to Ca). The relevant point here is that all evidence indicates that plutonium and americium concentrate in the food chain much *less* than radium (W. J. Blair and C. R. Thompson, *Science* **183**, 715, 1974); for example, their passage through membranes is thousands of times slower. Thus, there is nothing in von Hippel's criticisms that would affect my result.

Now for my so-called "errors." The limitations in comparing chemical with radioactive toxicities are pointed out at some length in my paper, but von Hippel's point is not one of them. It would require ingestion of much *less* radioactive waste to give 180 000 rem to the bone (which, incidently would cause a 69%—not 100%—probability of cancer according to the BEIR Report—not according to me) over its many decades of residence there, than to give a rapid dose of 400 rem to the bone marrow (these materials concentrate on the outside of the bone and give little dosage to the marrow). Animals injected with radium or plutonium have often received up to 50 000 rem to the bone (in one case 250 000) and they died of bone tumors, never of acute radiation sickness. The famous radium watch-dial painters who received up to 120 000 rem to the bone (an average of 17 000, enough to cause 6% of them to die of bone cancer) experienced no symptoms of radiation sickness. As I emphasize in my paper, the only reason I make comparisons with chemical poisons is as a counter to statements like "one year's waste could kill 30 billion people," and I never use it in calculations. On the other hand, compounds of arsenic and barium do have long-term health effects even in small dosage and their effectiveness may well be similar to that of radioactive poisons. Unfortunately, these things are not well enough understood for us to make numerical comparisons.

With regard to my other so-called "error," the rate of migration of strontium through deep underground aquifers, it should be clear to a careful reader that the rate given was not an absolute minimum, but rather something of a minimum for the bulk of the material. In the Idaho situation, only about 1% migrated as much as 1 km in a decade. Moreover, the Snake River aquifer is an extraordinarily fast flowing one (10 ft/day versus a typical 1 ft/day) and its depth is only about 150 meters versus 600 meters for the waste; at shallower depths pressures are lower, so fractures remain more open and transmit water faster with less ion exchange holdup. Incidentally, this waste was low-level, not high-level as von Hippel states.

While von Hippel is correct in pointing out that there are differences in the transport speeds of different ions, these do not affect my calculation of eventual effects. Except in the initial time for the integration which only affects the result by a factor of 3 and is completely taken care of by the leaching time for strontium alone, no time delays are pertinent. For example, the ratio of Ra/U in rivers is the same as in rock although the transport time through the ground is much longer than the 1600 year half life of radium. The whole process is in secular equilibrium, so transport velocities do not matter.

I agree that the Oak Ridge reports by Gera, Claiborne, and Jacobs are useful and interesting and I have urged their authors to write them for publication, but they do not give estimates for the probability of release, and hence do not give a quantitative estimate of the over-all hazard.

One of the problems with von Hippel's argument on underground siting of reactors is that he doesn't do a cost-benefit analysis, so let's do one here. The final version of the Rasmussen study, which includes the effects pointed out by the American Physical Society study, still gives an average of less than one fatality in its lifetime from each power plant. A few million dollars extra invested in medical research or treatment, or in public health, could easily save a life, and when we save money by buying a small car, or by driving on a long trip rather than taking a bus, we imply that our own lives (and those of our families) are not worth more than a few million dollars. Thus, if underground siting costs more than a few million dollars, the benefits do not justify the costs. I believe it would cost more than that.

Von Hippel's argument on underground siting of fuel reprocessing plants may have merit, but first he would have to show some mechanism by which the material could get through the 10 foot thick concrete shield walls whereas it would not get through a hundred feet or so of earth cover.

The basic issue is, however, one of time constants. Fifty years or so is a typical time constant for changes in surface structures and institutions, but for a rock 600 meters underground, a typical time constant for important changes is about 100 million years. Clearly, the relevant time constant for power plants and reprocessing facilities is 50 years or less, whereas for radioactive waste it is much longer, so there is no real comparison between them.

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Scientific court

I am writing with regard to your January editorial (page 120) espousing the scientific court proposed by Arthur Kantrowitz. I, too, would support such an experiment, but would add a bit of cautionary advice.

To be sure, a scientific court could contribute mightily toward dispelling any notions the public might have of the infallibility of scientists. The use of the court must not, however, be extended to the resolution of public-policy questions when the arguments of the competing sides rest on moral or value-charged bases. If the issue were to be purely scientific in character—for example, to what extent would the SST affect the ozone content of the upper atmosphere, or how loud would it seem to airport neighbors—then the court would be a most appropriate forum. If, though, the question were whether the United States should build such an aircraft, then I submit that a scientific court would be no more competent than any other body of citizens to adjudicate such a controversy. This latter sort of debate is properly political and not merely factual. Insofar as the public is informed about the actual scientific details by the court, just to this extent is the court fulfilling its mandate. But if the court were to presume to instruct the public—*ex cathedra*, as it were—then the public would indeed be foolishly taken in by such an assumption of false infallibility.

Every citizen has the right and responsibility to decide for himself great public policy questions. A scientific court should assist him in this attempt with clear awareness of its limitations. One must always bear in mind that facts, even indisputable ones, go only part of the way toward illumination of the wise course.

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AUTHOR COMMENTS: I agree that the Science Court must continuously exercise the utmost caution to avoid making public-policy recommendations. In my opinion, if it is not successful in avoiding such value-laden statements, its influence will be destroyed.

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Corrections

April, page 15—The first sentence in the letter titled "Monopole debate" should have read "I was dismayed to find a statement in your October issue to the effect that '. . . Julian Schwinger . . . developed a consistent *field* theory of monopoles'." The word "field" was omitted also in the version seen by the two respondents.

The June 1976 cover note should have included the following credit line: "© by the California Institute of Technology and the Carnegie Institution of Washington." Our apologies to the permissions department of the Hale Observatories. □