RE: "WHO MADE JOHN SNOW A HERO?"

The recent essay "Who Made John Snow a Hero?" by Vanderbroucke 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 their discipline. According to this account, good ideas ultimately triumph over bad, brilliant but neglected scientists of one era are recognized by later generation as being "ahead of their time," and science advances ever closer to the "truth" chiefly through improvements in reasoning and technique (3-4). As amply demonstrated by the burgeoning social history of science, however, this narrow view represents a seductive but inadequate stance (2-9). Instead, the insights 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 social'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 Vanderbroucke et al. of any discussion of the political history of the miasma/contagion debate (1) is both curious and troubling. As Ackert-Hinds (10) and others (5, 6, 11-13) have amply documented, we are at issue as much as simply "ject contagion" versus "bad air" spreading various vapors produced by "fluid." To the liberal inhabitants 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 involved an ascetic "ancient regime" mentality (10-13). Snow himself was acutely aware of the economic implications of his hypotheses (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, 13-13), along with the identification of "healthy carriers" (11), Snow's hopes were realized and the "germ theory" gained increasing acceptance. This complex history is a far cry from the assertion by Vanderbroucke et al. that "germ theory" triumphed primarily because "bacteriology emerged as a stronger science" (1, p. 969).

Secondly, in contrast to what Vanderbroucke et al. state (1, p. 970), Snow's conception of contagion disease was not the same as that of contagionists in the 16th century. As Henle attested in his now classic 1840 essay "On Mias- mata 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, and could be expelled by, sick people and, thus, could be transmitted not only by direct contact but also by air, water, soil, food, and a variety of filth (14-16). From the standpoint of epidemiology, 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 "mischaus" thinking that Vanderbroucke 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, 19), and the economic depression of the 1930s, which revived concerns about the effect of the "social environment" on health (5, 11, 19). The racist models of "host, agents, and environment," and now "sexual causation," have led to new insights regarding the causes of disease incidence (as opposed to simply the causes of sickness) in (16, 20, 21) and have recently been challenged by theories regarding the social produc- tion of disease (6, 8, 26-24). If epidemiologists are to "understand the development of our rea- soning" (1, p. 972) and the selection of our heroes (as Henle), it is clear we must pay attention to not only the biologic but also the social roots of epidemiologic thought.

REFERENCES
3. Fleck L. Genesis and development of a science, 450
often reprinted text “Snow on Cholera” (3), also disagrees with the month of birth. The month is recorded as March 15th: on the tombstones. My father, John Buckland, likely heard 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 paternal occupation (4), may also note that the original record reports that Snow’s father was a “labourer” rather than a “farrier” as recorded in the biography. The conclusion in months by Sir Richardson aptly reflects 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 church in York, England (All Saints, North Street) and indeed to make a contribution to its simple repair fund as, surely, this church is in great need of funds. The visitor might speculate how the red theme re-evoked in the beautiful stained glass might have influenced the young John Snow to pursue a health career. Additionally, an examination of the gravestones of this church provides a further clue to the family life of John Snow, which original researchers may wish to pursue. The finish results of John Snow is now housed at the Bethlem 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 whenever possible. The persistence of data recording errors in vital records continues to add to the challenge of occupational epidemiology (5).

REFERENCES


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I am extremely grateful to Dr. Dunn (1). His completion of the original "anonymous reviewer commences," which are so important to our paper (2), puts the record straight in several respects. The only reason why the writings of Masey were not cited was because I could not justify 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. Brunskii's graveuse 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 aridification 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 moods 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 local 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 itself. They are danger of forgetting outside influences on medi- cine. 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 at 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 itself and with its radical methodological program, is putting the cart before the horse." (5).

REFERENCES

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RE: "ENDOMETRIAL TRIAL 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 published report on 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