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Research Institute *Powering Progress through Innovative Solutions*

May 8, 1997

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Dear John:

I am writing to call your attention to the enclosed published correspondence (Physics Today) on the lack of realism in the fusion concept as a potential energy option in our national planning. As this is undoubtedly germane to the responsibilities of your task force, I thought you should be aware of this issue.

The essence of the Physics Today letter exchange is that some of us familiar with the engineering barriers unique to fusion and their unavoidable economic consequences, particularly those resulting from the effects of the very high energy neutrons and of the impossibility of extracting heat from within the reacting plasma, are convinced that the fusion power plant concept should not be included as an option in the strategies proposed to realistically meet our future national energy needs. Achievement of commercial operability, safety, and economics is not engineeringly feasible in the foreseeable future. This question of power plant feasibility and its engineering issues has been deferred by the many review committees that have focused primarily on the more timely complexities of plasmas and the scientific direction of fusion research. The Physics Today letters from the fusion community makes this evident by their concentration on the progress and hopes in plasma physics.

The question is not whether fusion research should continue but rather whether fusion power should be included in the mix of energy options that this nation should depend on for meeting changes in our national energy needs in the foreseeable future. Such a situation might arise from a decision to drastically reduce U.S. fossil fuel use for a variety of reasons, which would require a major increase in conservation and nonfossil sources.

The policy damage that has already occurred by the perception of fusion as being "almost-at-hand", a perception fostered by the fusion community, has been the diversion of attention and support from continuous improvement of nonfossil energy sources. The program to demonstrate a fusion reactor is now ending its fifth decade. The always repeated hope has been to develop in several decades a

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workable concept that could be beneficially used for the large scale generation of electric power. As briefly summarized in our beginning and ending letters in Physics Today this hope will not be fulfilled. Unfortunately, fusion has provided the political community a visionary escape from attention to the long range realities of current energy options.

Enclosed is a letter from Prof. Kenneth S. Pitzer, who gave the initial approval to the Princeton fusion project when he was Director of Research for the A.E.C. in 1949-51, expressing support for our views. We have not solicited supporting letters.

We share with our fellow physicists a recognition of the progress that has been made in the science of managing plasma phenomena. It is evident that this domain of science continues to be challenging and should be a continuing part of physics research.

I hope these views are helpful to the deliberations of your task force.

Sincerely,

Chauncey Starr

Chauncey Starr
President Emeritus

c: Joan Bok
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March 18, 1997

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Gentlemen:

I was very much interested and pleased to see your three LETTERS in Physics Today for March 1997. They provide very important information as well as sound conclusions for an overdue abandonment of "Fusion Power" as a practical goal and basis for major project expenditures.

It is now nearly 50 years since I first approved the initial project at Princeton when I was Director of Research for the A.E.C. in 1949-51. I frequently say in informal conversation that the prospects for fusion power have grown less promising and more distant as the years have passed and we have learned more. I was aware in a general way of the difficult and dangerous aspects of a fusion power plant outside of the thermonuclear unit itself, but I did not study these problems in any detail. Thus, I welcome very much your evaluations and reports.

Initially, controlled fusion was an exciting possibility that fully merited scientific exploration even at considerable cost. But, as each of you say in your concluding paragraphs, that time is long passed and any further expenditures should be limited to those merited on a "pure science" basis. I hope that you will express your views in other media in addition to "Physics Today."

Yours very sincerely,

Ken Pitzer

Kenneth S. Pitzer
Professor of Chemistry

LETTERS

Insurmountable Engineering Problems Seen as Ruling Out 'Fusion Power to the People' in 21st Century

It is now a half-century since serious work was initiated on developing a thermonuclear fusion reactor. Since then, a continuing series of experimental projects has been proposed for achieving power-in versus power-out breakeven. Projects that have been carried out have contributed to our scientific understanding, but they have been promoted primarily as steps leading to the use of fusion reactors in the large-scale generation of electric power. (See PHYSICS TODAY, "Letters," December 1996, page 11, and January 1997, page 95.)

That goal is unattainable. Although 50 years of research and many billions of dollars spent have failed to achieve the plasma conditions necessary for breakeven, the use of fusion reactors for central station power generation is made hopeless solely because of engineering considerations, not the physics involved. This is true regardless of the method of plasma confinement employed and of the choice of thermonuclear reaction.

