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REVIEWS AND COMMENTARY

Who Made John Snow a Hero?

J. P. Vandenbroucke,¹ H. M. Eelkman Rooda,² and H. Beukers³

This paper describes how and why John Snow's investigation of the transmission of cholera grew into an epidemiologic classic. The evolution of the interpretation of the work of John Snow was first studied in depth in the Dutch medical literature, and thereafter traced more superficially in the bacteriologic, hygienic, and epidemiologic literature of Germany, the United Kingdom, and the United States. From the oral tradition of teaching, as well as from the written sources, it is concluded that US epidemiologist W. H. Frost was responsible for the revival of the work of John Snow in the 1930s. Besides the obvious and enjoyable clarity of thinking and reasoning, epidemiologically and medically, of the writings of John Snow, his example well suited epidemiology of the 1930s since his convictions came very close to the bacteriologic paradigm of the day. *Am J Epidemiol* 1991;133:967-73.

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The tale of John Snow, the 1853 and 1854 London cholera epidemics and the Broad

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¹ Department of Clinical Epidemiology, Leiden University Hospital, Leiden, The Netherlands.

² Student of Health Sciences, Leiden State University, Leiden, The Netherlands.

³ Department of the History of Medicine, Leiden State University, Leiden, The Netherlands.

Reprint requests to Dr. J. P. Vandenbroucke, Department of Clinical Epidemiology, Leiden University Hospital, P.O. Box 9600, 2300 RC Leiden, The Netherlands.

The authors acknowledge the critical reading of an earlier version of this paper by Drs. Sander Greenland and Charles Poole. Further, we are deeply indebted to two anonymous reviewers and one editor who have witnessed part of the history we tried to describe. Given our indebtedness, we have quoted the comments of one reviewer

Street pump, is known today by every epidemiologist as "the epidemiologic classic." A controversy exists, however, about the real lesson which this historical episode harbors for the present practice of epidemiology (1-6). Indeed, different textbook authors emphasize different aspects of John Snow's work (7-12). Such differences are further heightened in the oral tradition of teaching. Many authors interpret Snow's work as a triumph of the epidemiologic method—by making geographic comparisons of cholera incidence and mortality, Snow arrived at a causal insight and intervention (the removal of the handle of the pump) long before any bacteriologic cause of the disease was known. Others use the story only as a nice

example of a natural experiment that closely mimicks the conditions of a randomized controlled trial. Still others have been intrigued by the somewhat neglected aspect of the large role preconceived theory played in Snow's work (1, 2)—in contrast to most of his contemporaries, Snow was a staunch believer in some little animalcule that would transmit cholera by contagion; more particularly, he held this theory many years *before* he made his renowned epidemiologic observations. Supporting evidence for this latter contention is that John Snow's famous 1855 book, *On the Mode of Communication of Cholera*, was a *second* edition. This book was preceded by his 1849 writings in which he had already expressed the same opinions (13, 14). (Besides his work on cholera, John Snow pursued productive and highly successful scientific work in anesthesiology, where his leadership was accepted, leading to attendance of the Queen and to a prestigious presidency of a medical society (15). He died at the young age of 45, only 3 years after publication of the second edition of his work on cholera.)

Given the diversity of emphasis and interpretation, and confessing to rather strongly one-sided views ourselves (5, 6), we set out to determine how and why the tale of John Snow grew into the classic example for epidemiology, and how and why certain interpretations came into being, so as to be continuously transcribed in modern textbooks. We started to wonder how John Snow's work really looked in the eyes of his contemporaries. Was his type of investigation with his type of hypothesis (drinking water as a source of infection) being followed by anybody? Was there recognition of his innovation, either methodologically or as to the cause of the disease?

To be able to study the question in sufficient depth, and to delineate the volume of source material to manageable size, we first restricted our query to the emergence of the work of John Snow in the Dutch medical literature. By studying popular tracts on health issues, medical journals, and textbooks on public health and hygiene, we tried

to trace when, how, and by whom the work of John Snow was first mentioned, and how the epidemiologic community took possession of it until it became sacrosanct. Thereafter we verified whether our findings would also hold true for other countries.

