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## Changing images of John Snow in the history of epidemiology

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### Summary

Ever since the end of the 19th century, the story of John Snow and his investigations into the contagiousness of cholera has fascinated epidemiologists. Several different lessons have been extracted from the interpretation and reinterpretation of Snow's work – according to prevailing insights. The story of John Snow continues to evolve, even into the 21st century.

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**Key-Words:** History – Epidemiology – Cholera – Water.

The purpose of this commentary is to ask, and try to answer, the questions: "Why did it take so long before people recognised the importance of John Snow's work?", and "Who was first responsible for this belated recognition?". I will first of all show how long a time it took before Snow was recognised as a "hero" of epidemiology. But in searching why and how it took such a long time, an even more intriguing idea developed. It seemed to me that the name of Snow was needed to replace a part of the history of epidemiology of which 20th century epidemiologists were a little bit ashamed: the period when epidemiology still believed in miasmata. In doing so, however, some of the sounder themes that epidemiology really inherited from miasmatic thinking (an inheritance which we prefer to forget), were suitably ascribed to John Snow. Somehow, the Snow which we know is a construct of several ideas from the history of epidemiology; some of which we are ashamed of, and others of which we are proud<sup>1</sup>.

### The role of "preconceived ideas"

Before embarking on the main theme, let me indicate that I have two interests in John Snow. The query about the way in which he became a hero of epidemiology was only my second interest. My first interest had to do with the origin of his ideas<sup>2,3</sup>. When reading John Snow and some of the commentators about his work, I was struck by the amount of *a priori* reasoning in his book "On the mode of Communication of Cholera". When you read it closely, you see that Snow was already a convinced contagionist and already believed that water was an important factor *before* he made his observations about the Broad Street Pump and about the water companies. Haven't philosophers already said for a long time that we only see what we know? Anyway, Snow

already had ideas about small amounts of cellular and self-replicating matter that would propagate disease. He had developed and strengthened these ideas, among others by simple clinical observation: case histories of children who shared beds in hospital and subsequently caught cholera from each other, and many similar observations that seemed to fit very well with his preconceptions about “germs”, contagion and disease. To the concept of “direct contagion” he added the idea that, when there is no immediate person-to-person contact, then water will carry the infection. And for that idea too, he already had “proofs” long before his epidemiologic observations.

If you want to see for yourself how much *a priori* reasoning there was in Snow’s work, you only have to read the first edition of his work on the “Communication of Cholera”. Few people realise that Snow’s celebrated book of 1855, which we know because it was reprinted so successfully in 1936 by Wade Hampton Frost, is only the *second* edition<sup>4</sup>. When you read the first, much smaller edition of 1849<sup>5</sup>, excerpts of which were also published in the London Medical Gazette<sup>6</sup>, you will find already essentially the same reasoning, the incrimination of drinking water. Then you understand that Snow made his observations with a very prepared mind. In the first edition, he makes already a first attempt, on very crude data borrowed from earlier epidemics of cholera, to point at differences between London districts and their water supply. He also has many anecdotes about pumps, overflowing cesspools etc. In retrospect, it was a “dress rehearsal” for the Broad Street Pump and the “water companies”. But even to a very benevolent observer like myself, it is clear that he was bending his data, or I should say, his anecdotes in the “right” direction. His was a highly single minded affair – and perhaps all scientists need a little of this single-mindedness to cling to one’s own theory, whatever the data and the objections of others. Even in his second edition, he “obliterates” part of the data: he only emphasises the first part of the epidemic that shows most clearly the association between drinking water and cholera; he avows that the second part of the epidemic is less clear in this respect, because by that time the spread of the cholera went on from person to person.

I am not the first to point to the role of preconceived ideas in Snow’s work. In the 1936 reprint of the second edition of Snow’s work (with the data that made Snow’s reputation) there is an introduction by Wade Hampton Frost, as well as a short biography by Snow’s contemporary and friend Richardson<sup>4</sup>. They wrote very clearly that Snow had very specific ideas “in mind” before he made his observations. Therefore, I do oppose certain epidemiologic interpretations that say that Snow just observed an association be-

tween water and cholera without having any inkling of the bacterial cause of cholera – 30 years before Koch made the discovery of the cholera bacillus. Such interpretations of the history of epidemiology lead to the notion that somehow “observing associations” is “the superior science”. This is to me a distortion of Snow’s writings. Quite on the contrary, what he describes as the background theory from which he set up his observations, is firmly rooted in “germ theory” convictions about causes of disease. I am neither the first<sup>7,8</sup>, nor will I be the last to make this observation. In a recent commemorative issue of the American Journal of Epidemiology, Winkelstein made exactly the same observations, based on other writings of Snow<sup>9</sup>.