The principal engineering factor eliminating any possible future application of fusion power is the unacceptably high capital cost that would be mandated by the fact that heat cannot be extracted from within the reacting region (as is done in fossil fueled boilers and nuclear fission reactors), but must be gathered outside of the plasma. Engineering limitations on maximum heat transfer rates and on the maximum average-to-peak ratio of the heat transfer rate would require the fusion reactor to be of huge dimensions for the relatively small amount of power produced.

Any workable plant design having an electric power output of interest to utilities would require gargantuan dimensions, expensive materials and a major amount of fabrication on site. The charges against capital investment alone would lead to a cost of power several times that available from traditional methods of generation. No utility would ever accept such an economic penalty, regardless of other presumed advantages. Nor would the public consumer.

It is unfortunate that this inherent

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restriction on the engineering extraction of plasma-produced energy eliminates the practical application of fusion. There are means to alleviate this limitation, such as by increasing the heat dumped to the divertor strike plates and by absorbing heat in a neutron-slowing blanket. For a given power output, heat deposited in a blanket reduces the amount of heat that must be removed at the vacuum vessel wall. But then every square meter of wall has the added cost of one to two cubic meters of solid and expensive neutron blanket and shield. None of these means can significantly reduce the capital cost per unit of electrical output.

The use of any thermonuclear reaction that releases neutrons results in yet another insurmountable engineering obstacle. The only even hopeful candidates, the deuterium–tritium and deuterium–deuterium fusion reactions, both release energetic and damaging neutrons. Aside from inducing radioactivity in the structure, these neutrons would cause a gradual dilation and embrittlement of the huge vacuum vessel. No material can provide an operating life that does not require periodic vessel replacement. And no electric utility would ever accept having to replace such a gigantic, radioactive and almost inaccessible component during the lifetime of the plant.

What is more, even if the vessel never required replacing, the long-term demands placed upon it would not be within the realm of possibility for the design engineer. The power plant's operation would depend on maintaining the vacuum integrity of a single vessel with a major dimension of at least 15 meters, fabricated with hundreds of joints and connections with auxiliary systems and subject to thermal stresses from variable and very high temperatures. Leaks would be unavoidable. Locating and repairing them by remote means in an inaccessible geometry would not even be imaginable to the power plant operator!

The arguments presented here were valid and made decades ago. Indeed, they were clearly set forth in a series of three articles written by William D. Metz over 20 years ago. Published in *Science,*[1] the articles garnered for Metz the American Institute of Physics—United States Steel Foundation Science-Writing Award. To quote his second article, "It sometimes seems necessary to suspend one's normal critical faculties not to find the problems of fusion overwhelming."

It is evident that, although the physics of the fusion reactor may eventually be made to work in principle, the engineering will not be made to work in practice. Engineering realities will eliminate fusion as an energy source for central station power just as they have the application of many other once-promising concepts, such as the nuclear fission reactor as a means of rocket propulsion and magnetohydrodynamics as a means of electric power generation.

In conclusion, it is not logical to continue to divert a substantial fraction of our physical sciences resources to the hopeless objective of fusion power. Certainly there is still much to be learned about the physics of plasmas, but no proposed experimental project should now be based on the premise that man-made thermonuclear fusion will contribute to meeting our future energy needs.

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"LOOK', I WOULD SAY TO LEONARDO. 'SEE HOW FAR OUR TECHNOLOGY HAS TAKEN US'. LEONARDO WOULD ANSWER 'YOU MUST EXPLAIN TO ME HOW EVERYTHING WORKS.' AT THAT POINT, MY FANTASY ENDS."

Fusion power to the people? Given that the first-mile mark—the scientific demonstration of "breakeven, plus"—is still unattained, the further road to commercial fusion power will be difficult, if not impossible.

The obstacles ahead appear to be technically far more imposing than those encountered on the road from the University of Chicago stadium to today's economically marginal commercial nuclear fission power. To begin with, there is a startling lack of a foundation of information on materials for fusion technology in the US, as made very clear in the 1993 Conn report.[1] In addition, the current level of support for materials research and facilities is totally inadequate, perhaps by a factor of hundreds, compared to the expenditures for fusion hardware and experiments; worse still, there seem to be no plans to change the priorities. Also, neither experienced design engineers nor cost estimators have been significantly involved in the US fusion program to guide the enthusiastic but unrealistic fusion power protagonists.