Our results were rather unsettling and can be briefly described as follows. John Snow's work grew into a "classic," entailing a detailed description of his "epidemiologic method," only between the 1930s and the 1950s, i.e., some 80 to 100 years after initial publication. During the first few decades after his original publication in the 1850s the work was hardly mentioned. In the Dutch medical literature there were occasional footnotes about a drinking water theory (usually without even mentioning Snow by name), but on the whole, the leading paradigm of those days was Von Pettenkofer's "Boden Theorie," a multicausal variant of miasma theory (16, pp. 658–9). Von Pettenkofer's work was enthusiastically described and paraphrased in lengthy articles with a wealth of supporting geographic data. His books and writings were abundantly translated and popularized for all possible purposes. The greatness of Von Pettenkofer's work and name was sung in all modes; it was held that it would almost be impossible to improve on or add to his work. His ideas completely permeated the Dutch medical society. It was a lone critic who showed in 1873 that there was only a poor correlation between an analysis of the composition of soil samples and mortality in different areas of the city of Rotterdam, the Netherlands (17). In the first decades after John Snow's initial publication, the drinking water theory only surfaced once, in an 1868 cautious report of a government committee (note that this was about the same time that Simon would have given the drinking water theory some credit in England, see below). When studying the other Dutch medical literature of the time, however, it becomes clear that the general mood was far from favorable.

The idolatry of Von Pettenkofer in the Netherlands reached its absolute height in the famous controversy with Robert Koch.

after the latter discovered a commaform bacillus in 1883 in Egypt and held it to be the cause of cholera (18, p. 149). Koch coupled his bacteriologic studies to epidemiologic observations in Egypt and India, and attached great importance to drinking water as a source of contagion. Neither Koch nor his collaborators ever mentioned John Snow's name or work, however (18, p. 162). Virchow, who originally held a miasma-like theory on cholera as being a generalized disease (19, vol. I, p. 124) (in contrast to Snow who held it to be primarily a local affliction of the gut) and who had opposed Snow's drinking water theory (19, vol. II, pp. 259-60), admitted defeat in 1884 (18, p. 168). However, Von Pettenkofer remained adamant in his opposition of Koch. He refused to accept the bacillus as a cause of cholera, certainly not as a sufficient cause, and in the end, probably carried away by his own argument, not even as a necessary cause to produce the disease (20). The controversy culminated in the episode where Von Pettenkofer drank an aliquot of pure *Vibrio cholerae* broth. His example was followed by a few other doubters, some of whom would later become celebrated bacteriologists and immunologists themselves, like the Nobel prize winner Metchnikoff (21). Rumor has it that Von Pettenkofer and one other doubter developed some diarrhea afterwards, but this was dismissed as not being the dreaded cholera, and the experiment was claimed a total victory by Von Pettenkofer.

Somehow, this autoexperiment proved to be Von Pettenkofer's swan song. From the Dutch medical echoes of the ongoing debate we learned that slowly, i.e., disease by disease, the miasma theory, and Von Pettenkofer's variants of it, lost scientific credibility as bacteriology emerged as the stronger science. From 1900 onward we found a gradual decline in the number of references to Von Pettenkofer's work and a gradual increase in the number of quotations to the "excellent" work of John Snow. We witnessed this process in successive editions of various Dutch textbooks on public health. In a 1913 volume we can read how refresh-

ing it is to find the clearheaded work of John Snow as a rare treasure amidst the volumes of theoretical trash that hygienism had produced; Koch's discoveries were held to bring the real understanding of the mode of spread of cholera and to put hygienic prescriptions on sound footing (22). The same author scorned hygienists for their mistakes (23)—for example, it was described how hygienists in their zeal to evacuate rotting organic material, ordered workers to dump the street dirt of the city of London (including human excreta) into the Thames River (from where the drinking water was obtained), thereby perhaps prolonging the epidemic or causing a second wave. At the same time, Snow was being praised for the equanimity by which he had born the stupid slandering by miasma hygienists, and contrasted in that respect to Semmelweiss. Still, in the 1935 first edition of the next leading textbook on public health in the Netherlands, Snow is not mentioned while Von Pettenkofer is cited eight times (be it mostly in the negative) (24). In the fifth edition, in 1955, Snow and Budd are mentioned as contagionists and opponents of Von Pettenkofer; Snow's work receives the epithet "classic," and there is interest in the description of his method (25). This late recognition of John Snow's work as a "classic" in the Netherlands seems to follow the reprint of his work in the United States, edited by W. H. Frost, in 1936 (13). The aim of such a reprint was to make an unavailable work widely available. Such unavailability might be due to a low initial popularity—John Snow's volumes were not kept in libraries, in contrast to Von Pettenkofer's. In the Netherlands, roughly 80 copies of Von Pettenkofer's books are still present, while only one original and two reprints of John Snow's 1855 second edition of the *Mode of Communication of Cholera* are officially on library shelves.

