A recent article in the International Journal of Epidemiology, 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'. First, we vehemently deny that anything stands in 'foundations' trivializing epidemiology; that the entire book and the chapter 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 W. H. Armstrong 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'. 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'. 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' discharge of persons affected with malignant cholera and in the atmosphere of infected places. [1] Later, he states 'That the cause of malignant cholera is a living organism of distinct species.' [2] That this organism—incapable of being halved—has never been described—is taken by the act of swallowing into the intestinal canal, and there become infinitely multiplied by the self-propagation which is characteristic of living things, etc., etc.' [3]

In a footnote in the middle of this pamphlet, Budd states: 'Dr Snow, whose ingenuous pamphlet fell into my hands while their 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 wanting to convey his views into a real discovery.' [4]

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 that these materials from the lining membrane of the air-tubes and transmitting these germs through the air.' [5]

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

Department of Epidemiology, School of Hygiene and Public Health, The Johns Hopkins University, 615 North Wolfe St, Baltimore, Maryland 21205, USA.


From ABRAHAM M LILIENTH and DAVID E LILIENTH

From DONALD CAMERON and IAN G JONES

Sec—the above is a curious leter. The first paragraphe contains an unwarranted assumption about our knowl-edge of nineteen-century epidemiology. The remain-ing ten have nothing to do with our paper except insofar as they illustrate afresh some of our complaints about "modern epidemiology". The Lilliefords 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 not mean that, but it is not an 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 microorganisms which affect people in conditions which are most likely to be found in the circumstances of the past. As Wide

Department of Cardiology, Macclesfield, Chester District, Worsley Park, Macclesfield SK10 2UW, 3/1.

Hampden 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-marked theory which he was eager to put to the rigt and with which the intermingling of two water supplies made possible.1 The Lilliefords' last paragraph shows that they still do not understand Snow's contribution, as we see it and as he saw it himself. As we did in our paper, the spread of cholera by water containing the deposits 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 cholera at the different water supplies. It was Snow who pointed out that cholera is kept alive in communities because of its spread within a small area. He mentions... The powerful influence which the drinking of water...
containing the sewage of a town empties over the spread of cholera, when that disease is present . . . ? (our emphasis). But of course if it were not for the spread by food and other fecal 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 general amount of attention from the profession than my views respecting its more immediate communication by the cholera parasite 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 refers to polluted water will not of itself explain the disease as an epidemic. There is no hint of this last point in the Lilliefield's 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 Lilliefield's 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 book, and articles written by modern epidemiologists, scorn of which we quote. We excerpt 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 epidemiologist. They are based on consistent biological and social theory.

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 Lilliefield's 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 book 'On the Mode of Communication of Cholera.' The Lilliefield comparison with cigarette-smoking lung cancer is a red herring. Snow did take action. He addressed himself to the workmen 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 causation 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
3. Snow J. Further Returned to the mode of Communication of Cholera: including some comments on the recent reports on cholera by the medical board of health. Medical Times and Gazette 1855. 51. 153-155. 84-89.