

## Invited Commentary: The Testimony of Dr. Snow

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Congratulations to Dr. Lilienfeld (1) for having discovered the transcript of Dr. Snow's testimony before the Parliamentary Committee. It proved almost emotionally moving to me to hear John Snow speak—or at least to read the more or less literal transcript of his words. A weighted opinion on the hows and whys of this epidemiologic witness episode might constitute the start of a medical historian's Ph.D. thesis. Let me nevertheless offer a few comments about different aspects.

### Scientific and political context

The overriding impression one gets from Snow's testimony is that of his extremely strong belief in germ theory and contagion, and his consequent contempt for anything close to the rivalling theory that miasmatic emanations cause disease. This apparently led to his starkly unwarranted generalization to factory fumes. Why was he so blind?

There is a particular context. The year of the testimony was 1855: That was also the year in which John Snow published the second and much celebrated edition of his book on the contagiousness of cholera via drinking water (containing the story of the Broad Street pump), based on his experiences in the epidemics of the preceding years (2). Snow came out of his investigations an even more convinced adherent of the germ and contagion theory than he already was. As Dr. Lilienfeld explains, the 1854 Parliamentary acts were originally motivated by the same cholera epidemics. To Snow, it must have seemed like blasphemy that the proposed Parliamentary acts invoked the rival miasma theory (disease emanating by fumes), which he had, at least in his mind, completely proven wrong. To him, the idea of combatting the fumes must have seemed like the complete inverse of the lessons learned from the epidemics he had so thoroughly investigated. When I imagine myself in Snow's shoes—i.e., having just published an account of some epidemiologic exposure and finding Parliament enacting laws completely contrary to my findings—I feel anger mounting. What Snow obviously missed was the political understanding that these Parliamentary acts might have been

provoked by the cholera epidemics but were aimed at much broader measures for improving living conditions.

There is also a general context. In 1948, the medical historian Ackerknecht (3) described the great irony that around 1850 the academic popularity of the theory on germs and contagion was at an all-time low—immediately preceding its most successful vindication by bacteriology. The germ and contagion theory was old, and it appealed (intuitively) to the ignorant masses but not to academics. Moreover, germs and contagion were in bad standing with merchants and industrialists. Contagionism had found its expression in quarantines. In the words of Ackerknecht: "Quarantines meant, to the rapidly growing class of merchants and industrialists, a source of loss, a limitation to expansion, a weapon of bureaucratic control that it was no longer willing to tolerate..." (3, p. 567). He went on to note that the liberal merchant class therefore sided with the enlightened theoreticians of the miasma theory. The latter group believed that one needs insight into time, place, and person to explain disease, and it led them to propose the cleaning of sewers, improvements in housing and nutrition, etc., instead of merely putting people in quarantine and not changing anything about their living conditions. Could it be—just a historical guess—that Snow seized the occasion to show the inverse, to ingratiate his theory with the wealthy and the powerful, by showing that his theory was not at all inimical to mercantile and industrial interests? Snow's opening words from the transcript published by Dr. Lilienfeld were: "...my opinion is, that measures to protect the public health would not interfere with useful trades..." (1, p. 5). On the contrary, in this instance, the theories on miasmatic fumes were contrary to those interests.

The seemingly cold indifference of Snow to the plight of the laborers in the factories and the surrounding population exposed to toxic fumes contrasts strongly with several things he wrote in the second edition of his book on the propagation of cholera (2). For example, in the last pages of the book, he proposes measures to stop the spread of contagion. They read like an amalgam of the best of both theories (contagionism and the miasma theory): He proposes better housing, better sanitation, and measures for providing clean drinking water and preventing person-to-person transmission. From those pages, compassion seems to emanate for the poor as well as for immigrants.

### Beliefs and interpretation of data

It is sometimes difficult to disentangle preconceived beliefs from data analysis and interpretation. Snow's work is

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a case at hand. I have previously demonstrated that he already held his germ-theory and drinking-water convictions before he made his observations, as can be seen from his earlier publications (4). More than that: When reading the second edition of his book, with its celebrated data on the Broad Street pump and the intermixing of water from different water companies, it becomes apparent that even his data analysis was guided by his preconceptions. Many tables giving the incidence of cholera by water company show data for the first weeks of the epidemic only. As Snow explained quite clearly, during the later weeks of the epidemic, cholera started to spread from person to person, and the difference between the water companies blurred. To us, nowadays, this is logic itself. To an adherent of the miasma theory at the time, however, this must have seemed like data-dredging (if the term existed then), since such an adherent believed neither in water contamination nor in person-to-person spread.

