British civil plutonium

SIR—We are pleased that the Central Electricity Generating Board (CEGB) has responded (*Nature* **318**, 406; 1985) to our paper (*Nature* **317**, 213; 1985) in which we concluded that 2.3. ± 0.8 tonnes (te) of plutonium is missing from the UK civil stockpile. The calculations that we published last September resulted from a very thorough study of all the criticisms of our earlier work made by CEGB at the Sizewell Inquiry. We are pleased to see that only two main CEGB criticisms remain, and we will deal with both of them in turn. The first criticism is indicative of a rather superficial reading of our paper. The second criticism contains an argument that can easily be shown to be incorrect.

(1) It is definitely incorrect to say that our claims for ± 2 per cent accuracy were not substantiated. We presented a number of tests that showed our results to be within ± 2 per cent of published figures or, if not, to be underestimates. They included the following.

(i) We predicted that the total plutonium in the cores of the magnox reactors on 31 March 1985 was 9.6 te. Our calculations were completed before the government came up with its own figure of 9.50 ± 0.25 te. Here ours is a 1 per cent overestimate. We pointed out that our procedure slightly overestimates plutonium in core but not total plutonium production.

(ii) We predicted that plutonium stocks on 31 March 1983 and 31 March 1985 would differ by 4.9 te. Government figures (quoted to nearest 0.5 te) differ by 5.0 te. Here ours is a 2 per cent underestimate.

(iii) Since Sizewell, most of our effort has gone into deriving $G(B)$, plutonium isotope production in kg per te as a function of burn-up, from first principle incorporating all factors mentioned by CEGB in its non-quantitative criticisms. We particularly object to the description of our new station-by-station calculation of $G(B)$ as a "simplified method . . . which uses approximate data culled from a variety of sources". The reference list in our paper shows that the data come mainly from UK Atomic Energy Authority and International Atomic Energy Agency sources.

Among the tests of $G(B)$ we reported were: When using appropriate parameters, $G(B)$ exactly reproduces published data on plutonium production as a function of burn-up in the Calder Hall reactors (agreement well within 1 per cent); Table 1 in our paper demonstrates that using parameters appropriate to the civil reactors, we get good agreement with isotopic ratios in CEGB's despatch data (differences of about 2 per cent for four stations and less than 1 per cent for the other four; averages agree to better than 0.1 per cent); use of "worst case" parameters reduces the plutonium total by only 1.5 per cent.

(2) We are surprised that CEGB should repeat again the claim that Wylfa should be included when comparing production and despatch figures. The background to this is that the Inspector asked CEGB to provide the Sizewell Inquiry with data on plutonium production after 1977. CEGB did not comply with this request, but instead, provided figures on plutonium despatched from each station for the six fiscal years 1978–84. At seven of the eight stations, the spent fuel is stored in ponds before despatch to Sellafield. As the spent fuel cannot remain in the ponds indefinitely, we believe that, provided the period considered (6 years) is much greater than the time the elements remain in the ponds (approximately 1.2 years on average), then the total plutonium despatched should be similar to that produced in the 6-year period starting one year earlier.

Wylfa, however, has a dry-store where spent fuel can remain indefinitely and where plutonium is accumulating. It is totally inappropriate to include Wylfa in any such steady-state analysis.

If there was a backlog in all the ponds at the start of the 6-year period and this backlog was removed by the end of the period, then more plutonium would be despatched in 1978–84 than was actually produced in 1977–83. This is what CEGB says happened, but it does not quantify the size of the backlog. Our calculations are consistent with this, because our production figures *underestimate* the plutonium despatched by 3.6 per cent. If CEGB's account of the UK/US plutonium exchange is to be correct, our figures have to be a 9 per cent *overestimate* of the actual figures.

There is an easy way for CEGB to discredit our calculations if they are so wrong. It could be done without risk to national security, as the 2 per cent error inherent in our calculations is equivalent to a large number of warheads. All CEGB needs to do is to publish the plutonium content of the spent fuel in the storage ponds at the start and end of the 6-year period or provide the information on plutonium production requested by the Inspector. CEGB refuses to do either, following advice from the Department of Energy, even though such data would help to resolve this controversy.

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How big was the Ark?

SIR—In a reply to my earlier letter (*Nature* **316**, 184; 1985) R.H. Clarke of Melbourne University (*Nature* **318**, 100; 1985) chose also to ridicule the following statement of mine published in the Melbourne *Age* in 1984: "first, does he realize that Noah's Ark was immense (450 × 75 × 45 feet)? Its tonnage would have supported 4,000 fully grown African elephant bulls. There would have been plenty of room for all had God put young animals on board."

Clarke says that such beliefs should not be imposed on the young and vulnerable, but my statement is ludicrous only if taken out of context. Because of the tremendous genetic potential for variation within the species, the total number of representative ancestral pairs that had to be on board, to account for the diversity of extant terrestrial fauna, did not have to be as large as might be imagined. We need only to think of *Canis familiaris* (dogs) to realize that in theory a single pair on the Ark could have been sufficient. Invertebrates could have come on board via their plant or animal hosts. Interbreeding followed by natural selection (or artificial breeding) would have run its normal course in subsequent millennia.

Microevolution can proceed very rapidly, as has been demonstrated in experiments by the Commonwealth Scientific and Industrial Research Organization where five pure dingo pups mated with domestic dogs resulted in 41 hybrids possessing a wide range of characteristics which matched the diversity of wild dingo-dog hybrids now prevalent in the Australian bush. Since biblical chronology places the launching of the Ark at about 4,500 years ago, there would have been ample time to provide the diversity of species now extant. It is also relevant to my case that classification at the species level does not reflect breeding boundaries, as many cases of potential interspecific hybridizations within a genus have been documented (for example *Canis, Felis, Drosophila*). Thus, while microevolution (which includes the case of the peppered moth) is rapid and observable, direct evidence for macroevolution, which seeks millions of years for change, is still non-existent. My paradigm, to use Kuhn's terminology, might be different from that of macroevolution, but the facts need not be.

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