

References and Notes

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Nuclear Waste Disposal in the Oceans

In their article "Disposal of nuclear wastes" (1) Kubo and Rose give an excellent updated assessment and discussion of many of the problems of disposing of radioactive wastes from nuclear fission reactors. They note that today no proved, operational long-term storage facility for high-level wastes is available. They find, however, several attractive technological options that have been given little consideration, and they conclude that the following options appear to be either usable or worth further exploration: mausoleums; disposal in mines, and perhaps in ice; in situ melt; and further chemical separations. They state, furthermore, that it is too early to assess disposal in space.

Kubo and Rose, however, appear to deal too lightly with disposal in the oceans as an option that is worth further exploration. They dispense with this option by noting that it "seemed unsafe for lack of adequate knowledge about all the consequences of failure—a situation that still obtains." The idea of dumping large amounts of high-level nuclear wastes into the oceans has in the past been met with strong opposition because of the unknown risks involved. International agreement would, furthermore, be required before such a program which had met certain criteria of safety could be implemented. Still, the potential of the oceans as dumping ground for the high-level nuclear wastes from fission reactors (after chemical removal and nuclear burning of the actinides) is very attractive. Petros'yants (2) summarized

the situation thus in 1972 (referring to the risks connected with disposal of nuclear wastes in the oceans): "The fears are unquestionably founded, and therefore it is essential, in the interest of protecting the seas and oceans from radioactive contamination, to conduct a series of extremely serious scientific studies before making the decision to utilize this method of disposal of solid radioactive wastes in the depths of the oceans. . . . The disposal of wastes in the ocean at depths of 5000 m and more is extremely attractive, but it is essential, of course, to make absolutely certain that this is a reliable and safe approach, and that radioactivity will not be scattered throughout the oceans. Such solutions will apparently be found: it is too attractive to use the enormous ocean expanses for the purposes."

One approach to disposal of high-level nuclear wastes in the oceans emphasizes the concept of containing the solidified wastes in containers for a period of approximately 700 years, which is sufficient for the fission products to have decayed to safe levels. It would probably be more expedient to rely instead on the low leach rate of the glasses employed to solidify the nuclear wastes, under the environmental conditions prevailing at the ocean floor. The individual glass bodies in free contact with seawater could then be made sufficiently small (of the size of tennis balls) so that the heat and radiation generated by the nuclear wastes would not disintegrate the glass bodies by cleavage or crystallization. Suitable glasses that have leach rates

ten times smaller than that of common bottle glass (at a pressure of 1 atm) are already known. Such low leach rates appear to be compatible with safe disposal of solidified high-level nuclear wastes after removal of the actinides (3).

The option of disposal of high-level nuclear wastes in the oceans does not appear to have received the attention it deserves. More work on this option and international cooperation are called for if we, who live in the western part of the world, want to assure ourselves that nuclear power is going to resolve our energy needs in the coming decades. We cannot afford to neglect such a relatively inexpensive option as long as we do not have an operational long-term storage facility for high-level wastes. Besides, some countries, of which Denmark may be an example, might have difficulty demonstrating, in time for the final disposal of their accumulated nuclear wastes in mausoleums, and to the satisfaction of their people and the international community, that they possess underground geological formations that are suited for safe disposal of high-level nuclear wastes.

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Since he wrote his comment, Nielsen and one of us have met in Cambridge, Massachusetts, to discuss the issue. We agree that a key to the issue is better separation of the waste components. After that, several hitherto nonoptions become at least conceivable, and perhaps possible. Ocean disposal is one of these.

As we emphasized in our article, high-level nuclear wastes fall into two general categories: (i) fission products, with half-lives of 30 years or less, which are for all practical purposes benign after 700 years; and (ii) the actinides, with half-lives of typically 25,000 years or more, whose hazard persists into geologic time. The two categories can be separated in the waste reprocessing plant, much more com-

pletely than had been planned in the past, at additional but apparently tolerable expense. It is both technically and economically feasible to recycle the long-lived actinides (plus iodine, if desired) through the nuclear reactor; thus, the waste requiring disposal becomes benign after 700 years. The separation task is not trivial, especially for materials contaminated with traces of actinides, but we believe that very substantial advances can be made even there.

