and the physics profession will suffer dearly if that time comes. Indeed, given the 50–100–year time scale now being suggested, why the air of wartime urgency that seems to drive the fusion reactor program? It would be far better if something like a third of the current US fusion budget were spent addressing these materials and engineering concerns by supporting programs manned by professionals trained and experienced in those topics.

If, by some unforeseen and happy chance, a radically different and more promising concept for fusion were discovered, at least the materials situation would be in much better shape. Toward that end, the scientific exploration of fusion could and should still continue, though at a more deliberate rate.

Rockwell International's William Parkins came to the unwelcome conclusion in the 1970s that fusion power will not be achieved practically.[2] And so does a recent assessment by the *Energy Economist*,[3] of the European Union's fusion program, all under the telling headline "Refugees from Reality." The great British physicist and one-time energy minister Walter Marshall has been quoted[2] as saying, "Fusion is an idea with infinite possibility and zero chance of success." After 25 years of service on advisory committees for several national laboratories for condensed matter science, materials and metallurgy, and several years of directing industrial research on composites and ceramics, I firmly agree with him.

Whereas controlled thermonuclear fusion exploration was a necessary, heroic venture into unknown science in its early days, and its potential as an energy source could be dreamed of, today we have 40 years of accumulated experience from which to demand a realistic critical evaluation of its potential societal utility. That requires that we physicists now look beyond our science to the practical aspects when we make claims, as demanded by our tradition of objectivity, in that continued public support for physics depends on the reliability of our public statements. Richard Feynman ended his appendix to the Challenger report by warning us: "For a successful technology, reality must take precedence over public relations, for Nature cannot be fooled."[4]

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It is unfortunate that the fusion community continues to perpetuate the myth that fusion is a foreseeably practical endgame for our energy resources. With the present concepts, it certainly is not. It is, of course, a fascinating scientific experiment and should be evaluated and supported in that light.

The feasibility of controlled fusion as a practical energy source has been viewed with deep skepticism by many who participated in the development of nuclear fission power plants. In the 1970s the Electric Power Research Institute (of which I was then president) maintained a fusion program until it became evident that fusion concepts could not be expected to achieve the basic requirements for commercial electricity generation. The Central Electricity Generating Board of the UK came to a similar judgment about the same time.

In the past decade, the fusion community has tried to establish the plausibility of future power capability on the basis of the Aries and Joint European Torus (JET) reviews, but they were superficial analyses with oversimplified assumptions, with promising performance to be demonstrated after a 40-year development program. This situation is very reminiscent of the optimism based on conceptual designs that pervaded the fission community in its early days. Back then, we were at least encouraged by the pilot reactor demonstrations of the basic physics and controllability of fission. Unlike the case with fusion, it was relatively easy to assemble working nuclear cores. Only as the development of practical power plants proceeded did the major engineering barriers to the safe, reliable and economic operation of fission reactors become apparent. They included the degradation of materials exposed to neutrons, corrosion and thermal cycling; shutdown heat removal; and the system interactions initiated by component failures that required the addition of complex defense-in-depth subsystems, including a containment building. The balance of plant reliability, public safety and economics was achieved only with difficulty.

It is now obvious that fusion power would face similarly severe barriers, even if the science of fusion controllability were demonstrated in coming decades. And it is difficult to envision fusion power ever approaching the economics of commercial fission power plants, even with an optimistic view of technical ingenuity.

In collegial support of national scientific R&D, criticisms of the future potential of fusion have been considerately muted by those knowledgeable about fusion's limitations. Even the utility industry has carefully limited its comments to merely a statement of its requirements. When public support for science was very generous, we could consider fusion research as part of our nationally supported exploration of science's frontiers, without regard to its eventual success. And scientific knowledge has been a valuable by-product of the fusion program. Unfortunately, with the stretching of the fusion program and the current national budget constraints, this is no longer the case. Most seriously, the present administration is publicly assuming fusion's long-term success as a policy basis for diminishing the development support for more realistic long-term alternatives, particularly nuclear fuel recycling and breeding.

The public has become increasingly cynical about the intellectual integrity and reliability of the physics community, and fusion is a case in point. It is now time for the knowledgeable community to more fully disclose the uncertainty of fusion as a national energy source, so that the public is not further misled and the politicalization of this area of science is not continued.


