Who Made John Snow a Hero?

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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 Street pump, is known today by every epidemiologist as “the epidemiologic classic.” A controvert exists, however, about the real lesson which this historical episode bares 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

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example of a natural experiment that closely mimicks the conditions of a randomized controlled trial. Still others have been in-
trigued 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 small animalcule that would transmit cholera by contagion; more partic-
ularly, he held this theory many years before he made his renowned epidemiologic obser-
vations. 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 suc-
cessful scientific work in anesthesiology, where his leadership was accepted, leading to attendance of the Queen and to a presti-
gious 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 inter-
pretation, 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 epi-
demiology, and how and why certain inter-
pretations came into being, so as to be con-
tinuously transcribed in modern textbooks.
We started to wonder how John Snow's
work really looked in the eyes of his contem-
poraries. Was his type of investigation with
his type of hypothesis (drinking water as a
source of infection) being followed by any-
body? Was there recognition of his innova-
tion, either methodologically or as to the
cause of the disease? To be able to study the question in suffi-
cient 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 text-
books 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 posses-
sion of it until it became sarscsmac. There-
after 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 de-
tailed 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 af-
fter his original publication in the 1850s
the work was hardly mentioned. In the
Dutch medical literature there were occa-
sional footnotes about a drinking water the-
ory (usually without even mentioning Snow
by name), but on the whole, the leading
paradigm of those days was Von Ppettenkofer's
"Boden Theorie," a multiausian variant of
miasma theory (16, pp. 658-9). Von
Ppettenkofer's work was enthusiastically de-
scribed and paraphrased in lengthy data.
His books and writing were abun-
dantly translated and popularized for all possible
purposes. The greatness of Von Ppettenkofer'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 cautionous
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 Ppettenkofer in the
Netherlands reached its absolute height in
the famous controversy with Robert Koch.
after the latter discovered a commaforal ba-
cillus 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 at-
tached 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).
Vircow, who originally held a miasm-al
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. 229-40), admitted defeat in 1884 (18, p.
168). However, Von Pettenkofer remained
adamant in his opposition to Koch. He re-
 fused 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 aliquote of pure Vibrio cholerae
broth. His example was followed by a few other doubters, some of whom
would later become celebrated bacteriolo-
gists and immunologists themselves, like the
Nobel prize winner Mechnikov (21). Ru-
nor has it that Von Pettenkofer and one
other doubter developed some diarrhea af-
terwards, 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
to Von Pettenkofer’s swan song. From the
Dutch medical echos of the ongoing debate
we learned that slowly, i.e., disease by dis-
ease, the miasma theory, and Von Pettenkofer’s
critics 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 clearedhead work of John
Snow as a rare treasure amidst the volumes of
theoretical trash that higynism had pro-
duced; Koch’s discoveries were held to bring
the real understanding of the mode of spread
of cholera and to put hygienic precepts 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 ma-
terial, 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 squanimity by which
he had born the stupid slandering by miasma
hygienists, and contrasted in that respect to
Semmelweis. 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 Pudd 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 “clas-
sic” 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 un-
available work widely available. Such un-
availability 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 epidemiolo-
gists 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 blinds 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 notions 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 abounded with geographic charts of the spread of diseases in cities, regions, and countries, usually showing how right miasm 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 strongest 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 sociocultural 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 admission 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, broken 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 Vibri0 cholerae in a water tank in India (18, p. 165). 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 "predispensing 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 bacteriological 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. 298-90). 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 reemergence 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. Maczy, 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 after the republication of Snow’s work, the work of another great con- tagonist 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 epidemiologists accepted a strange mixture of ideas: the meth- odological example of John Snow was re- viewed 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 develope such a mixture might have been that they preferred a multicausal way of thinking, but were forced under the uni- causal umbrella of successful bacteriological (40, 41). Further study of the history of epidemiology seems necessary, not only to satisfy our human curiosity, but also to un- derstand the development of our reasoning in this science.

REFERENCES
