population, wherever the ground is not occupied with the factories above mentioned, and it lies by the riverside, at an elevation of only two feet above Trinity high-water mark, yet the deaths from cholera in 1854 were only 29 to each 10,000 inhabitants, whilst in London at large they were 45 in 10,000; in the sub-district of Kennington, 1st part, less densely inhabited, they were 126, and in Clapham 103 in 10,000, the latter being a genteel, thinly inhabited sub-district, at the elevation of 21 feet. Again, the sub-district of Saffron-hill, with the slaughter-houses, knackers’ yards, and catgut factories of Sharp’s-alley on its eastern boundary, and the Fleet-ditch, at that time uncovered, flowing through it, suffered in 1854 a mortality from cholera of only 5 in 10,000; being one-ninth of that of the metropolis generally, and one-twelfth of that of the Belgrave sub-district, where the mortality was 60 in 10,000. These circumstances might be thought to prove a little too much, were it not that the prevalence of cholera is influenced by a variety of circumstances, and in London very much by the nature of the water supply; for in the short but severe epidemic of 1854, the chief medium of its propagation in the metropolis was water, containing whatever passed down the sewers from previous patients. The sub-district of Bermondsey, called the Leather-market, which contains a number of factories for skin-dressing, suffered, in 1854, exactly the same high mortality as the other five sub-districts in the South division of London, which, like it, were supplied exclusively with the impure water of the Southwark and Vauxhall Company. The conclusion to be drawn from all these facts is, that the vicinity of offensive factories leaves the cholera to pursue the same course that it would do in their absence.

Sackville Street, July, 1856.

Commentary: Snow’s paper on ‘offensive trades’—with the benefit of 150 years of hindsight

Jan P Vandenbroucke

Leiden University Medical Center, Department of Clinical Epidemiology, PO Box 9600, 2300 RC Leiden, The Netherlands. E-mail: J.P.Vandenbroucke@lumc.nl

The paper by John Snow about ‘offensive trades’ is little known. Its re-publication is most worthwhile, because it is a good antidote: understanding this paper, and in particular understanding its background, is a good antidote against ‘potted histories’ of epidemiology that see Snow as a lone hero who saw ‘right’ while the rest of the world erred.

The general background of the cholera debate in the middle of the 19th century in Britain has been described extensively and insightfully in 2002 by George Davey Smith, in a paper in which he clearly shows the complexities of medical progress. Several persons held opinions like Snow, often equally well reasoned. William Budd was one among the more famous contemporary medical scientists who also ascribed cholera to contaminated water, with a reasoning that came close to Snow’s—which even led Snow to issue a kind of ‘priority claim.’ Others did not think that contaminated water was the complete story, but nevertheless saw some important role for water in the transmission of the disease. One such author was John Sutherland, whose work originally appeared in an appendix of a larger report about cholera in 1848–49 and was reprinted in 2002 in the IJE, together with Davey Smith’s and other commentaries. Sutherland also published tables about the
drinking water of persons who were struck with cholera, made maps but reasoned mainly about general sanitary conditions. Finally, the controversy about the origin of cholera was influenced by general social and cultural ideas, and by the opinions and prejudices held by medical scientists—not just about cholera, but about society in general.\textsuperscript{2,7–9}

For the micro-history of the 1856 paper, it is necessary to go back to 1855, the year in which Snow testified before a parliamentary committee about the Nuisances Removal and Diseases Prevention Amendments Bill. That Bill aimed to clear the environment from toxic fumes, especially fumes that were emitted from chimneys in certain manufactories, located in populated areas in towns. If enacted, the law would have dire consequences for some manufacturers who might have to close shop. A direct reason for Parliament to become active again about disease prevention was the 1854 cholera epidemic—but as things often go with parliaments, it tried to cast its net wider. Episodes of the famous London fog (or smog) due to all kinds of fumes, mainly from fossil fuels, which plagued London from the 19th century to the late 1950s, must also have influenced the notion that action ought to be taken against emissions of toxic fumes.

**Snow’s testimony before the Parliamentary Committee**

The story of Snow’s appearance before the Parliamentary Committee was described in detail by David Lilienfeld, who described the wider political background of the Bill and Snow’s testimony.\textsuperscript{10} In Lilienfeld’s paper the full text of the exchange between Snow and the committee is reproduced. It is very interesting for a 21st-century epidemiologist to almost hear Snow ‘speak’.

