the real causes of ill-health. It was not yet convinced of the evils of water pollution and it was certainly not convinced that it was guilty, as Snow claimed it was, of causing harm to those Londoners who drank its water. But just this was Snow's point:

*...if the Southwark and Vauxhall Water Company had been able to use the same expedition as the Lambeth Company in completing their new works, and obtaining water free from sewage, the ... epidemic of cholera would have been confined in great measure to persons employed among the shipping, and to poor people, who get water by pailful direct from the river Thames or tidal ditches.*

Snow did excuse the water company to some extent, however, for he pointed out that there were others who were also to blame:

*The persons ... most instrumental in causing the increase of*



*cholera ... [were] precisely those who ... [had] made the greatest efforts to check it, and who ... [had] been loudest in blaming what they considered the supineness of others.*

Here Snow was criticizing a practice that was advocated by those who firmly believed in the "sanitary idea," — men such as Edwin Chadwick, Southwood Smith and John Simon. According to this idea, feces lying in cesspools or sewers gave off "unpleasant gas," and so were "dangerous and pestilential nuisances." Thus arose the practice of flushing "the removable causes of disease," into whatever river happened to be nearby — "into the river," Snow pointed out, "from which two thirds of the inhabitants, till lately, obtained their supply of water."

Flushing the sewers, a practice that had been carried out in 1849, was not practised in 1854, but Snow identified another problem: recently "increased quantities of water ... [had] been supplied at more frequent intervals, causing the water-butts to overflow for hours together, and having the effect of washing the evacuations more quickly into the river, from whence they were disturbed again to the community sooner than usual." In this way, Snow thought, the water of the Southwark and Vauxhall Company had become "more than usually dirty, and full of living things." So, Snow concluded, "almost everything that has been done with a view to check the progress of cholera ... had the effect of increasing it." (An exception was the filtration and settling of water practised by the Chelsea Water Company, which "by careful filtration and by detaining the water in their reservoirs, rendered it in a great degree innocuous.")<sup>53</sup>

Putting all his results together, Snow could reach only one conclusion: "the houses supplied with the water from Thames Ditton, by the Lambeth Company, continued throughout the epidemic to enjoy an immunity from cholera, not only greater than London at large, but greater than every group of districts, except the north and central groups."<sup>54</sup> (North of the Thames the mortality was caused more by "the relative crowding and want of cleanly habits of the people, and by the accidental contamination of the pump-wells, than of the supply of the water companies"<sup>55</sup>— circumstances that Snow examined when cholera struck Broad Street at this time.) But if one had the misfortune to live in a house supplied by the Southwark and Vauxhall Company, one's chance of dying was higher than elsewhere: 14 times higher in

the first four weeks of the epidemic and still nearly 6 times higher in the 14 weeks down to October 14. Water quality was important; water polluted by sewage was unhealthy and liable to cause cholera when cases of the disease existed.

Thus Snow, as a result of his investigation in the districts south of the Thames, obtained "very strong evidence of the powerful influence which the drinking of water containing the sewage of a town exerts on the spread of cholera, when that disease is present...."<sup>56</sup> How sound was his evidence?

His evidence was epidemiological, and that is the strength of his study. But in this pre-bacteriologic era, he could, of course, find no bacteriologic evidence, and that constituted a weakness of his case. As E.A. Parkes noted, "this array of evidence" was weakened by "the want of proof of contamination of water."<sup>57</sup> A *priori* evidence, of course, indicated that the water supplied to Londoners south of the Thames was polluted, for this had been shown microscopically by Dr. A. Hassall in 1850,<sup>58</sup> and a comprehensive microscopic examination of samples of water from different companies in 1851<sup>59</sup> had revealed the existence in these samples of all manner of "living things." Snow, describing Hassall's analysis of Thames water, wrote that "he found in it the hairs of animals and numerous substances which had passed through the alimentary canal."<sup>60</sup> So he had concluded that the water supplied by the companies on the Surrey side of London (the then separate Southwark and the Vauxhall, which were originally two companies before they amalgamated, and the Lambeth) was "by the far the worst of all those who take their supply from the Thames."

The problem of identifying water that was polluted by human waste Snow attempted to overcome by testing for the sodium chloride concentration, which would differentiate the two brands of water (the sodium chloride was a byproduct of human metabolism). As far as he could tell by relying on his own chemical expertise, this test seemed infallible.

