Commentary: Nobody loves a critic: Edmund A Parkes and John Snow’s cholera

Tom Koch

Department of Geography (Medical), University of British Columbia, 1984 West Mall, Vancouver, BC, Canada, V6T 1Z2
E-mail: tomkoch@kochworks.com

Accepted 20 June 2013

Science isn’t about being right. It is about convincing others of the correctness of an idea through a methodology all will accept using data everyone can trust. New ideas take time to be accepted because they compete with others that already have passed the test. New thinking needs a strongly favoured methodology and an iron-clad application if it is to triumph, replacing the old.

Journal critics are the first line of defence against ideas and research projects that seem promising but have yet to be vetted, their methods analysed carefully. Despite the importance of that service, the critic’s role is typically disparaged because—let us be frank here—nobody likes critics. If they praise something they’re assumed to be sycophants and if they disparage published work they’re dismissed as merely grumblers.

History is not kind to critics. Its writers typically dismiss where they do not simply ignore those whose careful reviews argue caution in the face of works destined to become, in the future, classics. Think, here, Prince Peter Kropotkin whose naturalist studies focused upon the limits of Charles Darwin’s evolutionary theory and the direction in which research based upon it would be best directed. Only today—more than 130 years later—is the importance of his critique being acknowledged.

There are good critics, of course, even great ones. The best are not only prominent in their field but also stylish essayists whose careful insights educate the general and the professional reader alike. Harvard biologist Richard Lewontin is a current example, an essayist who enfolds each review within an erudite recital of the state of the science being discussed. The result leaves the reader (and author) gasping: ‘I wish I had said that.’

As an example of a good critic unfairly dismissed by history think Edmund A Parkes, the British physician and researcher who reviewed John Snow’s famous 1855 text, *On the Mode of Communication of Cholera*. In a seven-page, approximately 7800-word essay, Parkes carefully considered and found wanting Snow’s argument that cholera (and plague, and typhoid fever) was solely waterborne. Although the myth of Snow’s brilliance insists his critics were wrong, a careful reading of Parkes’ concerns insists that the myth of Snow is overstated. Yes, cholera is a waterborne disease. But were we to read Snow’s work with attention but without foreknowledge we, too, would find its argument incomplete.

This review of the 19th century debate over cholera has more than historical significance. It pits a simplistic, focused explanation against one that was broad and multifactorial. And, too, it demands attention be paid to the researcher’s methodologies and their sufficiency, not just results. Finally, it pits the myth of the lone researcher against the reality of science as a complex, communal, interactive process. In a time of rapidly evolving, epidemic zoonotics, the lessons of that earlier debate are as contemporary as the evolving state of the mutating coronavirus that so concerns us today.

**Edmund A. Parkes**

The choice of Edmund Parkes assured a knowledgeable and informed review of Snow’s privately published 1855 text. A former military physician with ‘considerable experience of tropical diseases’, including cholera, in 1849 Parkes was elected a special professor of clinical medicine at University College where he was also appointed as a physician. He had published at least three works on cholera: ‘On Asiatic and Algide cholera’, written during his service in India, was published in 1847; ‘Intestinal discharges in cholera’ and then ‘Early cases of cholera in London in 1848’. A frequent contributor to the medical journals of the day, he also authored several well-regarded textbooks. And, from 1852 to 1855, he was editor of the then prestigious *British and Foreign Medico-Chirurgical Review* in which his review of Snow’s work was published.

Expert in both chemistry and physiology, he was an authority in the area of ‘modern hygiene’ which today we call public health. His textbook, the *Manual of Practical Hygiene*, went through four editions in his lifetime. An eighth edition, published in 1891, was translated in several European languages.

1553
And so, like Snow, Parkes was an extremely well-published medical author, researcher and sanitarian. He was similarly engaged in the study of cholera and published in the field before Snow’s first monograph on cholera in 1849. It is hard to think of anyone better qualified to review Snow’s 1855 tome.

**John Snow**

Snow’s work, as G Davey Smith so nicely put it, ‘appeared amidst a veritable spate of speculation, experiment, investigation and recommendations regarding cholera’. Following the first global pandemic, which began in India in 1817, a generation of researchers assembled and reviewed a mass of evidence. On that basis they debated from the 1830s through the 1860s the nature of cholera, the mechanisms of its local transmission and the general means of its broad diffusion. Snow was not the only author to propose a waterborne disease. William Budd was another. Most, however, did so within a more or less multifactorial context that acknowledged then prevalent miasmatic theories of airborne disease generation and diffusion.

