tends to have a predictable effect on smoking cessation. However, from a social perspective, it is fairly certain (to within relatively narrow confidence limits) that how many cases of lung cancer or cardiac events will be avoided if 10,000 persons stop smoking. What this illustrates is that social benefit is more certain than individual benefit. In other words, if we are looking at it from a social perspective, it is more consistent (when we are making health decisions on a societal level) to use a discount rate which is lower than the simple aggregate of individual discount rates.

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Reply

I welcome the opportunity to explain some of the “misconceptions” and “misunderstandings” detailed by Dr Marshall.

1. Economics for some may be the “science of choice” but the Oxford English Dictionary is a little more specific and describes it as “the practical and theoretical science of production and distribution of wealth”.

2. I had not intended in the introductory paragraph to claim that economic theory applied to health employment. I simply cited an observation in a widely used economic text that the history of economics is pretty much the history of paid employment.

3. In Dr Marshall’s third point (rewritten and expanded since I wrote my original reply) there seems to be a misunderstanding about the role of theory in science. In the cycle of “systematic enquiry, conjecture, and re-evaluation”, do we not accept the theory that best fits (explains) the observations, question the theory that fails “to fit all observations”, and reject the theory when a new one seems to fit the observations better—and thus did exit the “flat earth society”?

4. Economics has certainly contributed to our understanding of community behaviour within the environment of paid employment. The suggestion was that other social sciences might contribute more (before or) outweigh the environment of paid employment.

5. I understand that the theoretical basis of neoclassical economics is the individual—and in my naive view that may be the origin of the limitations. The problem, however, is not neoclassical theory, because it overdoes the extrapolation from the individual to the community (the Oxford English Dictionary describes “economy” as the administration of resources of a community). That is the substance of my thesis.

6. Dr Marshall’s discussion seems to demonstrate the dominance of “money” and the “perfect market” in economic theory.

7. It is possible that the illusion appears to show the importance for economics of “finance”. Since this non-health care example has been introduced, may I suggest that it shows simply that the purchase of a large “good” like a house is spread over time: there is no need to “ignore” or “assume”.

8. It is most certainly not my perception that people who do not produce are not valued. The inclusion of the “human capital model” and the decision rules of “rational economic man” was to demonstrate that, as a moderately “compassionate caring society”, we do not restrict health care planning choices to middle aged men in gainful employment.

9. I recognised the possibilities of estimating implicit social value in health care decision making some 20 years ago, and the idea was very rapidly adopted in the early days of health economics. Before that cost-benefit analyses (mostly in the field of transport economics) tended to enter an empirical or arbitrary social value and then compare cost-benefit analyses. It seems to me to be more logical to turn it round and estimate an “unknown” from an established equilibrium or status quo.

In conclusion, the closing sentence of Dr Marshall’s letter that “social benefit is more certain than individual benefit” seems to endorse my thesis that “society’s future is more certain than an individual’s future”. This might suggest that Dr Marshall may be beginning to drop some of the trappings of neoclassical economics—sovereignty of the individual and an assumption that society’s choice may be predicted by the individual’s. Let us hope that other economists will follow and continue to question the wisdom of discounting the future.

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Ionizing radiation and offspring sex ratio

Sir—Dickinson et al report a high sex ratio (proportion male) of offspring born to men exposed to ionizing radiation at Sellafield. These authors were undecided in their interpretation. I should like to suggest that their data represent strong further evidence that ionizing radiation does directly (but admittedly weakly) affect offspring sex ratio.

If ionizing radiation induced sex linked lethal mutations, one would expect irradiated fathers to produce an excess of sons, and irradiated mothers to produce an excess of daughters.4 In accordance with this thesis, Cox reviewed the literature and reported that in 12 data sets describing the offspring of irradiated fathers, there were 10 in which sex ratios were raised in contrast with those of controls: and in 10 sets describing the offspring of irradiated mothers, 9 reported lowered offspring sex ratios (χ² = 9, p<0.005).