This situation will be a recipe for catastrophe when the time comes to transfer science to usable technology;

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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4. Quoted in Richard P. Feynman, *What Do You Care What Other People Think?: Further Adventures of a Curious Character*, W. W. Norton, New York (1988), p. 237.

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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

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LETTERS

Development of Fusion Power Seen as Essential to World's Energy Future; Critics Respond

Curiously, the US fusion research program seems to be a Rorschach test that divides external viewers into disparate groups of technological optimists and pessimists. Individuals who are out of touch with the fusion program seem to have exaggerated views of either the promise or the problems of the program. The three letters about fusion that appeared in the March issue of PHYSICS TODAY (page 15) come from those who overstate the problems.

On the other hand, we, as members of the fusion research community, regard the fusion energy effort as a very challenging task that is well worth doing and, based on the significant progress made to date, is likely to succeed.

In fact, the record of accomplishment of the fusion program has been outstanding and steady. One after another, significant challenges—both scientific and engineering—have been overcome as they have been encountered. For example, turbulent transport in plasmas once seemed hopelessly complex and the trends discouraging. But in the early 1980s, new operating regimes with reduced transport were discovered in tokamaks. In the late 1980s, fusion scientists undertook a major national initiative to understand transport and learn ways to reduce it. Today, we have highly promising physics-based models of transport and are succeeding in experiments to control the transport of particles and energy in plasmas.

The list of accomplishments is already long, and is growing. Multimegawatt neutral beams and other means have been used to heat plasmas to tens of kilo-electron volts. Ingenious operating regimes have been devised to control instabilities in plasmas with many degrees of freedom. Remarkable approaches for diagnosis and feedback control have been developed and applied. Clever techniques allow current to be driven in tokamaks or even allow the plasma to generate its own sustaining current. Some problems peculiar to the use of tritium–deuterium fuel have turned out to be smaller than anticipated, while others remain to be investigated. Meanwhile, sophisticated yet practical theoretical, experimental and engineering developments of general usefulness have resulted, such as a formulation of soliton waves and the development of theoretical models of turbulence, imaginative diagnostics to measure magnetic fields in plasmas with fast moving ions and advanced techniques for fabricating superconducting magnets.

One after another, external reviewers have commended the fusion research program—one of the most reviewed of all federally sponsored scientific programs—and occasionally have expressed some surprise in discovering just how advanced and high quality it is. One of the most independent and thorough reviews was conducted by a panel of the President's Committee of Advisers on Science and Technology (PCAST) in 1995. In its report, the panel identified various significant challenges to achieving fusion power, but saw no insurmountable technological impediments.[1] It also pointed out that the prospect for a practical energy source made the fusion effort well worthwhile.

Does the record of accomplishment in fusion research guarantee future success? Of course not. Followers of philosopher David Hume have long questioned the paradox of induction, but we have no better basis for making decisions. Rather than surrendering in the face of uncertainty, fusion researchers and their reviewers do look into the future and ask how perceived scientific and technological hurdles can be overcome or avoided.

How do we move forward from the present? Although the history of science offers some striking examples of solutions being developed before the problems they solved were even recognized, generally speaking, problems are not solved until they are encountered. To overcome scientific—and especially engineering—problems, one needs to build and operate experimental facilities to test ideas. In fusion, no major experimental facility has been built in the US in more than a decade.

It is far from certain that fusion will be sufficiently developed in time to meet a large share of the worldwide energy demand by the middle of the next century. Nevertheless, there are good reasons to believe that success will be achieved, particularly in view of Europe and Japan's strong commitment to fusion R&D. The technological challenges are real and large, but we would argue that they are no larger or more insurmountable than the problems associated with every other means of supplying reliable, affordable energy to a world of ten billion people, each of whom will demand at least a quarter as much energy as each American now consumes.

In whatever way this enormous challenge is eventually met, the energy picture of the world will be very different from what it is today. It is our view that fusion should and will be an essential part of that 21st-century picture.

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William Parkins, James Krumhansl and Chauncey Starr argue that electric power production from fusion reactions is impractical. Some of their statements are erroneous and some indicate that they are uninformed about the status and accomplishments of fusion research throughout the world during the past decade.

