knowledge of most cancers, that biomarker may be only tenuously linked to the disease, so that the overall relevance of the intervention becomes difficult to assess. Nevertheless, interventions like that of fat reduction and breast cancer risk being conducted as part of the Women's Health Initiative in the U.S.A., will probably be vital in weeding out the non-causal associations thrown up by observational studies and in establishing the relevance of particular pathways to the determination of an individual's overall risk of disease [5].

It would be unfortunate if the reader of this review were to come away with no impression of the other avenues of research which may eventually prove vital. A host of exciting possibilities are being provided by different approaches. At a conference on Food and Cancer, sponsored by the Food Chemistry Group of the Royal Society of Chemistry held in Norwich, U.K. in September 1992 [6], a wealth of papers on potential mechanisms by which both nutritive and non-nutritive components of the diet may alter cancer risk was presented. A considerable number of studies were concerned with the problem of the oxidation and conjugation of non-nutritive dietary factors, and the inhibition, induction and activation of the enzymes/isozymes responsible for the biotransformation of compounds foreign to the body. It appears that relatively small amounts of these xenobiotics can have dramatic influences on the existence and availability of certain metabolic pathways. Otherwise harmless endogenous compounds (e.g. steroid hormones) and their metabolites may be raised to the status of procarcinogens in the presence of pathways induced by xenobiotics. If this is the case, are we expecting too much of the broad brush techniques of diet cancer epidemiology?

The article by Miller and his colleagues might be criticised for failing to acknowledge the exciting contributions that other research approaches are providing — even if the global picture is still very indistinct. Epidemiologists and laboratory scientists need to be guided by each others' findings if answers to problems as complex as diet and cancer are to be discovered with least delay.

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IN 1981, DOLL AND Peto provided an estimate of the proportion of cancer deaths in the United States attributable to diet of 35%, with, however, a wide range of acceptable estimates, from 10 to 70% [1].

The substantial amount of epidemiological research published over the last 12 years seems to have confirmed, at least in first approximation, the point estimate given in 1981, and somewhat restricted its range of acceptable estimates. There is, however, still scope for discussion on how wide a range can now reasonably be accepted.

Miller and colleagues (pp. 207–220), at the end of their review, provide a series of apparently precise estimates of population attributable risks and hence potential incidence reduction. These, for instance, would be of 68% for stomach cancer through reduction of nitrite, cured meats and salt-preserved foods and increase of fruit and vegetable consumption, or of 27% for breast cancer through reduction of fat and increase of vegetables.

Although we now have sufficient knowledge to restrict the original Doll and Peto's range of acceptable estimates [1], perhaps to somewhere between 20 and 50%, I am not sure that any such precise estimate for potential incidence reduction can be offered. For instance, the 27% breast cancer reduction might be consistent with the results of most [2] (though not all [3]) case–control studies, but is certainly inconsistent with the findings of most cohort studies [4–6]. Miller and colleagues indicate that "when in cohort studies less details can be collected than is possible in case–control studies, there may be much misclassification of fat intake". Further, case–control studies which relate to current or recent diet may be more appropriate to investigating some aspect of diet with a short-term (promoting) effect on the process of breast carcinogenesis [7]. One could further discuss advantages and disadvantages of case–control and cohort studies, but when the general results of the two major analytical epidemiology approaches are so inconsistent, any precise estimate of risk remains open to criticism.

This line of reasoning has at least two main implications, one in the short term on indications for prevention, and another in broader terms for perspectives of research. In principle, if our knowledge is still unsatisfactory, our focus should in fact be more on research than on prevention, and *vice versa*. In practice, other considerations should also be taken into account, including some general cost/benefit assessment of preventive indications—for cancer as well for other major diseases—and some evaluation

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on the perspectives of further epidemiological research to offer more reliable and accurate assessment of risk for general dietary patterns and specific nutrients or micronutrients.

Over the last decade, epidemiological research on diet and cancer has appreciably increased in quantity and quality, and has provided a number of relevant new evidences. It is, however, difficult to evaluate how much new information can be provided in the near future by further improvements in dietary assessment, and in the methodology and design of case–control and cohort studies. In any case, problems of collinearity between several components of diet, and inherent limitations in any assessment of dietary history, will almost certainly continue to pose serious difficulties for quantifying the role of specific nutrients and micronutrients. Some of these difficulties, at least for micronutrients, will possibly be solved only through randomised intervention trials. In the absence of evidence from controlled intervention studies, we are now not yet in the position of recommending dietary supplements, but also a general recommendations to avoid use of dietary supplements can be open to criticism, and may (hopefully) be changed in the near future.

Finally, descriptive epidemiology should remain a basic framework for monitoring the impact of dietary changes. Attention should be paid both to absolute values of current rates and to their trends over time. Miller and colleagues, for instance, indicate that "incidence of breast cancer in eastern Europe is approximately half that of high risk countries of western Europe". However, in 1985–1989, overall age-standardised (world standard) breast cancer mortality was 22.35/100 000 women in the 12 countries of the EEC, 20.44/100 000 in other western European countries, and 16.57/100 000 in eastern European countries (i.e. a difference in mortality of only 20 to 25% between western and eastern Europe) [8]. Trends in mortality over time, moreover, have been more unfavourable in eastern than western Europe since, in 1960–1964, rates were 9.67/100 000 women in eastern Europe, 17.54/100 000 women in the 12 EEC countries, and 17.96/100 000 women in other western European countries. In eastern Europe, over recent calendar periods, mortality trends have been comparatively even more unfavourable at younger ages. It is, therefore, difficult to make any quantitative inference on a more favourable dietary pattern for breast cancer in eastern as compared to western Europe, in terms of fats or other nutrients. Likewise, any quantitative estimate of potential incidence reduction only on the basis of some selected overall age-adjusted rates may be severely misleading.

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MILLER AND COLLEAGUES have provided the scientific community with an admirable and succinct review of the epidemiological evidence relating diet to the aetiology of cancer. Building from this, they have ventured into the contentious area of making dietary recommendations for the general public and provided, in Table 1, quantitative estimates for the impact of their recommendations on the reduction in cancer incidence. An exercise such as this inevitably has shortcomings, and it would be all too easy to lose sight of the major contribution made in this review by criticising specific points of detail. It is, however, only by considering the detail that gaps in our present understanding will be highlighted, and future research issues prioritised.

The point of detail which I want to discuss concerns the recommendations about stomach cancer. It may be argued that dietary advice to reduce the risk of a cancer which is the second most common in the world, and which is the only specific cancer site to have a recommendation exclusively devoted to it (number 5, limit the use of salt and the consumption of salty, salt preserved foods and nitrites) is no mere point of detail. Also, examination of the figures in Table 1 shows that the incidence of stomach cancer could potentially be reduced by 68% from dietary changes alone. This is greater than the equivalent incidence reduction for any of the other 12 sites (or groups of sites) listed — the figures of 80% for lung cancer, and 90% for cancers of the upper gastrointestinal and respiratory tracts including major effects from eliminating smoking.

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