LETTERS TO THE EDITOR

Research into purchasing health care: time to face the challenge

Sir—I should like to support the overall thrust of the paper in the December 1996 issue of the Journal,¹ which is to encourage greater transparency in research on NHS health care purchasing. However, I should like to rectify the impression given in this paper that the national evaluation of GP total purchasing pilot projects (the latest extension of the fundholding concept) will miss the opportunity to compare GP total purchasing with other approaches to purchasing. The study design includes a variety of comparisons depending on the service area in question but includes comparisons between total purchasing projects and populations whose services are purchased by health authorities both with and without the presence of standard fundholding. Since some of the comparison health authorities include locality commissioning schemes within their boundaries, it will be possible, albeit in a limited way, to include these alternative devolved approaches in the reporting of the evaluation.

The evaluation team has endeavoured to incorporate appropriate comparisons, although it is true to say that the NHS Executive's prime interest in commissioning the work was to assess the marginal costs and benefits of total purchasing over standard fundholding. A leaflet describing the study design is available from the King's Fund Policy Institute and a preliminary report, Total Purchasing: A profile of national pilot projects, was published by King's Fund Publishing in January 1997 and is available from the Bournemouth English Book Centre (tel 01202 715555).

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Discounting the future: influence of the economic model

Sir—Unfortunately almost everything Dr West writes about economics seems to be founded on misapprehensions.¹ Perhaps this explains his evident distaste for the theory and its practitioners. Yet economic theory is important. Anyone who wants to contribute to health policy needs to understand it or risks being ignored. Despite this, Dr West manages to reach a conclusion with which I can readily agree. Nevertheless, it is important to point out some of his more glaring misunderstandings.

1. Economics is not “the study of the economy” (I am not even sure what Dr West means by “the economy”), it is more correctly the study of human choice under conditions of scarcity. It is axiomatic in economics that there is always scarcity, therefore it might be correctly described as the study of choice. If I choose A over B, then to me, A is more valuable than B.

2. Economic theory does not apply only to paid employment as West implies. It is certainly easier to model economic activity which involves a universal currency of exchange, just as it is easier to describe the epidemiology of heart disease if all cardiac events corresponding to a single definition are regarded for the purpose of analysis as equivalent. But even if the history of epidemiology is that history of the classification of illness, epidemiology is certainly more than this. Economic models are just as applicable to human activities which do not involve currencies. Indeed a “Robinson Crusoe economy”—a single person, producing what he himself consumes—is the starting point for many of its explanatory models.

3. Economics is considered a “science” because it has a tradition of systematic enquiry, theoretical conjecture, and refutation. Economics, also has a core of theory. In this respect it is quite unlike epidemiology—which is simply an investigatory methodology—and rather more like physics (where the core theory is of natural selection). If we reject the core of economic theory, there is little point in using economic analysis to throw light on our decisions, as it is central to the kind of economic analysis that is used in health economics. Of concern is the fact that West uses the results of economic analysis to dispute the theory. This presumes that he is using another method to assess these results, but he fails to explain what this model is.

4. It would be equally sensible to suggest that because an epidemiological investigation produced a result which was counter-intuitive or which we did not like we should reject epidemiology.

4. Economics is one of a number of social sciences, it would be absurd to suggest (as West does) that one social science provides a “better” description of human activity than another. Differing social sciences describe different aspects of human behaviour, just as pathology and epidemiology describe different aspects of disease. Economics, for example, ignores questionnaires and for economic theory to be normative rather than simply descriptive, requires a number of explicit value judgements.

5. The theoretical basis of economics is individual choice. However, what any individual chooses is rational to them. This is called consumer sovereignty. People prefer things differently at different times. This has always been the case, if only because crops are seasonal, because life is uncertain, and because our wants and desires change with time. The discount rate is simply a means of reflecting time preference. The discount rate may be positive, negative or zero. Robinson Crusoe will live in the winter when he cannot grow food, so his discount rate during the summer when food is abundant may even be negative. In other words, one potato in December when he may be starving, may be worth 10 him when he has many.

