

Commentaries

Wider “causal thinking in the health sciences”

Mervyn Susser offers us in this issue¹ another² timely warning about the future of epidemiology. An expression of the same concerns was echoed by the British Medical Research Council recently in its consultation about the future strategy for funding epidemiology. Put at its simplest these concerns arise firstly from the past successes of the methodology of epidemiology in determining vital causal relations from a quagmire of artefact and delusion; ever present in observing human health and health improvement. Secondly, that such approaches seem to have gone about as far as they can; with some important exceptions.

Despite its sophistication and obsession with sheer methodological rigour, epidemiology always appears a blunt tool, relative to laboratory medicine for example, but none the less has had an enormous amount to offer. The unique importance of epidemiology is, as Susser says, elucidating the problems of causal relations in health from actual observation of important and complex processes where the precise mechanisms are often poorly understood; from exposure to disease and from intervention to improvement. That is its obsession. And “risk factor” epidemiology remains an essential insight into the full nature and potential of improving human health, but it is clearly not sufficient.

The pressure is on and the question for epidemiology is how can it deal with the reality that “productivity declines; and young minds go elsewhere”. What in fact is to become of epidemiology? I hope we will see the important development of ecoepidemiology, as Susser suggests, because that is one vital part of the epidemiological paradigm—but there are others too. The serious study of health in populations in all its manifestations certainly demands “technical capabilities and analytical tools beyond the present day resources of epidemiology”. What is indeed required is a systematic development of the appropriate theoretical basis for public health—which is epidemiology—embracing the entire causative chain for better health.

It is worth looking at the professional development of epidemiology as well as the scientific and medical history. Jerry Morris asserted epidemiology as the core discipline in public health,³ as indeed it is. To understand and be a specialist in public health requires knowledge about the effective and efficient prevention of avoidable ill health, in all its manifestations. This certainly requires knowledge about risk factor epidemiology, but increasingly it requires knowledge about evidence in health on the determinants of risk, and health enhancing factors, be they social, biological, ecological, economic or political.

Yet to be a specialist in public health a crafty confidence trick has been perpetuated on the health of this nation, and others too, apparently in the name of standards and of effectiveness. Institutional public health in the UK today is a closed shop and it has marginalised epidemiology as core, without adequate reference to the really appropriate skills and competencies.⁴ Assuming (without evidence) that clinical medicine should be dominant, public health is now essentially (and inevitably) health service management.⁵

Simultaneously the strangest state of affairs has existed during my professional life in which students of postgraduate courses in epidemiology had to be medically qualified and yet taught by specialists of other disciplines. But, for

the above reasons, the training of doctors in specialist epidemiology was simultaneously disparaged by public health, while management was given a greater importance. All other core intellectual disciplines have had to accept a purely supportive role to public health.

That is a sure way to guarantee the premature demise of a vital intellectual enterprise; subordinate it to the parochial career aspirations of a few and ignore the development of the true theoretical underpinning. The essential paradigm has suffered for too long under these ridiculous constraints.

Geoffrey Rose drew our attention a while ago⁶ to the “prevention paradox”—which roughly stated says that preventive interventions only benefit a minority of the “beneficiaries” directly and those who benefit are not necessarily individually identifiable. But this is not a paradox at all, but is clearly an anathema to the clinical method, directed at specific individuals benefiting from clinical interventions. All of public health is like that, and thus all of public health simply cannot be embraced by the individual risk factor model and its commensurate clinical emphasis.

Rose gave an enormous amount to public health but what he also gave was a high class MSc course in epidemiology open, until very recently, only to medically trained people. And that way the false notion of a paradox and the simplified health belief model remained dominant and constrained the theoretical basis for public health. We became very good at analytical studies identifying, without bias, the risk factors for this and that—all essential stuff—but the policy implications gradually became more and more obscure, under that model. That was because “behaviour” was not accorded the importance it deserves in the underlying analysis of appropriate public health strategy.

Without an adequate theory exclusive professions still need quick “solutions”, hence embracing management takes on a particular attraction, in this case, of health services especially. Thus other core disciplines, had perforce to remain marginal and merely supportive and many “young minds” did (and continue to) go elsewhere because the professional barriers were simply too high. Once one discipline becomes dominant it will certainly fail to perceive the need for change. But the time, Susser tells us it seems, has now at last finally arrived.

The focus on risk factors ignores the true complexity of the behavioural effects and disparages important health promoting factors. The concentration on individuals overlooks social and structural group specific influences—which are actually often dominant. People never do important things ignoring their own overwhelming context. The dominant theoretical developments in epidemiology have effectively ignored the true dialectic that exists between people’s actual chances and their real possibility of making choices. This is palpably not an entirely individual business because both the realities and the possibilities are determined by status and context, themselves in turn variously real and perceived. Style, as Weber tells us,⁷ is indeed important.