So there is good prior evidence that ionizing radiation has the effects described. The data of Dickinson et al4 confirm with this generalisation. The sex ratio of offspring born to their most heavily irradiated men (201 sons, 144 daughters) is highly significantly different from that of the offspring of the women who had received preconceptual radiation (454 sons, 461 daughters) (χ² = 7.1, p<0.01). So the data of Dickinson et al4 add to the already existing strong evidence that ionizing radiation induces sex linked lethal mutations in man which are reflected in the offspring sex ratios of irradiated parents.

The effects of ionizing radiation are quite different from those of non-ionizing radiation in this regard. For instance, men working in high voltage power stations or substations reportedly produce an excess of daughters,5 possibly as a result of hormonally modulated effects of such radiation. In general, illness and adverse industrial/occupational exposures to men are associated with low testosterone and/or high gonadotrophin levels,6,7 an effect which may be quite strong (presumably as a consequence of the increased adrenal disturbance).7—Thus as far as I know—ionizing radiation is the only reproductive hazard which causes men to sire an excess of sons.

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Reply

We thank Dr James for his letter and, in particular, for reminding us of the study by Schull and Neel published in 1958 that irradiated fathers and mothers would be expected to produce excesses of sons and daughters respectively. As knowledge of genetics progressed, Schull and Neel reconsidered this theory,4,8 their most recent postulate being that while maternal exposure would, in principle, reduce the sex ratio, it is difficult to predict what effect irradiation of fathers might have since, “With the recognition that one X chromosome is inactive in the somatic cells of women (the Lyonisation phenomenon), it became clear that sex linked mutations in males are unlikely to have a dominant lethal effect in females.”4

James also brings to our attention the study by Cox, who claims that 10 out of 12 data sets describing the offspring of irradiated fathers showed an increased sex ratio in exposed groups.9 All the studies of paternal irradiation to which Cox referred were summarised in table 4 of our paper, where we gave the minimum detail necessary for critical assessment. Such a review must inevitably be dominated by Schull and Neel’s study of 53 691 children born during 1947-57 to the survivors of the atomic bomb dropped on Hiroshima and Nagasaki.1 Subsequent to Cox’s review, Schull et al updated this work by further considering the 47 624 children born during 1956-62 to the Japanese atomic bomb survivors,2 the relevant results again being summarised in table 4 of our study.1 Many of the smaller studies appeared to classify children as “exposed” if they were born, rather than conceived, after paternal irradiation.7,10 Several were questionnaire based studies1,2 whose response rate ranged from 37% to 64%.11 The studies of the atomic bomb survivors2 considered children conceived at least 18
months after the bombings and thus excluded the effects of radiation doses received by fathers during the period of spermatogenesis—i.e. immediately before conception—whereas we reported an association with the dose estimated to have been received by fathers in the 90 days before conception. Referring again to table 4 of our paper, it will be seen that only in the study of chronic exposure is it likely that the father was irradiated in the immediate preconceptional period and that these studies yield equivocal results.

James' post-hoc comparison of the children born to mothers and fathers who had received a preconceptional radiation dose is invalid: our analysis considered a priori hypothesis comparing both groups independently with the remainder of the population, after allowing for the effect of year and paternal employment at Sellafield. Since the sex ratio of children of mothers with a preconceptional radiation dose was 0.985 (95% CI: 0.865, 1.121), not significantly different from that of children of mothers without such a dose, 1.046 (95% CI: 0.961, 1.140), our data provide no evidence to support the hypothesis that irradiated mothers are more likely to produce girls.

One of the caveats concerning our results which we would like to reiterate is our concern about the estimation from annual dose summaries of the diagnostic radiation doses received during the 90 days before conception—a process which inevitably leads to dose misclassification. While random misclassification generally biases the results towards the null hypothesis, in this instance it is quite possible that there is differential dose misclassification and hence uncertainty about the direction of bias. Measurement error is clearly an important issue in this regard and is an area we are actively exploring.