Thorough as it was, Snow's epidemiological evidence was, however, not foolproof. Parkes identified some of the flaws in Snow's investigation.<sup>61</sup> One concerned the 334 deaths from cholera in the period from July 8 to August 5. Parkes made two points. First, if the deaths had occurred in *all* the districts supplied by the two water companies, separately or jointly, the comparison between the death rates might not be valid. Parkes' argument ran as follows. First, the Lambeth Company supplied

much of the water to "a good neighbourhood on elevated ground," (including the health districts of Streatham, Foresthill, and Sydenham), while the Southwark and Vauxhall Company supplied the greater part of "the poorer, lowest, and marshiest district in London." Second, if these deaths related only to the subdistrict supplied conjointly by the two companies, there were still grounds for objection, and he asked two pertinent questions: whether one had to rely on the chemical test that Snow had used, and whether it was possible that the water from Thames Ditton never contained *more*, and the Battersea *less*, chloride of sodium. Snow had noted that the sodium chloride concentration of the Southwark and Vauxhall water did vary on occasion; he said that in the latter part of January 1851 the concentration was only one-twentieth of that in September 1854 and one-fifteenth of that in November 1854 – findings reported by the Government Commission on the Chemical Quality of the Supply of Water to the Metropolis.

Parkes also objected that the information about the numbers of houses supplied by the two water companies was incomplete. Snow did give the total numbers supplied by the companies, but these numbers applied to entire districts and not to the special districts where the two supplies were "intermingled." Parkes' point was that the Thames Ditton water was supplied to so few houses, that the small number of deaths from cholera was merely proportional to the smaller number of houses. This was a valid point, but it was one that Snow could counter. He was aware of this weakness in his argument, and answered Parkes' objection two years later, in the following manner:

*The results of my inquiry into the supply of water was, of course, obtained separately for each district and subdistrict in which the inquiry was made, and were so published; but I was unable at the time to show the relation between the supply of houses in which fatal attacks took place, and the entire supply of houses in which fatal attacks took place, and the entire supply of each district and subdistrict, an account of the latter circumstance not being known. I expressed myself as follows in an article which I published [on October 7, 1854] soon after my inquiry was made: "I hope shortly to learn the number of houses in each subdistrict supplied by each of the water companies respectively, when the effect of the impure water in propagating cholera will be shown in a*

*very striking manner, and with great detail." This information did not, however, come within my reach till recently, and not even then with all the accuracy I could desire. In the recent Report on the Cholera Epidemics as affected by the Consumption of Impure Water, lately written by Dr. Simon, and published by the General Board of Health, there is a statement of the number of houses supplied by each of the water companies respectively in each district and subdistrict.*<sup>62</sup>

This rebuttal was published in the *Journal of Public Health* in October 1856.<sup>63</sup> It is important in corroborating Snow's argument. As Frost pointed out, it "confirmed it in detail and shows his keenness in statistical analysis."<sup>64</sup> This article complements Snow's monograph of 1855, just as the monograph complements his two earlier letters of September 2 and October 7, 1854. Snow's hypothesis stood up, whatever aspect was being analyzed. Thus Table I of the article, which provided the detailed "results of the Author's personal Inquiry in Twenty-One Sub-Districts," yielded a mortality ratio for the first seven weeks of the epidemic for the two water companies per 10,000 houses supplied, of 6.94:1 (47.2:6.8) for the Southwark and Vauxhall and Lambeth companies, respectively. Almost identical was the ratio (47.2:5.7) derived from Dr. Whiting's investigation, as shown in Table II. Table V gave the results of the entire epidemic; the ratio was 5.9:1 (160:27). Table IV provided data on the mortality for 31 subdistricts, this time related to the population; the ratio for the two companies was 9.26:1, respectively. Thus Snow himself was quite satisfied that his study was reliable, and he was satisfied enough to conclude the article of October 1856 as follows:

