What future for fast reactors?

SIR—In their article[1] last year, Keepin and Wynne raised questions about the integrity of mathematical modelling in the energy study[2] by the International Institute for Applied Systems Analysis (IIASA) called *Energy in a Finite World*. (Specific references in what follows are to page numbers in the published report.) The implied accusation is that the modelling is biased to emphasize the potential role of the fast breeder reactor, but that only small changes in the parameters involved show this solution to be unnecessary. This requires a rebuttal.

Keepin and Wynne's big mistake is to have confused mathematical modelling of scenarios with the rest of the book. Our conclusions are not to be found in the chapters on mathematical modelling, which are not meant to anticipate the conclusions, which are themselves given at the end of the book (chapter 25).

The conclusion that matters there is that "it could be done" (p. 771). The conclusion is not that it will, should or even can be done. The use of the conditional rather than the indicative verb refers implicitly to the overall assumptions, that the world will not experience major catastrophes, that, overall, it will behave rationally and cooperatively and that the gap between developed and developing countries will be closed, albeit slowly. In other words, we asserted that primary energy supply *could* meet future energy demand.

Whether the real world develops in this way is deliberately left open. Indeed, we conclude that "the reality of political, societal and institutional problems will probably make the situation more grim than has been described in our two scenarios" (p. 778). So the study is meant to serve for purposes of orientation and provision of perspective, not as a prediction of reality, as was recognized when the study was considered "sensible" in an editorial note[3] in *Nature*.

The conclusion that "the demand for liquid fuels is a principal driving force of the energy problem" (p. 779) was a surprise not in the minds of the authors when the study was begun in 1973. They indeed expected that fast breeders would assume a major role, if not the major role, in the future; the study showed something different, a problem within the energy problem.

So much, the authors learned; they had come a long way, which led to fossil, not nuclear, fuels. If the study makes a case at all, it is the case for fossil fuels. On the global scale, even low quality fuels were recognized to be important, whence our concern for environmental issues and the issue of the cleanliness of fossil fuel usage (pp. 781, 804). Since then, the cleanliness of fossil fuel usage has indeed become a political issue (the SO_2, NO_x case). But in the late 1970s, when the study was concluded, that was not in everybody's mind.

In contrast with these explicit conclusions, there is no such explicit conclusion about the fast breeder, which enters only indirectly. Nuclear power must fill a gap when, by 2030, a total of between 24 and 36 TW-years per year of primary energy must be provided in our scenarios (p. 787). If one wants to postpone the use of nuclear power, these sources are already stretched to their limits. After all, our study was not considering just the United States as were Keepin and Wynne, but the whole globe. Indeed, our study even supposed that the United States would take on the role of coal supplier for Western Europe and Japan.

But if nuclear power is to assume the role of providing 5–8 TW-yr yr^{-1}, it will be necessary to have a large number of breeders. They will follow the learning curve, so that their costs will decrease. This is a top/down approach and not in logical contradiction with a bottom/up approach which, in a different context, would allow that there would not be many breeders, so that learning would not take place.

This dichotomy between a top/down approach and a bottom/up approach points to what is inevitably lost when there is no substantial number of breeders. We might not be able to provide 3–4 kW per head of the world's population. Indeed, the study notes that "nuclear waste and proliferation issues could limit the build-up of nuclear energy over the next fifty years" (p. 779) and so is quite explicit on this issue.

Keepin and Wynne's underlying proposition is to do without nuclear power by not using energy, that is by energy conservation. In the countries of the Organization for Economic Cooperation and Development (OECD), this leads mostly to the substitution of energy by capital and to drastic changes of lifestyle. Our study concludes that "only radical changes of lifestyle could lead to very low energy demand" (p. 773); we also say that "our Low Scenario implies strong — but probably more realisable — energy conservational measures" (p. 774). We continue to have strong doubts about the so-called soft-energy paths, but it is possible that their advocates could arrive at a comprehensive and global energy study, such as *Energy in a Finite World*, dealing not just with the OECD countries or only the United States but with the world as a whole, including the developing countries. Such a study might change our minds.

So far in this rebuttal, we have referred only to chapter 25 of our study, which contains our conclusions. What then was the function of mathematical scenario writing in the study? Mathematical scenario writing was but one tool among others. The study considered various strata with both qualitative and quantitative insights. The quantitative insights, mathematical modelling matching energy demand and supply in various parts of the world, had then to be synthesized with the insights derived from other levels of the study, elements in the formidable task of drawing conclusions.

Keepin and Wynne have thus not recognized the structure of our study. They separated a particular part of it and made it appear as if the conclusions were derived exclusively from that.

The more specific questions of mathematical modelling in general and linear programming in particular have been addressed in an earlier rebuttal[4]; reference is made to that for readers who are interested in these details.

This letter was received in the *Nature* office on 1 February 1985.

W. HÄFELE

Kernforschungsanlage Jülich GmbH,
Postfach 1913,
D–5170 Jülich, FRG

1. Keepin, B. & Wynne, B. *Nature* **312**, 691–695 (1984).
2. Häfele, W. in *Energy in a Finite World* Vol. 2 (Ballinger, Cambridge, Massachusetts, 1981).
3. *Nature* **290**, 349–350 (1981).
4. Häfele, W. & Rogner, H.-H. *Policy Sci.* **17**, 341–365 (1984).

Radiation risks

SIR—As a member of the team cooperating with the Medical Research Council in its study of mortality among people working for the UK Atomic Energy Authority, may I say how pleased my colleagues and I were to see in *Nature* (22 August, p.666) a balanced and generally accurate resume of the papers recently published in the *British Medical Journal*. So much could certainly not be said for much of the national press.

There is, however, one inaccuracy that could lead to misunderstanding. The number of deaths from prostate cancer (28 not 38 as you report) within the authority is precisely the number that you would expect on the basis of national rates (see Table III of the paper) and is clearly not excessive. The excess you mention refers only to a subgroup of workers who had been monitored for tritium; six deaths occurred when the expectation was only 0.67. Whether or not this is fortuitous is not known, although further investigations are to be conducted. It should be noted, however, that the overall mortality of tritium workers is exceptionally good, being only 60 per cent of the nationally based expectation (see Table VII).

L. SALMON

Environmental and Medical Sciences Division,
Atomic Energy Research Establishment,
Harwell, Oxfordshire
OX11 0RA, UK