letters

Peer review revisited

There can be no doubt as to the importance of a healthy dialogue between the scientific community, government and society. In "The Emperor's new Clothes—1981" (July, page 34) the author decries the dearth of scientific peer review of public policy decisions. However, the article's emphasis on scientific peer review ignores the fact that Congress is largely composed of generalists and of necessity is responsive to a multitude of constituency interests, needs and pressures. As a consequence, public policy decisions are not and cannot be arrived at solely on the basis of scientific merit, although such review is useful for matters of technical complexity. Public policy decisions must be based on economic, social and political factors as well. The simple fact is that Congress too is subject to review, that of the public it serves.

In taking an example from the highly technical and often emotionally charged arena of nuclear energy, the author sought to illustrate that a proper peer review of the facts surrounding the need to develop a commercial breeder reactor and carry out a technology demonstration through the Clinch River Breeder Reactor Project, would show that there is "no reason to pursue commercialization of this technology."

It is ironic that the author chose to underscore this issue with a technical subject about which there are such highly diverse yet authoritative opinions. The article's conclusions, based on the opinions of one segment of the scientific community, require a healthy dose of faith in the "science" of predicting the future electric-energy needs of this country. When faced with the responsibility for—and direct accountability to—the public, a policy decision to support the financing of CRBR and continued development of the breeder concept (as was recently made by the Congress) can be arrived on the following basis:

- While the scientific community is in discord over the need for a commercial breeder, they do at least agree on our technical ability to build one;
- There are non-technical factors which demand consideration.

The scenario is rather simple. If there is a low electric growth for the next 20 years, as the author contends, or if a revitalized economy does not dramatically push up electric demand, then cancelling the development of CRBR would be warranted.

If, on the other hand, there is greater than predicted electric growth because of a revitalized economy or innovative ways to use electricity efficiently to displace oil, and if the as-yet-unproven new technologies fail to provide the additional quantities of electricity our society needs, then *not* having a proven, developed breeder technology available could invite severe economic penalties.

Because nuclear breeder technology is well-established in this country and around the world and because breeders can provide vast amounts of energy according to the evidence of other nations' successes and the best scientific review, it appears to be prudent insurance to support a significant program.

In arriving at this social/technical policy decision it seems clear that the relatively modest financial costs of having a proven source of energy available far outweigh the incalculable social and economic costs of not having enough. In this case, the confidence of the scientific community in projections (in reality, best-educated guesses) of the need for more electricity should moderate the vigor with which we pursue such technologies as the CRBR, but should not dictate the demise of such technologies.

The author, at least by implication, overlooks the importance of other social and political ingredients in public-policy decisions. Indeed the author denigrates "national prestige" as a justification for engaging in the CRBR development.

While national prestige may be difficult to measure and quantify, it does play an important role in public-policy decisions; such is the nature of governments and people.