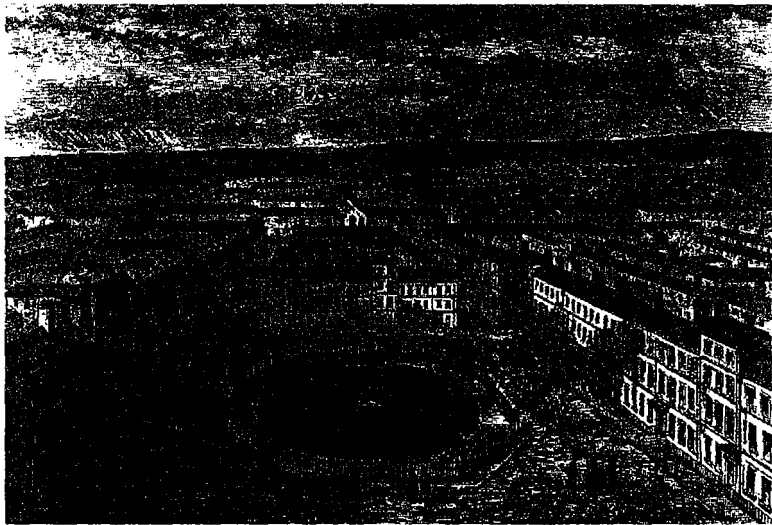
*It is probable that, when the facts brought to light by this inquiry are sufficiently known, no one will deny the influence of impure water in promoting the mortality of cholera; but it must not be supposed that it is mere impurity of an ordinary kind that causes the disease, for there are innumerable facts to prove that ordinary impurities have no such effect, and that it is only when the specific morbid matter of the disease gains access to the water that cholera is propagated.*

Epidemiologists since Snow have admired his investigation

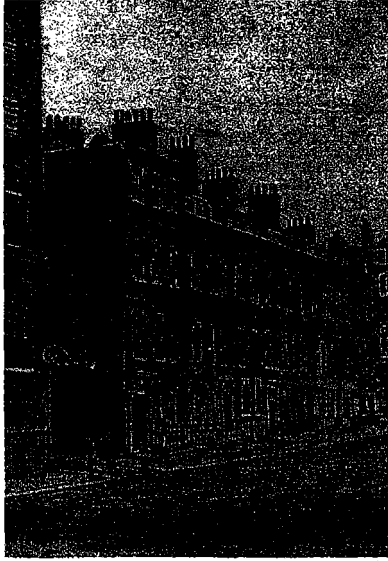
into cholera in the south districts of London in 1854 and have concluded that his epidemiological evidence was sound. Frost, for example, praised Snow's analysis as "a nearly perfect model," adding that "his argument has the permanence of a masterpiece in the ordering and analysis of a kind of evidence that enters at some stage and in some degree into every problem in epidemiology."<sup>65</sup> Snow's work has stood the test of time, which in itself answers the question as to whether Snow's evidence was sound. In this respect, Feachem's comment on the timelessness of Snow's work is pertinent: "This historical perspective is of more than academic interest, for those epidemiologists who today propose a primarily waterborne mode of transmission for cholera draw heavily on their justification on ... 19th century outbreaks and, in particular, place much emphasis on the pioneering epidemiological studies of John Snow."<sup>66</sup>

### **Impurity of An Extraordinary Kind: The Broad Street Outbreak**

While Snow was investigating the impact of cholera south of the Thames, what he called "the most terrible outbreak of cholera which ever occurred in this kingdom [of Great Britain]"<sup>67</sup> suddenly struck an area near one of London's graceful squares



*Golden Square, London (about 1740)*



*Broad Street, London (mid-19th century)*

north of the Thames – Golden Square – and including in particular the residential area in and around Broad Street. This outbreak started at the end of August 1854. It was fortuitous that Snow should have been on hand – Broad Street was only a few blocks from his home in Sackville Street, near Piccadilly – because his knowledge and understanding of the spread of cholera contributed to the management of the outbreak. His account of this outbreak and his part in investigating it, which he described in his monograph of 1855, further enhanced his status as an authority on cholera. It also made the outbreak one of the most celebrated episodes in the history of epidemiology, and *On the Mode of Communication of Cholera* became an epidemiological classic.

Although the Broad Street outbreak, is, in general, familiar as an episode in the history of medicine and particularly of epidemiology, many of the details of the outbreak and of Snow's role in solving the problems it raised are not. Much has been written on the Broad Street outbreak, but much that has been written about it is erroneous.<sup>68</sup> In particular, the myth has arisen that the epidemic ceased immediately after Snow, understanding

the cause of the outbreak, simply removed the handle of a water-pump in Broad Street. Here legend has usurped fact, for the statements in this sentence are untrue: Snow very soon realized that pollution of the well water was one of the causes of the outbreak, but he did not know how the well had become polluted, and this was ascertained, not by Snow, but by the local curate, Henry Whitehead; Snow himself did not remove the handle of the pump, though he was the one who recommended that it be removed; and the epidemic was on the wane before the handle was removed. Because of the profound significance of Broad Street outbreak both to the history of epidemiology and to the understanding of Snow's role as an epidemiologist, the story must be retold, and in detail.

