

tional status: findings from South India settlements. Chapter 2—Doctoral Thesis, Harvard University, 1984.

³ Hebert J R. Environmental Determinants of Nutritional Status of Children: Findings From Three Madras Communities. Doctoral Thesis, Harvard University, 1984.

⁴ Esray S A and Habicht J P. Nutritional anthropometric indicators for evaluating water and sanitation projects. Paper presented to: International Workshop on Measuring the Health Impact of Water and Sanitation programmes. Cox's Bazaar, Bangladesh, 21–25 November 1983.

From ABRAHAM M LILIENFELD and DAVID E LILIENFELD

Sir—In a recent article in the *International Journal of Epidemiology*,¹ Cameron and Jones considered some aspects of John Snow's contribution to epidemiology and mistakenly accused us of missing the point of John Snow's contribution to science: and 'in doing so they trivialize it as they trivialize epidemiology'. This criticism is based on the 1½ pages devoted to John Snow in a chapter of 20 pages on the history of epidemiology, in our book, 'Foundations of Epidemiology'.² First, we vehemently deny that *anything* stated in 'Foundations' trivializes epidemiology; nay, the entire book and the chapter on history actually glorifies our discipline. Indeed, any member of the epidemiological community familiar with the literature of epidemiology would be well aware that neither of us, in any paper, presentation, or in any correspondence, has ever trivialized either John Snow, his contribution to epidemiology, or epidemiology in general.

The fact, sir, is that as much as Wade Hampton Frost had, we recognized John Snow's logical organization of the observational facts that led to his theory of water transmission, as well as, his testing of the theory by observations using comparisons of mortality rates in what he recognized to be a natural experiment. We emphasized Snow's use of the quantitative approach in analysing cholera mortality because Snow's approach was rather different from what other epidemiologists did at that time. We are forced to assume that Cameron and Jones have a limited view of what work was actually being done by epidemiologists during this period of the 'Greening of Epidemiology'.³ The idea that water was involved in cholera epidemics was held by several individuals. William Budd, of typhoid fever fame, wrote a pamphlet in 1849, entitled 'Malignant Cholera. Its Mode of Propagation and Its Prevention'.⁴ This pamphlet begins with the paragraph 'In the Medical Gazette of last Friday, an announcement was made by Dr. Brittan of the very important discovery that peculiar microscopic objects exist constantly in the characteristic 'rice-water' discharges

of persons affected with malignant cholera and in the atmosphere of infected places.'⁵ Later, he states '1. That the cause of malignant cholera is a living organism of distinct species.' and '2. That this organism—in shapes hereafter to be described—is taken by the act of swallowing into the intestinal canal, and there becomes infinitely multiplied by the self propagation which is characteristic of living things, etc, etc.'⁶

In a footnote in the middle of this pamphlet, Budd states: 'Dr Snow, whose ingenious pamphlet fell into my hands while these materials were preparing for publication, has been led, by the consideration of particular instances of some of the facts above alluded to, to the same conclusion as to the part which water plays in the diffusion of the disease. Of being the first to develop and to publish this very important conclusion he must, therefore, have the whole merit. To no part of this merit do I lay the slightest claim. In Dr Snow's illustration of the entire subject of the propagation and prevention of cholera, there is, besides, much that is so apt, and in such accordance with the truth, that the detection of the actual cause of the disease, and the determination of its nature, were all that was waiting to convey his views into a real discovery.'⁷

In another note, he states 'The establishment of this doctrine for cholera opens a wide field of discovery in other diseases. It leads at once to the persuasion that dysentery is a disease of the same class, is propagated in the same way and may probably be prevented by similar means. It suggests also the suspicion that whooping cough and influenza are diseases of the same order, produced that is by the growth and propagation of microscopic beings at the expense of materials of the human body, but drawing these materials from the lining membrane of the air-tubes and transmitting their germs through the air.'⁸

We know that Budd and the Bristol group of microbiologists had seen a 'fungus' in the discharges and their theory was known as the 'cholera-fungus theory'. Their observations of the 'cholera-fungus' were not substantiated upon inquiry by a Committee of the Royal College of Physicians.

An outbreak of cholera resulting from polluted water had occurred in Manchester in 1849 and was reported

by Sutherland.⁹ In an earlier paper, we reported some of the details of this outbreak.¹⁰ In fact, Snow refers to it briefly.¹¹ Sutherland later stated, 'It is believed by some that the water that induces cholera contains the specific poison of cholera in it.' (We think he was referring to Snow, Budd, and others.) Further, he stated, 'The matter in dispute is really of no practical value, for if it be a fact that the use of impure water is injurious to the public health, the manner of its action is of very secondary importance at least for practical purposes.'¹² One need only clean up the water supplies. Snow, on the other hand, disagreed since he strongly thought it was necessary to have knowledge of the actual specific morbid agent that was conveyed in the water. This disagreement reminds one of the cigarette-smoking-lung cancer controversy in that there are those who feel it is necessary to have knowledge of the specific mechanism involved in carcinogenesis before taking preventive action.

Given that in 1849, in addition to Snow, there were others who implicated the water supply, some of whom clearly stated that a living microorganism was involved, what did John Snow do that was unusual? He conducted a field study, taking advantage of what we epidemiologists call a 'natural experiment' and analysed epidemiological data. Frost, who 'rediscovered' Snow's work and felt that it was a 'nearly perfect model', used the actual data comparing the mortality rates among the houses receiving their water from different water companies in his introductory

course on epidemiology. It was the utilization of such data, together with the incident at the Broad Street pump, that was the unique contribution made by John Snow. In addition, John Snow realized the relevance and importance of these data in testing his hypothesis; this was what made Snow's contribution the 'nearly perfect model'.