For others, water was part of the cholera story but for Snow it was the whole story. Thus he proposed not only a novel theory of cholera as a water- rather than airborne disease but advanced simultaneously a general theory of waterborne disease that broke with accepted theories of disease as fundamentally airborne. The onus was on Snow, therefore, to make the case for his theory through his studies of cholera.

In 1849 Snow published a monograph arguing cholera as solely waterborne. His argument was based upon what recent biographers have called ‘a complex blend of epidemiological evidence, pathological observations, and bold analogies’. Two local outbreaks, one at Surrey Court, Horsleydown, and another at Albion Terrace, were the epidemiological centrepiece of this publication. In both microstudies, Snow concluded contaminated local water sources were the source. Other investigators drew different conclusions based on an air- rather than waterborne disease theories.

Snow admitted his evidence was ‘scattered and general a nature’, and his theory was thus offered ‘not as matters of certainty, but as containing a greater amount of probability in their favor than any other’ (quoting Snow on the Mode of Communication Cholera). Almost immediately, he published two further papers attempting to flesh out the evidence and between 1849 and 1856 published frequently in the field, arguing time and again that cholera was solely the result of contaminated water and transmitted by oral-faecal contamination. Period.

**The review**

Parkes began his review this way: ‘None of our readers can be ignorant of the opinions of Dr Snow on the communication of cholera by means of drinking water, nor the perseverance and energy with which he has sought for facts to corroborate this view’. The text under review, Parkes added, recapitulated ‘all the evidence [he] has hitherto published, with the addition of certain facts lately acquired’. The first part of Parkes’ essay covered the older data, carefully reviewing 11 separate cases of localized cholera outbreaks described as evidentiary by Snow. Time and again, Parkes complained that Snow continually stated possibilities as certainties and suppositions as fact. In a review of the Horsleydown data, for example, ‘instead of leaving the origin of the first case uncertain’, Parkes writes, Snow assumed without conclusive evidence its source; at Albion Terrace he insisted without proof that cholera ‘must’ have entered local drainpipes in the water even though other investigators argued different explanations.

‘The point’, Parkes wrote in exasperation, ‘is to prove the fact of water being the agent, and not to assume it, and then to seek for some other explanation of these cases for which the presumed contamination cannot account’. And here Snow’s methodology, and the data it required, were wanting. Time and again, relevant data by which Snow’s theories might have been rigorously considered within the science of the day were lamentably absent. ‘Now, certainly in no less than seven of the eleven cases’, Parkes concluded, ‘the evidence to prove the fact of water being the agent, and not to assume it, and then to seek for some other explanation of these cases for which the presumed contamination cannot account’. And here Snow’s methodology, and the data it required, were wanting.

**New cases**

Snow’s 1855 text included two critical new studies. The first was the ferocious, if localized, Broad Street outbreak and the second his study of the South London epidemic, both occurring in 1854. In the latter study, Snow proposed a ‘natural experiment’ in which cholera mortality would be related at various scales to different water sources in the South London cholera epidemic of that same year. This ambitious idea was later advanced as a rationale for his epidemiological fame.

**Broad Street**

Perhaps the most famous of Snow’s examples, his analysis of the Broad Street outbreak exemplified for Parkes the author’s conceptual and methodological limits. Snow employed two very different types of analysis, both then common in disease studies. The first included a recitation of case reports and the second was cartographic. Famously, Snow applied for the mortality reports from the General Register...
Office and had the first weeks of the outbreak's mortality included on a map of the Broad Street area with, of course, the location of water pumps that provided district water. 'It would clearly appear', agreed Parkes, 'that the center of the [cholera] outburst was a spot in Broad-street, close to which is the accused pump; and that cases were scattered all round this nearly in a circle, becoming less numerous as the exterior of the circle approached.'

But to Parkes the centric distribution appeared more likely to present the image of an airborne rather than waterborne pattern of disease diffusion. 'If it were owing to the water,' he asked, 'why should not the cholera have prevailed equally everywhere the water was drunk?' And whereas the epicentre of the outburst was clearly in the vicinity of the pump 'there are, indeed, so many pumps in this district that wherever the outbreak had taken place, it would most probably have had one pump or other in its vicinity.'