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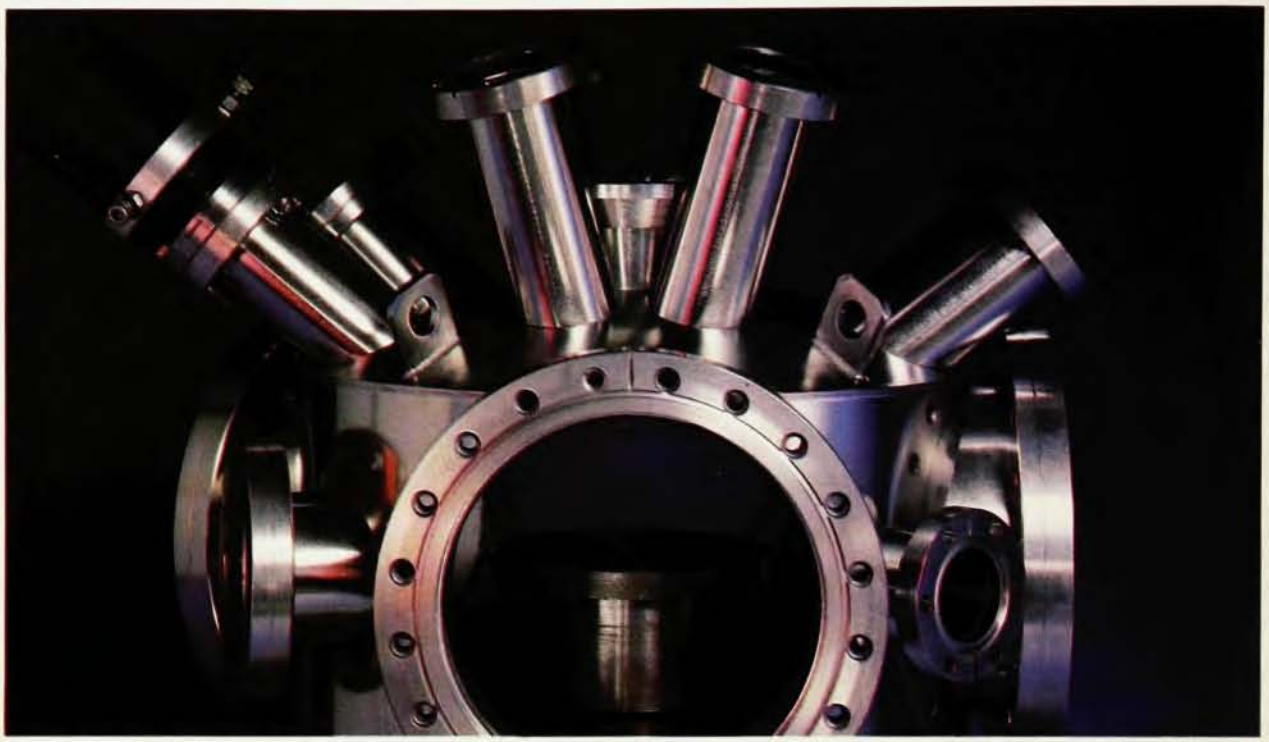

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strong platform necessary from which to negotiate more effectively nonproliferation goals. This philosophy of negotiating from a position of strength and leadership is in fact a cornerstone of the President's recently announced nonproliferation policy.

The issues relating to both commercial nuclear energy and the limiting of weapons proliferation remain critical and demand unprecedented public involvement from the scientific community. But it is an involvement that the scientific community should approach with the understanding and acceptance that non-technical factors often carry equal or greater weight than that of scientific merit.

Nonetheless, improving interactions between science and government remains an important facet of national dialogue, and the Congress always welcomes the well-considered opinions of the scientific community. Indeed, it would be of benefit to us all if the scientific peer-review process were made more effective in public policy areas and especially if it were apparent to generalists in Congress and the public, how it worked.

MARILYN L. BOUQUARD
Subcommittee on Energy Research and Production
Committee on Science and Technology
US House of Representatives
Washington, DC
8/81

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Frank von Hippel argues (July, page 34) that government reports often ought to be peer-reviewed more thoroughly, but he says nothing about reviewing the reviewer. He gives as one of his examples the American Physical Society critique of the Reactor Safety Study. The APS study estimates that a very serious reactor accident might cause 10 000 additional delayed cancer deaths among the 10^7 people exposed, mostly to cesium-137 deposited on the ground. The total exposure is 7×10^7 man-rems spread over 30 years, for an average exposure of 7 rems per person or 230 mr per year. Of those exposed, about $\frac{2}{3}$ receive an integrated dose below 10 rem, $\frac{1}{3}$ a dose above 10 rem. The estimate of 10 000 additional cancer deaths is based on a strictly linear dose-response relation, with 1.3 cancers produced per 10^4 man-rems of exposure.

Von Hippel does not indicate that in applying the linear hypothesis at the dose rates of several hundred mr per year, at which the bulk of the dose is delivered, the APS study goes much beyond what is known. Though the uncertainties in use of the linear hypothesis are acknowledged in the APS study, these caveats and uncertainties (which are enormous at these low dose rates) tend to be overlooked in the final critique. The strong impression remains from von Hippel's paper that the Reactor Safety Study grossly underestimated the number of delayed cancer deaths; a more accurate statement would be that if the linear hypothesis is correct, then the Reactor Safety Study has underestimated the number of deaths that would result from the postulated release of radioactivity. The size of the release may be substantially overestimated if the evidence from Three Mile Island that water tends to hold up cesium iodide is sustained by further observation.

I believe von Hippel's points would have been stronger had he conceded that the peer reviewer, just as what is reviewed, is fallible.