Enough on my first interest in the interpretation of John Snow’s work. I related it with some purpose, however, as it will come back. This first interest drew me into the second, because I started wondering how the interpretation of Snow might have evolved over time. I already knew that in his time he was a clear contagionist, was in a minority position (perhaps because people thought that his observations were merely made to confirm his prejudices), and that the publication of his book was almost a financial loss to him.

### The emergence of Snow in the medical literature of the Netherlands

To delineate my search, I started to study the influence of Snow in the Dutch medical literature. However much I was prepared for surprise, my findings were still unsettling. In the Dutch medical literature, Snow’s work only grew into a “classic” between 1930 and 1950: some 80 to 100 years after the initial publication. During the first decades after his publication, his work was hardly mentioned. In the Dutch medical literature, i.e. in the contemporary issues of the leading medical journals in the Netherlands, there were occasional footnotes about a drinking water theory – mostly without mentioning Snow by name. On the whole, the leading paradigm of the 1850 to 1880s and even 1890s was Von Pettenkofer’s “Boden theory”, a multicausal variant of miasma theory<sup>10</sup>. Von Pettenkofer’s work about cholera was enthusiastically described and paraphrased in lengthy articles with a wealth of very supportive geographical data. His books and writings were abundantly translated and popularised. The greatness of Von Pettenkofer’s work was sung in all modes and it was often said that it would almost be impossible to improve on his work. His ideas completely permeated Dutch medical society. A very lone critic in 1873 showed that there was only a poor correlation between the composition of the soil and mortality in different areas of Rotterdam, the

Netherlands<sup>11</sup>. In the first decades after John Snow's initial publication, his drinking water theory only surfaced once in the Netherlands in a cautious 1868 report (some 15 years after the facts!). Mind, that this is about the same time that Simon in England started to give Snow some credit<sup>1</sup>.

The idolatry with Von Pettenkofer reached an absolute height at the time of his celebrated controversy with Robert Koch about the commaform bacillus that the latter had discovered in 1883 in Egypt and had designated as "the cause" of Cholera<sup>12 p. 149</sup>. Von Pettenkofer, who initially might have made some room for bacteria in his theory, was by that time so entrenched that he devised his famous experiment in which he swallowed pure cholera broth and successfully proved that neither he nor his co-experimenters got sick (among his co-experimenters were famous people like Metchnikoff who later converted himself to work with Pasteur)<sup>13</sup>. Ironically, this very successful experiment became Von Pettenkofer's Swan song. Strange if you think of it: that a successful experiment can be the beginning of scientific defeat.

From about 1900 onwards, when bacteriology emerged as the "stronger science", we find a gradual decline in the number of references to Von Pettenkofer, and a concomitant gradual increase in references to Snow. In a 1913 Dutch book of public health, we can read how refreshing it is to read the clear-headed 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 real understanding, and underscored Snow's findings<sup>14</sup>. The same Dutch author goes as far as to scorn hygienists for their mistakes: for example, he describes how hygienists had so much zeal to clean the city that they ordered to dump the street dirt of the city of London (including human excreta at that time) into the Thames River (from where the drinking water was obtained). So, it was written in 1914 that those hygienists might have prolonged the epidemic! At the same time Snow is praised for the equanimity by which he supported miasmatic slandering<sup>15</sup>. Still in a 1935 new book on public health in The Netherlands, Snow is not mentioned<sup>16</sup>. Von Pettenkofer is mentioned eight times, be it always in the negative. It is only in 1955, in the fifth edition of the same book on public health in the Netherlands that we read for the first time unequivocally that Snow's work is a "classic"<sup>17</sup>! In 1955, one hundred years after the original publication! At the same time, attention is drawn to the equally forgotten work of Budd on Typhoid Fever. In between, of course, there had been the reprint of Snow's work by Frost in the USA. The aim of the reprint was to make the work widely available. And it succeeded. The original work by Snow had almost been lost – for example in the Netherlands, there are still roughly 80 copies of books by Von Pettenkofer in libraries;

but only one original, and two reprints of Snow's 1855 second edition of the *Mode of Communication of Cholera*. Of the first edition of 1849, there must only be a few copies all over the world.

