

My Interview With John Snow

Kenneth J. Rothman

26 April 2004

Allen J. Wilcox
Editor-in-Chief
EPIDEMIOLOGY
Snow Building, Suite 606
331 W. Main Street
Durham, NC 27701

Dear Allen,

I know you savor the irony that the journal's editorial office is now located deep in the heart of tobacco country, and that the building itself is called the Snow Building. Last night, just before I went off to bed, I happened to be looking at a photograph of the building's portico on my computer. During the night, a heavy storm passed through eastern Massachusetts, making for restless sleep, and I tossed my way through strange dreams during much of the night. As usual, I have forgotten the content of the dreams; all I know for sure is that the last image on my mind when my head hit the pillow was the Snow Building façade. In the morning, when I returned to my study, although the computer was off, I found a new document face down in my printer tray. I thought you would be interested to see it.

Yours,
Ken



Correspondence: Kenneth J. Rothman, Department of Epidemiology and Division of Preventive Medicine, Boston University Medical Center, 715 Albany St., Boston, MA 02118. E-mail: krothman@bu.edu

Copyright © 2004 by Lippincott Williams & Wilkins

ISSN: 1044-3983/04/1505-0640

DOI: 10.1097/01.ede.0000134546.70252.e7

INTERVIEW

KR: Dr. Snow, thanks so much for granting me this opportunity to meet with you. I can hardly believe that I located you. Do you mind if I take notes?

JS: No, not at all. I have to say that I am surprised that you succeeded in finding me; I do not get around much. How did you happen upon me?

KR: I really don't know. My best guess is that I am dreaming, but even so, I would really like to find out where this dream goes. As it happens, you have been on my mind of late—not that John Snow is ever too far from the mind of any epidemiologist. But recently I have been reading the newly published biography of you by Vinten-Johansen and colleagues—Cholera, Chloroform, and the Science of Medicine, A Life of John Snow.¹ It pulls together many details about your life and work. After reading it, I began to reread some of your own writing, and then I mused about having the chance to talk to you directly. These musings occurred mainly after long dinners when I had consumed too much wine. Sorry, no offense intended...

JS: None perceived. My abstemious habits regarding alcohol and the consumption of flesh were never motivated by moralism. I did not and do not proselytize. Nor do I chastise. You see, my temperance has been nothing more than a personal choice, as your intemperance is your personal choice.

KR: Touché. So you are still abstemious after all these years?

JS: I continue to avoid alcohol, but kidney problems have persuaded me to moderate my stance on eating meat. But an occasional meat meal is more than adequate for anyone. I noted that Tony McMichael predicted that everyone will join me in reducing their flesh intake as the planet becomes saturated with hungry people.

KR: You refer to Planetary Overload²—a compelling book. I see that you are keeping up with the literature.

JS: I do try. There is a bit much of it lately, even for someone who professes no greater love than learning.

KR: Do you also attend meetings?

JS: Hardly ever, although I wish I could get out more often. I know the value of attending professional meetings. Without them, I would have been much less a student of medical science. In particular, I esteemed the energetic debates of the Westminster Medical Society, especially in its early days when the attendance was more limited and the discussion considerably more heated.

KR: From my reading of Vinten-Johansen et al.—I'll call them V-J for short—I gathered that you were something of a tiger in your famous medical debates with the notables of the London medical world. Yet I got the impression that you were shy and even aloof with patients in your general practice. One might infer from that description that your

bedside manner was, how should I put this, underdeveloped? Would you agree?

JS: Underdeveloped? You resort to circumlocution, but in any event, the charge is simply rubbish. It was not a lack of bedside manner at all. I simply did not affect the haughtiness so fashionable among the London doctors who gazed down the social ladder at their patients below. Doctors then as now are apt to feign exactitude in their knowledge that does not befit an honest appraisal. Open talk about uncertainties was not taken to in a kindly way; it was perversely seen by some other physicians as a type of arrogance. I daresay that I would have been happy as a patient to have had a doctor such as myself. And, by the way, I do not think of myself as shy nor aloof. I would have described myself as “earnest and engaging.”

