

LETTERS TO THE EDITOR

RE: "WHO MADE JOHN SNOW A HERO?"

The recent essay "Who Made John Snow a Hero?" by Vandembroucke et al. (1) serves as a timely reminder that epidemiologists, like many other scientists, have a penchant for simple and at times self-serving interpretations of the development of our discipline. According to this account, good ideas ultimately triumph over bad, brilliant but neglected scientists of one era are recognized by later generations as being "ahead of their time," and science advances ever closer to the "truth" chiefly through improvements in reasoning and technique (2-4). As amply demonstrated by the burgeoning social histories of science, however, this narrow view represents a seductive but inadequate stance (2-9). Instead, the concepts that scientists use, the questions they ask (or ignore), their openness to "discovering" or recognizing new findings, and their tendency to accept or reject hypotheses and theories are all deeply and inherently connected to their world views, which in turn are shaped by their society's dominant culture, ideology, and politics, as well as by the position of scientists within that society (2-9).

It is for this reason that the omission by Vandembroucke et al. of any discussion of the political history of the miasma/contagionist debate (1) is both curious and troubling. As Ackerknecht (10) and others (5, 6, 11-13) have amply documented, more was at issue than simply "direct contagion" versus "bad air" spreading noxious vapors produced by "filth." To the liberal upholders of the miasma theory, contagion implied the need for quarantine of ships and other commerce, a strategy that invited undue government interference in the economy, threatened profits, and reeked of an autocratic "ancien regime" mentality (10-13). Snow himself was acutely aware of the economic implications of his hypothesis (14) and earnestly hoped that other less intrusive means could be found to prevent transmission. With the consolidation of industrial capitalism and a shift toward more individualistic approaches to social welfare and public health policy in the 1880s (5, 6, 10-13), along with the identification of "healthy carriers" (5, 13), Snow's hopes were realized and the "germ theory" gained increasing acceptance. This complex history is a far cry from the assertion by Vandembroucke et al. that "germ theory" triumphed primarily because "bacteriology emerged as a stronger science" (1, p. 969).

Secondly, in contrast to what Vandembroucke et al. state (1, p. 970), Snow's conception of

contagious disease was not the same as that of contagionists in the 16th century. As Henle stressed in his now classic 1840 essay "On Miasmata and Contagia" (15), prior theories of contagion held that the actual disease was passed directly by sick to healthy persons. The theory advocated by Henle and Snow, however, proposed a radically new and conceptually different approach: disease-causing agents were microscopic living creatures that replicated inside of, and could be excreted by, sick people and, thus, could be transmitted not only by direct contact but also by air, water, soil, food, and a variety of fomites (14-16). From the standpoint of etiology, this new hypothesis encouraged investigators to search for agents in bodily excretions and the substances with which they came into contact, while from the standpoint of therapy, this approach fostered the development of remedies to treat the source—as opposed to only the symptoms—of the infection (16).

Subsequently, epidemiologists were forced to move beyond the "unicausal" thinking that Vandembroucke et al. apparently view as the hallmark of bacteriology (1, p. 972), and seemingly favor for epidemiology as a whole (17), by two events: the discoveries that not all exposed people became infected and not all infected persons became ill (11, 18), and the economic depression of the 1930s, which revived concerns about the effect of the "social environment" on health (5, 11, 19). The richer models of "host, agent, and environment," and now "web of causation," have led to new insights regarding the causes of disease incidence (as opposed to simply the causes of cases) (5, 6, 11, 20, 21) and are presently being challenged by theories regarding the social production of disease (6, 9, 20-24). If epidemiologists are to "understand the development of our reasoning" (1, p. 972) and the selection of our heroes (and heroines), it is clear we must pay attention to not only the biologic but also the social roots of epidemiologic thought.

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In the conclusion of their essay about John Snow, Vandenbroucke et al. (1) quote at some length an anonymous reviewer who describes the role of Wade Hampton Frost in bringing Snow to the attention of epidemiologists. The reviewer also touches on Kenneth Maxcy's use in teaching of the writings of both Frost and Snow. But what is omitted—from the essay and the quotation—is a reference to the 1941 Commonwealth Fund collection of Frost's papers, edited by Maxcy (2). In the opening paragraphs of his introduction to this volume, Maxcy takes note of Frost's admiration for the achievement of Snow and describes the impact that Snow's work had on Frost's career in research and teaching. It is evident that Maxcy came to the same conclusion some 50 years ago that Vandenbroucke et al. have reached today.

There are two other pertinent additions to the list of references provided by Vandenbroucke et al., Sartwell in 1976 (3) and Lilienfeld in 1983 (4). The latter author concluded his remarks thus: "It can be truly said that from Snow to Frost, there was none like Frost."