While we—members of the ARIES team—agree that fusion technologies have not been fully developed, measurable progress has been and is being made. With declining budgets for energy research during the past several years and a decreased sense of national urgency about exploring alternative sources of energy, the US fusion program has been redirected to emphasize the further exploration of fusion science, rather than the acceleration of development of fusion technologies. Nevertheless, the program has maintained active research on advanced materials and strong collaborations with our international partners on technology development—for example, on the International Thermonuclear Experimental Reactor (ITER)—and also has engaged in a significant effort to advance research on the design of fusion power plants.

A series of such power plant conceptual design studies during the past 15 years (for example, ARIES) has been used to guide fusion research. Twenty years ago, fusion power plants were envisioned as large, pulsed systems based on conventional steel technology with complex geometries that were extrapolated from then-existing experimental devices. Since then, the desire for a more commercially attractive product has spurred intensive research that has resulted in smaller machines through improved plasma performance, steady-state operation through new methods of current drive and a better understanding of high-performance, low-activation materials, such as vanadium alloys, which provide both high power-handling and high temperature (hence thermal conversion efficiency)—all of which are essential factors in attaining cost competitiveness.

These conceptual design studies have demonstrated that such fusion power plants would be passively safe (no need for evacuation plans), generate only low-level radioactive waste and have good prospects for repair and maintenance. The studies have involved the participation of substantial industrial partners, such as McDonnell Douglas Aerospace, General Atomics, Stone and Webster and Raytheon Engineers and Constructors, as well as national laboratories and universities. Through such joint industry-university-laboratory partnering, market reality and credibility, as well as state-of-the-art plasma physics and engineering, are being fully addressed.

In the most recent power plant design study, ARIES-RS, guidance from and interactions with US electric utilities and industry leaders resulted in significant progress being made in defining possible solutions for a fusion power station to meet the utilities' criteria of technical credibility and attractiveness as a commercial product. In September 1996, in his dual capacity as chairman of the Fusion Power Plant Studies Utility Advisory Committee and chairman of the Electric Power Research Institute's fusion working group, Steven Rosen of Houston Lighting & Power wrote a letter to the US Department of Energy's director of energy research, Martha Krebs, in support of these activities. Referring to safety, waste disposal and maintainability of the ARIES-RS design, he stated that "the ARIES-RS conceptual tokamak power plant design has many of the features we, as end-users, find attractive in a future power system."

We certainly would not claim that the engineering problems associated with fusion electric generation stations are easy to solve. However, the central problem is not a matter of very large thermal or mechanical loads, in that many commercial systems handle much higher heat and mechanical loads. Rather, at issue are the environmental conditions in a fusion device that require new engineering solutions.

The need to develop new material technologies is not unique to fusion. Most novel major industrial endeavors have required development of new materials (for example, lightweight materials for aerospace and radiation-resistant materials for nuclear fission).

The size of a fusion power plant is not set by the maximum surface heat flux as Parkins argues. About 80% of the deuterium–tritium fusion power is carried by neutrons that leave the plasma and deposit their energy in the power-producing blanket that surrounds the plasma. Thus, most of the fusion power appears as volumetric heat and not as surface heat load. The size of the power plant is set mainly by the efficiency of the magnetic confinement scheme in supporting the plasma pressure and containing the plasma energy—areas in which there have been substantial recent advances. The surface heat fluxes can be high only in certain areas where the plasma exhaust is concentrated on material walls. The physics of plasma boundary layers and plasma–material interactions is under extensive investigation.

In sum, the engineering challenges are well known, and the possible engineering solutions can be found in an extensive and growing literature on the subject.[1]

The ITER activity has resulted in the detailed engineering design of a fusion device with a burning plasma that can be built and operated. Obviously, a substantial amount of effort will be required in the future to develop, demonstrate and optimize engineering components needed for a commercial power plant. The challenges are difficult but they should be judged in terms of the immense payoff of developing commercial fusion power with attractive safety and environmental features. The choice before us is whether to shy away from these problems because they appear difficult, or to follow the vision of leadership from previous generations to provide for future generations.

Reference

1. See, for example, the list of the ARIES team's publications that is maintained at the ARIES World Wide Web site, http://aries.ucsd.edu/ARIES.html.

