

In this number

Evaluation is a theme running – in different ways – through the editorials and several articles this month. One editorial board member from The Netherlands discusses the meaning and uses of the “epidemiologic transition theory”. The conclusion seems to be that as a theory it still has some way to go to demonstrate its utility. Another editorial board member from Spain evaluates the experience of his own country in coping with an AIDS epidemic. There are lessons for all European countries here.

Within the field of obstetrics two articles undertake more traditional evaluation: the first of a regionalised multi-tier service in The Netherlands, and the second an overview of the provision of caesarean section in the UK. Not all obstetricians will find themselves willing to accept the

conclusions of either paper, and it is probably true that Cochrane-initiative-acceptable, randomised controlled trials are still needed to make a formal evaluation of these areas of service.

Another five papers in this number relate to various aspects of cardiovascular disease. Three of the papers investigate serum lipids and the other two consider risk factors where intervention is more problematic – gender, socioeconomic status, and geography. The conclusion of the paper from Norway about measuring cardiovascular disease risks is relevant to the evaluation of intervention studies, but for the other four, further work and thought are needed before their relevance to intervention and prevention is clear.

Editorials

The epidemiologic transition theory

The “epidemiologic transition theory” was first formulated in a paper published by Omran in 1971.¹ This theory provides a stylised description and explanation of the mortality component of the “demographic transition”, the spectacular decline firstly of death rates and then of birth rates which has been observed in all currently industrialised countries.² In the epidemiological transition theory, the historical development of mortality over time is characterised by three phases: the “age of pestilence and famine”, the “age of receding pandemics”; and the “age of degenerative and man-made diseases”. It is the transition from a cause of death pattern dominated by infectious diseases with very high mortality, especially at younger ages, to a pattern dominated by chronic diseases and injuries with lower mortality, mostly peaking at older ages, that is seen to be responsible for the tremendous increase in life expectancy.^{1, 3-6} In countries in western Europe and northern America the shift started early and took approximately 100 years. This was called the “western” or “classical model” of the epidemiologic transition. In a number of other countries, notably Japan and eastern Europe, the transition started later but proceeded much more quickly (the so called “accelerated model”). Finally, in many third world countries the transition started even later and, unlike that in currently industrialised countries, has not yet been completed (the “delayed” or “contemporary model”). Omran attributed the decline of mortality to a complex of factors closely linked to “modernisation”. For the western model, socioeconomic progress, leading to a rise in living standards, was presumed to be a very important contributing factor, whereas for the accelerated and delayed models, public health and medical technologies were considered relatively more important.^{1, 3-6}

The concept of an epidemiologic transition (sometimes also referred to as “mortality transition”, or “health transition”) has become popular among demographers and geographers.⁷⁻¹⁰ While it is also well known to many public health professionals,¹¹ it is surprisingly less familiar to

epidemiologists – as is shown by its absence from most epidemiology textbooks and from the International Epidemiological Association’s *Dictionary of Epidemiology*.¹² Perhaps for this reason it has never been subject to the rigorous scrutiny it deserves. In this comment, which will be limited to the “western model” of the epidemiologic transition, I will argue that the concept is ill defined, and cannot therefore be put into operation without ambiguity. The problems come to light rather acutely when one wants to locate in time the beginning and end of the epidemiologic transition.

Should the beginning and end of the epidemiologic transition be based on trends in all cause mortality?

In Omran’s publications, the epidemiologic transition was never clearly defined. His words were the following: “Typically, mortality patterns distinguish three major successive “stages” of the epidemiologic transition: (1) The Age of Pestilence and Famine, when mortality is high and fluctuating (2) The Age of Receding Pandemics, when mortality declines progressively and the rate of decline accelerates as epidemic peaks become less frequent or disappear (3) The Age of Degenerative and Man-Made Diseases, when mortality continues to decline and eventually approaches stability at a relatively low level ...”.¹ If we take this literally, the beginning of the epidemiologic transition lies back in prehistory, because mortality has always been “high and fluctuating”, but it is unclear what changes are supposed to have occurred during the first stage. It seems more appropriate to locate the beginning of the epidemiologic transition *between* the first and second stages, and that of course is what most researchers have done. According to Omran, in most industrialised countries the second stage began “about the middle of the 19th century”.⁴

It is obvious from the citation given above that Omran tended to identify the beginning and end of the transition on the basis of trends in all cause mortality. Given that the whole notion derived from the concept of a demographic