To summarise, studies of the possible association between paternal preconceptional irradiation and an altered sex ratio do not yet satisfy the Bradford Hill criteria for inferring a causal relationship.

We would like to take this opportunity to thank Mr Les Scott, Dosimetry Data Management Research Institute, for estimating the preconceptional radiation doses based on which the study was based.

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NOTICES

11th Conference of the European Health Psychology Society, 3–5 September 1997, Bordeaux, France, for further information contact: 11th EHPS, Congress Rive Droite, 23 rue Baudrinent, 33100 Bordeaux, France. Tel: +33 05 56 32 82 29. Fax: +33 05 56 32 79 73.

International Congress on Biomedical Peer Review and Global Communications, 17–21 September 1997, Prague, Czech Republic. For further information, contact: Annette Flanagin, Workshop on Peer Review, 515 N State St, Chicago, IL 60610, USA; fax: +312 464 5824; e-mail: jama-peer@aamae.org.

BOOK REVIEWS


The fourth edition of this well established text bears little relation to the thin tome that I used at university in the early 80s. The book has been expanded and is well laid out with good illustrative diagrams and extensive use of bullet points. The latest edition has been excessively updated and includes a new chapter on health targets and particularly emphasis on the priority condition identified in The Health of the Nation. Many of the latest changes in the Health Service are described, although the pace of change is such that the most recent movers, of district health authorities and family health service authorities, have not been included.

In general the book is an excellent introduction to the subject and any criticisms are fairly minor. In the chapter on survey methods, it was sometimes not clear whether the term "bias" was being used in a general or technical sense with a definition only appearing towards the end of the chapter. The discussion of this important concept could have been expanded within one section rather than spread through subsequent chapters. In discussing Health Service structure, there is a heavy emphasis on describing the Health Service in England, the other countries in the United Kingdom meriting only one sentence. One diagram is erroneously labelled as illustrating the Health Service in the United Kingdom and in France. In fact, it only shows the structure in England. A further chapter giving some general international perspectives in health service organisation could also have been usefully added.

Notwithstanding these comments, the book should be a recommended initial text for medical students, and serve as useful summary of the subject for clinicians wishing to update their knowledge. Postgraduate students commencing study in public health might also find the book useful.

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Latin America, as other world regions, is changing its demographic and epidemiological pictures. The adult population is growing and it is essential to analyse health conditions of this group. This volume is focused on the mortality of that segment of the population. The book originated from the First Scientific Seminar of the Adult Mortality Committee held in Chile in 1991. In spite of the information on old, 90s, and latest data, the diversity of subjects, and diverse author approaches, it is a good summary of the burdens and contrasting situations characterising the changes in this region.

The book has four parts. The first presents a comprehensive overview of the book content and an expert's paper with a complete description of trends and demographic characteristics of Latin America. The four papers comprising the second part concentrate on the different methods of studying adult mortality and on the limitations of data sources in underdeveloped environments. Part three contains itself with current changes in the epidemiological patterns in selected countries. The corresponding analysis is a reminder of the complexity of the transition phenomena in countries with vulnerabilities, conflicts, and increasing social conflicts. The negative effects of global crisis on public health programmes and the big contrasts within the countries, are important considerations made by the authors. The last section is the longest and is concerned with specific causes of death and their prevention.

The quality of the papers is variable—it is worth mentioning, however, the remarkable paper on maternal mortality. The reduction in cardiovascular mortality seen in some countries is difficult to explain because of the scarcity of effective preventive intervention and health promotion programmes. The excessive deaths due to all kinds of violence are striking, even more when it is considered that violence ranks first in morbidity and mortality in most countries. Some of the conclusions arrived at by the authors in this section are fascinating and challenging, particularly those by Frejka and Atkin on abortion.

This book is a valuable contribution to the subject of adult health in Latin America. However, several questions are still unanswered.
Ionizing radiation and offspring sex ratio.

W H James

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