ALVIN M. WEINBERG
Institute for Energy Analysis
Oak Ridge, Tennessee
8/81

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Frank von Hippel's article "The Emperor's new Clothes—1981" states, regarding the (Nuclear) Reactor Safety Study (RSS) and the "Lewis" Report *Risk Assessment Review Group Report to the U.S. Nuclear Regulatory Commission* (NUREG/CR-400), that: (1) The NRC response to the Lewis report was "to reverse its position completely and publicly" with respect to its former support of the RSS; (2) this was a triumph of the peer review system. Implicitly throughout his account, von Hippel sees justice to have finally prevailed in unmasking the calculations of accident probabilities in the RSS that he characterizes as "so uncertain as to be virtually meaningless."

The NRC, the public press and von Hippel treat the Lewis Report with very selective inattention.

The *body* of the Lewis Report says, in its main points:

- ▶ There were some bad statistical treatments (such as, the "square-root bounding model").
- ▶ The error bounds should have been considerably larger (both up and down).
- ▶ Some serious common-mode failures may have been overlooked.
- ▶ The short-term and long-term fatalities were somewhat muddled, or hard to distinguish.
- ▶ WASH 1400 in some ways was *too* pessimistic—for example in considering the effects of beneficial actions by people, hard to predict or quantify.
- ▶ The peer review was too flimsy.
- ▶ WASH 1400 should not be used blindly, just as a source of numbers to quote, in assessing reactor safety. Different reactors are different, other uncertainties need to be evaluated, and so on.
- ▶ Nevertheless, WASH 1400 was a valuable piece of work, and advanced

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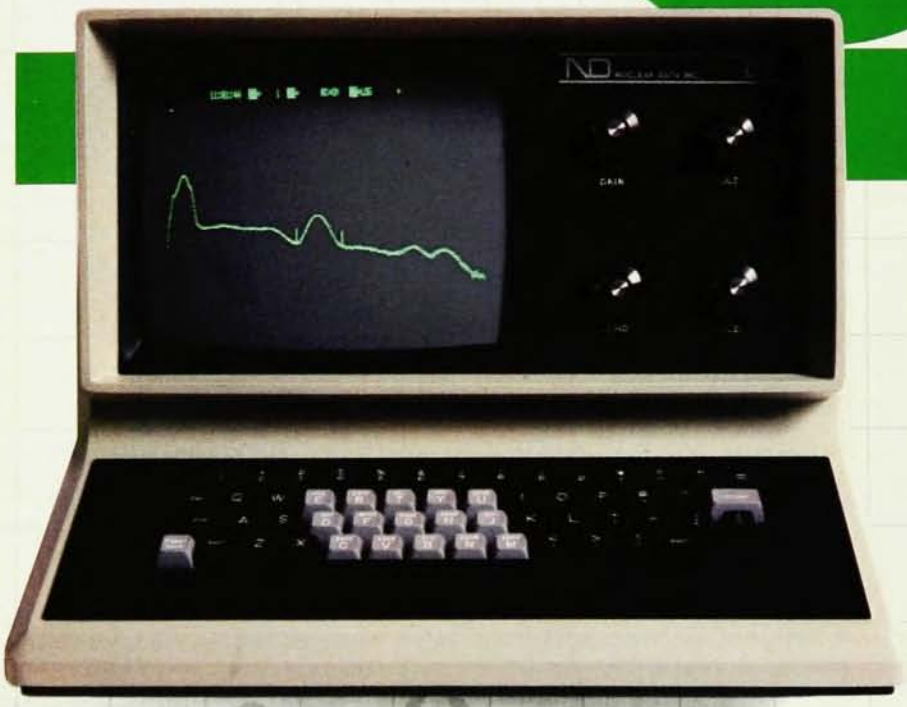

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the methodology and laid a good foundation.

▶ In a most unusual section (pages 41–43) the report deals with allegations by Dan Ford of the Union of Concerned Scientists that the workers on WASH 1400 acted dishonestly, in suppressing information. I quote the last paragraph of the Lewis Report on their page 43; it dismisses the UCS allegation thus:

> We consider allegations of dishonestly to be deserving of the most serious and careful study; each must be backed up by evidence commensurate with the seriousness of the charge. It is understandable that an incident such as this should raise doubts among the skeptical as to the overall honesty of the RSS effort. However, this allegation of dishonesty is without merit in our view. Furthermore, we do not accept the argument that this incident can be used as a "case study," generalizable to reveal lessons as to the intellectual integrity of the larger RSS effort.