The outbreak began during the night of August 31 and September 1, 1854. As the Reverend Henry Whitehead demonstrated, it originated in No. 40 Broad Street.<sup>69</sup> This house abutted, in front, on to the pump from which many of the people in the area (and some people further away) obtained their drinking water. At the end of August, a baby girl in this house became ill with diarrhea and, on August 28, 29 and 30, her mother had emptied "considerable quantities" of water containing the diluted "dejections" of the infant into the adjoining cesspool. The baby died on September 2, of "exhaustion after an attack of Diarrhoea four days previous to death." Whether the infant truly had cholera cannot be proved, though her illness preceded the explosive outbreak of cholera in the Broad Street area by about 48 hours and her death was recorded as being among the deaths from cholera. However, as examination later revealed, the basement of this family's house had "atmospheric connection," with the street sewer, and the cesspool was so "choked and defective" that its filthy fecal contents percolated into the soft black soil around, and hence into the well itself. Pollution of the well was inevitable, and it is likely that the infant's illness and the mother's actions were all that was needed to initiate an outbreak of cholera that killed some 600 persons within a few days.

The infant's illness, the mother's actions, and the manner in which the Broad Street pump was polluted were not known to Snow at the beginning of September 1854. However, once it became obvious that cholera had struck his area, he wasted no

time in finding out what was happening. The following paragraphs from Snow's monograph summarize his response:

*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 commencements of the outbreak, it might possibly have been still more impure. I requested permission, therefore, to take a list, at the General Registrar Office, of the deaths from cholera, registered during the week ending 2nd September, in the subdistricts of Golden Square, Berwick Street, and St. Ann's, Soho, which was kindly granted. Eight-nine deaths from cholera were registered, during the week, in the three subdistricts. Of these, only six occurred in the four first days of the week; four occurred on Thursday, the 31st August; and the remaining seventy-nine on Friday and Saturday. I considered, therefore, that the outbreak commenced on the Thursday; and I made inquiry, in detail, respecting the eighty-three deaths registered as having taken place during the last three days of the week.*

*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. In five of these cases the families of the deceased persons informed me that they always sent to the*



pump in Broad Street, as they preferred the water to that of the pump which was nearer. In three other cases, the deceased were children who went to school near the pump in Broad Street. Two of them were known to drink the water; and the parents of the third think it probable that it did so. The other two deaths, beyond the district which this pump supplies, represent only the amount of mortality from cholera that was occurring before the irruption took place.

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 everyone connected with the deceased persons; and in six cases I was informed that the deceased persons did not drink the pump-water before their illness.

The result of the inquiry then was, that there had been no particular outbreak or increase of cholera, in their part of London, except among the persons who were in the habit of drinking the water of the above-mentioned pump-well.

I had an interview with the Board of Guardians of St. James's parish, on the evening of Thursday, 7th September, and presented the above circumstances to them. In consequence of what I said, the handle of the pump was removed on the following day.<sup>70</sup>

Snow had quickly identified the problem, and the removal of the pump handle was a logical step — and a step that had been taken before to combat cholera in London.<sup>71</sup> By the time the pump was disabled, however, the attack was beginning to lose its severity, as the following table <sup>72</sup> shows:

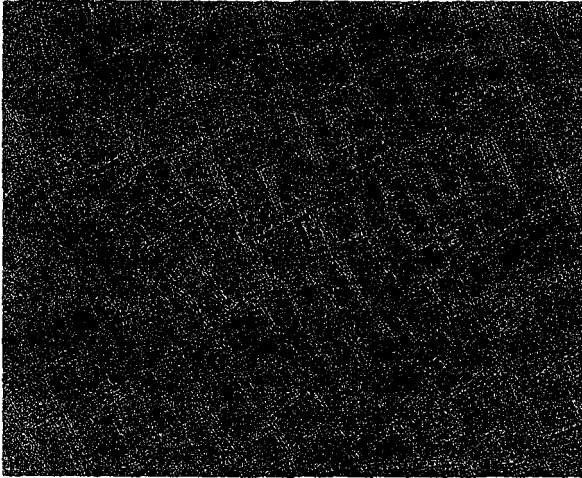
	August		September													
	31	1	2	3	4	5	6	7	8	9	10	11	12	13	14	
No. of deaths	3	70	127	76	71	45	37	32	30	24	18	15	6	13	6	
No of fatal attacks	56	143	116	54	46	36	20	28	12	11	5	5	1	3	0	

