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.)

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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.

References

1. W. D. Metz, Science **192**, 1320 (1976); **193**, 38 (1976); **193**, 307 (1976).

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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]

References

1. *Neutron-Interactive Materials (NIM) Program Review*, report of the Fusion Energy Advisory Committee, Panel 6 (R. W. Conn, chair), Department of Energy Report no. DOE/ER-0593T(1993).
2. W. E. Parkins, Science **199**, 1403 (1978).
3. Energy Economist, November 1996, p. 181.
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