In my paper [1] in the June issue of the Journal I pointed out some problems inherent in the New Zealand absolute cardiovascular risk strategy for deciding who should receive antihypertensive drug therapy for mild-to-moderate hypertension. This paper was the subject of comment by MacMahon [2], Zanchetti and Mancia [3] and Swales [4] and I should like to respond to some of these comments. Incidentally, another paper critical of the New Zealand guidelines has now been published by another group in New Zealand [5].

The most serious of the problems with the absolute risk strategy is the tremendous increase in cardiovascular risk as age increases. Application of the absolute cardiovascular risk strategy leads logically to a concentration of therapy in the elderly and the very old, and to a relative neglect of hypertension in the middle-aged. I suggested two manoeuvres that could mitigate this. First, the proposed threshold of risk (10%) should be calculated on a time frame that is inversely proportional to age, for example 10% in 20 years for a person aged 40 years, 10% in 10 years for a person aged 60 years and 10% in 3 years for a person aged 80 years. MacMahon [2] appears to agree with the general idea of this but believes that the calculations should be based on estimates of disease-free survival and on estimates of the likely effects of antihypertensive therapy on this. Second, I suggested that consideration should be given to basing the calculations not on total absolute cardiovascular risk but on ‘marginal absolute hypertensive risk’, namely the difference in absolute risk between a hypertensive and a normotensive person of the same age and having the same other risk factors. The intention here was to avoid the overwhelming effect of the large amount of cardiovascular disease that occurs even in normotensive elderly people and to allow for the decline in relative hypertensive risk in old age. MacMahon [2] states that the estimated absolute reduction in cardiovascular events (used in the New Zealand guidelines) is a proportion of the marginal hypertensive risk, the implication being that use of marginal hypertensive risk would not confer any benefit. On the subject of lessened relative hypertensive risk in the very old, MacMahon pointed out that the data on this are becoming somewhat problematic and I accept this. Nevertheless, I believe that the concept of marginal hypertensive risk is worth investigating and I hope that someone will take it up so that we can see what the data look like.

Swales [4] is of course right that I am combining (not confusing!) the judgemental and the scientific. However, guidelines are inevitably based on a mixture of things: scientific knowledge, value judgements and (unfortunately) economic constraints. He feels that emphasis on antihypertensive treatment in the elderly and the very old is justified and that there is no need to modify this in any way other than by exercising normal clinical caution. He does seem, however, to be slightly worried about the proposed reduction in antihypertensive therapy in middle-aged people and to be in agreement with the need for some modification in this area. He, like MacMahon, would prefer to base any modifications of the guidelines on life expectancy rather than on adjustments to the time frame. It may well be that estimates of life expectancy or disease-free survival would give a useful basis for guidelines and I look forward to seeing this line of thinking developed. I have, however, three areas of doubt: the results may well be rather complicated for everyday use; changes in life expectancy are necessarily always averages and they tend to be small; and whether ‘life expectancy rather than an increase in morbid events is a concept that is more easily grasped by the patient’ [4]. Patients do not, I suspect, think in terms of life expectancy except in very broad outline. A 50-year-old person is not worried about whether he or she will live to be 80 years and 3 months or 80 years and 9 months old. The thinking of such a person is much more likely to be about whether there will be an early heart attack or, even more dreaded, a stroke.
Swales strongly opposes ‘introducing empirical mathematical adjustments [which] are simply cosmetic’ and he describes proposals such as mine as fundamentally misguided. The trouble is that we are faced here and now with guidelines based on the absolute risk strategy which he [6] and others have praised but which alter dramatically the influence of age on the decision to treat. He himself mentions that most national and international guidelines ‘perform an unacknowledged adjustment whereby elderly subjects at the same absolute risk are less likely to be treated than younger patients’. It is a little difficult to see why it is such a bad thing to apply an openly acknowledged empirical (but, I think, reasonably logical) mathematical adjustment to the absolute risk strategy. One could of course opt out by accepting the liberal aspects of the guidelines (in the elderly) and ignoring the restrictive aspects (in the middle-aged). However, this cannot be said to be an ideal solution and it ignores the very real possibility that there will at some stage be some form of cap on overall expenditure.

Swales mentions ‘the inadequacy of risk-factor profiling as a sole basis for decisions concerning an individual patient’; this seems to represent something of a change from his previous approving comments [6] and I welcome this. He indicates, in fact, that the New Zealand guidelines go considerably beyond that which he terms ‘the primary purpose of guidelines, which is to provide a guide to delineate minimally acceptable levels of care’. I am not sure that everyone would agree with this definition of guidelines but I also am wary of the tendency for guidelines to become more prescriptive. In any case, my own view is that the New Zealand guidelines have useful aspects but that some modifications are necessary. These guidelines cannot (at least in New Zealand) be wished away and it is clearly likely that other national and international guidelines will embody some, at least, of the same principles.

Swales [4] quotes me unfairly, I think, when he states that I express ‘doubts concerning the legitimacy of using trial results to calculate the reversal of risk produced by treatment’. The use of trial results for such a purpose is of course perfectly legitimate; the point I was making was that the use is often uncritical and inappropriate. MacMahon [2] and Zanchetti and Mancia [3] agree that the trial results underestimate the benefits of antihypertensive treatment.

Whither now? More precise definition of risk may help, as Swales says, to render the present uncertainty a thing of the past, though I am inclined to think that it may take some time to reach this desirable state. Acquisition of the necessary data will certainly be quite a challenge. The clinical handling of the various components of total risk will be an even greater challenge and will continue to give rise to difficult but fascinating scientific, economic and ethical questions.

References