6. A particular discount rate is not part of any economic “creed”, although it is assumed (and corroborated by observation) that time has an influence on our choices. There is no requirement that social conditions should be stable (although stability or its absence affects the discount rate). If we conduct economic analysis to evaluate the implications of collective decisions, the discount rate should simply be a reflection of collective time preferences. If people borrow and lend money in a perfect market, according to economic theory, the real interest rate (the interest rate minus the inflation rate) should reflect an equilibrium. This is the “price” of money in the future: the financial discount rate. If you lived in a world where all goods could be traded in perfect markets, this would be equivalent to the discount rate in any sense.

7. Only a very naive economist would suggest that financial markets are free from distortions. As a result there is a divergence between the financial discount rate and the theoretical “free market” discount rate. Firstly, income from lending (savings) is taxed which skews conditions of supply and demand, this is offset by higher interest rates. Secondly, interest rates in national banks are set by government and do not reflect market rates. In welfare economics the debate therefore focuses on whether the financial discount rate should be higher or lower than the financial discount rate. Nevertheless, we all use the principle of time preference. If I invest 100 a month for 25 years (ignoring inflation and assuming a 3% real interest rate) a sum of £43 000 when I am finished, despite the fact that I only invested a total of £30 000. I will therefore be able to pay for my very “real” house of 25 years, although I could not afford it now. The same applies to paying for “real” doctors, “real” buildings etc.

8. West discusses the human capital model of valuing life at some length. However, he quite erroneously relates this to the principle of time preference. Because the basis of economics is individual choice, the human capital model for valuing life is widely considered to be unsound. All valuations in economics are subjective valuations and economists are always about deriving valuations, never about imposing them. The phrase “society stands to gain most . . . which West uses, implies an “objective” evaluation of social benefit, this has no place in economics. The value system which informs economics is explicitly subjective—consumer sovereignty. Social benefit in welfare economics is simply the sum of individuals’ subjective preferences. Contrary to West’s personal preference, there are many valid value people for a wide variety of reasons, not simply for their income generation. A more theoretically consistent model for evaluation of anything which is not bought and sold (such as health) is through a preference function. This means that we derive the people’s own evaluations from their behaviour. If, by their actions or statements, a person clearly puts a higher value on the life of a child than on their own, then, to that real, a child’s life is more valuable than their own.

Given all this, how do I arrive at the same conclusion: that we should use a zero discount rate?

Why people prefer “jam today” to “jam tomorrow” is an interesting question. Part of the answer lies with the attitudes to uncertainty. We prefer jam today because to- morrow we may not enjoy it. There is a great deal of economic theory devoted to describing behaviour under conditions of uncertainty. If I am a smoker, I may or may not benefit from stopping. My decision to stop depends (among other things) on how I weigh up the present disadvantages of stopping against the future possibility that I may benefit. Individuals vary in their assessments of these outcomes. Anything which alters an individual’s perception of the facts and benefits

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 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 considering a social perspective, it is more consistent (when we are making health decisions on a social 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 "misunderstandings" and "misconceptions" 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 all levels of 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 refutation", 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 outset) to the environment of paid employment.

5. I understand that the theoretical basis of neoclassical economics is the individual—and in my naïve view that may be the origin of the limitations of the 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.

The crux of my argument is that 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 (money) 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 ratios. If explained 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 of this. I should like to suggest that their data represent strong further evidence that ionizing radiation directly (but admittedly weakly) affects 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. In accordance with this theory, rising, 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 al 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 al 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, an effect possibly as a result of hormonally stimulated effects of such radiation. In general, illness and adverse industrial/occupational exposures to men are associated with low testis torem and/or high gonadotrophin levels, and the producing sex ratio (presumably as a consequence of the hormonal disturbance). 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 theory which Schull and Neel put forward 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,4,5 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 re-recognition that one X chromosome is inactive in the somatic cells of women (the Lyonization 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.6 All the studies of paternal irradiation to which Cox referred were summarised in table 4 of our paper,4 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 1945–57 to the survivors of the atomic bomb blast dropped on Hiroshima and Nagasaki.7 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,8 the relevant results again being summarised in table 4 of our study.9 Many of the smaller studies appeared to classify children as "exposed" if they were born, rather than conceived, after paternal irradiation.6–10 Several were questionnaire based studies10 whose response rate ranged from 37% to 64%.11 The studies of the atomic bomb survivors12 considered children conceived at least 18
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*J Epidemiol Community Health* 1997 51: 339-340
doi: 10.1136/jech.51.3.339-a

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