REFERENCES

- 1 Cameron D, Jones I G. John Snow, the Broad Street pump and modern epidemiology. *Int J Epidemiol* 1983; **12**: 393-6.
- 2 Lilienfeld A M, Lilienfeld D E. *Foundations of Epidemiology*. 2nd Ed. New York, Oxford University Press, 1980.
- 3 Lilienfeld D E. The greening of epidemiology: Sanitary physicians and the London Epidemiological Society (1830-1870). *Bull Hist Med* 1979; **52**: 503-28.
- 4 Budd W. *Malignant Cholera: Its Mode of Propagation, and its Prevention*. London, John Churchill, 1849.
- 5 *Ibid*, p 3.
- 6 *Ibid*, p 5.
- 7 *Ibid*, p 19.
- 8 *Ibid*, p 30.
- 9 British Parliamentary Papers: Health; Infectious Diseases; Cholera 1847-50, Vol. 1, Shannon, Ireland, Irish University Press Series, 1970, pp 394-6.
- 10 Lilienfeld D E, Lilienfeld A M. Epidemiology: A retrospective study. *Am J Epidemiol* 1977; **106**: 445-59.
- 11 Snow J. *On the Mode of Communication of Cholera*. 2nd Ed. London, John Churchill, 1855. Reprinted in: Snow on Cholera, New York, Hafner Pub Co., 1965, p 31.
- 12 Quoted in Snow J. Further remarks on the mode of communication of cholera; including some comments on the recent reports on cholera by the General Board of Health. *Medical Times and Gazette* (New Series), 1855; **11**: 84.

From DONALD CAMERON and IAN G JONES

Sir—The above is a curious letter. The first paragraph contains an unwarranted assumption about our knowledge of nineteenth century epidemiology. The remaining six have nothing to do with our paper except insofar as they illustrate afresh some of our complaints about 'modern epidemiology'. The Lilienfelds set out to establish what is not in dispute: that others besides Snow had ideas about water pollution as a cause of cholera. We did remark upon it but, it is not a point we would wish to emphasize. On the contrary, we say, and are supported by Snow's own view, that his theory is much more fundamental than this. It is that cholera is caused by living, reproducing micro-organisms which affect people in conditions which are most likely to be found in the circumstances of the poor. As Wade

Hampton Frost says '... it is important to remember that when Snow undertook his personal investigations in the epidemic of 1854 he already had in mind a definite and well matured theory which he was eager to put to the rigid test which the intermingling of two water supplies made possible'.¹

The Lilienfelds' last paragraph shows that they still do not understand Snow's contribution, as we see it and as he saw it himself. As we said in our paper, the spread of cholera by water containing the dejecta of cholera patients, was not Snow's theory. It was a deduction from his theory. The deduction and the theory itself received additional support from his statistical enquiries on the mortality of those taking the different water supplies. It was Snow who pointed out that cholera is kept alive in communities because of its spread within domestic situations. He mentions '... the powerful influence which the drinking of water

containing the sewage of a town exerts over the spread of cholera, *when that disease is present . . .*'² (our emphasis). But of course if it were not for the spread by food and other domestic means cholera would never have gained access to such countries as the UK or the US, there would be no infection of water supplies, there would be no explosive epidemics. Remember, Snow says, 'The division of my views on cholera which refers to its communication through the medium of drinking water, has apparently obtained a greater amount of attention from the profession than my views respecting its more immediate communication by the cholera poison being swallowed without water. While I speak on this division of the subject, however, I must beg the society to bear in mind also the other part of my views, first alluded to, for I am well aware that the part which relates to polluted water will not of itself explain the disease as an epidemic.'³ There is no hint of this last point in the Lilienfelds' book or in any of the other works we quoted.

Lastly we wish to comment on our remark about the trivializing of epidemiology. This was not directed particularly at the Lilienfelds' book but at a large part of modern epidemiology. Our criticism is not based on reading 1½ pages devoted to John Snow or even 20 pages of history. It arose out of a study of a large number of text books and articles written by modern epidemiologists, some of which we quote. We excepted many books on the epidemiology of the communicable diseases not because we think that there are two epidemiologies. We do not. These books are much less often empiricist in outlook. They are based on consistent biological and social theories.

What we most object to in modern epidemiology is

its concentration on phenomena, its unwillingness to look at what is behind appearances, its obsession with technique and its lack of commitment to any consistent theory. This last is illustrated well by the Lilienfelds' preference for Sutherland's views rather than those of Snow. Of course Snow was right. His theory gave him access to a whole range of preventive measures of which the contemporary empiricist would be ignorant. These are set out at the end of his 'On the Mode of Communication of Cholera'.⁴ The Lilienfeld comparison with cigarette-smoking-lung cancer is a red herring. Snow did take action. He addressed himself to the vestrymen who removed the pump handle or had it removed. Epidemiologists who know that it is necessary to understand the biological, sociological, economic and political mechanisms and processes involved in the production of lung cancer, in order to prevent this condition which is almost certainly increasing rapidly throughout the world, would not hesitate to embark on whatever preventive action can be taken with the very limited knowledge that is officially conceded now.

REFERENCES

- ¹ Frost W H (ed). Snow on Cholera. New York, The Commonwealth Fund, 1936; XV.
- ² Snow J. On the mode of Communication of Cholera. London. Churchill, 1855, p 74.
- ³ Snow J. Further remarks on the mode of Communication of Cholera: including some comments on the recent reports on cholera by the general board of health. *Medical Times and Gazette* 1855: 2: 31-35, 84-88.
- ⁴ Snow J. On the mode of Communication of Cholera. London. Churchill, 1855, pp 133-6.