KR: Sorry, I meant no criticism of your doctoring. In fact, I read that earlier this year you were voted the best physician of all time by readers of Hospital Doctor Magazine, with Hippocrates coming in second. But let me follow up on your comment about your practice. It seemed from my reading that later in your career your general practice diminished while you specialized more in administering anesthesia and began to devote more of your time to your research efforts. Were you being pulled toward research or were you discouraged in some way with patient care?

JS: First of all, let us be clear on one thing: demand for my services as a physician never flagged. Over the years I spent as a London doctor, I gradually made more time for my research endeavors, but this change was my choice. I was one of the best anesthesiologists in London, if not the best, because of the care I took to understand the process. I narrowed my practice to anesthesia in part because it afforded me the opportunity to conduct my inquiries, but even more so because by showing London doctors how the anesthesia should be administered, I was spreading the most important new medical knowledge of the day.

But never mind all that. I must say that I'm a bit surprised by your question. Is it not obvious to you that research accomplishes more good and brings greater health to humanity than more intensive doctoring would ever accomplish? I devoted time to my inquiries, embracing the scientific method, to improve the lot of all patients. I am sure you understand the concept that success in science can prevent people from becoming patients in the first instance.

KR: OK, let's talk about research. Did you consider your anesthesia investigations, and your refinement of the clinical practice of anesthesiology, to be a more important contribution to medical science than your efforts to understand the communication of cholera?

JS: Both are important topics, for completely different reasons. I know that you epidemiologists care more for the cholera story, which I turned to after I completed my major work with ether and chloroform. As important as cholera was,

however, I would argue that my contribution to understanding narcotism—what everyone now calls anesthesia—had at least an equal importance. Surgeons might have gone for who knows how many years administering ether or chloroform by spilling it on a handkerchief. At the outset they had practically no understanding of how the gas worked its effect. My experiments on the equilibrium levels of gases in air and blood showed how to calibrate the inhaled gas level judiciously to achieve the intended level of narcotism, all at minimal risk to the patient. Look here, I spent nights and weekends for months on end measuring changes in respiration rates, temperatures, concentrations, symptoms, and all else I could think of in every vertebrate species to be found in London. In an amazingly short order, I knew how to administer these gases safely and effectively. I presented my findings at meetings, published them in scholarly works, and showed the fruits of this knowledge to the practitioners of London. This work, I am not too shy to say, transformed medicine irrevocably. No longer was the definition of a good surgeon one who could amputate a limb in 30 seconds. Imagine how much this did for patients. Was my time not better spent working out the principles of the administration of these gases than peering at the tonsils of Soho?

KR: Some would say that it was your role in administering chloroform to Queen Victoria that was the signal event in revolutionizing the use of anesthesia. Ironically, you administered it to her by handkerchief.

JS: I did indeed. To put it crudely, as you Yankees might, I was indeed reluctant to place an inhaler over the snout of the Queen. I used a linen handkerchief, folded unobtrusively, and imbued with scant traces of chloroform. We used the chloroform for analgesia, not for anesthesia, of course. Very little was required to blunt the pains of labor, and I used as little as possible to relieve her Majesty's stress. It was effective. It was also symbolic, opening the door of acceptance to chloroform. But acceptance did not come immediately. Wakley, that annoying gadfly, editorialized in the *Lancet* that it was irresponsible to submit the Queen to the vagaries of such a dangerous gas, and even suggested that the report of the queen receiving anesthesia was false—that the queen was tricked into thinking she received chloroform. Of course, she did receive the chloroform, and there was nothing irresponsible about it. She was never unconscious, she was never subjected to any extra risk. At least on this point, as on so many others, Wakley was nearly alone in his scolding, blathering away and behaving like a loudmouth. Of course, he held the attention of the medical world of London, and he did not hesitate to write anything he took a fancy to in that little journal of his. It is remarkable how much power accrues to arrogant bullies who happen to have the power of their own publication with which to disperse their rants. But truth be told, I sort of admired him for all his hectoring and grand-

standing. Right or wrong, he was never hesitant to call things as he saw them.