Turning now to the summary of NUREG/CR-0400, we find most of the above sentiments expressed as follows, in this order:

▶ WASH 1400 was a conscientious and honest effort.

▶ There are a number of sources of both conservatism and non-conversatism, which are difficult to balance.

▶ The dispersion for radioactive material and the biological effects model should be improved and updated before they are applied in the regulatory and licensing process.

▶ The methodology, which was an important advance over earlier methodologies applied to reactor risks, is sound and should be used more widely.

▶ WASH 1400 is inscrutable—difficult to follow—executive summary is a poor description.

▶ The fault-tree/event-tree methodology is sound, and both can and should be more widely used by NRC. The implementation in WASH 1400 was a pioneering step, but leaves much to be desired.

There was no mention of the UCS claim in the summary.

Now what did the Nuclear Regulatory Commission do, on receipt of this report? They wrote to every known recipient of WASH 1400, saying that the commission has reexamined it's (*sic*) views regarding the study in light of the Review Group's critique. In their words:

"While praising the study's general methodology and recognizing its contribution to assessing the risks of nuclear power, the Review Group was critical of the Executive Summary, the procedure followed in producing the final report, and the calculations in the body of the report."

(Then followed 31 lines of "major failings" and 16 lines of "major achievements—despite its shortcomings").

Then the commission said it:

"withdraws any explicit or implicit past endorsement of the Executive Summary"

"agrees the peer review process was inadequate—take whatever correction action is necessary . . ."

". . . accepts the Review Group's conclusion that absolute values should not be used uncritically . . . The Commission does not regard as reliable the Reactor Safety Study's numerical estimate of the overall risk of reactor accident."

". . . commission correspondence involving WASH 1400—being reviewed and corrective action as necessary will be taken"

". . . expects the staff to make use [of component parts of the study] as appropriate, that is where the data base is adequate . . ."

". . . has provided additional detailed instructions to the NRC staff concerning continued use of risk assessment techniques . . ."

What happened in the public press? The general account was that the Lewis Report had discredited the Reactor Safety Study and the NRC had accepted the verdict. The Union of Concerned Scientists claimed victory. Von Hippel appears to support these selectively distorted interpretations.

The rest of von Hippel's article is better.

DAVID J. ROSE
Massachusetts Institute of Technology
8/81 *Cambridge, Massachusetts*

THE AUTHOR COMMENTS: Representative Marilyn Bouquard doesn't seem to realize how enormous the growth in US electicity demand would have to be to justify a commitment to the breeder reactor wihtin the next decades. It is shown in reference 1, for example, that the US could build up in the next 30 years to an annual level of *nuclear* generated electric power production equal to 1.6 times *total* US electricity production in 1980 and maintain that level of nuclear electricity production until well past the year 2080 before the grade of uranium ore being mined in the US fell low enough to justify economically commercializing the breeder. The most recent upper bound projections by the Department of Energy of US nuclear power growth over the next 40 years are approximately one half this large,[2] pushing the breeder that much further beyond any reasonable planning horizon.

I must confess also that I cannot understand Bouquard's argument that it would provide the US a "strong

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platform to more effectively negotiate non-proliferation goals" to take the lead in commercializing an unnecessary and uneconomic technology which involves the separation and processing of huge amounts of nuclear weapons materials.

In this context the continued high priority given to the plutonium breeder in the US energy research and development program can only be understood as a triumph of the porkbarrel politics to which Bouquard seems to refer when she states that Congress "of necessity is responsive to a mulitude of constituency interests, needs and pressures." Indeed, the fact that she is both the Chairman of the House Subcommittee on Energy Research and Production and the representative from the Congressional district in which the Clinch River Breeder Reactor plant is to be built suggests that in this area Representative Bouquard knows whereof she speaks.