The outbreak reached its zenith on September 2, and by

September 7 the number of deaths had fallen to a quarter of the number on September 2. Similarly the number of fatal attacks had started to decrease before the pump-handle was removed. These facts must be noted, in view of the myth that removing the handle was what caused the outbreak to cease; for example, Benjamin Ward Richardson, Snow's friend and contemporary biographer, wrote that "the pump-handle was removed, and the plague was stayed."<sup>73</sup>

Snow himself explained the fading of the epidemic on grounds other than that of the removal of the pump handle. He wrote: "there is no doubt that the mortality was much diminished ... by the flight of the population, which commenced soon after the outbreak; but the attacks had so far diminished before the use of 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."<sup>74</sup> With respect to the population, Snow had observed that "the duration of cholera in a place is usually in a direct proportion to the number of the population,"<sup>75</sup> and he used this observation to support his general concept of the spread of cholera. He remarked that the connection between the duration of an outbreak and the population in the area was "a relation which points clearly to the propagation of the disease from patient to patient; for if each case were not connected with a previous one, but depended on some unknown atmospheric or telluric condition, there is no reason why the twenty cases which occur in a village should not be distributed over as long a period as the twenty hundred cases which occur in a large town."<sup>76</sup> The rapid exodus from Broad Street, which soon became deserted and lifeless, may well have been the principal reason why the epidemic began to wane so suddenly.

Snow's chief finding was that cholera was prevalent among those who had drunk the water from the Broad Street pump. He illustrated this by constructing a map of the area.



*Snow's map pinpointing cholera deaths (bars) in areas around Broad Street (summer 1854)*

In this map the Broad Street pump was the central point and black bars marked the sites of the houses where either a death from cholera or a fatal attack had occurred. The concentration of deaths was proportional to the proximity of the houses to the pump. Snow admitted that there were "necessarily" some deficiencies in the completeness of the map, for it was impossible to trace all of the deaths. For some of the persons who died in hospital, the number of the house was not registered; for those who died after their being moved to St. James's Workhouse it was, likewise, not registered; and for those workers and others who contracted cholera in the Broad Street area and died elsewhere, the place where they had been taken from was not given in the returns of death. As a result, Snow concluded, "the full extent of the calamity will probably never be known."<sup>77</sup> Even so, the map clearly illustrated the pattern of the epidemic. The two striking facts were these: "the deaths either very much diminished, or ceased together, at every point where it [became] nearer to send to another pump than the one in Broad Street," and "the deaths [were] most numerous near to the pump where the water could more be more readily obtained."<sup>78</sup> Any deficiencies in the overall findings, Snow thought, "probably do not

detract from the correctness of the map as a diagram of the topography of the outbreak.<sup>79</sup> He also thought that, had the localities of these "few additional cases" been ascertained, "they would probably be distributed over the district of the outbreak in the same proportion as the large number which are known"<sup>80</sup> In any event, Snow's map of the Broad Street outbreak remains one of the earliest spot maps of epidemiologic significance.

Snow explained the less obvious conclusions that could be drawn by studying the map. For example, the pump on Broad Street was not the only pump in the area. There was one in Little Marlborough Street, but its water was so impure that many people avoided using it, and those who died in houses near it had consumed water from the Broad Street pump instead. (The water of the Broad Street well was seemingly — but only seemingly — pure and refreshing most of the time, probably because of its content of carbon dioxide, which produced the fermentation of organic matter in the water.) There was another pump in Rupert Street, but some of the streets nearby — "as the crow flies" — were in practice "a good way removed" from the pump because it was situated at the dead end of Rupert Street. The other pumps were situated on the periphery of the area, and cases near them could have been explained on grounds other than consumption of their waters — for example, the general preference for the water from the Broad Street pump.

Snow also stated that it must have been "obvious" that those who died could have taken the Broad Street water without their friends knowing this. Such was Snow described in the following passage:

*The water was used for mixing with spirits in the public houses around. It was used likewise at dining rooms and coffee-shops. The keeper of a coffee-shop in the neighbourhood, which was frequented by mechanics, and where the pump-water was supplied at dinner time, informed me (on 6th September) that she was already aware of nine of her customers who were dead. The pump water was also sold in various little shops, with a teaspoonful of effervescing powder in it, under the name of sherbet; and it may have been distributed in various other ways with which I am unacquainted. The pump was frequented much more than is usual, even for a London pump in a populous neighbourhood.<sup>81</sup>*

In answering potential criticisms concerning cholera among those who apparently did not take the Broad Street water or its absence in some of those who lived or worked in Broad Street, Snow noted "certain circumstances" bearing on the subject of the outbreak of cholera that had to be mentioned.<sup>82</sup> Snow described these, case by case, and in doing so built up a convincing case that confirmed his general hypothesis that cholera was spread by the pollution of water.

