

Letters to the Editor

Cervical cancer and early sexual intercourse

From Mr. Richard Peto:

SIR—Dr. Buck (1) considers various explanations for the relevance of early exposure to sexual intercourse to subsequent incidence rates of cervical cancer. All these explanations invoke some putative special susceptibility of the adolescent cervix. However, early exposure would be expected to be very relevant even if no such special susceptibility existed.

Suppose an organ (the cervix, in this case) is exposed for a certain amount of time to a repeated carcinogenic insult. After a number of years of such treatment have passed, there is an appreciable probability that a pathological cancer will appear in that organ in the coming year. The point to note is that this probability (the 'incidence rate' of pathological cancer) increases very sharply with duration of exposure to the insult; for example, with lung cancer, where the start of the insult can be timed, incidence rates increase roughly as the fourth power of duration of smoking, but *given* this duration the age of the subject doesn't matter. This strong relationship with duration of insult can, however, give the mistaken impression that some special susceptibility of the young must be invoked. Again, consider lung cancer incidence rates after various durations of smoking. Among men who smoke one pack of cigarettes a day from the age of 20 for the rest of their lives, the risk of lung cancer in their 40th year of smoking will be about three times as big as the risk in their 30th year of smoking (since $40^4/30^4 \approx 3$). This isn't because ageing itself matters, it's just because the duration of past smoking is longer in the older men. An exactly equivalent observation would be to note that in a group of regular smokers of one pack per day who are all aged 55, those who started to smoke at 15 had an incidence rate of lung cancer three times greater than that of the men who started smoking at 25. Smoking between the ages of 15 and 25 thus triples the risk at 55, and this might misleadingly suggest that young adult lungs are especially vulnerable, while in fact the exact opposite is true—the effects of cigarette smoke on the lung tissue of regular smokers depend only on duration of exposure and, given this, are wholly independent of the age at which the exposure occurs.

This is found in both experimental animals and humans (2) and so one would expect, for example, to find (if the adolescent cervix is no more vulnerable than the adult cervix) that women of 40 who started intercourse at 15 have more than twice the incidence rate of cervical among otherwise similar women who only started intercourse at 20.

The age distribution of cervical cancer is, in middle and old age, complicated by the effects of age-related changes in sexual habits as well as, perhaps, by some genuine age-related changes at the menopause. Nevertheless, the *necessary* strong relationship between the age when intercourse begins and subsequent risk should be recognized in any interpretation of the *observed* relationship between these factors, even if, as in Professor Buck's article, this interpretation is merely a teaching example.

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Popper's Philosophy for Epidemiologists

From Dr. Carol Buck:

SIR—I am indebted to Michael Davies (1), Alwyn Smith (2), Michael Jacobson (3), Richard Peto (4) and Andrew Creese (5) for taking the trouble to comment on my paper *Popper's Philosophy for Epidemiologists* (6). Being mindful of space limitations, I shall not attempt to reply to all the points that they have made.

Andrew Creese's letter is concerned with *what* scientists investigate, whereas my paper dealt with *how* scientists conduct their investigations. I think he is wrong in suggesting that my article invites the assumption that science advances in a historical and political vacuum. I do not believe this nor did I intend to imply it. I agree with him that scientists have an obligation to direct some of their effort towards society's current problems. But, as Popper has emphasized, a problem cannot be attacked scientifically unless it lends itself to the

formulation of testable hypotheses. For this reason some of society's concerns lie at present outside the domain of science, a fact which we may regret but for which we cannot, in my view, blame Popper.

Michael Davies and Michael Jacobsen both chide me for downgrading induction, although in different ways.

Dr. Davies insists on the old prescription: 'formation of hypotheses by observation and inductive reasoning, testing by deduction and experiment'. His chief example is Baker's serendipitous discovery of the relationship between adulterated wine and colic in Wurtemberg. He appears to believe that the occurrence of serendipity destroys Popper's premise that all observations are made to test some hypothesis already in mind. Not everyone believes this, and certainly not Pasteur who said, 'In the fields of observation, chance favours only the mind that is prepared'.

Mr. Jacobson's criticism attacks my implied dismissal of statistics as 'a set of numerical devices'. I am sorry if this is what I implied, because my respect for the contribution of statistics to epidemiology is deep and of long duration. I agree fully with Jacobson that statistical inference is an inductive process indispensable to the interpretation of data and hence to the testing of hypotheses. Moreover, I do not interpret Popper's views as being in conflict with the principles of statistical inference. Popper's point is that induction plays little part in the *generation* of hypotheses.