Farrokh Najmabadi
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Parkins, Krumhansl and Starr offer a provocative argument against the viability of fusion energy. However, to examine their implications that fusion will not be a competitive energy source in the 21st century—and, presumably, beyond—we must address two important questions. First, Competitive compared to what? Second, Which fusion reactor is not competitive—that is, what do the writers envisage as our ultimate commercial fusion product?

After our present reserves of fossil fuels have been eliminated as a result of exhaustion, environmental constraints or sequestering for other, more useful ends, there will remain only two energy sources indigenous to Earth and available for central baseload electricity generation in the long term: breeder fission and fusion. So the question of ultimate viability is really one of the fission breeder reactor versus the fusion reactor. Arguably, fusion comes out ahead in terms of safety and environmental factors, waste disposal, nonproliferation and fuel availability.[1] Therefore, when looking at the competitiveness of fusion relative to fission, the key question is, To what extent do fusion's tangible advantages compensate for the perceived disadvantages of the cost and complexity of the fusion power core? I submit that this question has not yet been satisfactorily addressed by either the world fusion community or its detractors. Moreover, the question *cannot* be answered until our physics research programs have fully identified the concept for our ultimate fusion reactor product.

Parkins, Krumhansl and Starr are no doubt basing their perceptions on the reactor potential of the conventional tokamak, which in fact has proved to be an excellent research tool for studying high-temperature plasmas and which will enable us to examine thermonuclear plasmas realizing significant energy gain. For this reason, the International Thermonuclear Experimental Reactor (ITER),

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a current international design study of a burning fusion plasma experiment, is rightly a tokamak. The conventional tokamak ultimately may not lead to a fully viable commercial reactor for various reasons, including those espoused by Parkins *et al.* However, Parkins is incorrect in declaring that his conclusions are "true regardless of the method of plasma confinement and of the choice of thermonuclear reaction." Rather, I would say there's more than one way to skin a cat. Just what the ultimate way will be for fusion is uncertain at this relatively early stage of development.

The following are just two examples of what can be considered paradigm shifts in methods of realizing fusion energy. First, "inertial fusion energy"—whereby a small fusion fuel target about the size of a pea is compressed with an energetic pulse from a laser or heavy-ion accelerator—provides a route to a power plant concept fundamentally different from a tokamak or other concepts of that class.[2] There would be no large, expensive superconducting magnets exposed to potentially damaging fusion radiation. This unique feature would allow lifetime fusion chambers to be designed with renewable liquid coolants facing the targets, instead of solid, vacuum-tight walls that could suffer damage due to the fusion heat and radiation. This would permit use of low-activation coolants, and structural materials would qualify for on-site, near-surface burial at the end of the plant's life.[3] Furthermore, it could eliminate the need for an expensive R&D program on advanced materials. Thus, Parkins's perceived problems of "thermal stresses," "leaks," "vacuum integrity," "embrittlement," "periodic vessel replacement" etc. would be sidestepped.

Second, there is a class of fusion concepts that, in principle, obviate the need for the very high temperatures required for conventional thermonuclear fusion. They operate by Coulomb barrier reduction. One such concept is muon catalysis,[4] which employs the negatively charged muon to screen the repulsive Coulomb potential between fusion fuel nuclei. Although muon catalysis itself appears to fall short of economic viability due to muon loss, such concepts potentially offer very large increases in fusion reactivity at very low temperatures by perturbations of the Coulomb barrier penetration probability.[5] They suggest a potential avenue to explore further for a step-change approach to fusion energy. They certainly look nothing like Parkins *et al.*'s concept of a conventional fusion reactor.

I conclude by contending that advances leading to a clearly economic fusion reactor product will probably lie in seeking advanced physics solutions rather than simply engineering the nuts and bolts for the present conventional approach. Thus, the smartest investment of our fusion research dollars is to press for innovation in and understanding of the physics of various advanced concepts in breadth, in parallel with pursuing our studies of burning fusion plasmas in the tokamak.