KR: I understand what you mean about editors. But perhaps we could discuss your work on cholera? You realize that it has assumed mythic proportions for generations of epidemiologists?

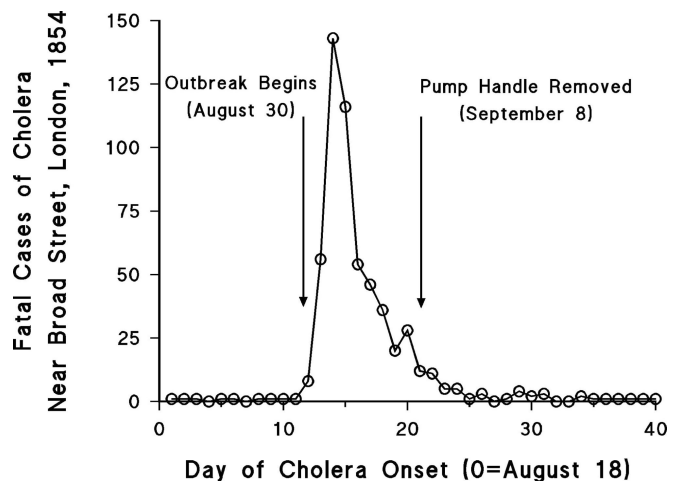
JS: I am gratified for the attention, to be sure, although some of the facts have been distorted with time.

KR: Nowadays you are revered by many in medicine and public health as the man who stopped the Broad Street outbreak by taking the handle off the pump. But that's not exactly what happened, is it?

JS: Well, I did not physically take the handle off the pump, if that is what you mean, although I did bring it about.

KR: That isn't what I meant. I realize that you argued your case before the Board—the Board of Governors and Directors of the Poor of St. James Parish, to be precise—and that they, acting on your advice, removed the handle of the pump the very next day. That was on Friday, September 8, 1854. But you yourself published a meticulous account of the epidemic curve, showing the daily deaths from cholera around Golden Square. This list was Table 1 in the revised version of your best-known work, On the Mode of Communication of Cholera.³

Anyone can see from the listing of daily deaths that the epidemic was nearly over when the pump handle was removed. I brought with me a graph that I made from your table. This graph shows where the epidemic was when you, or I should say the Board, removed the pump handle. But the myth surrounding this event has it that you stopped the epidemic by having the pump handle removed. Within a few years, Richardson, the physiologist and historian, wrote of the event: "The pump-handle was removed, and the plague was stayed." Isn't the reality closer to that old saw that if you want to look successful in stopping an epidemic, the right time to intervene is when the epidemic curve begins to fall?



JS: You should not look to me to defend what my good friend Richardson wrote about it or what others after him have perpetuated. It is true that the epidemic curve had fallen nearly back to normal by the time the Board removed the pump handle, which I clearly explained in my 1855 pamphlet. In fact, if you read what I wrote, you will find that I mentioned the dropoff in deaths, and I speculated that it could have been due to one of two possibilities—from either the spontaneous cleansing of the pump water or from the flight of the surrounding population, which departed in droves as soon as the epidemic commenced.

But even so, I would not agree with you that the removal of the pump handle was without effect. Really, I thought you modern-day epidemiologists were smarter than that. How do you know what would have happened if the pump had remained in operation?

KR: *Do you mean to say that you think the epidemic curve would have started climbing again?*

JS: Who knows? I admit that your clever graph makes it look as if removing the pump handle did not affect the course of events at all. But how can you be sure that the epidemic would not have flared again? Thanks to the persistent sleuthing of the Reverend Whitehead, we have some evidence that a second wave of deaths may have been averted by removing the pump handle.