I appreciate the representative's statement at the end of her letter that "Congress always welcomes the well-considered opinions of the scientific community." This statement does not adequately describe, however, the difficulty of reaching Congress with well-considered policy analysis when political commitments have already been made. In the case of the breeder, for example, the fact that breeder advocates have for years chaired the Congressional subcommittees which decide the nation's energy research and development priorities has resulted in analysis unfavorable to the breeder program receiving only token hearings by Congress or none at all. The result is that the bad news about the breeder programs has had to filter into Congress "through the cracks" with concerned individuals educating members of Congress and their aides one at a time.

The reader might obtain the impression from Alvin Weinberg's letter that it was the APS Study Group on Light Water Reactor Safety which first assumed the linear proportionality between radiation dose and cancer risk at low radiation doses. This is not correct. For at least a decade the "linear hypothesis" has been the most frequently used basis for extrapolating to low doses the cancer and genetic effects of radiation observed at higher doses.

Specifically, at the time of the APS group's review of the NRC's Draft Reactor Safety Study in the summer of 1974, the standard reference in this area was the 1972 report published by the National Academy of Sciences, *The Effects on Populations of Exposures to Low Levels of Ionizing Radiation*. This re-

port used the linear hypothesis and its estimates of the long-term effects of low doses of radiation were used in both the *Draft Reactor Safety Study* and in the report of the APS group. The difference between the two groups was not in the calculation of the relationship between radiation doses and their long-term effects, but, as I explained in my article, in the calculation of the doses. Here, as I also explained, the Reactor Safety Study group ultimately accepted the APS group's correction.

Weinberg should have made clearer in his letter, therefore, that he believes that *both* the NRC Reactor Safety Study and the APS group may have made a major error in using the linear hypothesis. This subject is the focus of a very long-running debate in the community of scientists studying radiation-health effects—a debate in which both Weinberg and I have been interested spectators.

The current state of play in this controversy is briefly as follows: The most recent (1980) National Academy of Sciences (NAS) report on the subject[3] uses a "linear–quadratic" model which, for an exposure to 10 rads of whole body gamma radiation, leads to an estimated cancer death risk of 0.08–0.23 percent. The corresponding number predicted from the linear coefficient used by the APS group was 0.13 percent.[4] The NAS linear–quadratic hypothesis was derived primarily, however, from a comparison between the incidences in the radiation-induced cancer observed at Hiroshima and Nagasaki. Recently it has been established that the radiation doses at these two cities had been incorrectly calculated. (PHYSICS TODAY, September, page 17 and reference 5) It is obvious, therefore, that we have not yet heard the last word on this matter.

David Rose accurately summarizes the Risk Assessment Review Group's report on the Reactor Safety Study to the Nuclear Regulatory Commission and the NRC's subsequent public statement on the matter. I must comment, however, on his conclusion at the end of his letter that may account in "The Emperor's New Clothes—1981" supports the "distorted interpretations" in the press that the Risk Assessment Review Group's report "had discredited the Reactor Safety Study and the NRC had accepted the verdict."

I think that the matter can be clarified by distinguishing between the four different layers of the 2000-page-long Reactor Safety Study: The Executive Summary (12 pages), The Main Report (267 pages), the Appendices, and the Technical Appendices that were attached to many of the Appendices.

The Executive Summary of the *Reactor Safety Study* and its figures comparing the risks of reactor accidents with other risks is the only part of the report

which was seen by all but a relatively small number of people with a professional interest in reactor safety. It can only be characterized as a public relations document and was heavily used as such by the electrical utilities and the nuclear industry to convince the public that the risk from reactor accidents is absolutely trivial. There is no doubt that this Executive Summary *was* discredited as a result of the Risk Assessment Review Group's report and by the NRC's public statement. On the other hand, many people—including myself—find the bottom layer on which the Reactor Safety Study was built, the technical appendices to the appendices, quite useful. They were generally written by specialists and, while they contain errors and omissions, were *not* discredited by the Risk Assessment Review Group. The intermediate layers of the Reactor Safety Study are intermediate in intellectual integrity between these two extremes.

As a final comment I would like to reiterate my personal opinion that the Emperor has never been less well clothed than today. This applies especially to the area of nuclear weapons policy. Never has there been a greater need for large numbers of concerned scientists to educate themselves about the real dangers and opportunities associated with modern technology and then to use that knowledge to empower concerned citizens and policymakers to define and bring about a more hopeful future.

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FRANK VON HIPPEL

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