Mr. Jacobson's paper offers an argument against my expectation of a positive association between habitual abortion and cervical cancer if both are related to a deficiency of progesterone. He demonstrates that unless both of the associations with progesterone are strong, the correlation between abortion and cervical cancer can be zero, or even negative. I am grateful for being appraised of this restriction upon the behaviour of correlations among three random variables.

Richard Peto suggests that a special susceptibility of the adolescent cervix need not be invoked to explain the high relative risk associated with coitus under the age of 17 and that duration of exposure would suffice. It seemed to me that his model would not account for the fact that when age at first coitus is dichotomized at under 20 years versus 20 years and over, the relative risk is almost the same as when the dichotomy is made at 17 years. (I have made these comparisons using data from six studies summarized by Rotkin (7).) My colleague in the Department of Mathematics, Dr. Jon Baskerville, has examined this question. He finds

that the lung cancer model, with incidence related to the fourth power of duration of smoking, gives a steep rise in risk ratios as the point of dichotomy is advanced along the age span. However when he postulated a different model, in which incidence increases in simple proportion to the length of exposures, the risk ratios increase so slowly as the point of dichotomy advances that the relative risk for under 20 years versus 20 years and over is only marginally greater than that for under 17 years versus 17 years and over, as in the calculations that I made from Rotkin's data. However, one can achieve the same result from Peto's model by adding to it a special susceptibility under the age of 17. It is unlikely, therefore, that the issue can be settled by an appeal to models. The hypotheses of special susceptibility in adolescence would be refuted if we found that women who started sexual activity at 15 and stopped at 25 had no greater risk of cervical carcinoma than women who started at 20 and stopped at 30. Perhaps we should start looking for some convents that do not require virginity as a condition of entry. This need not be regarded as a completely facetious suggestion if one recalls the epidemiological significance of Gagnon's observation in 1950 on the rarity of cervical cancer among nuns (8).

Since my paper appeared, a friend has told me that I may be wrong in regarding as bizarre the assumption that orthodoxy is equally frequent among New York and Israeli Jews. According to his sources of information, the proportion of observant Jews is thought to be about 30 per cent in both areas.

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What is an Epidemiologist?

From Mr. Derek Mowbray:

SIR—Your editorial, which appeared in the December 1975 issue of the *Journal* (1), begs for some form of response. You pose two questions. First, why should doctors continue to dominate the speciality of epidemiology? Second, what is the role of the epidemiologist?

You conclude that the skills required to study epidemiology are not always similar to those required to be qualified as a medical practitioner. It would thus seem clear that doctors will continue to dominate the speciality as long as epidemiological posts are established for them within a medical framework. A change in this situation will presumably only come about when posts are filled by those exhibiting the skills to study epidemiology—regardless of their primary discipline.

Your second question raises issues of definition. To be categorical in denying the epidemiologist the joy of seeing his (research) work implemented by himself implies that those who do implement the findings of epidemiological study are not, themselves, epidemiologists.

You state that the epidemiologist, as a planner, is not trained for management. Any textbook on management reveals that a function of management is planning or directing, or establishing objectives. Thus as a planner he is undertaking a function of management, and as you point out, management is concerned with implementation. Your statement that the epidemiologist should 'never be the one who implements'—because he is a planner—would appear not to be the conclusion which could be logically drawn from the language which you use.

What I believe fails to be drawn from your editorial is the separation of service perspective from an academic research perspective. If this had been drawn out, your second question would take on a different and more complex meaning. Implementation is concerned with organizational

change, and much has been written about the role of research and development units in organizations, and about alternative organization change strategies which need to be adopted in any given circumstance. The need for flexibility in the relationship between research and the organization is a message which is very strong, and the message of your editorial is clearly one of inflexibility.

Another issue which confuses me is that you state that the health administrator has the responsibility for implementing research findings, and yet earlier you state 'to implement is to manage'. Health service administration is concerned, in this country, with the carrying out of tasks within parameters which are clearly defined. These parameters are either written or are established by a means of negotiation; in any event the discretion of health service administrators is limited.

Management, on the other hand, has much wider discretionary functions, and is, consequently, not a function which is left solely to the administrator. In this country management is achieved through a close relationship between disciplines who collectively are responsible for the management functions of co-ordination, planning and control. Thus, implementation is the responsibility of more than just the health administrator. It could be part of the responsibility of an epidemiologist who is also concerned with the management of health services.

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