Snow provided neither the type of comparative nor even descriptive statistics, then widely employed, that might have strengthened his mapped argument. In the 1854 Broad Street study the local curate, Rev. Henry Whitehead, published in 1854 a monograph replete with descriptive statistics of the outbreak and its relative impact on citizens. Without that type of analysis Snow's mapped argument was open to multiple interpretations.

Recognizing this deficit, perhaps, in an 1855 report to a parish inquiry Snow remapped the data, correcting several small errors, and included an irregular polygon based on greater proximity to the Broad Street well than to all others (Fig. 1). He did not, however, use this to calculate relative mortality among persons living within this area. Nor did he create other polygons around other pumps in a manner that would permit comparisons of mortality based on population between the Broad Street and other pump regions. As a result the map was inconclusive and, in Snow's study, statistics that might have strengthened his case were largely absent.

Parkes summed up the more general limits of Snow's argument this way: 'The weak points in this array of evidence are, I suppose, the want of proof of

Figure 1 In a later map Snow included a 'nearest neighbor' irregular polygon to enclose the population closest to the Broad Street pump. He did not, however, use this to calculate comparative mortality ratios that would have helped his case.
contamination of water, or rather, the evidence in favour of its purity; 2nd, the deficiency in negative evidence, that there was no other local cause which produced the partial outbreak; and 3rd, the fact that the disease ran rapidly to its acme, and then declined, while the water supply remained the same'.

These were serious deficiencies. Indeed, Snow admitted, examining the pump's water, 'I found so little impurity in it of an organic nature that I hesitated to come to a conclusion'. In 1854 and early 1855, available evidence did not support the idea that the well itself had been contaminated. 'It will have been observed', wrote Parkes, 'that the contamination of the pump water with drains, or by any other method, is not even attempted to be proved....' If the reasons for the outbreak's rapid onset were unclear so, too, was any rationale for its decline.

Snow's stated goal, Parkes lectured, was 'not to prove that bad water acts as a predisposing cause, but that the water contains itself the cause of cholera. To prove so weighty a fact, we require not only positive, but negative evidence...that no other circumstance existed which could explain the attack except the contaminated water'. Snow briefly considered but as briefly dismissed then current arguments implicating the foul-smelling, new sewer lines added to the district in 1851 or 'bad airs' emanating from the 1665 plague burial sites that had been punctured by the new sewers. Indeed, Snow's map did not include the sewer lines— included in three other contemporary maps—and misrepresented both the size and location of the former plague burial pit whose southeast corner was a short block from the Broad Street pump (Fig. 2). Snow's report was published before an 1855 engineering study that revealed a break in the brickling of the Broad Street well and visual evidence that materials flowed from a local cesspool into it. Also in 1855, Reverend Whitehead identified the index case in his report for a parish inquiry into the outbreak, a family that lived near and used that cesspool for its wastes. Had Snow not published prematurely, before those data were available, and had he added even minimal comparative and descriptive statistics, his case would have been more persuasive.

The South London epidemic
Snow's study of the South London epidemic, affecting thousands rather than hundreds of Londoners, was similarly deficient. Ambitiously, he attempted to distinguish mortality on the basis of two different water company jurisdictions, the 'good' waters of Lambeth Water Company and the 'bad' waters of the Southwark and Vauxhall Company that drew its water from Battersea in an area of Thames River pollution.

Figure 2 In the Broad Street study Snow located the 1665 plague burial site as a small oval at the northwest of the map. It was, however, a large area whose southeast corner was a block from the Broad Street pump. New sewer lines had been added to the district in the early 1850s.
In service of this study, Snow had drawn a map of the epidemic area of South London in which the water service jurisdictions were identified. He then attempted to argue that the mortality in areas supplied by the Southwark and Vauxhall Company was far greater than in the area supplied by the Lambeth Water Company, which drew its waters from the cleaner area of Ditton. Unfortunately, when he was writing the book, he later admitted, '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 each district and subdistrict, on account of the later circumstances not being known'.