Under these circumstances where there is no need to consider geologic times, ocean disposal appears worthy of additional consideration. However, several problems remain, some of which are also discussed by Nielsen. (i) The technology is not yet good enough. In particular, devitrification and consequent dispersion into the environment of the glassified wastes are possible; making the waste pellets small does not reduce the damage caused by short-range particles. (ii) Regions with appropriately small ocean currents and other desirable qualities are hard to find. (iii) The oceans are increasingly subject to national and international control, so decisions about their use are no longer unilateral. (iv) Opposition may arise because people just don't like the idea. (v) Some mistakes would lead to very serious consequences.

These problems are not necessarily unsolvable; if ocean disposal becomes

possible, we suspect that some kind of burial will be better than simple dumping. Note also that better separation of the various waste categories makes most other disposal options (such as salt mines or granitic structures) more attractive, too, and the whole assessment alters.

Nielsen makes an important point about the nuclear waste problem in Europe. The matrices of sites for nuclear power plants and fuel reprocessing plants and of acceptable disposal sites just do not match up within the national boundaries of Europe, and transnational solutions are required. Some aspects of this have been discussed elsewhere (1).

The U.S. Atomic Energy Commission (and other agencies elsewhere) could help to clarify views about disposal options by saying more about the possibility of better nuclear waste separation.

Nielsen agrees with us on most of the points made here.

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Wilson's Disease and Copper-Binding Proteins

The application of Scatchard's method (1) to estimate the number and association constants of a protein's binding sites for a small molecule is valid only if the binding reaction is reversible and if the measurements are made at equilibrium. Since these criteria do not seem to be satisfied by the binding of copper to the hepatic protein described by Evans *et al.* (2), doubt must be cast on their conclusions.

The copper-binding protein, metallothionein, studied by these authors is very similar to the hepatic copper-binding protein, L-6-D, isolated and investigated by Morell *et al.* (3). Both proteins were extracted from homogenates of human liver by 0.025M phosphate buffer and, although one was purified by ethanol-chloroform precipitation and the other by chromatography, both were then soluble when dialyzed against distilled water or 0.005M phos-

phate buffer and both were lyophilized from the dialyzed solution.

The copper of L-6-D, as isolated from the liver, is bound so tightly to sulfhydryl groups that it cannot be freed by dialysis or ion exchange resins. The copper of the copperthionein studied by Evans *et al.* is also very tightly bound. In preparing apoprotein for their experiments, they removed the copper from the purified metallothionein in solution by chromatography at pH 2. Then, to obtain their experimental results, they dialyzed the apoprotein against several different concentrations of copper sulfate at pH 7.4. If equilibrium exists between copper and copperthionein at pH 7.4, then it should have been possible—and would have been preferable—to remove copper from the protein by dialysis at this pH. Since the copper of another copper-binding protein, ceruloplasmin, is ir-

reversibly bound to the protein at pH 7.4 but is readily freed by dialysis below pH 3 (4), it seems essential to demonstrate directly the reversibility of the copper-protein bond of copperthionein under the conditions of the experiments.

However, whether or not copperthionein and L-6-D are similar, it is almost certain that copper is bound as cuprous ions to the sulfhydryl groups that characterize both proteins (5). So if equilibrium did exist between the free cupric ions of copper sulfate and the protein-bound cuprous ions, a complex, reversible oxidation-reduction reaction involving reversible binding of cuprous ions would have to be postulated. Without experimental evidence for the existence of such a system (5), equilibrium can hardly be assumed, and a Scatchard plot to quantitate and characterize the binding sites cannot be used.

If equilibrium was attained, the binding constants reported are each eight times too large. The intercept on the abscissa is nk —and not simply k —where n is the number of identical binding sites on the protein and k is the intrinsic association constant of each for copper.

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Scheinberg begins his comments by stating that the results of Evans *et al.* (1) are doubtful because the criteria of reversibility and equilibrium for the binding reaction were not met. He then launches into a discussion of L-6-D, the relevance of which I fail to see. No binding studies have ever been reported for this protein, and what data is available cannot be used to refute the results of Evans *et al.*

I agree that experiments at a lower pH would have been preferable, but