### The motives to make John Snow a "hero"

The motives of 20th century epidemiologists for making Snow a hero to replace Von Pettenkofer, became clear to me when studying the opposition between bacteriology and miasma theory. The new "Science of Bacteriology" had swept some of the greatest heroes of public health off their feet. The only historical figure of the 19th century to which epidemiologists and public health officials of the early 20th century could still relate to was John Snow, because he had proved to be "right" in his application of the "germ theory". Ironically, in his own time, the position of Snow's theory was a marginal one.

Epidemiologists of the first half of the 20th century have succeeded very well, maybe too well, in screening off the heritage of Von Pettenkofer. In introductions to epidemiology only Snow is mentioned, giving young students the impression that he was an important leader of epidemiology and public health in the middle of the 19th century. Thereby, the greatest irony of it all is that Snow's work is almost always described as a triumph of a type of thinking that is quite miasmatic or hygienic in character. It is forgotten that Snow had very firm convictions about germ theory and contagionism. His work is almost described as a victory of "black box" epidemiology, looking for environmental causes, without having any inkling of the biologic background. Somehow, the spirit of miasma still succeeds in blurring our view of Snow. What is more, all the credit that should go to hygienic thinking is brought to Snow. All hygienic precepts in combating cholera, i.e., not only those about drinking water, but also those about personal cleanliness, sewage disposal, etc. are now ascribed to Snow because he proved the contagion.

### Snow in the international literature

It might be argued that too much of the above is based on a study of Dutch medical literature only, and that the Dutch are too close to the Germans, hence the all-pervasive influence of Von Pettenkofer. The first historian who called attention to the fact that Snow's work was so slowly and erratically accepted was Garrison in his 1929 textbook in the USA<sup>18 p. 661-2, 781</sup>. There we also learn that Britain was the country where Snow's work was most readily accepted.

Already in 1866, only 11 years after the publication, Simon would have reported favourably about it to the Queen's council. Yet, we have Simon recognising in the 1890s how it took him 30 years to understand the "rightness" of Snow<sup>19</sup>. Nevertheless, even British epidemiology remained ambiguous. We have seen that in 1936 Frost secured the reprint of Snow's work, because he found it so important, and it proved an instant success. Yet, one year earlier, in 1935 Greenwood, the first professor of Epidemiology at the London School of Hygiene, published his magnum opus, "Epidemics and Crowd Diseases", about the history of epidemiology, partially written for the lay public and partially for professional audiences<sup>20</sup>. In Greenwood's account of the epidemiology of cholera in this 1935 book, neither Snow, nor the drinking water theory are mentioned<sup>20 p. 165 et seq!</sup> To Greenwood the real epidemiologic hero of the past was Creighton – Charles Creighton who in the 1890s had published extensively on the history of epidemics in Britain from the year 600 to the 1800s<sup>21</sup>. Creighton was a staunch follower of Von Pettenkofer; he was not only one of the last anti-contagionists, but also much opposed to small-pox vaccination. In his great book on the history of epidemics, a book that is still readable, both for its style and for its scrupulously factual accounts, Creighton sneered at Snow in a footnote as "one who had seized upon the occasion of a pump"<sup>21 vol II, p. 854</sup>. And still in 1929 Garrison – who had somewhat more sympathy for Snow – continued to describe Creighton as the founder of modern British epidemiology<sup>18 p. 742</sup>. In Germany, Virchow originally opposed the germ theory on cholera<sup>22 vol I, p. 124; vol II, p. 259–60</sup>; he admitted defeat in 1884<sup>12 p. 168</sup>. But even worse: Robert Koch who gave the ultimate demonstration that the drinking water theory was right by the isolation of the *Vibrio Cholera* in a water tank in India, even Koch did not mention Snow in his early papers<sup>12 p. 162</sup>. Either Koch did not know his writings, or Snow was thought to be irrelevant – we will never know. So much for the importance of epidemiology.

### Wade Hampton Frost

It will have become clear that the person responsible for making John Snow a hero was W.H. Frost. I ended one of my papers about Snow by citing an anonymous reviewer, who really seemed to have personal background information<sup>1</sup>. The review was typed by old-fashioned type-writer. I have never known who the reviewer was, and I repeat the quote here:

*"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 eight 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<sup>23 p. 6</sup>. He also gave a lecture at the American Epidemiological Society praising the Snow studies<sup>24 p. 3–22</sup>."*

The use of the "Snow exercise" has since that time spread over the world<sup>25</sup>. I was introduced to it by Hans Valkenburg, Professor of Epidemiology at Rotterdam, the Netherlands, when I became a member of his department, because it was used as a class exercise for medical students. I took it over to Leiden University, the Netherlands, but was forced to abandon it because medical students opposed it as "too lengthy, outdated and a mere exercise in reading".