KR: *You must be referring to the Lewis family. I read something about that family's experience in the V-J book about you.*

JS: Yes, I do refer to the Lewis family. Look at your graph. There was no more than one fatal attack around Golden Square on any day before August 30. On that day, there were 8 fatal attacks. The next day 56, and the day after that 143. It is obvious that something critical happened to ignite the epidemic just before August 30. That critical event was clearly the contamination of the well. As the Reverend Whitehead learned from his intensive investigation after the epidemic, poor Mrs. Lewis' infant daughter came down with diarrhea on August 28 and died on September 2. During that interval, the infant passed copious quantities of "rice-water" evacuations, repeatedly soaking her nappies. Her mother kept them in cold water before washing, and poured that water into the cesspool in the street in front of their dwelling. Months later, after Whitehead suggested that Mrs. Lewis' infant might have been the source of the pump contamination, the cesspool in front of Mrs. Lewis' building was excavated. They found it to be "misconstructed" in a way that caused the sewage to back up, and the brickwork lining the cesspool and its drain was decaying. This underground cistern, full of cholera evacuations from the unlucky Lewis infant, sat less than 3 feet from the bricks that lined the well. The infectious content from this cesspool, contaminated with the choleric "rice-water" excreta of the Lewis infant, dripped steadily into the well.

KR: *All this was discovered after you published your 1855 essay, wasn't it?*

JS: Yes, exactly.

KR: *So the infant's death on September 2 meant that after that date, the cesspool was no longer contaminating the water of the well with cholera excretion. Are you certain that was the reason that the epidemic came to a halt?*

JS: It fits, does it not? At the time, the miasmatics, which is to say most of the hygienists with any influence, were too obtuse to put all the evidence together. They were worried much more about the exhalations of the cholera victims, along with imaginary effluvia of long-buried corpses and the vapors of foul sewage rather than the sewage itself. Of course, in hindsight, it is easy to be confident of what was truly happening, as my theories about how cholera is transmitted have been confirmed beyond any doubt. We know for certain that it was the contaminated well water in Golden Square that caused the many hundreds of deaths, and it seems almost certain that the Lewis infant was the index case in the epidemic. The infant's excreta were delivered into the well water and from there into the mouths of those who drew their water from the pump.

KR: *Is that the same route by which the nonexistent second wave of deaths, the deaths that you claim to have averted, would also have been caused?*

JS: On September 8, the very day that the pump handle was removed, Mr. Thomas Lewis, a policeman by trade, who was father to the poor Lewis infant and husband to Mrs. Lewis, became sick with cholera. On that day, Mrs. Lewis once again began to fill the cesspool with excreta from a cholera victim and to infuse the well with the infective agent. But fortunately, the pump was by then out of operation, and the epidemic was halted.

KR: *It makes a compelling story. Had you known of it in time for the publication of your essay, would you have included it?*

JS: Most certainly. Each of these facts is a piece of the puzzle. As it was, I did describe in minute detail some cholera victims who had little or no connection with Golden Square but for the misfortune of getting a draught of water from it by some odd means. There was the man from Brighton whose brother died in the epidemic on September 1. The Brighton man came to visit in Golden Square for no more than 20 minutes, during which time he ate a small lunch of rumpsteak along with a tumbler of brandy and water, and then departed for Pentonville where he died of the cholera on September 3.

KR: *The brandy didn't sterilize the water?*

JS: Apparently it was too little. Then there was also the widow from Hempstead who had not been near Broad Street for months but who so liked the water there that she had a bottle delivered to her by cart. She had a delivery of some water on August 31 and she died of the cholera the following day. These are compelling facts, are they not? But the real

case for the mode of communication was made by the overall findings from my epidemiologic investigation.

KR: You refer to your natural experiment comparing the customers of the two competing water companies, the Lambeth Company and the Southwark and Vauxhall Company? Do you realize it is the most oft-quoted epidemiologic study ever undertaken?

JS: It is sweet consolation for the criticism I faced then for my supposedly radical views. But it still amazes me that the old boys of the London medical world could not see the evidence for the proof that it was. Perhaps proof is too strong a word, but all the evidence was laid before them, and they would not shake their rigid views. The miasmatic theory was awfully vague, and yet they clung to it. The cholera evidence, as you know well, was overwhelming, and yet because what we knew about the water supply could not account perfectly for every case of cholera, they were loath to accept water as the mode of transmission.