Parkes made very clear that without those data Snow's *experimenta cruxes* could not go forward. ‘Snow endeavours to meet this difficulty by giving, from the Parliamentary return, the number of houses supplied respectively by the two companies. But this return applies to the entire districts, and not to the special district where the supplies are intermingled, so that really we are in doubt whether the Ditton water is supplied to half of this special district, or to a quarter or a tenth part’. The required data became available after Snow’s book went to press. They were then carefully analysed by the Board of Health’s Dr John Simon in a study ‘presented to both houses of Parliament by Command of her Majesty’ and published in 1856. In a statistical tour de force he found that there did appear to be, as Snow predicted, a 3:1 difference in mortality ratios at the scale of general water supply jurisdictions (Fig. 3). But that difference disappeared when data were analysed at finer registration district and subdistrict levels. Without better methods of statistical analysis or a definitive identification of a cholera agent, Simon concluded, water was implicated as at least a contributing source, but its acceptance as the sole source of the outbreak could not be proven.

Snow responded to Simon’s paper with his own. But Simon was right. Absent a kind of Bayesian analysis, and the statistics available were insufficient to provide conclusive proof, and without the identification of and a means of testing for what we now know is the bacterial agent, the quality of the water could not be adequately ascertained. The result to Parkes, and most of his contemporaries, was conclusive: ‘We were unable to do more than conclude that he [Snow] had rendered the transmission of cholera by water an hypothesis worthy of inquiry; we cannot draw any other conclusion from his researchers on water supply that the predisposing effects of impurity of water are also rendered highly probable’.

**Discussion**

Clearly, Snow did not prove his case. In both the Broad Street and the South London studies, Snow failed to include the type of carefully constructed mortality ratios based on population that would have permitted comparison between the areas of ‘good’ and ‘bad’ water. In neither case did he vigorously address alternative disease theories, even where the geography of the outbreak insisted they be seriously considered. This was especially galling in Snow’s South London study where, Parkes noted, Snow paid no attention to ‘Parr’s Law’ demonstrating an inverse relation between altitude above sea level and cholera mortality. ‘He alludes, indeed, to, but speedily dismisses the important law of elevation’, wrote Parkes, ‘demonstrated by the Registrar-General; and refused, indeed, to admit the effect of elevation and refers the differences of prevalence entirely to the water supply’.

Some of the faults of Snow’s 1855 tome could have been easily avoided. Had he constructed other, distance-based polygons around other pumps in the Broad Street area, he might have constructed relative mortality ratios that implicated the central pump more strongly. Had he delayed publication until the pump’s casing could be carefully investigated—and the index case identified—the contamination of the Broad Street pump would have been evident rather than deduced. And as he admitted later, his ‘grand experiment’ failed because he rushed to publication before the required data were

---

**Table**

<table>
<thead>
<tr>
<th>Death-Rates per 1,000</th>
<th>In Houses enumerated in 1854 as receiving their Water-supply</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>From the Southwark Company. From the Lambeth Company.</td>
</tr>
<tr>
<td>Cholera</td>
<td>1848-9. 12.5  11.8; 1853-4. 3.7  13.0.</td>
</tr>
<tr>
<td>Diarrhoea</td>
<td>1848-9. 2.9    2.7; 1853-4. 2.1    3.3.</td>
</tr>
</tbody>
</table>

**Figure 3** In his analysis Dr John Simon found that, at a general level, cholera mortality could be related to different water suppliers. At a finer scale of analysis this relationship broke down, however. Therefore, he believed the findings suggestive but not conclusive.
available. When the data from the General Register office were made available they were, as Dr Simon correctly noted, suggestive but inconclusive using the analytical tools of the day. Parkes did what a good reviewer is supposed to do: he carefully considered the evidence presented and weighed its presentation and relevance given the author’s objectives. The myth of Snow’s brilliance is thus transformed from a hero story into a cautionary tale. Snow had a good idea. Indeed, he had a great idea. Cholera is waterborne, after all. But in presenting this idea, and a disease theory tied to it, he failed to employ the best methodologies of the day (cartographic or statistical) and in his enthusiasm rushed to print, again and again, before necessary data were available. In arguing his theories he rarely gave more than grudging attention to other disease theories or to the data that seemed to support them. He refused to modify his arguments or change his methodology, when presented with the work of others that seemed at odds with his own.