All in all, 20th century epidemiology has accepted a strange mixture of ideas: the methodological example of John Snow was revived but it was intertwined by a continuation of the multicausal way of thinking by Von Pettenkofer. The basic reason for this attitude of 20th century epidemiologists might have been that deep in their hearts they preferred a multicausal miasmatic way of reasoning, but that they were nevertheless forced under the uni-causal umbrella of bacteriology. Snow was the best compromise.

### Note added in print: a selection of papers since 1996

The above text is a rather faithful rendering of the talk that I gave in 1996 in Annecy, France. Fortunately, the literature about John Snow continues to grow, and has been enriched with many new insights, and even discoveries. I want to mention in particular the paper by Lilienfeld who discovered a transcript of a testimony that Snow gave before a British parliamentary committee<sup>26</sup>; in that testimony, Snow adamantly refuses to take into consideration that "toxic fumes" could cause any disease whatsoever, and seems to take a political stand that sounds "reactionary" to modern readers. I tried to comment how we might interpret this episode<sup>27</sup>, which is so reminiscent of many of today's discussions in occupational health<sup>28</sup>. Next, there was the paper by Brody et al.<sup>29</sup>, which builds upon their earlier work<sup>30</sup>, to draw

attention to the neglected role of map-making in the work of Snow. It resulted in a flurry of letters by different authors in the correspondence pages of *The Lancet*<sup>31</sup>. Most recently, as to this writing, and as to my limited capacity to select and read all relevant literature, there was the editorial by Davey Smith and Ebrahim<sup>32</sup>. These authors draw our attention to one important sentence by Snow, in which he mentions that anticontagionist theories of cholera causation were in a hygienic sense as useful as what he believed to be the final truth. Thus, in line with Snow, Davey Smith and Ebrahim agree that some of the theoretical positions of believers in the miasma theory led to beneficial public health action. Nevertheless, they write that in the end, it is "better to be right for the right reasons". Finally, what else could one expect: a brief look at the internet shows that over the past years dozens of web pages have been devoted to Snow<sup>33</sup>!

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#### Zusammenfassung

##### Das sich wandelnde Bild von John Snow in der Geschichte der Epidemiologie

Seit Ende des 19. Jahrhunderts fasziniert Epidemiologen die Geschichte von John Snow und seinen Untersuchungen zur Ansteckungsgefahr bei Cholera. Aus heutiger Sicht wurden verschiedene Lehren aus der wiederholten Interpretation von Snows Arbeit gezogen. Die Geschichte von John Snow setzt sich aber noch weiter fort, bis ins 21. Jahrhundert.

#### Acknowledgement

The text of this commentary is based on a talk given at the symposium "Measuring our scourges – the history of Epidemiology" at Annecy, France, July 1-10 1996, and on a previously published text<sup>1</sup>. A note covering a selection of papers since 1996 has been added.

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#### Résumé

##### Evolution de l'image de John Snow dans l'histoire de l'épidémiologie

Depuis la fin du 19ème siècle, l'histoire de John Snow et de ses recherches sur la contagiosité du choléra a fasciné les épidémiologistes. Plusieurs leçons ont été tirées de l'interprétation et de la réinterprétation du travail de Snow, qui ont varié selon les perspectives dominantes. L'histoire de John Snow continue à évoluer, même au sein du 21ème siècle.