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It seems to me very probable that 150 years ago, equally "insurmountable" difficulties could have been identified when contemplating the possibility of heavier-than-air flight—an undertaking that had baffled humankind for a couple of millennia, at least. Yet it was not really very long, once the proper pieces for understanding the fluid mechanics had fallen into place at the end of the 19th century, until airplanes and then jet aircraft became commonplace (even though many travelers still have no understanding of the fluid mechanics involved). The necessary engineering proved very inventive, once the understanding of the basic phenomena was truly in hand, though until then there would have been no point to it. One is glad now that Osborne Reynolds, Ludwig Prandtl, N. Joukowski and the Wright brothers didn't give up.

The three letters on the alleged impossibility of fusion power seem to me to have much in common with what has been the true shortcoming of the controlled-fusion program: sweeping generalizations and proof by assertion, largely on the part of people impatient with, or even unacquainted with, the demanding task that has to come first: getting the plasma physics right. From the point of view of one who has been in basic plasma physics for four decades, the problems seem to lie not in the aspiration toward fusion, but in the premature and heedless way that extravagant promises and publicity, unsupported by detailed physical understanding, began to dominate and direct the program about 1970. Unrealistic hopes were raised, especially in those in no position to appreciate the primitive level of understanding of the plasma state that prevailed. Basic plasma research, which some came to see as unnecessary, had less and less of a voice in what experiments were done. Now that those high hopes have been dashed, the backlash is understandable, but is no more valuable than the oversold reactor promises were.

For me, the lesson is that what is needed is a renewed awareness of how subtle and tricky continuum mechanics can be—plasmas are harder than fluids to understand, and our understanding of fluids has already taken up about 200 years and is far from complete. A rededication is now in order—to getting the subtleties of plasma physics and magnetohydrodynamics right, and leaving the alleged "reactor scenarios," or the impossibility thereof, to writers of science fiction. Members of Congress cannot be expected to separate the two, but the physics community should be able to.

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Fusion engineering issues are indeed daunting, as the three writers emphasize. But then, so were fusion plasma physics issues when the program started. Now, the challenge for the future is twofold. On the one hand, it is to use the understanding of plasma physics developed in the tokamak and other experimental devices to optimize and simplify the confinement configuration from a power plant perspective. On the other hand, it is to focus creativity and inventiveness on engineering and technology, comparable to the earlier focus on plasma physics. Implementing these two thrusts will address the fallacy underlying your correspondents' comments—that today's fusion engineering and technology are necessarily prototypical of an eventual power plant.

In the quest for fusion power, success is neither assured nor near at hand. Nonetheless, virtually every scientifically developed nation is engaged in fusion research. Of course, other energy options should also be pursued in parallel, both to meet energy needs in the nearer term and to provide a range of options for energy planners in the longer term. It must be recognized, however, that there are very few if any long-range options that promise fusion's combination of wide fuel availability, safety and environmental attractiveness.

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US R&D policy should be determined by the more broadly based judgment that emerges from intensive reviews of the status and prospects of specific programs, especially when conducted by review groups whose membership comes primarily from outside the scientific program under review. The US fusion energy sciences program has already received at least three such reviews in this decade. The first, in 1990, was chaired for the Department of Energy by Guy Stever, a former president of Carnegie Mellon University. The Stever committee ended up strongly supporting the nation's effort to develop fusion energy, and its recommendations were the basis for the policy included by then Secretary James Watkins in the energy policy enacted into law in 1992. The second review, in 1992, conducted under the auspices of a DOE Energy Research Advisory Board subgroup chaired by Charles Townes of the University of California, Berkeley, was also supportive of the effort.

The most recent external review—and the highest-level review in the long history of the fusion program—was conducted in mid-1995 by a special panel formed by the President's Committee of Advisers on Science and Technology (PCAST) and chaired by John Holdren of Harvard University. (Stewart Prager of the University of Wisconsin—Madison and I were the only panel members with research interests directly in the area of plasma physics and fusion energy; I was also the panel's vice chairman.)

The PCAST panel considered in depth all the issues raised in your March issue—and more. It held numerous meetings over a four-month period, and made sure that it heard a full range of representative opinions from all segments of the US R&D community. For example, portions of many sessions were held with just one or two people at a time to ensure that all views were heard and understood in an unencumbered atmosphere.

In the end the panel concluded that "funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond."[1]

Subsequently, on 6 December 1996, PCAST chairman John Young wrote to President Clinton suggesting five priority R&D issues to address in his second term. Energy R&D was one of those, and the pursuit of fusion energy was again endorsed within that recommendation—and for the same reasons and at the funding level of $320 million originally recommended by PCAST in 1995.

