

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-

swers. All too often there has been an overreliance on a single statistical outcome to declare success or failure, in lieu of adequate comprehensive scientific interpretations.

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2. A. S. Dennis, J. W. Gelhaus, M. R. Schock, "Rainfall anomalies in a randomized seeding project," *Preprints, 3rd Conference on Weather Modification* (American Meteorological Society, Boston, Mass., 1972), pp. 300-303.
3. S. A. Changnon, *Bull. Am. Meteorol. Soc.* **61**, 546 (1980).

The title of Richard A. Kerr's article "Cloud seeding: One success in 35 years" (Research News, 6 Aug., p. 519) would have better reflected the state of the subject if it had ended with a question mark. It has yet to be proved that even one cloud seeding experiment has produced significant modification to precipitation on the ground (although there is no doubt that cloud seeding can modify cloud structures).

Caution, and still more caution, is the keyword in evaluating the effects of cloud seeding, even in the case of the Israeli experiments, which appear to have provided firm evidence for a positive effect. These experiments were, in large part, designed, executed, and analyzed by the same team. Independent validation and replication is required before the results of those experiments can be considered as proof.

It is possible that some cloud seeding experiments have modified precipitation at the ground to degrees that were not detectable statistically within the time periods of the experiments. Improved understanding of the physical events involved in the formation of precipitation in both natural and artificially seeded clouds, and, most important, of the means for documenting these events, can reduce the times required to obtain statistically significant results.

Cloud seeding to modify precipitation is a challenging task, perhaps one of the most difficult to be tackled in this century. Many of the problems encountered will be faced in other attempts to determine whether it is possible to control large-scale geophysical events (for example, earthquakes). After some 35 years of research efforts, we now have

the tools to determine whether or not we can control precipitation. It would indeed be a tragedy if work on this subject were brought to a halt.

Finally, I would like to point out that the reanalysis of the Climax data, which Kerr attributes to me, was in fact, carried out by Arthur L. Rangno, a member of my research team.

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Extraterrestrial Intelligence: An International Petition

The human species is now able to communicate with other civilizations in space, if such exist. Using current radioastronomical technology, it is possible for us to receive signals from civilizations no more advanced than we are over a distance of at least many thousands of light years. The cost of a systematic international research effort, using existing radio telescopes, is as low as a few million dollars per year for one or two decades. The program would be more than a million times more thorough than all previous searches, by all nations, put together. The results—whether positive or negative—would have profound implications for our view of our universe and ourselves.

We believe such a coordinated search program is well justified on its scientific merits. It will also have important subsidiary benefits for radioastronomy in general. It is a scientific activity that seems likely to garner substantial public

support. In addition, because of the growing problem of radiofrequency interference by civilian and military transmitters, the search program will become more difficult the longer we wait. This is the time to begin.

It has been suggested that the apparent absence of a major reworking of the Galaxy by very advanced beings, or the apparent absence of extraterrestrial colonists in the solar system, demonstrates that there are no extraterrestrial intelligent beings anywhere. At the very least, this argument depends on a major extrapolation from the circumstances on Earth, here and now. The radio search, on the other hand, assumes nothing about other civilizations that has not transpired in ours.

The undersigned* are scientists from a variety of disciplines and nations who have considered the problem of extraterrestrial intelligence—some of us for more than 20 years. We represent a wide variety of opinion on the abundance of extraterrestrials, on the ease of establishing contact, and on the validity of arguments of the sort summarized in the first sentence of the previous paragraph. But we are unanimous in our conviction that the only significant test of the existence of extraterrestrial intelligence is an experimental one. No a priori arguments on this subject can be compelling or should be used as a substitute for an observational program. We urge the organization of a coordinated, worldwide, and systematic search for extraterrestrial intelligence.

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