### "Hired gun"

One can find Snow's attitude mirrored today. For instance, studies that report adverse consequences of pharmacologic preparations are often challenged on the basis of potential biases such as "channeling," "attrition of susceptibles," "preferential prescribing," "class effects," or "confounding by indication." Such biases may be real, but they are often unproven or logically unfounded: They exist only as potential objections.

The open discussion of such issues is important, but it is worrisome that these arguments seem to be raised most often by persons on the side of manufacturers with a financial stake in the outcome of these discussions. It is therefore extremely interesting to hear John Snow start his testimony with an argument often used by persons who testify on behalf of industry or other interest groups, one that goes more or less like: "I was not lured into these opinions by lucre but was chosen because I already held these opinions." Certainly, that is often how things go. However, the selection for testimony of experts who are known to hold certain preconceived opinions is akin to publishing an unbalanced literature review, with potentially more serious consequences.

### Snow and his contemporaries

The picture of Snow and his contemporaries remains complicated. The extent to which Snow's opinions on germ theory were accepted in his (short) lifetime or immediately thereafter remains difficult to judge. Certainly, there were people who referenced them, as is also demonstrated by Lilienfeld, and certainly quite a lot of people started to dream up combination theories that could cope with all objections (such as the concept that miasmas were also, or even sometimes preferentially, transported by water). However, Snow did not truly sway contemporary opinion, and his theories were not greeted as a complete breakthrough in the way that they are often presented as being in epidemiologic textbooks (4).

Snow's relationship to *The Lancet* is even more difficult terrain. The *Lancet's* founder, Thomas Wakley, was offi-

cially the Editor from 1823 to 1862, but history has it that in 1851 he collapsed, exhausted from his medico-political fights, and retired temporarily to Madeira (5). His son, the even more irascible James Goodchild Wakley, joined the *Lancet* in 1852 and served as Editor from 1862 to 1886 (5). Thomas Wakley certainly leaned to the side of contagionism in the 1830s, but he remained critical (see below). Since Snow's first writings on cholera, along with the first edition of his book, were published in 1849, and the second edition of the book was published in 1855, a slow labor of love by a medical historian would be required for us to see whether the *Lancet's* editorial opinions about contagionism or about Snow in particular changed around 1852 when the younger Wakley moved in. (Ackerknecht suggests in a footnote (3, p. 581) that the contagionist *Lancet* was somehow already unfavorable to Snow in 1849, and hints that the *Lancet* softened its contagionist stand later on.)

Perhaps, again, the *Lancet's* Editor saw the broader point that Snow so dearly missed. Even if the journal was siding with contagionism, the Editor might have seen that removing toxic fumes from factories was a good thing to do, whatever theory it was based on, and that it was politically rather foolish to cling so much to a single theory as to forget the greater good and have one's scientific reputation exploited by the manufacturers.

### Miasma and contagion

The fundamental problem with the miasma and contagion theories at the time was that both failed to answer certain questions. This was already seen by Thomas Wakley in the 1830s (3). For instance, if disease came about by germs, why did not all persons attending the sick get sick? Most ironically, this argument can be turned backwards: If diseases come about by emanations from the soil, why do not all persons in the same household (including those attending the sick) get sick? However, do we, 150 years later, understand why certain persons develop disease—for example, meningitis? That is, do we understand why people become ill preferentially after moving (e.g., army recruits coming together, schools starting after holidays, fugitives in camps); why the epidemics are more devastating amidst poverty and malnutrition; and, most enigmatically, why most contacts will remain only temporary carriers of the causative germ?

The best historical judgment about Snow might follow the lines of the 1927 obituary on Charles Creighton that was written by Major Greenwood (the first professor of epidemiology at the London School of Hygiene and Tropical Medicine) (6). Creighton was a historical epidemiologist who had remained a complete anticontagionist well into the 20th century. Against all reason, he never believed in bacteria, out of conviction that bacteria alone could not explain why diseases occurred at certain times and places and among certain persons—and he was therefore contemptuous of Snow (4). Nevertheless, Creighton wrote a factually most wonderful history of epidemics in Britain. Greenwood wrote about Creighton: "He despised the pseudo-historian who awards praise or blame in accordance with the extent to which the opinions or descriptions of the old writer can be identified with those of modern idols of the market place" (6,

pp. 240–1). Perhaps this epitaph about an unreasonable anti-contagionist might lead us to a measured retrospective judgment about the unreasonableness of Snow.

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