The practical lessons of Snow’s failure to convince his contemporaries is a warning to those who, convinced of their theories, seek to publish prematurely or to promote boisterously research whose proofs are only partial. Of equal and perhaps greater importance is the necessity of a defence of Snow’s critic, an argument against the near universal myth of Snow the great and lonely hero. At a 2013 conference celebrating the 200th anniversary of Snow’s birth, I argued the obvious limits of Snow’s work and the appropriateness of his contemporaries’ criticisms and cautions. And, too, I argued the falsity of the claims made on Snow’s behalf by modern historians. Snow did not create ‘show leather’ epidemiology or mapped cartography. He did not create the experimenta cruxes. He did not prove cholera was waterborne. Other contemporary researchers—Reverend Henry Whitehead, William Farr, John Simon etc.—were more exciting and better epidemiological researchers than Snow. And yet we persist in the modern myth of Snow, a myth born in a 1901 textbook on the Principles of Sanitary Science and Public Health, and later expanded in the 1930s as a broad truth.

But, ‘We need heroes, don’t we?’ a member of the conference audience asked plaintively. It does not matter if Snow was a hero or a chump, in other words. We need to believe in the solitary genius as we face the disease challenges of our own time. Snow is a symbol, in other words, and the truth of his work as a researcher does not matter. My reply is a suitable conclusion to this essay and its relevance: ‘Yes, we need heroes. But we don’t need a Lone Ranger who singlehandedly saves the day.’ When we make the hero a solitary figure we forget the cooperative nature of medicine and public health. There are no solitary heroes in the struggle with endemic and pandemic disease, just the many who struggle to treat them and understand their nature.

If we are to learn anything from Parkes’ careful review and from Snow’s passionate if incomplete work, it is this: research is collaborative and cooperative, not individualistic. It includes our fiercest critics and most ardent supporters. In the dilemma that is endemic or epidemic disease, it’s not about us, our ideas, but about the disease and our communal means of understanding these conditions. To ignore Snow’s failings because we want a simple hero is to assure the failure of the science we promote and practise. It is to assure that public health disasters will follow.

Conflict of interest: None declared.

References
11. Jameson J. Report on the epidemick cholera morbus, as it visited the territories subject to the presidency of Bengal, in the years 1817, 1818 and 1819. Drawn up by order of the government, under the superintendence of the medical board. London: Government Gazette Press, 1819.
Commentary: Confronting unexpected results: Edmund Parkes reviews John Snow

John Eyler

Program in the History of Medicine, University of Minnesota, 4609 Gustafson Dr., Gig Harbor, WA, 98335, USA.
E-mail: eyler001@umn.edu

Accepted 5 August 2013

John Snow’s On the Mode of Communication of Cholera¹ 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.²

In short, Parkes was an established authority on epidemic diseases and hygiene. In fact Snow cites Parkes’s pathological work in his second edition.³ 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 contagious 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 contagious 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.

Parkes's commissioned study appeared as a 25-page article in the British and Foreign Medico-Chirurgical Review in 1849. 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 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 was 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 workhouse 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 where 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'. 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. 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. 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. By this time informed medical opinion was changing.

Conflict of interest: None declared.

References

Commentary: Edmund Alexander Parkes, John Snow and the miasma controversy

Beverly P Bergman

Institute of Health and Wellbeing, University of Glasgow, 1 Lilybank Gardens, Glasgow G12 8RZ, UK.
E-mail: b.bergman.1@research.gla.ac.uk

Accepted 16 September 2013

The advancement of scientific knowledge has often been characterized by controversy, but perhaps none has been more vehement than the conflict between supporters of the rival 'miasma' and 'contagion' theories of disease transmission. During the 19th century, a series of cholera pandemics originated in India and the second of these, which reached Britain in 1831, intensified the debate. At the time of the 1854 outbreak of cholera in London, which was part of the third pandemic, both theories still had their champions within the evolving specialty of public health. This carefully argued, highly critical but ultimately flawed commentary on John Snow's seminal publication on the transmission of cholera by water provides an insight into the depths of that controversy in the years before the discovery of the role of microorganisms in communicable disease.
At the time of writing the commentary, its author, who styled himself simply E.A. Parkes, was Editor of the British and Foreign Medico-Chirurgical Review and Professor of Clinical Medicine at University College Hospital, London. A strong advocate of the miasma theory, Parkes would need much convincing to change his views. Far from being a crusty academic set in outmoded ways, he was still only 36 years of age and experienced beyond his years, his relative youth concealing a brilliant mind. Although a practising physician at the time of its publication, he was later to become one of the outstanding Victorian pioneers of public health.