The motives for the belated sanctification seem clear. Apparently many epidemiologists and public health officials of the first half of the 20th century were overawed with the success of bacteriology. That new science had swept the greatest hero of public health

of the 19th century off his feet (Von Pettenkofer had committed suicide in sad familial circumstances at great age in 1901) (16, p. 658). The only historical figure of the 19th century to whom epidemiologists and public health officials of the 20th century could have recourse was John Snow, because he had proved to be "right" a mechanism that too often blurs our view of our medical past (26). Ironically, in his own time John Snow was not a mainstream epidemiologist or public health official, but, rather, a holder of eccentric views that went back to dark 16th century theories on contagion of Fracastorius. Certainly, in the Netherlands, his publications received no particular attention nor caught the imagination in the turmoil of the great cholera debate. He was one among many. Even his methods were not particular. The medical journals of his time abound with geographic charts of the spread of diseases in cities, regions, and countries, usually showing how right miasma theory and Von Pettenkofer were.

Epidemiologists of the first half of the 20th century succeeded in screening off the heritage of Von Pettenkofer and other hygienists so completely that now only John Snow's memory survives as the historical paradigm, giving today's epidemiologists the impression that he was the important leader in public health in the previous century, with a method of investigation that was unique and widely acclaimed. Thereby, the strangest peculiarity is that while the heritage of Von Pettenkofer was quietly buried, John Snow's work is almost always described in terms that are very close to classic hygienic thinking. John Snow's work is often discussed as the victory of the study of environmental association, rather than the victory of holding contagionist and localist convictions at a time when these were in the minority. In this way, Von Pettenkofer's spirit still succeeds in blurring our view of John Snow's work. Some early warnings about the "right" interpretation, e.g., in W. H. Frost's introduction to the 1936 reprint of Snow's work (13, p. 15) and in the note by Snow's biographer Richardson (13, p. 34), were completely lost (6).

It might be argued that the detail of some of our above reasoning is based on evidence from the Dutch medical literature only. This literature might be heavily influenced by the German literature because of language similarities, geographic proximity, and socio-cultural reasons. Indeed, we know that the foreign medical tracts which were most often translated into Dutch during the second half of the 19th century were predominantly German. There is ample evidence that similar processes existed in other countries, however. Garrison, in his 1929 textbook on the history of medicine, already hinted that the publication of *The Mode of Communication of Cholera* almost proved a financial loss to Snow, and described how slowly and erratically the drinking water theory gained ground (16, pp. 661-2, 781). Perhaps England was the country where Snow's work was most readily accepted. Already in 1866 Simon would have reported favorably to the Queen's council. Yet, we have Simon witnessing in 1890 how it took him 30 years to recognize the "rightness" of Snow (27). Highly illustrative of a continuing ambiguity of British epidemiology, however, was the description of the upsurge of bacteriology and the downfall of Von Pettenkofer in Greenwood's *Epidemics and Crowd Diseases*, a *magnum opus* on epidemiology and its history as of 1935 (28, p. 60). Greenwood quoted other early 20th century British epidemiologists, such as Crookshank and Hamer, who scolded the "mental paralysis" that overtook the epidemiologist when dealing with bacterial causes of disease. According to Greenwood, Von Pettenkofer's fame declined not because anybody had shown him wrong, but because nobody was interested in his *kind* of argument anymore (original emphasis). In Greenwood's 1935 account of the epidemiology of cholera (28, p. 165 et seq.), neither John Snow nor the drinking water theory are mentioned. The real hero to all three Britons (viz., Crookshank (29), Hamer (30), and Greenwood (28)) was Charles Creighton who had published extensively on the history of epidemics in the 1890s (31). Creighton was a follower of Von Pettenkofer and only

sneered at John Snow as one who had seized upon the occasion of a pump. . . (31, vol. II, p. 854). Given the admiration of early 20th century British epidemiologists for Creighton, who was also a much abused opponent of smallpox vaccination, Garrison, in 1929, wrote: "He [Creighton] died a lonely, forsaken man and has just come into his own as the founder of modern British epidemiology" (16, p. 742). Who, among today's epidemiologists would still write the same? For Germany, we already noted the opposition by Virchow and the failure of Koch to mention Snow, even if Koch himself gave a demonstration that the drinking water theory was right in a beautiful observation on the isolation of *Vibrio cholerae* in a water tank in India (18, p. 163). Snow's work could have been ammunition in Koch's struggle with Von Pettenkofer, yet it was not used.