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A recent article documented the process by which John Snow became recognized as the father of modern epidemiology (1). I would like to point out that even his own vital data have been erroneously documented by his intellectual descendants. For example, students of the history of epidemiology and anesthesiology will already be aware that the original gravestone of John Snow, located in Brompton Cemetery, initially recorded the incorrect year of his birth as 1818 instead of 1813, a nice example of a digit confusion coding error. This was corrected during post-war restoration of the gravestone (2). I recently discovered that the exact month of John Snow's birth, recorded as June 15, 1813, by his friend Sir Benjamin Richardson in the classic, authoritative, and

1813 2 nd Feb	10 th Feb	Sarah Daughter of	Thos. Dwyer	Cooper	North Street	Printer	G. Moore Minister
1813 10 th Feb	10 th Feb	Charles Son of	John Mason	Wilson	South Street	Labourer	G. Moore Minister
1813 14 th Feb	14 th Feb	Mary Daughter of	Thomas Barnes	Beaumont	South Street	Labourer	G. Moore Minister
1813 23 rd Feb	23 rd Feb	John Daughter of	John Mary	Sinton	South Street	Printer	G. Moore Minister
1813 15 th Mar	15 th Mar	John Son of	William & Frances	Snow	South Street	Labourer	G. Moore Minister
1813 17 th Mar	17 th Mar	Sarah Daughter of	John Sarah	Allen	North Street	Glazier	G. Moore Minister

FIGURE 1. Parish record entry, All Saints Church, North Street, York, England, confirming the birth of John Snow on March 15, 1813. (From the original in the Borthwick Institute of Historical Research, University of York, York, England. Reproduced with permission.)

often reprinted text "Snow on Cholera" (3), also disagrees with the month of birth. The month is recorded as March 15th on the tombstone. My father, John Brunskill, kindly located the original entry for me in the church register, which confirms that John Snow was born on March 15, 1813 (figure 1). Occupational epidemiologists, who are concerned that vital records may not accurately record parental occupation (4), may also note that the original record reports that Snow's father was a "labourer" rather than a farmer as recorded in the biography. The confusion in months by Sir Richardson was probably with the month of Snow's death, June 1858. This verification will allow those of us who annually commemorate the birthdate of John Snow to do so now with confidence as to the correct month.

Those who wish to research this source are encouraged to visit the charming church in York, England (All Saints, North Street) and indeed to make a contribution to its steeple repair fund as, sadly, this church is in great need of funds. The visitor might speculate how the oral themes recounted in the beautiful stained glass might have influenced the young John Snow to pursue a health career. Additionally, an examination of the graveyard of this church provides a further clue to the family life of John Snow which original

researchers may wish to pursue. The parish records are now housed at the Borthwick Institute, University of York.

I believe that these sobering lessons of the fallibility of human data recording will continue to encourage us all to validate our sources wherever possible. The persistence of data recording errors in vital records continues to add to the challenge of perinatal epidemiology (5).

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THE FIRST AUTHOR REPLIES

I am extremely grateful to Dr. Dunn (1). His completion of the original "anonymous reviewer comments," which are so important to our paper (2), puts the record straight in several respects. The only reason why the writings of Maxcy were not cited was because I could not lay my hands on the original during the revision of the paper and I do not like to quote material I have not seen myself. Dr. Brunskill's graveside evidence (3) leaves us with the tantalizing question of what other clues might be present in John Snow's family life.

While Dr. Krieger's letter (4) contains many delightful additional references which will undoubtedly stimulate others to read and make up their own minds, there is an intellectual undertone which I do not wish to leave unanswered.

For greater clarity of my remarks, it is useful to draw a somewhat artificial contrast between two straw men, representing two antagonistic views about the interpretation of the history of medicine. The first view holds that the history of medicine is only an application of general history and that the evolution of medical ideas follows directly from the evolution of society in general. The second view holds that there exists a true physical reality, irrespective of our minds or our society, which medicine—among other sciences—is slowly unveiling. Proponents of the first view will be likely to emphasize (like Dr. Krieger) how medical opinion in the past suited societal and economic needs. They run the danger of total subjectivism, however, seeing all medical opinion as the expression of the whim of the moment. Hardliners of the second view emphasize the evolution of medical ideas in itself. They run the danger of forgetting outside influences on medicine. Dr. Krieger has rightly discerned our tendency to hold to the second viewpoint. By way

of defense, I like to quote a witty apology of this rather old-fashioned view from the preface of a recent excellent book on the evolution of ideas about the difference in functioning of the two hemispheres of the brain: "At the same time, I am rather wary of that class of currently fashionable studies in the history of science and medicine that puts all the stress on social relations, competition for resources, and vested class and professional interests—almost wholly neglecting the cognitive goals of the social activity being studied. Although indubitably important and often exciting, it seems to me that much of this sociologically oriented work both is missing out on one of the most fascinating parts of the history of science (the science itself) and with its radical methodological program, is putting the cart before the horse" (5).

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RE: "ENDOMETRIAL CANCER AND AGE AT LAST DELIVERY: EVIDENCE FOR AN ASSOCIATION"

We have read with interest the paper by Lesko et al. (1) reporting on an inverse association between age at last delivery and endometrial cancer risk seen in an American case-control study. A last delivery after the age of 40 years was associated with a 60 percent risk reduction compared with a last delivery before age 25 years. The authors state: "the only published data relevant to this relation are from two case-control studies

reported in the 1960s" and refer to Wynder et al. (2) and Stewart et al. (3). However, in 1988 we published results based on a Norwegian prospective study of 62,079 women among whom 420 cases of cancer of the uterine corpus were diagnosed during the 20 years of follow-up (4). In that paper, we described a strong protective effect of late age at last birth.

Table 1 shows results based on the total of 606