Edmund Alexander Parkes (1819–76) was born on 29 March 1819, the son of a Warwick worsted manufacturer, William Parkes (1788–1840) and his wife Frances née Byerley (1785–1842). The family was well known in the English Midlands. They were related by marriage to several of the prominent families of the Enlightenment; William’s cousin had married the granddaughter of the theologian and chemist Joseph Priestley (1733–1804), whereas Edmund’s mother Frances was a great-niece of the potter Josiah Wedgwood (1730–95) and hence connected to the Darcys. They were Unitarians, as were many of the great intellectual families of the period. While Edmund was still a young boy, his father’s business failed and Edmund was sent to London to complete his education at Christ’s Hospital, a charitable school established in 1552 to provide a high quality education to children with academic potential whose parents could not otherwise afford it. Subsequently he worked as an assistant to his uncle Sir Anthony Todd Thomson (1778–1849), Professor of Materia Medica and Therapeutics and Joint Professor of Medical Jurisprudence at University College London, and it is likely that it was his uncle’s influence which secured Parkes’ admission as a medical student to University College London. He continued to work in his uncle’s laboratories during his student days, developing the research and experimental skills which would stand him in good stead throughout his career. He was academically gifted and won a number of prizes as a student.

Parkes graduated MB in 1841 at the age of 22 and chose to begin his medical career in the army; in 1842 he was gazetted Assistant Surgeon with the 84th (York & Lancaster) Regiment. His military duties took him to India and Burma and provided him with the material for his MD thesis ‘Remarks on the Dysestency and Hepatites of India’ which was published in 1846, after he had left the army and returned to civilian practice. While in India he had witnessed two severe epidemics of cholera and in 1847 he published a work ‘On Asiatic and Algide Cholera’, followed by papers on ‘Intestinal Discharges in Cholera’ and ‘Early Cases of Cholera in London’. The physician Sir William Jenner (1815–98) was later to comment on these papers in an address to the Royal College of Physicians: ‘Having regard to the age of their author, the circumstances under which the materials for them were collected, and their intrinsic merits, these two works are among the most remarkable in medical literature."

In 1855 Parkes was nominated by the British Government to establish a civilian hospital in Turkey for casualties evacuated from the war in the Crimea (1854–56). Together he and the engineer John Brunton selected and surveyed a suitable site at Renkioi on the Dardanelles where the world’s first prefabricated hospital, designed by Isambard Kingdom Brunel, was to be erected. Well appointed, well ventilated and staffed by nurses provided by Florence Nightingale, the hospital suffered none of the problems of many of the Crimean War hospitals and deaths were mercifully low. Nonetheless, during his time caring for Crimean War casualties, Parkes became aware of the toll of preventable disease attributable to poor hygiene, including cholera, and he was to be a key witness at the subsequent commission of enquiry. One of the recommendations of the commission was that teaching in ‘military hygiene’ (public health) be provided to all newly appointed military doctors, and in 1860 Parkes relinquished his post in London on his appointment as the first Professor of Military Hygiene at the Army Medical School. His textbook A Manual of Practical Hygiene was the first formal textbook of public health and, although initially written as a text for his students, it quickly became internationally renowned in the wider community. Eight editions were published up to 1941, under a succession of editors, and it was translated into other languages including Japanese; a special edition was prepared in New York for the US market. As a textbook it was ahead of its time; many of the issues covered are familiar to a modern public health practitioner, including air and water quality, nutrition and exercise.

Parkes’ review of Snow’s work, published in April 1855, was almost certainly written shortly before his departure for Renkioi. Already it clearly shows the thought processes which were later to underpin his role in public health. Early in his review, Parkes stated that his object was ‘to see the strength of [Dr Snow’s] evidence’, a phrase which would come into common usage nearly one and a half centuries later in the era of evidence-based medicine. He then proceeded to a discussion of hypothesis testing, as befitted a scientist trained in research methodology under a professor of jurisprudence, and here he found Snow’s arguments wanting. In noting the different numbers of occupants of two properties affected by cholera (Case 1), he demonstrated an understanding of the importance of rates in epidemiology as well as the problem of confounders (here, differences in structure and ventilation between the two blocks of dwellings). In considering Parkes’ understanding of these issues, we must bear in mind that the science of what we now know as
epidemiology was only just coming into being at that time, based on the work of William Farr (1807–83) and others. Even the word ‘statistics’ was of relatively recent origin, having been introduced into the English language only 70 years earlier by Sir John Sinclair of Ulbster (1754–1835), author of the *Statistical Account of Scotland*, following a visit to the Continent where he had seen the German practice of ‘statistik’. Early examples of ‘statistics’ were largely narrative in nature, the mathematical and analytical approach only developing in the late 19th century; significance testing would be a new concept in 1885, nearly 10 years after Parkes’ death.13