Snow's case-by-case presentation of the argument that cholera was waterborne was essential in view of the strength with which people still clung to other concepts of the transmission of cholera — even though it was clear to Snow, and a few others, that "all the instances of communication of cholera through the medium of water [including the Broad Street outbreak] .. resulted from the contamination of pump-well, or some other limited supply of water."<sup>83</sup> One such fanciful concept was woven around an epidemic of plague and a pit in which the victims had been buried two centuries earlier. (The pit was supposedly to have been dug near the area of Little Marlborough Street, "just out of the area in which the chief mortality occurred."<sup>84</sup>) More often blamed were "effluvia" from the sewers passing into the streets and houses, but, as Snow noted, "that is a fault common to most parts of London and other towns."<sup>85</sup> Snow added that there was nothing peculiar in the sewers or drainage of this limited spot in which this outbreak occurred, and that Saffron Hill and other localities, which suffered much more from "ill odours," had been attacked very little by the cholera.<sup>86</sup>

The Broad Street epidemic was significant in many respects, but none more so than its contribution to the hypothesis that cholera was spread chiefly by water, which Snow was attempting to validate in 1854. (Other means of spread, such as contamination of food by flies, which Snow himself recognized,<sup>87</sup> did not invalidate his hypothesis, which embraced factors such as lack of hygiene.) Snow's own investigation of the Broad Street outbreak, in addition to his investigation south of the Thames, did much to confirm him in his belief and to convince others that cholera was waterborne rather than airborne, but he obtained further support for his view in a second investigation of the Broad Street outbreak.

This second investigation was conducted by a team of local parishioners. This team — the Cholera Inquiry Committee of the parish of St. James, Westminster — brought Snow in as a member and as, scientifically, the leading investigator. The committee, numbering 17 members, reported its findings and conclusions in the *Report on the Cholera Outbreak in the Parish of St. James, Westminster, during the Autumn of 1854*, which was published in July 1855,<sup>88</sup> soon after Snow's own monograph was published. Together, these two publications had a synergistic effect, as the reviewer of both publications remarked when he wrote that "the publication of these books must exert considerable influence on sanitary reform, and in fact prove the position, higher to scarcely demonstrated, that zymotic diseases are, to a certain extent, removable by sanitary measures."<sup>89</sup>

Snow, one of six physicians on the committee, together with three of these doctors, visited 316 houses in the area. Many of these houses he knew well, for some of his patients lived in them. By visiting the area he gained an excellent idea at first hand of the havoc wreaked by the outbreak of cholera, and this local knowledge, together with the knowledge he had acquired from his investigation into cholera south of the Thames, enabled him to contribute constructively to the vestry committee's investigation. His participation in the vestry committee's study also confirmed his concept of the way in which cholera spread in a community. An excerpt from the report Snow presented to the committee on December 12, 1854, illustrates this, and emphasizes his multifactorial concept of the manner in which cholera spread through a community:

*I wish it to be understood that I do not attribute every case of Cholera to the use of polluted water. It is my opinion that every case is caused by swallowing the peculiar poison or morbid matter of Cholera, which has proceeded from a previous patient sick of the malady; but this morbid matter need not be in water, and there are facilities for its being accidentally swallowed, and propagating the disease, without the aid of water. This is more especially the case in the crowded dwellings of the poor, where a number of persons live, sleep, cook, and eat in one room. I do not, therefore, attribute every case of Cholera in the parish to the water of*

*the pump well in Broad Street, but certainly those which constitute the great outbreak which took place at the end of August, and which suddenly raised the mortality of this disease from about five in a week to nearly 500.*<sup>90</sup>

The opportunity that Snow had to study the Broad Street outbreak was, therefore, valuable to him in consolidating his concept of cholera. It also enlarged the body of data that enabled him to prepare the second edition of his monograph on cholera, which he completed just one day before he submitted his report to the vestry committee.

