Commentary: Confronting unexpected results:
Edmund Parkes reviews John Snow

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John Snow’s *On the Mode of Communication of Cholera* is one of the most famous works in the history of epidemiology. It first appeared as a modest pamphlet in 1849 in the midst of Britain’s second epidemic of cholera. A second and more substantial edition appeared in 1855 following another but smaller outbreak, and it is this second edition for which Snow is remembered today. Edmund Alexander Parkes was a logical choice to review Snow’s second edition. As a member of the Army Medical Service in India and Burma, Parkes had seen cholera where it was endemic, and he had published substantial pieces on the pathology and therapy of cholera, dysentery and hepatitis. When he returned to England in the middle 1840s and received his appointment as an assistant physician at University College Hospital, London, he had a reputation as an up-and-coming medical authority on epidemic diseases and hygiene. By 1855, when he reviewed Snow, he was first physician at University College Hospital and Professor of Clinical Medicine at University College. He later became Professor of Military Hygiene at the Army Medical School at Netley. Before his early death at age 57, he would serve as editor of the *British and Foreign Medico-Chirurgical Review*, as Goulstonian Lecturer to the Royal College of Physicians and as a member of the General Medical Council, and be elected Fellow of the Royal Society of London.

In short, Parkes was an established authority on epidemic diseases and hygiene. In fact Snow cites Parkes’s pathological work in his second edition. Parkes’s views were certainly informed and mainstream in the mid 1850s. His critique of Snow’s work is consequently useful in helping us understand how Snow’s contemporaries reacted initially to his cholera theory, and perhaps more importantly, it underlines the magnitude of the conceptual changes Snow’s work represents.

In order to fully understand Parkes’s review, it is important to realize that when Asiatic cholera appeared in Britain for the second time in 1848–49, the newly-created General Board of Health engaged Parkes to undertake a study of the first cases to appear in London. The idea was to use these cases
to test the theory that cholera was contagious. Parkes himself suggested the study be undertaken for this purpose. The question was an active and important one in the 1840s. Medical opinion was strongly divided on whether diseases such as cholera could be transmitted person to person, with the majority of the profession holding that they were not communicable. Almost no one doubted that a small number of diseases such as smallpox, syphilis or rabies should be classified as contagious, since they were known to be caused by inoculable material which passed between persons, but many other important epidemic and endemic diseases, including cholera, typhoid fever, typhus, yellow fever and bubonic plague, seemed to defy a contagionist explanation. Although it is no coincidence that the anti-contagionist sentiment grew with the political influence of merchants and manufacturers who favoured free trade and opposed the quarantines and isolation which a contagionist model would suggest, there were very good critical or observational reasons for holding that contagion, as then understood, could not adequately explain the behaviour of some of the most important epidemic diseases.4

Parkes’s commissioned study appeared as a 25-page article in the British and Foreign Medico-Chirurgical Review in 1849.5 It is good to consider this article in order to understand the frame of mind that Parkes brought to his review of John Snow’s work 6 years later. In opening his 1849 report, Parkes sought to place himself in the scientific vanguard by arguing that the experience of medical observers during the preceding several decades, especially those working in the tropics, had undermined a strict contagionist explanation of several important epidemic diseases including cholera. In view of this new evidence, Parkes identified himself as a modified contagionist. By that he meant that he had no doubt that cholera was caused by a specific material cause, a poison, but he sought to demonstrate that strict contagion could not explain cholera’s epidemic behaviour. In this study for the General Board of Health, Parkes chose to investigate the first 31 cases of cholera reported to the Board. His approach was overtly critical. He insisted that for a study of this type to be successful every ‘reputed case’ must be known and every ‘reputed case’ investigated. Almost all cases, he insisted, had been investigated by the agents of the General Board of Health. Parkes had seen many of the patients himself. He had examined the records of all cases and excluded cases that were doubtful. He thought, for example, that the first three reported cases might not have been cholera. He found that the remaining 28 early cases in London occurred in 10 districts widely separated geographically. No case could be shown to have been in contact with a previous case in another district. He went to some lengths to show how absurd it was to imagine a series of accidents that would have caused even the two most casual contact between these cases: a gas works labourer in Lambeth, a sailor in Horsleydown, the wife of a coal porter in Fleet Street, a prisoner on a hulk at Woolwich or a patient on a hospital ship at Greenwich, etc. He drew particular attention to the cases on the hulk and the hospital ship. These patients had been in what amounted to quarantine from the rest of London, yet they were among the very earliest to contract the disease.