In his discussion of Snow’s case reports, Parkes critically questioned Snow’s reasoning, assessing the weight of evidence for water-borne vs airborne transmission of cholera and generally finding Snow’s evidence unconvincing. Case 10 is the well-known Broad Street pump outbreak for which Snow is best remembered. Snow’s own account showed that, in the absence of visible contamination of the water from the pump, he was hesitant to implicate it; but he found the evidence of the spatial distribution of the cases compelling, concluding that the amount of infective material in the water must have been too minute to be visible. Parkes, on the other hand, wished to see evidence not only that all those who drank from the pump were affected, but also that those who drank elsewhere were unaffected, demonstrating that he had an understanding of the principles underpinning both positive and negative predictive values. He disagreed that the map firmly established the role of the pump as source and noted that ‘it looked more like the effect of an atmospheric cause’, a surprising observation as there is no evidence of any downwind effect. Air movement was crucial to the miasma theory; chapters in his textbook show that Parkes was familiar with meteorology, having probably gained that knowledge from the chemist and meteorologist Professor J.F. Daniell with whom he was acquainted while a student.7 10 Snow’s conclusions, based on his own empirical observations of the distribution of cases, have been fully vindicated by modern mapping techniques using geographic information systems (GIS)14 and his cholera map is now widely used as a teaching example.15

With Koch’s discovery of the nature of disease-causing organisms still some years in the future, both Snow and Parkes were missing a vital link. It would be inappropriate to be too critical of Parkes’ review of Snow’s writings in the light of later knowledge. Rather, the review should be seen as the prescient work of a physician who would later become influential in the field of public health. His discussion of Snow’s work presented a careful analysis of the findings and demonstrated many of the principles familiar to a modern-day epidemiologist, at a time when public health as a specialty was barely in its infancy. Parkes retained an open mind and, as further evidence came to light later, backed up by experimental evidence, he eventually embraced the water-borne nature of the spread of cholera. At an intermediate stage in his changing views, in his report to the Privy Council on his investigation of the outbreaks of cholera in Southampton in 1865 and 1866 he conceded that a satisfactory explanation was afforded by the intestinal discharges of patients entering the sewage system, but then explained the transmission on the basis of gases from the sewage pervading houses and causing further disease;11 but by 1869 the third edition of his textbook acknowledged that cholera was indeed transmitted through contamination of water by the bodily evacuations of affected patients.

The final two paragraphs of the review say much about Parkes himself; as a scientist, as a physician, as a political lobbyist but above all as a gentleman, graciously acknowledging Snow’s diligence even while disagreeing with his conclusions. Sadly his hope that ‘if [Snow’s] discovery should be established, the prevention of cholera would be easy’ has yet to be realized, notwithstanding that our understanding of the causative organism now even encompasses the sequencing of its genome.16 If only public health could be so simple.

Conflict of interest: None declared.

References
Commentary: Two views of cholera

Nigel Paneth

Departments of Epidemiology & Biostatistics and Pediatrics & Ituman Development, College of Human Medicine, 909 Fee Road, Michigan State University, East Lansing, MI 48824, USA. E-mail: paneth@msu.edu

Accepted 14 October 2013

Edmund Parkes was a well-qualified choice to provide a critical assessment of John Snow’s second edition of the Mode of Communication of Cholera in the British and Foreign Medico-Chirurgical Review. A former military surgeon, Parkes had extensive experience of cholera in Asia. His monograph, Researches into the Pathology and Treatment of the Algide or Asiatic Cholera, had been published in London 8 years previously, and was dedicated, we might note, to the editor of the Review, his good friend John Forbes. Parkes’ book was built around 47 autopsies of cholera victims conducted during cholera epidemics in Burma and Madras in the 1840s. Working in a military hospital, Parkes tended to see patients in the advanced stages of disease, and was more impressed by the cold, clammy appearance of his cholera patients (algide = cold) than by their copious diarrhoea, which several of his patients no longer had by the time he saw them. Although Parkes was aware of the marked thickening of the blood in cholera patients, unlike Snow he did not attribute it to dehydration from diarrhoea. For Parkes, the most important pathological finding was pulmonary vascular congestion, which we might now recognize as a reflection of dehydration and shock, whereas for Snow the key pathological event was the massive intestinal fluid loss. As to the communicability of cholera, Parkes asserted in 1847 “I have never observed any indication of contagion.”