We are all entitled to our opinions, and in a rapidly developing area of science and technology research, such opinions may vary. The way to develop consensus and appropriate policy is to rely on in-depth, dispassionate review and debate, followed by documented judgment. The advice of PCAST should be seen for what it is, a reflective and considered judgment, and should be given the weight it deserves.

Reference

1. J. P. Holdren et al., *The U.S. Program of Fusion Energy Research and Development*, report of the Fusion Review Panel, President's Committee of Advisers on Science and Technology (PCAST), (1995).

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The three letter writers have devoted much of their lives to fission, as I have to fusion, so there is some turf to protect on both sides, but it's important to remain collegial. Chauncey Starr hired me when he was dean of engineering at UCLA, and, when he left to found the Electric Power Research Institute, he was nice enough to appoint me chairman of EPRI's Fusion Advisory Committee. In spite of his dedication to the breeder reactor, he had the vision to give strong backing to fusion research; he was the last UCLA engineering dean to do so.

The fission and fusion people can work together, and here is how they can do that. The timetable for fusion has been greatly extended by the state of the economy. We need a power source to sustain us until the engineering problems of fusion can be solved, and that is a sound basis for supporting fission, which will never be a permanent energy source because of the nuclear waste problem. But it is a different matter if we need to put up with the waste for a limited time—say, 50 years—until fusion reactors come on line. The experience of the fission community will be of great value in this mission. However, we must also have sufficient support from Congress to maintain our level of expertise and scientific momentum.

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My perspective is that of someone involved from the earliest development of fission power in the US Navy's nuclear propulsion program, in the Liquid Metal Fast Breeder Program, in the Electric Power Research Institute's Advanced Light Water Reactor program and in the US magnetic fusion program.

I agree that development of fusion to the stage of practicality is difficult. However, an entire future for humanity that would be energized largely by fission breeder reactors is unacceptable as long as there is any possible alternative. Fusion power remains such an alternative.

Finally, given the profound importance of the subject of energy, I find it very hard to understand why scientists and engineers attack each other's programs instead of attacking public and official apathy and insisting that energy development be given wholehearted support in every reasonable way. What more useful good can physics have than developing the ultimate energy resource to its full potential?

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PARKINS, KRUMHANSL, AND STARR REPLY: The vision of fusion power has been accepted by a certain segment of the scientific community for so many decades that fusion power's eventual feasibility has become a sacred mantra for justifying an enormous scientific endeavor aimed at achieving control of the fusion process. It is therefore not surprising that our challenges to this mantra should provoke the above strong responses, particularly as so many lifelong careers have been invested in this quest.

It should be noted that the thrust of these responses supports the validity of the main points made in our three letters. The writers plead for continuation of the programs on the science needed to achieve a controllable and usable fusion process. They point to past progress in the understanding of plasma physics and the ingenuity of their experimental equipment as harbingers of great progress to come. Their optimism and commitment are certainly to be expected.

However, it is also apparent from

the writers' comments that there has not been a serious investigation and development of the many systems required for a practical power plant. They repeatedly refer to "conceptions" of plant configurations, which are hardly a meaningful basis for serious evaluations. The utility industry's comments on the ARIES design (cited in the letter from Farrokh Najmabadi *et al.*) was merely polite approval of the performance goals set for an operational plant. The many formal reviews of the US fusion program have similarly focused on research directions, not ultimate feasibility. The reality is that the engineering development of a fusion power plant has not been undertaken, and that the many issues raised in our three letters have not been faced or resolved.

Our letters did not address the wisdom of the scale of resource allocation appropriate to exploring the science of fusion. That is a matter of priorities for the national scientific community to determine based on the value of understanding plasma physics. The massive investment in fusion research during the past decades has been devoted almost entirely to the physics endeavor to demonstrate that as much energy can be released from a plasma as is required to heat it.

Our point was that even if the science of a controlled fusion process is eventually understood and demonstrated, any usable power application will face engineering barriers that appear much more extreme than those faced by the current quest for "breakeven." There is no reasonable possibility of achieving the target of practical feasibility in the foreseeable future, even with an intensive engineering R&D program.