Snow's contributions, based on his great knowledge of cholera that had been built over the years, were especially valued by the vestry committee, but so were those of the curate, Henry Whitehead. Whitehead's role was most constructive. His investigation, in an area whose residents he knew so well, was especially fruitful. It was Whitehead who traced the outbreak to its origin in No. 40, Broad Street; he discovered this sometime after the actual outbreak, when he was looking through Returns of Deaths in the area<sup>91</sup>. What Whitehead found was of the same order of significance, in terms of factual weight, as Snow's investigation. Whitehead's investigation was fruitful in another respect: "entertaining at first adverse views, [he] ended his special investigation of Broad Street by a remarkable confirmation of Dr. Snow's numerical results."<sup>92</sup> The following summary of Whitehead's findings, taken from the vestry committee's report, slightly abridged, gives a sense of the curate's work:

- 1... *of the 90 fatal attacks among its resident population, 84 took place between 31 August and 6th September, 56 between 31st August and 2nd September, and 50 on September 1st and 2nd.*
- 2... *of the 90 deceased persons 45 positively drank the water shortly before illness; and that of only 13 altogether is it at all confidently said that they did not drink it. Moreover, that of the above-mentioned 84, the non-use of the water is asserted of only 8; and of the 56 persons attacked between 31st August and 2nd September, it is positively affirmed of only 2 that they did not drink this water.*

- 3... that undoubtedly of 100 persons residing on Broad Street, who were attacked with Cholera or Diarrhea (including dead and surviving), 80 drank the water, whilst 20 are affirmed not to have drunk it; whereas out of 336 persons living in that street and who were not attacked with either disease, only 57 had drunk the water, whilst 279 had not.
- 4... that there is a great probability that the numerical proportions were even more remarkable than this, all cases involved in any doubt having been rejected.
- 5... that in regard to the two factories situated next door to each other, both equally well arranged in regard to other sanitary conditions ..., the workmen of one in which the mortality was high had the water for drinking purposes, whilst those of the other never drank it, and entirely escaped, - the former fact being strengthened by the circumstances that the family of the proprietor never used the water and did not suffer.
- 6... that, in addition to the contrast pointed out by Dr. Snow as regards the exemption from Cholera on the part of the 70 men employed at the Brewery where the water was not drunk, and the amount of suffering amongst the 200 persons engaged at a neighbouring factory where the water was drunk, - a contrast even more remarkable is found between the workmen of this Brewery and those engaged on the closely adjoining unfinished lodging houses called Ingestre Buildings; for amongst these latter the water was in use, and Cholera proved fatal to 7 out of 35.
- 7... that of 97 people residing in 10 houses in which no attack occurred, 87 did not drink the water at all, whilst the remainder did not drink it during the height of the outbreak, or drank it either in small quantities or mixed with spirits.
- 8... that in a great number of particular instances ... the evidence of an injurious influence exercised by the water becomes strengthened as the inquiry becomes more strict and searching.



- 9... *that the want of good sanitary arrangements in certain houses operated by compelling the residents to resort to the pump for drinking-water; and that, on the contrary, in certain instances where the drains were in good order, the cisterns were clean and the inhabitants did not send to the pump.*
- 10...*that through the district generally the aged and infirm, when isolated, escaped, not merely because they had more house accommodation but because they did not use the water, having no one to send for it.*
- 11...*that on looking beyond Broad Street to certain cases at a distance from the pump, a remarkable amount of evidence still presents itself in support of the facts observed in its immediate vicinity.*<sup>93</sup>

Whitehead's investigation was valuable because of "his previous knowledge of the district, the people and the outbreak, and by the gradual and cautious character of his investigation." His investigation neatly complemented the investigation of Snow, who "seized the important advantage of an early inquiry."<sup>94</sup>

The vestry committee's report augmented the data that could be applied to the solution of the cholera problem in the middle of the 19th century, for it provided much factual information that supported the waterborne hypothesis and it presented a cogent argument in favour of this hypothesis. The following excerpt from the vestry committee's report gives a sense of these citizens' rational approach to the problems they faced:

*... the weight of both positive and negative evidence appears to be clearly and unmistakeably in one direction viz. — to show that the water had some preponderating influence in determining an attack. If it be supposed that the drinking of Broad Street water by those who died was, a mere coincidence dependent on, and to be expected from, the fact that so many persons in the neighbourhood, especially in Broad Street, constantly drank it, it must be remembered that this fact of coincidence also bears with greater force on any mere atmospheric hypothesis; for whereas of those in Broad Street itself who during the great outbreak coincidentally breathed*