So the true paradox in epidemiology is quite profound. It is high time that public health stopped behaving as if one

single dominant paradigm was good enough. It is not, and the theoretical basis for public health is overdue for a constructive reformulation and enhancement of epidemiology. Let us make absolutely sure that the intellectual basis is never again constrained by professional straight jackets to sort out any single group's special career aspirations—it is far too important for that. Susser is offering us one clear opportunity, among several, to improve the health of our communities, taking in the true nature of those communities. These must imply both multi-disciplinarity, but also greater methodological pluralism, where the synergistic opportunities for intellectual development are, in principle, immense. Contemporary public health must nurture and exploit them.⁸

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- 1 Susser M. Does risk factor epidemiology put epidemiology at risk? Peering into the future. *J Epidemiol Community Health* 1998;52:608–11.
- 2 Susser M. *Causal thinking in the health sciences: concepts and strategies in epidemiology*. New York: Oxford, 1973.
- 3 Morris JN. *Uses of epidemiology*. 3rd ed. London: Churchill Livingstone, 1975.
- 4 McPherson K, Fox J. Public health: an organised multi-disciplinary effort. In: Scally G, ed. *Progress in public health*. London: Royal Society of Medicine, 1997: 269–91.
- 5 Powles J, Day NE, McPherson K, et al. National Centre for public health is needed. [Letter]. *BMJ* 1997;315:54.
- 6 Rose G. *The strategy of preventive medicine*. Oxford: OUP, 1992.
- 7 Weber M. *Essays in sociology*. Gerth HH, Wright Mills C. London: Routledge and Keegan Paul, 1958.
- 8 Chief Medical Officer's Project to Strengthen the Public Health Function in England. A report of emerging findings. London: Department of Health, 1998.

Our conscientious objection to the epidemiology wars

Battle lines have been drawn for a war among competing visions of epidemiology. What are these visions and why are they in conflict? Consider the highly simplistic schema, one general, the other illustrative shown in figure 1.

Social epidemiology focuses on societal determinants of disease, risk factor epidemiology on behaviours and exposures, and molecular epidemiology on biological mechanisms. Susser¹ calls these levels the macro, the individual, and the micro.

Adherents claim supremacy for their favourite level over the others. The most bitter dispute is between the two farthest extremes: the macro and micro levels. Some advocates of macroepidemiology, following Tesh's claim that the societal level is "fundamental" and the others "superficial,"² join her in questioning the very concept of multicausality.³ Krieger and Zierler⁴ argue strenuously on behalf of "social production of disease theories." Shy's "witness for the prosecution"⁵ redefines epidemiology to exclude from its purview all determinants of disease other than the macro: "a study of the distribution and societal determinants of the health status of populations." Vandenbroucke,⁶ an advocate of the micro level, sees the other levels as doomed reincarnations of miasma theory. In rebuttal, proponents of the macro level criticise the shortcomings of molecular epidemiology.^{7, 8}

There is one point on which the micro and macro camps agree, however, and that is their mutual disdain for risk factor epidemiology. Between society and biology, the middle ground of behaviours and exposures is caught in the crossfire. Macroepidemiologists accuse risk factor epidemiology of victim blaming; biomedical reductionism; and failure to recognise the social, political, and economic context in which health related behaviours and exposures occur.^{3-5, 9, 10} For the micro camp, the study of behaviours and exposures in relation to disease is "black box" because

it pays insufficient attention to pathogenic mechanisms.^{6, 11} But in comparison with risk factor epidemiology, macroepidemiology and its study of social, political, economic, and cultural determinants of disease has an even larger and more complex set of intermediate variables with which to contend. If the micro camp is critical of risk factor epidemiology for being black box, and the macro camp is critical of risk factor epidemiology for being individualistic, imagine what they must think of each other!

These debates are interesting, and may even be important. It is tempting to join the fray, but we wish to avoid the trap of mounting a reactionary defence of the extant methods of risk factor epidemiology or the results it has produced thus far. Although we believe that risk factor epidemiology has much of which to be proud,^{12, 13} we believe even more strongly that it is still in its infancy as a field of scientific inquiry, still rapidly evolving in a positive direction. Of greater salience to the larger debate, we do not believe that risk factor epidemiology is the only epidemiology. We support arguments in favour of macroepidemiology and microepidemiology as well. If conscripted to take sides in a war among these three epidemiologies, we would protest as conscientious objectors.

Whom would we ask to argue our case before the draft board? We would choose Mervyn Susser. He has the scope of vision to see beyond internecine squabbles and to define an epidemiology for all of us. Although he chooses to single out risk factor epidemiology for criticism in the title of his essay,¹ his avowal of second thoughts in its first sentence suggests an implication we would like to make explicit: Susser's vision of a multilevel epidemiology, with its apt metaphor of Chinese boxes, is anathema to anyone who would promote any one level—macro, individual, or micro—as the only level, as the most important level, or as the fundamental level, at which epidemiology should function. Like Terris¹⁴ and Stallones,¹⁵ who lauded MacMahon's "web of causation"¹⁶ as an antidote to narrow reductionist thinking, we now praise Susser's Chinese boxes for their timely reminder that societal, lifestyle, and molecular explanations of disease are interconnected and mutually reinforcing, not stark alternatives locked in mortal combat against each other.⁴ Only the name that Susser gives to his conception, "ecoepidemiology," suggests that he is part of the macroepidemiology camp; some members of that camp use a similar sounding term, "ecosocial epidemiologic theory," to refer to their proposed programme, which "embraces population-level thinking and

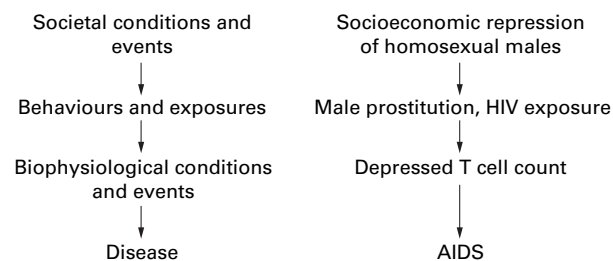


Figure 1 Oversimplified schema of levels of epidemiologic study.