LETTERS

The Clinch River Project

Eliot Marshall's article "The perils of Clinch River" (News and Comment, 8 Oct., p. 137) is a fairly lucid discussion of the precarious status of this nation's major breeder reactor demonstration project—the Clinch River Breeder Reactor (CRBR). There are some statements in the article, however, that tend to perpetuate certain misconceptions about the project's viability and intent; they also lend credence to the erroneous allegations of the project's opponents. First, there is a great deal to show for the more than $1 billion—which includes about $180 million of the more than $300 million of private sector funds pledged to the project—already spent on the CRBR to date. The design of the plant is almost 90 percent complete, and about 70 percent of hardware procurements have been completed, with $700 million of components already delivered or ordered. The design is internationally acknowledged to be one of the most advanced in the world, including a unique heterogeneous core design to improve fuel utilization efficiency and some of the most advanced safety features in nuclear power plant design. In all, more than 1000 technology innovations have been incorporated in the CRBR design.

It is also important to keep sight of the fact that this is a research and development project, and as such is subject to first-of-a-kind cost uncertainties. In fact, the increases in cost since the baseline estimate of $1.7 billion are very similar to those experienced with coal conversion demonstration projects. Opponents choose to ignore this, as well as the fact that more than 60 percent of the project's cost escalation since 1974 has been due to politically motivated delays. It is a gross inequity to attach imputed interest (on the national debt) to the project's total cost estimate. No other federally funded R & D project incorporates this interest charge in their budget estimates.

Arguments against the breeder that cite decreased energy demand presume the continuation of the present global recession far into the future—a prospect that does not fare well with the public's desire for a better tomorrow. Also, it is important to note that rate of growth of electricity demand is still increasing, historically following the growth in the gross national product. According to the Department of Energy, the present growth rate of about 1.5 percent to 3 percent implies a doubling in the number of nuclear power plants by the next century.

The present status of the pace and timing for the development of commercial breeders emasculates arguments that are based on increased projections of uranium availability. Mitigation of the technical, safety, and licensing issues alluded to in the article, as well as the normal construction and operational certification schedules will preclude economic entry of the breeder into the marketplace much before 2030, if we assume initiation of CRBR construction this year. Incorporating further delays of ten or more years, as the opponents advocate, would assure that the breeder reactor would not be available when needed.

It is most disheartening to continually see quoted the ramblings of the project's most ardent detractors without also having presented for balance the views of those of us who have labored long and hard to preserve this crucial component of our nation's energy supply arsenal. I am puzzled as to how such an article could be written with no attempt to solicit the views of the leadership of the Committee on Science and Technology or even to contact the appropriate staff regarding a project authorized by the committee.

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Cloud Seeding

Richard A. Kerr's article about the Florida Area Cumulus Experiment-phase 2 (FACE-2) (Research News, 16 July, p. 234) raises some concerns about the complex and sometimes controversial field of planned weather modification.

First we are concerned about the tenor of the material presented in Kerr's article. The title "Test fails to confirm cloud seeding effect" and the initial paragraph suggest a broad negative outcome for weather modification that does not appear to be supported by the totality of the article. How do the FACE-2 results apply to cloud seeding in general? Cumulus clouds in Illinois, North Dakota, or Washington differ considerably from those that produce rains in the semitropical climate of Florida, where FACE-2 was conducted. These regional differences are quite likely related to differences in the approach to be used to modify cumulus clouds and the rain process in other climatic zones of the United States. Thus, the outcome of FACE-2 can only be viewed as a "final convincing demonstration" for cumulus clouds in a tropical climate. At the end of the second paragraph, Kerr states that "[t]hese disappointing results do not necessarily reflect on attempts to coax more precipitation out of clouds elsewhere," but the impression is made early that FACE-2 was a major failure and the final death knell for hopes to modify cumulus clouds.

Our second concern relates to the role of statisticians in the field of weather modification. In the early part of Kerr's article, quotes from two statisticians indicate that the FACE-2 confirmatory-type experiment failed to achieve the prestated percentage change objective (a 25 percent increase in precipitation). Yet the same two statisticians are later quoted as saying that, "More than likely, the results show a treatment effect" and "FACE-2 is also suggestive, as was FACE-1, of a treatment effect. It's encouraging." The role of the statisticians in the FACE-2 design and the inability of the National Oceanic and Atmospheric Administration (NOAA) to complete an adequate confirmatory experiment are evident in the fact that the 3-year sample size of 51 experimental days was less than the original design value of 62 days (*1*). A FACE-2 group report is quoted as stating that "when all of the relevant elements are examined, it appears that FACE-2 is a risky confirmatory experiment." The problem of the "outlier," the day of extremely heavy rain that fell in the no-seed category, has long been recognized in the field of planned weather modification (*2*). One wonders why the confirmatory experiment was not better designed to exclude such extreme rain events.

In essence, the FACE-2 experiment did not confirm the statistical change of FACE-1; the design did not allow for exclusion of heavy rain outliers; the results are encouraging; and NOAA did not operate FACE-2 long enough to get the correct sample size. These facts make it difficult to present a conclusive picture about weather modification capabilities—even in Florida.

The entire FACE effort reflects many of the features that have caused other weather modification projects in the United States to achieve uncertain results (*3*). Most often this presumed failure has related to inadequate support, both for preexperimental research and then for pursuing the experimentation adequately to achieve meaningful an-