Consider this: During the next hundred years, when we will still have available all the fossil fuels, fission power and renewables, a practical fusion plant will have to achieve and demonstrate sustained performance reliability for several decades, need only a few scheduled outages for maintenance, meet stringent environmental criteria and be economically competitive with other electricity sources. We believe the present fusion concepts will not be able to meet any of these requirements. It is therefore misleading both the general public and the policymakers to include fusion in our national energy strategy for a dependable mix of electricity sources adaptable to uncertain future circumstances.

Our letters mentioned some of the special technical problems that arose in the development of successful fission plants and that would have to be faced in engineering any nuclear plant. The combined effects of those problems compound the difficulties of designing a practical operating system. Experience with conventional nuclear fission power plants, of which several hundred have been operating internationally for decades, provides useful insights into the unique aspects of nuclear engineering. Analogies with historical nonnuclear large-scale engineering developments such as aircraft and rocket vehicles are only marginally relevant. The hopes of the fusion community cannot rest on such analogies.

But relevant history does provide a powerful message that must not be ignored. After the discovery of fission was announced in 1939, Hans Bethe came forward with the first theory of energy production in the Sun through fusion. In less than five years after the discovery of fission, fission reactor production plants of hundreds of megawatts were operating that could have been converted to power generation. In the case of fusion, however, after a half-century of research effort, the first demonstration of an operating fusion reactor is still far away. And even if such a demonstration should ever occur, it would be only an academic achievement. As has been pointed out, additional insurmountable obstacles stand in the way of any practical application of fusion power.

Nature (which cannot be fooled, as Dick Feynman reminded us) imposes fundamental constraints that mankind cannot change and must accept. In the case of fission, a remarkably fortuitous set of technical properties made today's nuclear power industry possible. In the case of fusion, a very unfortunate set of constraints appears to obviate any future power industry based on the fusion principle.

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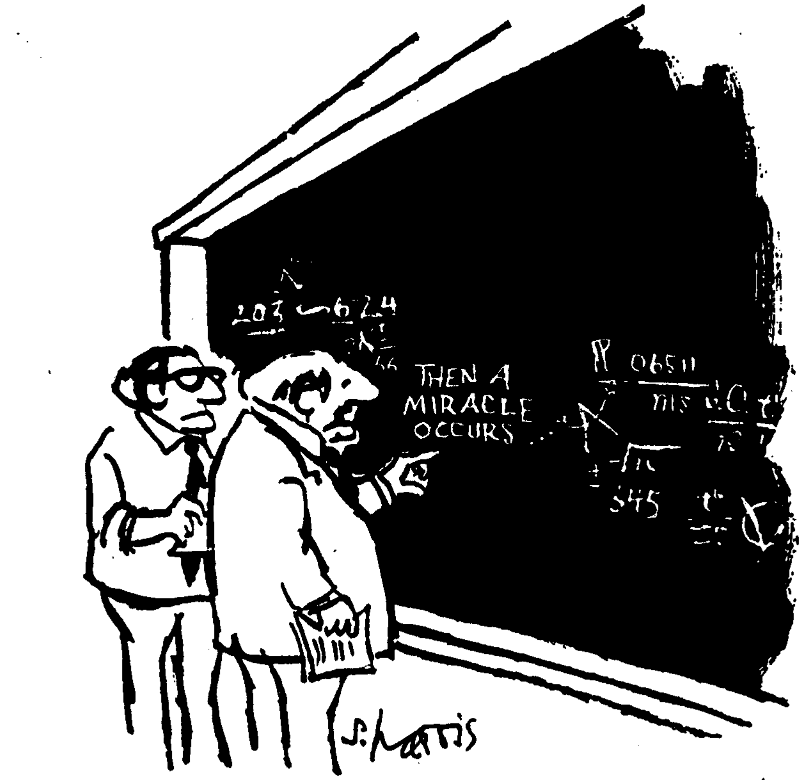

"I THINK YOU SHOULD BE MORE EXPLICIT HERE IN STEP TWO."

Canada Is Chided for Abandoning Its TASCC

Recently, the international nuclear physics community was shocked by the news that the Canadian government had closed the Tandem Accelerator Superconducting Cyclotron (TASCC) in Chalk River, Ontario (see PHYSICS TODAY, February, page 59, and March, page 69). This facility, in