With the benefit of hindsight we might suggest that the cases reported to the newly created General Board of Health probably represented the tip of the epidemic iceberg, and we might find the proximity of several of the reported cases to the sewage-contaminated Thames particularly interesting. Parkes considered neither. He did consider the possible transmission of the cholera poison through the air, only to quickly dismiss it. If the poison were an inorganic gas, it would quickly dissipate. If it were an organic particle, it would be quickly oxidized. Furthermore, if it spread between persons through the air, the cases should be concentrated near the earliest case. Instead it appeared that the poison would have to have passed harmlessly over whole neighbourhoods before affecting the next person. Parkes concluded that key to understanding cholera’s epidemiology lay in environmental conditions. These he believed would be found to affect both the reproduction of the cholera poison and the susceptibility of the human body. In 9 of the 10 localities where cholera first appeared, environmental conditions the anti-contagionists blamed for epidemic outbreaks prevailed, ‘moisture, effluvia, and impure air’. He acknowledged that the same dangerous conditions existed in other neighbourhoods where cholera did not occur, and he postulated the existence of a ‘choleraic constitution’ to explain which of the unsanitary districts suffered early in the outbreak.

Clearly then Parkes was not sympathetic to Snow’s theory and, as a critical reader, he subjected Snow’s volume to very close scrutiny and pointed out weaknesses or lapses which a modern reader, who knows that Snow was right, would be likely to miss or gloss over. The earliest evidence Snow marshalled were reports of small cholera outbreaks in which one house or group of houses experienced cholera and an adjacent house or group did not. Snow then suggested ways in which the well or cistern used by the cholera victims may have been contaminated, whereas the water used by the adjacent properties escaped such contamination. With a couple of exceptions, Parkes found such evidence worthless. Snow usually told his readers nothing about the number of people at risk in both groups, nor did he describe other factors which informed medical opinion held to be implicated in cholera outbreaks. He could not actually prove that the suspect water had been contaminated by the intestinal discharge of a cholera patient. In the worst examples, Parkes held, Snow’s explanation assumed the conclusion he was trying to prove.
Parkes next turned to the two examples for which Snow is most famous today. The outbreak near Broad Street, Golden Square, was another local outbreak Snow suspected was caused by a contaminated well. The episode differed from the ones Snow had already reported on both in scale and in the attention Snow devoted to it. Parkes correctly summarized Snow’s evidence from Broad Street. Working from the records of the 89 cholera deaths, which took place in 1 week in the three sub-districts surrounding the Broad Street pump, Snow tried to show that most of the dead were known to have drunk from the pump or, because of the location of their home or their path to school, were likely to have drunk from the well. On the other hand, individuals known to have access to alternative sources of water, the inmates of a workshop or the workers at a local brewery, for the most part escaped the disease. Parkes conceded that Snow demonstrated that there was an intense outbreak in the neighbourhoods near Broad Street and that people who did not drink from the Broad Street pump did not contract cholera; however, Parkes contended, Snow did not provide evidence that the suspect water was contaminated with cholera poison. Other wells whose water was more grossly impure did not apparently cause cholera. Snow also did not rule out other possible causes of the outbreak, nor could he account for the sudden decline in cholera cases before the pump handle was removed. Parkes observed that London was served with many public pumps, so that wherever an intense outbreak of cholera occurred, it was likely that there would be a pump near its centre. The clustering of cases did not prove transmission by water; Parkes, in fact, found the distribution of cases in Golden Square more consistent with the air-borne transmission of cholera poison.