Snow’s major clinical experience of cholera was as a teenage apprentice assigned to the Newcastle coal mines in 1832–33. He thus saw cholera at the front lines, and it is tempting to see here the difference in perspective between the primary care practitioner and the hospital specialist that still permeates modern medicine. Parkes thus came to Snow’s book with some strongly fixed views, especially on pathology and communicability. In the book, Snow described with almost eerie exactness the pathophysiology of cholera—understanding, even then, that cholera kills via massive fluid loss from the intestines. This coherence with modern understanding, we must acknowledge, allows us to be drawn easily into Snow’s argument on the mode of communication, which depends entirely on viewing cholera as a disease of the gut. Fecal-oral transmission made no sense to someone like Parkes who saw cholera as centrally a disease of the heart and lungs.

Parkes was as meticulous a dissector of arguments as of bodies, and he wielded his scalpel on each of Snow’s many examples of waterborne transmission, finding them all wanting, arguing that the case was not fully made, either because airborne transmission was not excluded or, more saliently, that Snow had a tendency to present numerator data when it was attack rates with appropriate denominators that were called for. He argued that the geographical distribution of cases around the Broad Street pump in Snow’s iconic map was perfectly consistent with a miasmatic, airborne interpretation of the diffusion of cholera. Perhaps Parkes had not yet seen Snow’s revised outbreak map, published in the Parish report on the outbreak 6 months after his book. That map
included an irregularly-shaped dotted line circumscribing the area that was closer to the Broad Street pump in walking distance than to any other street pump. The line enclosed nearly all the cholera deaths in the outbreak.  

Although most of Parkes’ arguments seem of the death-by-a-thousand-cuts variety that all who have been subject to grant application reviews are familiar with, there is one place where Parkes thought he detected a fatal flaw. The comparison of the two water supplies of South London, where the epidemic was most severe, occupied, as Parkes points out, half of Snow’s book. One of the water companies (Lambeth) had its Thames intake far upriver from London, whereas the other (Southwark and Vauxhall) took its water from mid-London, just downriver from discharging sewer pipes. And in a part of South London, those two water supplies were thoroughly intermixed, with neighbouring houses having either one supply or the other in apparently random fashion. Snow viewed the area with the intermixed supply as representing an ‘experiment on a grand scale’, because a miasmatic, airborne explanation of cholera could hardly explain differences in mortality within a population breathing the same air.

But in presenting cholera mortality rates denominator to the number of houses supplied by each company, Snow did not restrict his attention to the intermingled districts of South London. Rather, the mortality rates he provided were those of all recipients of the two water supplies, and this included, notably, four wealthy and elevated districts without any cholera deaths whose water supply was entirely from the Lambeth Company.

Snow had pointed out, perhaps less clearly than he might have, that whereas he knew how many houses the two companies supplied in toto, he did not know those numbers for the intermingled area. Thus the argument Snow made, and the data he used to support it, were not entirely consistent with one another.

Weighed against this were Snow’s data showing that fewer than 4% of South Londoners lived in the privileged Lambeth-only area, and that Southwark and Vauxhall water recipients had more than five times the cholera mortality of the rest of London, excluding the Lambeth-supplied. And when, in 1856, Snow published a paper with the previously missing denominators, the powerful differences in mortality remained.  

Snow’s case for fecal-oral and waterborne transmission was not airtight. As Parkes pointed out, Snow showed examples of cholera outbreaks that seemed most reasonably explained by waterborne transmission, but frequently failed to take the argument further to show that no alternative explanation was possible. Snow appealed more to judgment than to formal scientific proof.

Although the arguments Snow advanced for his ideas contained a measure of advocacy, he had a strong and clearly demarcated hypothesis, fully falsifiable, which he tested, as well as he could, with the material available to him. He did not shy away from examples that might, at first glance, have contradicted his thesis, but wrestled with them. In pushing past the frontiers of what he securely knew, in not waiting until every possible objection could be answered, he laid the foundation for preventing waterborne diseases.

References