In general, it is difficult to believe that the evolution of Dutch medical appreciation of John Snow's work would be much different than that in other countries, except for a slight tendency of Dutch medicine of the time to always try to keep a middle ground, leading to a cautious conservatism, i.e., a mixture of miasma theory and bacteriology at the end of the 19th century (32). Such mixtures of ideas also existed elsewhere, leading, for example, to opinions that water was a "predisposing cause" of cholera, among many others, rather than containing a specific contagious agent (33). The existence of many such intermediate beliefs in the period of transition between miasma theory and the acceptance of necessary bacteriologic causes may have further hampered our view of John Snow's original work.

The problem with many miasmatic and hygienic theories was that they were so adaptable that they might almost fit with anything. Von Pettenkofer's work is itself already a compromise between strict miasmatic theory and elements of contagionism, even if some of the elements were mutually inconsistent (34, pp. 288-90). Another good example is the work of William Farr on cholera. Farr is quoted by Snow and was helpful in providing Snow with some of his

material (13, pp. 61, 104, 118-19). Nevertheless, Farr regarded sewage-contaminated water as a modifying influence only (35, pp. 109-10). Farr's pet theory was that the elevation of the district was the main culprit. Although indebted to Farr, Snow felt obliged to attack this theory (13, p. 97).

Further international studies of the history of epidemiology will be necessary to trace the dynamics of the 20th century reemergence of John Snow. In such studies, attention should be paid to international comparisons, especially discrepancies. For example, how do we explain that the critical reprint of John Snow's work was edited by Frost in 1936, only 1 year after Greenwood's thorough account of the history of epidemiology in which the latter seemed totally unaware of Snow's existence? It is hard to understand how the history of epidemiology was so different on both sides of the Atlantic in the middle 1930s. Certainly, Frost's thinking, e.g., on tuberculosis, was much closer to bacteriology and germ transmission (36) than Greenwood's (28, p. 342). Although the real dynamics of these differences still escape us, we tentatively propose a motive for the reemergence of John Snow—many 20th century epidemiologists admired the great sanitary movement of the previous century and wished to continue this great endeavor; at the same time they felt ashamed of the underlying theory of most of 19th century's public health; in this dilemma they seized on about the only 19th century epidemiologist whose theoretical stand was acceptable; this led to an emphasis on "how he did it," in order to avoid further mistakes.

The most likely candidate for having introduced Snow as the main historical example of epidemiologic thinking is Wade Hampton Frost. An anonymous reviewer of this paper communicated the following: "Snow's studies of cholera were introduced to America, and perhaps the rest of the world, by Wade Hampton Frost, the first Professor of Epidemiology at the Johns Hopkins School of Hygiene and Public Health. Not only did Frost republish the papers, but he introduced the studies to his classes. This practice was continued by his

successor, Dr. Kenneth F. Maxcy, who as editor of the eighth edition of the Rosenau text on *Preventive Medicine and Public Health* described in detail the Broad Street pump study. Material from Frost's introduction and republication of Snow's paper was used as a class problem in the introductory course in epidemiology. This practice was continued by the third chairman, Sartwell, whose description of the case-control method used in comparing two London populations was described in the ninth edition (37, p. 6). He also gave a lecture at the American Epidemiological Society praising the Snow studies (38, pp. 3-22)."

A few years before the republication of Snow's work, the work of another great contagionist had been reprinted, William Budd's 1873 work on typhoid fever (39). Although both reprints were available, it comes as little surprise that Snow's work evolved into the great historical example, since his reasoning is more numerical, closer to modern epidemiology, and his writing more succinct.

All in all, 20th century epidemiology accepted a strange mixture of ideas: the methodological example of John Snow was revived and intertwined with a continuation of the multicausal type of thinking exemplified by Von Pettenkofer. The basic reason why 20th century epidemiologists felt compelled to develop such a mixture might have been that they preferred a multicausal way of thinking, but were forced under the unicausal umbrella of successful bacteriology (40, 41). Further study of the history of epidemiology seems necessary, not only to satisfy our human curiosity, but also to understand the development of our reasoning in this science.

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