*the air only one in ten died, on the other hand, of the coincident water drinkers, who of course were fewer in number, a much larger proportion was fatally seized. If some idiosyncrasies or resisting power can be assumed to explain the escapes amongst the air-breathers, it must equally be admitted in aid of those of the water drinkers who did not suffer; and it is obvious that the demands upon so unsatisfactory an explanation are much smaller in the latter than in the former case. It if be urged, in explanation of an atmospheric influence, that Cholera might be conveyed exclusively to some by a partial distribution of an impure air, it may be replied that no consideration of the streets, local levels, sewer-grates, house drains, or direction of the wind, will explain the existence of such partial atmospheric impurity, whereas the individual use of the water has been actually traced, and its consequences may not unreasonably inferred.<sup>95</sup>*

Thus the vestry's cholera inquiry committee served, as did Snow's own investigation, together with the accounts of cholera world-wide that Snow discussed in his monograph, to confirm Snow's hypothesis. As the report noted, "the Committee finds support from the gradually accumulating evidence collected in other localities, as to the important influence of contaminated water in increasing Cholera ... "<sup>96</sup> Others were beginning to reach the same conclusion. The Registrar-General, for example, was quoted as saying that "the balance of mortality is heaviest in every district and in every week against the impure water to an extent that leaves little room for doubt on the mind."<sup>97</sup> Dr. John Sutherland, in alluding to the Registrar-General's inquiries, concluded that "it is difficult to resist this statistical evidence of the predisposing effect of the Battersea water, and of the loss of life which has arisen from its use"<sup>98</sup> — even though here he appeared to believe that polluted water was only a predisposing factor. Snow must have been gratified by such indications of the change in thinking about the causation of cholera, not only because his own views on cholera were at last more generally becoming vindicated but also because it would become possible to prevent the occurrence of the disease that had slaughtered so many thousands of people in his own lifetime.

Snow's studies of cholera did much to change the thinking in England about the various ways in which cholera was transmitted. This change, however, took many years to take hold, and, because Snow died not long after the 1854 epidemic and the publication of the second edition of his monograph, he was not able to witness the virtual eradication of cholera from England and to appreciate that cholera would only return to England in epidemic form once more—as “the final catastrophe.”<sup>99</sup> Nor was he able to continue putting forward logical arguments to convince those, like Sir John Simon, who clung to the miasmatic doctrine of cholera, even after this final epidemic was shown, notably by Farr,<sup>100</sup> to be clearly spread by polluted water. Yet the final triumph was his, even though his early death prevented him from personally witnessing victory over cholera in his own country.

Snow was one of those few Victorians who understood that epidemic disease, while related to the dismal social and economic conditions, was primarily caused by specific agents rather than an undefinable and unprovable “miasma” — and acted on this understanding. He was one of that small band of Victorian physicians — men like Farr, Budd and Richardson, for example — whose persistence in seeking the truth (even though they did not see it as clearly as Snow) finally overcome the inertia of erroneous beliefs and misguided ideas and of vested interests. Until the end of his life Snow continued to exert the influence that came with his now considerable authoritative stature, though the last few publications concerning public health<sup>101-105</sup> have none of the significance and influence of his earlier work. But Snow will always be remembered for his concern with the health of his fellow citizens, and a paragraph from the second edition of his monograph enhances our remembrance of him. This paragraph will serve not only as a succinct summary of Snow's ideas and work on cholera but also as a clear indication that his thinking was shaped not only by his acute clinical perception as a physician but also by his epidemiological sensitivity to the manifold picture of the interrelationship between disease and society:

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*All the evidence proving the communication of cholera through the medium of water, confirms that with which I set out, of its communication in the crowded habitations of the poor, in coal-mines and other places, by the hands getting soiled with the evacuations of the patients, and by small quantities of these evacuations being swallowed with the food, as paint is swallowed by house painters of uncleanly habits, who contract lead-colic in this way.<sup>106</sup>*

Through his efforts, Snow had brought sense to the thinking of his colleagues on cholera. His own clear thinking elucidated the cause of the disease, and it initiated the institution of measures that would result in its prevention. Countless men, women and children would benefit. In medicine the art is long and a physician's life is short; but John Snow's art was such that even his short life was long enough to permit him to initiate the final battle against the "blue death." That triumph is the measure of this one man's untiring efforts.

## **Part Three**

# **LEGACY**

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