References

- 1 Vandenbroucke JP, Eelkman Rooda HM, Beukers H. Who made John Snow a hero? *Am J Epidemiol* 1991; 133: 967-73.
- 2 Vandenbroucke JP. Which John Snow should set the example for clinical epidemiology? *J Clin Epidemiol* 1988; 41: 1215-6.
- 3 Vandenbroucke JP. Is "The causes of cancer" a miasma theory for the end of the twentieth century? *Int J Epidemiol* 1988; 17: 708-9.
- 4 Snow J. On the mode of communication of cholera. 2nd ed. reprinted in: Frost WH, ed. *Snow on Cholera*. New York: The Commonwealth Fund, 1936.
- 5 Snow J. On the mode of communication of cholera. London: Churchill, 1849.
- 6 Snow J. On the pathology and mode of communication of cholera. *London Med Gazette* 1849; 9: 745-53 and 923-49.
- 7 Buck C. Popper's philosophy for epidemiologists. *Int J Epidemiol* 1975; 4: 159-68.
- 8 Cameron D, Jones IG. John Snow, the Broad Street pump and modern epidemiology. *Int J Epidemiol* 1983; 12: 393-6.
- 9 Winkelstein W. A new perspective on John Snow's communicable disease theory. *Am J Epidemiol* 1995; 142 (suppl): S3-S9.
- 10 von Pettenkofer M. Zum gegenwärtigen Stand der Cholerafrage. München: von Oldenbourg, 1887.
- 11 Ballot AM. Onderzoek naar de sterfte te Rotterdam en hare oorzaken. *Ned Tijdschr Geneesk* 1873; 2: 8-157.
- 12 Brock TD. Robert Koch, a life in medicine and bacteriology. Berlin: Springer, 1988.
- 13 Altman LK. Who goes first? The story of self-experimentation in medicine. New York: Random House, 1987.
- 14 Saltet RH. Voordrachten over gezondheidsleer. Haarlem: F. Bohn, 1913.
- 15 Saltet RH. Theorieën en voorbeelden uit den strijd tegen de besmetting. Haarlem: Bohn, 1914.
- 16 van Loghem JJ. *Algemeene gezondheidsleer*. Amsterdam: Kosmos, 1935.
- 17 van Loghem JJ. *Algemeene gezondheidsleer*. 5th ed. Amsterdam: Kosmos, 1955.
- 18 Garrison FH. An introduction to the history of medicine. 4th ed. Philadelphia, PA: W.B. Saunders, 1929.
- 19 Simon J. English sanitary institutions, reviewed in their cause and development and in some of their political and social relations. 2nd ed. London: Smith, Edler & Co, 1897: 263.
- 20 Greenwood M. Epidemics and crowd diseases. London: Williams and Norgate, 1935.
- 21 Creighton Ch. A history of epidemics in Britain. Cambridge: Cambridge University Press, vol I: 1891; vol II: 1894.
- 22 Virchow R. Collected essays on public health and epidemiology. Rather LJ, ed. Canton, MA: Science History Publications, 1985.
- 23 Sartwell PE, ed. Maxcy-Rosneau Preventive Medicine and Public Health. 9th ed. New York: Appleton-Century-Crofts, 1965.
- 24 Sartwell PE. Trends in epidemiology. In: Stewart GT, ed. *Trends in epidemiology: application to health services research and training*. Springfield, IL: C. C. Thomas, 1972: 3-22.
- 25 Vandenbroucke JP. Colon cancer and ulcerative colitis: an epidemiologic exercise revisited. *J Clin Epidemiol* 1992; 45: 923-4.
- 26 Lilienfeld DE. John Snow: the first hired gun? *Am J Epidemiol* 2000; 152: 4-9.
- 27 Vandenbroucke JP. Invited commentary: the testimony of Dr. Snow. *Am J Epidemiol* 2000; 152: 10-2.
- 28 Sandler DP. John Snow and modern-day environmental epidemiology. *Am J Epidemiol* 2000; 152: 1-3.
- 29 Brody H, Rip MR, Vinten-Johansen P, et al. Map-making and myth-making in Broad Street: the London cholera epidemic, 1854. *Lancet* 2000; 356: 64-8.
- 30 Paneth N, Vinten-Johansen P, Brody H, Rip M. A rivalry of foulness: official and unofficial investigations of the London cholera epidemic of 1854. *Am J Public Health* 1998; 88: 1545-53.
- 31 Correspondence. Snow and the Broad Street pump: a rediscovery. *Lancet* 2000; 356: 1688-9.
- 32 Davey Smith G, Ebrahim S. Epidemiology - is it time to call it a day? *Int J Epidemiol* 2001; 30: 1-12.
- 33 Non-exhaustive list of web pages devoted to Snow, as of April 10, 2001:  
<http://www.ph.ucla.edu/eppi/snow.html>  
<http://www.cdc.gov/ncidod/dbmd/snowinfo.htm>  
<http://www.sph.umich.edu/epid/GSS/pub.html>  
[http://www.sph.unc.edu/courses/course\\_support/case\\_studies/JohnSnow](http://www.sph.unc.edu/courses/course_support/case_studies/JohnSnow)  
[http://www.sph.unc.edu/courses/course\\_support/case\\_studies/JohnSnow/epilogue.html](http://www.sph.unc.edu/courses/course_support/case_studies/JohnSnow/epilogue.html)  
[http://www.anesthesia-history.org/displays/gallery/port\\_sno.htm](http://www.anesthesia-history.org/displays/gallery/port_sno.htm)

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