KR: Max Planck, the great physicist, who coincidentally was born in 1858 just a few weeks before your fatal stroke, wrote that "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." That would seem to apply in your case.

JS: It did not apply for the adoption of anesthesia, which spread like an uncontrolled fire. But for the transmission of cholera it would seem apt. The worst of it was that the sanitarians, from Chadwick on down, although well-intended, were apt to make the situation worse instead of better.

KR: That sounds counterintuitive. How did improved sanitation make things worse?

JS: The problem with the sanitarians was their preoccupation with sewage, because it emanated foul odors. They did not like the cesspools that harbored waste under city streets for the offensive smell that wafted up from them. They cared more for regularly cleaning out the sewers than they did for cleaning up the river. In their view, stench was the root of epidemic diseases. Their preoccupation with flushing waste out of city buildings and into the river was a serious, even catastrophic problem, because they paid no heed to what happened afterward, as long as the foul odor was mitigated. Most believed that the dilution of the sewage in the river was sufficient to cleanse it, but their nose was their primary meter of success. Unfortunately, the desire to flush waste called for pumping more water from the river back to the homes and businesses from which the sewage started. The sanitarians' emphasis on clean sewage simply recycled the waste water, perhaps a bit diluted and less foul of smell, but still potent nonetheless, back into the mouths of the citizenry. I suspect that this sanitary advance killed more people than it saved, at least until the water intake was changed to an upriver source.

KR: Your work is often linked to that of William Farr, the influential medical statistician. Was he one of those sanitarians you speak of?

JS: Farr was no Chadwick. Chadwick, even when he was the Sanitation Commissioner of London—which he was during the big cholera epidemics—was dreadfully unpopular. He was exceptionally priggish and arrogant. He wanted to build Eiffel towers everywhere just to pump the pure air at high altitudes down onto the streets. But I hasten to add that he was well-motivated, and a lifelong champion of the poor and downtrodden. For that I was glad to see that he was knighted in 1889, the year before he died, even if it was so very late in coming.

Farr was also headstrong, but less so. Even after my publication on cholera in 1855, Farr and his "Committee for Scientific Enquiries in Relation to the Cholera Epidemic of 1854," reviewing the matter, still thought that the cause of the epidemic was foul air. His report concluded that drinking water from the well in Golden Square was not associated with cholera. Farr's own data on altitude and cholera mortality pointed clearly to the water, at least to my way of thinking, but Farr seemed to believe that something in the air drifted down to lower altitudes in high concentrations. It sounds silly. But Farr's work was instrumental, even if his views were a hindrance for a time. Eventually, Farr came around to understanding the importance of drinking water in distributing the cholera poison. For me, that is the most important characteristic of a good scientist—the willingness to change one's mind.

KR: What eventually led to Farr changing his mind?

JS: I would like to think that my cholera study in the south part of London—the so-called "natural experiment"—influenced him. But he was a careful thinker and brought much evidence to bear on the question. As much as I would like to go into detail about it, I fear that I am a bit tired right now. Perhaps we can continue our discussion another time?

KR: Of course. You have been very generous with your time. Thank you for your perspectives.

JS: You are most welcome.

ACKNOWLEDGMENTS

Peter Vinten-Johansen and Jan Vandenbroucke were extremely helpful in keeping my imaginary Dr. Snow consonant with the real one.

REFERENCES

1. Vinten-Johansen P, Brody H, Paneth N, et al. *Cholera, Chloroform, and the Science of Medicine: A Life of John Snow*. New York: Oxford University Press; 2003.
2. McMichael AJ. *Planetary Overload: Global Environmental Change and the Health of the Human Species*. Cambridge: Cambridge University Press; 2003.
3. Snow J. On the mode of communication of cholera. In: Snow J, ed. *Snow on Cholera*. New York: Hafner Publishing Co; 1965:ii-139.