Snow appeared before the Committee in March 1855 on behalf of the manufacturers who produced the toxic fumes aimed at by the Bill. His single mission was to deny that toxic fumes could cause disease. He did so forcefully, and from the transcript looks completely prejudiced and intransigent. Accordingly, Lilienfeld’s paper had a very 21st-century title: ‘Snow, the first hired gun’.\textsuperscript{10} In a comment to that paper, I wrote that Snow’s prejudices can be understood at least in part.\textsuperscript{11} The same year, 1855, he published the second edition of his book on the transmission of cholera, with many new data that proved—at least in his own mind, though not in the minds of most others—\textsuperscript{2,11}—that contaminated drinking water was the main cause of cholera outbreaks. These views he had already published extensively about, and was impatient with those who did not see his point.\textsuperscript{2,12} As such, the notion that toxic fumes would cause disease must have looked to him akin to the miasmatic views of his anti-contagionist opponents. Snow was known to overgeneralize his findings, as he thought that many other diseases (like plague) would be caused by similar waterborne or ingestion routes, for which there was little evidence at the time, and which have been shown to have totally different causes later in medical history.\textsuperscript{2} The work of William Budd seems much more reasonable in this regard.\textsuperscript{2} Also, Snow was fairly convinced that only medically qualified persons should decide about the causes of disease, after much careful reasoning, which precluded the possibility that any worthwhile insight would come from well-meaning lay persons (say, like parliamentarians).\textsuperscript{2} Still, one may hold against Snow that he missed the wider point, i.e. that there might be other causes for disease than contaminated water transmission, and that outlawing the general habit of emitting manufacturing fumes in populated areas might be beneficial to health.

One authority that severely reprimanded Snow for his stand was the editor of the *Lancet* (anonymously, as was the habit at the time), in a stinging editorial in June 1855, wherein Snow was named and shamed in person; this editorial is also quoted verbatim in Lilienfeld’s paper.\textsuperscript{10} It was not that the *Lancet* was anti-contagionist—on the contrary, the *Lancet* had repeatedly defended contagionist views, i.e. Snow’s views, be it that they also regularly gave space to opposing views.\textsuperscript{2,11} The editors had apparently thought quite deeply about the different arguments pro- and anti-contagion. Whatever, the *Lancet* editor attacked Snow for the content of his testimony.

**The 1856 paper**

Maybe because of the journalistic principle of ‘hearing both sides’, the *Lancet* gave Snow the chance to defend his views. That was what Snow did in the 1856 paper on ‘offensive trades’.\textsuperscript{1} He stated that he had proven his point by data: offensive fumes are not toxic at all, not even to the people closest to the emissions, the workers in those very factories: these workers even had lower death rates!

Snow looked at the mortality in several ‘offensive trades’ and compared the mortality with other trades and with general mortality. To do so, he used the number of deaths in males ‘20 years and above’ in those trades and divided by an estimation of the person-years of males ‘20 years and above’ in the same trades. As the mortality data were gathered over an 18-month period, he multiplied the number of males aged 20 and above in those trades (from a census) by 1.5 to approximate the person-years; this led to his calculation of annualized death rates which were lowest in the ‘offensive trades’—much lower than the general population.

Snow immediately admitted that the way he did his calculations differed from the advice of the Registrar General, since the age composition of the different groups might be different, and age should in principle
be taken into account. However, he pleaded that the
data about the age structure were not yet available,
but that the results were uniform over time. He stated
that not taking the age structure into account did not
matter, since his calculations amounted to observing a
number of males as if they entered the different
trades from before age 20 and remained in those occu-
pations. Any 21st-century student of epidemiology
who has just finished his or her first introductory
Epidemiology 101 course might see the fallacy: Snow’s reasoning would be right if he had
constructed a life table, or calculated an overall cu-
minutive incidence from age 20 onwards, based on
age-specific incidences. His reasoning is wrong about
the amassed mortality and person-years of persons of
age ‘20 and above, for which the age structure is not
known’. Still, he tried to argue against potential
objections by stating that the bias resulting from a
comparison without taking age into account, would
be against finding lower mortalities in the offensive
trades; to make that point he transformed the annual
incidence of death into a life expectancy from age 20
years onwards (by simply taking the inverse of the
mortality rate, as if there would be a complete
steady state). This piece of reasoning is rather obscure
since, if anything, the inverse ought to be true: the
general population would contain many more elderly
and infirm persons. Even in Snow’s own discussion of
the data there is some wavering, when he writes that
the higher mortality in some non-offensive trades
might be due to many men entering those professions
at much older ages.