Parkes pointed out that Snow did not give some of his evidence the emphasis it deserved, and he described the case of the Hampstead widow and her niece a ‘most extraordinary case, which, if there is not some fallacy, is certainly unanswerable.’ Both women drank water brought to them from the suspect well, and both of them died of cholera shortly thereafter, although they lived in districts were there were no other cholera deaths. Since he remained sceptical, Parkes apparently thought there must be some fallacy.

Finally, Parkes came to what he acknowledged was Snow’s most original and substantial evidence. This was the outbreak among the customers of two water companies serving south London. One district was served by both companies which competed house by house for customers. In other respects, the two groups of households were identical in environmental and socio-economic circumstances. Furthermore, recently there had been a significant change in the purity of the water supplied by one of the companies. During the previous cholera epidemic, the one of 1848–49, both companies drew their water from the Thames near London Bridge and pumped it unfiltered and untreated to their customers. Before the epidemic of 1853–54 the Lambeth Water Company moved its inlet upstream above Thames Ditton, where it was above tidal influence and the grossest sewage contamination. The Southwark and Vauxhall Company continued to pump water from the old source. It seems then that this was the ideal circumstance to test the waterborne theory.

The incautious reader today easily concludes that Snow’s evidence was conclusive. Parkes correctly pointed out that, in this second edition, Snow did not have the evidence he needed. Parkes emphasized two great weaknesses. First, as Snow acknowledged, it was difficult to learn the water supply of individual houses. Of necessity, Snow worked from records of cholera deaths supplied to him by the General Register Office. When he visited a house where a cholera death occurred, the occupants often did not know the name of the company that supplied their water. Snow devised a simple chemical test for the saline content of the water on the assumption that the water under tidal influence would have a high salt content. Parkes objected that there was wide variation in the saline content, depending on the state of the tides when the water was drawn. More seriously and less obviously from Snow’s account, Snow did not know the number of people exposed to the two water supplies in the mixed district. In fact he did not even know the number of houses supplied by the two companies in the mixed district. Parkes’s careful reading found that the cholera mortality figures Snow gave were for all customers of the two companies and not only for the customers living in the district served by both companies. This was fatal weakness, because customers of these two companies living in different districts served by only one company might be subject to differences in environmental factors thought to be relevant to cholera’s prevalence: elevation, soil condition, organic waste, housing density, occupation etc.

Throughout his review Parkes treated Snow’s theory as highly speculative. Early in his review he rejected Snow’s pathological reasoning that since a cholera attack begins in the gut, its cause must be swallowed. Toward the end of his review Parkes pointed out that Snow also thought, even in the absence of evidence, that other diseases are waterborne, including yellow fever, intermittent fever (i.e. malaria), plague and typhoid fever. Still, Parkes had to admit that Snow had provided highly suggestive evidence, and he was willing to accept contaminated water as a predisposing cause of cholera, i.e. one of several factors to be considered in explaining cholera outbreaks. This admission brings us to a fundamental difficulty Snow’s contemporaries had in accepting his conclusions on cholera. These contemporaries were used to looking to multiple causes to explain outbreaks of diseases like cholera. These included a variety of environmental factors, especially those that were believed to
influence the state of air. Snow, on the other hand, was proposing that cholera had one and only one cause, the passage of material from the intestine of a cholera patient to the stomach of the next victim. As Parkes observed in opening his review, ‘Dr. Snow believes not only that cholera is propagated by means of water, but that it is solely and exclusively so propagated’.6 This seemed a very rash conclusion, one that ran counter to informed scientific judgment.

A number of medical men who had made a serious study of cholera outbreaks, including William Farr, who had assisted Snow in his study in south London, were willing to add sewage-contaminated water to the list of factors producing intense cholera outbreaks.7,8 But it took some time for Snow’s theory to gain full acceptance. Some of the objections to his evidence in the second edition were soon eliminated. Local investigators showed how the well served by the Broad Street pump had been contaminated and identified a probable index case, and Snow acquired the data he needed on the numbers of households served by both water companies in the mixed districts.8 Then in the next cholera epidemic, the one of 1866, after Snow’s death, Farr himself presented strong statistical evidence that an intense outbreak in east London was caused by sewerage-contaminated water.7 By this time informed medical opinion was changing.

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References