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# Four Factors and One Criterion Are Key to Improving Peer Review

Some major US research-sponsoring agencies are making concerted efforts to improve their proposal evaluation and selection processes. PHYSICS TODAY (January 1997, page 52) and *The Scientist* (9 December 1996, page 1) report efforts by the National Science Foundation and the National Institutes of Health, respectively, to modify their proposal evaluation processes mainly by altering the evaluation criteria. Both of these articles imply that the specific criteria selected represent a dominant factor in the quality of the proposal evaluation process.

I believe the less tangible aspects of the research evaluation process are far more important in determining product quality than a specific mix of review criteria, although review criteria selection should receive careful consideration. My belief, based on examining the peer review literature, conducting many peer review experiments and managing hundreds of peer reviews, leads me to offer the following conclusions about the factors critical to high-quality peer review (whether of proposals, programs, manuscripts, faculty or dissertations).[1,2]

The most important factor is the organization's commitment to high-quality reviews, and the associated emplacement of rewards and incentives to encourage such reviews.

The second most important factor is the review manager's motivation to conduct a technically credible peer review. The review manager guides the questions and discussion in a panel review, summarizes the reviewers' comments and recommends follow-on actions. In some organizations, the review manager has the latitude to select the review process and criteria, and in all organizations presently has the latitude to select reviewers by a nonrandom process. If the review manager does not follow, either consciously or subconsciously, the highest standards in selecting reviewers, the review's outcome could be substantially influenced before the review process begins.

The third most important factor consists of the reviewers' competence and objectivity. Each reviewer should be technically competent in his subject area, and the competence of the total review group should cover the multiple facets of research issues (specific research area reviewed; allied research areas; technology, systems and missions potentially impacted by the research). In addition, the group's expertise should not be limited to subdisciplines of the specific research area under review (which addresses the question of whether the job is being done right), but should be broadened to the area covered by the overall proposal's highest-level objectives (which addresses the question of whether the right job is being done). With this broadened structure, the review group could address the larger question of whether the right job is being done right, and would be more likely to provide equitable consideration to revolutionary new paradigms.

The fourth most important factor is evaluation criteria selection. For evaluating basic research proposals, the three main criteria are research merit, research approach and team quality.[1]

For research sponsored by a mission-oriented organization, a fourth criterion related to mission relevance is useful. To ensure this mission relevance criterion does not filter out the more basic research oriented proposals, a very liberal interpretation of mission relevance is necessary. For basic research, a nearer-term relevance criterion (such as transition or utility) correlates better with overall proposal quality score than does a longer-term criterion.[1]

Use of an essential final overall research quality criterion makes it possible to incorporate the effects of unlisted criteria that the reviewer feels may be important for considering a specific proposal. For example, suppose reviewers felt an agency proposal was more appropriate for sponsorship by industry than by government. Then the proposal could receive a low overall rating, even though the listed component technical criteria were rated highly.

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# Avoid Bumpy Road to Removing Lake's $CO_2$; Just Pay the Piper

In his letter proposing that limestone be used in an acid-neutralization scheme for Lake Nyos (February, page 15), Dan Golomb accurately describes the quantities needed and some of the potential problems related to its use. Indeed, limnologists have looked into, but generally rejected, this approach.

One major obstacle is Cameroon's transportation system. The terrain around Nyos is granitic, so any type of carbonate rocks would have to be imported over long stretches of roads that not only are poorly maintained but can disappear during the annual monsoon. Imagine Golomb's thousand limestone-filled trucks trying to negotiate the road shown in the accompanying photo (taken by my colleague Jack Lockwood).

Golomb mentions the expense of pumps and fuel as being a problem in the currently planned mitigation scheme whereby deep, gas-rich water is piped to the surface of the lake and degassed. But it is precisely the insignificance of this expense that makes degassing by pipe attractive; indeed, that would be the preferred technique even if a large limestone quarry were proximal.

As mentioned in the article by Ray Ladbury (PHYSICS TODAY, May 1996, page 20), the pumps would be needed only to initiate the flow of bottom water toward the surface. Once gas exsolution reaches a sufficiently vigorous rate, the fluid would flow (rapidly) up the pipe due to buoyancy. The successful pipe-degassing tests carried out at both Lake Nyos and its smaller cousin, Lake Monoun, used generators and pumps of fairly modest capacity. Scaling up to more or bigger pipes as needed to fully degas the lakes would cost much less than importing limestone. Once installed, the pipes should function without maintenance for many years, a key condition for success in the third world. After completely degassing the lake, the pipe installation would also be available for continuous flush-