To explain the lower mortality in the ‘offensive trades’, relative to the general population and other trades, Snow invoked the idea that perhaps the men entering the offensive trades were somehow more
temperate, had healthier habits and did not have a
sedentary life style (sic). For this remark he is some-
times credited with foreshadowing the ‘healthy
worker effect’. However, these sentences seem to
me more like an attempt to explain away the fact
that the mortality in the offensive trades was not similar to that in the general population (what one
would expect under the null hypothesis of no
effect). From the late French medical historian
Mirko Grmek, I once learned that methodological
concepts, like the concept of the healthy worker
effect, cannot be said to exist before they are coined
and recognized by at least a part of a scientific com-
munity: that is, until there is a school of thought that
fosters and propagates the concept. A single throw-off
remark in one paper shows some insight, but does not
really suffice, as no school of thought followed up on
this concept after his paper.

**A verdict on Snow?**

Like in my previous commentary,11 I have a tendency
to be lenient on Snow. It is possible that he had
several shortcomings—as a person and as a scientis—but we cannot know as we know few details
about his life. We know that he was generally un-
happy because he felt ostracized by the medical sci-
entific establishment for his opinions about
contaminated water and cholera, although he was
successful in anaesthesia.12 So, he might have taken
any occasion to propagate his views and show that
others were wrong, before the Parliamentary
Committee and in his 1856 paper. Moreover, he did
not have the benefit of an 21st-century Epidemiology
101 course; basic epidemiological knowledge was not
laid out for him in easy bits to digest, but he had to
learn and plod along slowly by himself and by reading
the works of others (Snow’s calculations of incidence
rates and his derivation of life expectancies from
them showed that he had a good grasp of the
theory behind these notions). From the transcript of
his testimony to Parliament, it transpires that he felt
ideologically close to the manufacturers, as he
thought their useful work should not be hampered
by ill-founded views on the origin of diseases.
Whether or not that can be held against him, as it
might have further clouded his views, is a matter of
opinion. Such conjunctions of social and scientific
opinion seem to be of all times.11

The re-publication of Snow’s 1856 paper remains
a salutary lesson about the way that scientific insight
into the causes of disease makes progress: by many
detours and mistakes, by half-theories, and combina-
tions of theories—intermixed with social and cul-
tural influences and prejudices. Thus, even someone
like Snow whose views on one particular matter
proved ultimately to be right, could be overly self-
assured and lose caution when applying his views to
different contexts—and be dramatically wrong in his
opinions enunciated in the name of science. That is
what we may say now, safe in the knowledge that
benefits from hindsight based on 150 years of medical
progress.

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Sandra Hempel

www.sandrahempel.co.uk. E-mail: palewell@globalnet.co.uk

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John Snow’s 1856 paper ‘On the Supposed Influence of Offensive Trades on Mortality’1 was published against a background of debate about the role of offensive smells, or miasma, in the morbidity and mortality of the population. Much of the argument focused upon the transmission of fevers and epidemic diseases and whether they were contagious: especially cholera, which during the previous 25 years had killed over 100,000 people in Britain in three nationwide epidemics.

In 1856 the miasmatists were in the ascendant. Ten years earlier Edwin Chadwick, who was to become head of the Board of Health, had told MPs: ‘...we may say that, by depressing the system and rendering it susceptible to the action of other causes, all smell is disease’.2 By 1856 though, John Snow had accumulated a sizeable body of evidence to support his theory that cholera was an intestinal disease spread via the oral-faecal route, mainly through polluted drinking water.3 In the mid-1850s many accepted that polluted water sometimes had a part to play, but only as one of several possible contributory factors rather than as the primary cause.

The summer of 1854 saw the famous incident of the Broad Street pump when Snow persuaded the parish authorities in Soho to remove the pump handle in an attempt to stop a particularly virulent cholera outbreak. More importantly that summer, however, Snow had carried out his Grand Experiment in South London, comparing the numbers of cholera deaths in those households that obtained their water from the Southwark and Vauxhall company with those in households supplied by the Lambeth company. Southwark and Vauxhall pumped their water unfiltered from the Thames close to where the sewage of the capital was discharged. Lambeth, however, had moved its works to rural Thames Ditton, beyond the reach of the filth. In the first 4 weeks of the epidemic, mortality was 14 times greater among the Southwark and Vauxhall customers than among Lambeth customers, Snow found.2

Also in the summer of 1854, the British Government recruited its own team of doctors, chemists, microscopists [microbiologists] and meteorologists to conduct a wide-ranging investigation of the causes, transmission and treatment of epidemic cholera. The experts finally concluded that ‘after careful inquiry’, they saw no reason to adopt Snow’s belief that cholera was largely waterborne. Cholera, they said, was not contagious but caused by ‘a wandering ferment’ in the atmosphere.4

Three months before this report was published in 1855, John Snow appeared before a parliamentary committee studying the proposed Nuisances...