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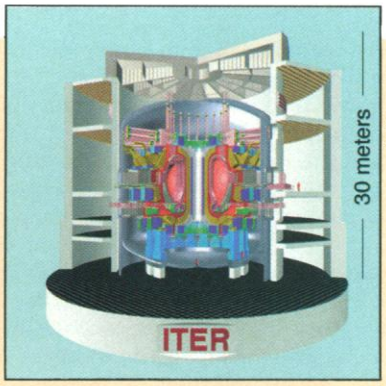
LETTERS

Testing one's mettle



How does scientific technique stand up to the requirements of the real world? A letter by nine physicists says that a decision to build the International Thermonuclear Experimental Reactor (ITER, depicted at right) would be "a commitment to a highly specific direction of development from which it would prove increasingly difficult and embarrassing to depart." A second letter enumerates problems in physics and engineering that should be addressed before construction of a large-scale fusion reactor such as ITER is attempted. A third, more general, letter asks, "Can the fusion reactor concept *ever* be a practical, cost-effective method" for generating electricity?" Other writers discuss the difficulties of assessing student learning and of comparing test scores and who deserves to receive funds for medical research.

Fusion Prospects

This letter relates to the article by Andrew Lawler about the International Thermonuclear Experimental Reactor (ITER) initiative (News & Comment, 19 Jan., p. 282). We have long believed that fusion has an inestimable potential as a long-term energy source. Eight of us have devoted much of our working lives to fusion and to the physics of tokamaks in particular. Through personal participation, we have each gained a warm respect for the high level of international cooperation that has been a hallmark of fusion research.

It is important to distinguish three different aspects of ITER:

1) The ITER organization, with policy and direction set by the ITER Council;

2) The ITER Engineering Design Activities (EDA) effort, a 6-year international program established in July 1992 to perform the detailed physics and engineering design for an engineering test reactor based on the ITER Conceptual Design Activities Final Report (1991); and

3) The decision to be made by the four parties to the ITER agreement about whether or not to actually *construct* this machine. (The decision is scheduled to be made after July 1998.)

To achieve its stated goal of ignition and long-term burn, the ITER Conceptual Design report proposed an enormous machine that was a large extrapolation of existing tokamaks. The EDA has since performed a useful but expensive exercise in detailing and costing this machine, which is currently referred to as the "Interim Design."

At present, it is not known how to construct a fusion reactor economically. To enter directly into the actual construction of the Interim Design—a single machine of grandiose scale and cost in time, human effort, and money—suggests otherwise and would establish a commitment to a highly specific direction of development from which it would prove increasingly difficult and embarrassing to depart.

The Interim Design would take four giant steps simultaneously, each of them untested: Huge size, ignition, long pulse with fusion-grade plasmas, and very large volume superconducting magnets operating at their ultimate magnetic fields. To focus the world's fusion research efforts on the construction of this machine, which would not produce its first plasma until 2008 or even later, would constitute a high-risk choice.

The stated target for the Interim Design was to achieve ignition and remain in a state of equilibrium burn for 1000 seconds while producing 1500 megawatts of fusion power. Experimental data and theoretical understanding do not support the achievement of this performance under the proposed operating regime. Separate and serious concerns have been raised that the actual plasma and fusion performance in this machine would not even reach the fallback target, significant long-term power amplification. Such a failure of an internationally sponsored machine would be a humiliating setback for the entire fusion world.

The EDA has already defined the Interim Design sufficiently, and a broad-based independent assessment could be made now, rather than after July 1998.

The ITER Council and the EDA leadership have not given serious attention to strategic alternatives for international collaboration, other than moving along doggedly into the construction of the Interim Design.

The learning curve in fusion science is steep. For example, great advances in experiment, theory, and computation have occurred just in the past 5 years. With support, this rate of progress may be expected to continue. The speed of major device construction should be scaled to the appearance of new ideas and fundamental improvements (and we should use history as a guide).

We place the highest priority in fusion research on the maintenance of robust national programs [including the multinational Joint European Torus (JET)]. Scientific advances of enormous potential value to economical fusion power have grown out of these programs. Efforts that use existing facilities, or those that can be built in the near future, would be our best use of time and money. We suggest that the ITER council start immediately to identify new strategies to give us well-targeted and cost-effective ways to advance the world's quest for this enormously desirable energy source.

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For the United States to concentrate its efforts on the construction of ITER, which by my estimates would require at least twice the $8 billion cited by Lawler, would halt significant progress in domestic thermonuclear research. It is tantamount to a suicidal plan that would discredit nuclear fusion as an economically viable form of energy production.

The current ITER design is based on the most optimistic extrapolation of experimental results for plasma confinement, plasma beta (the ratio of plasma to magnetic pressure), and plasma purity. To guarantee a minimum of performance, the design has been pushed to such a grandiose scale that its major and most sophisticated components would have to be manufactured in situ, as no road is large enough for their transportation. In spite of this, ITER would not ignite if any one of the aforementioned parameters falls below its assumed value by as little as 10 to 20%. Moreover, a single plasma disruption and consequent abrupt termination of tokamak discharge, a phenomenon that happens daily in tokamak reactors, could destroy the inner core of ITER. This raises the strong possibility that ITER may never achieve its goals.

The designing of ITER has served to indicate the major problems in physics and engineering that must be addressed before the construction of a tokamak fusion reactor is attempted: The former include an improved plasma confinement at large values of beta, which would lead to a more compact and cheaper reactor, as well as an improved plasma stability, which could lessen the danger of plasma disruptions; the latter include the development of low activation materials, and a better divertor design. These problems are being tackled in experiments and are the focus of proposed near-term facilities. The construction of ITER, by absorbing

all the available funds, would inevitably prevent development in these critical areas.

From Lawler's article (p. 282), it appears that ITER finds its strongest support in a "wealthy and influential association of major corporations. . . ." This sounds like an ominous repetition of history, as our problems today with nuclear fission power plants originated when the nuclear industry decided to bring to prominence the first fission reactor concept that appeared to work. Similarly, the adoption of this probably faulty device would have catastrophic consequences for the development of nuclear fusion energy.

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Several recent letters proclaim once again the superior promise that thermonuclear fusion offers for future large-scale generation of electric power (D. E. Baldwin and T. C. Simonen, 13 Oct., p. 220; E. M. Campbell and J. C. Browne, 12 Jan., p. 130; and N. A. Davies, 12 Jan., p. 133). They point out the significant progress made, the benefit that the so-called "multiple mission" fusion offers, and the peripheral advantages that would accrue. While somewhat exaggerated, these arguments are correct, but they miss the point (see W. E. Parkins, Letters, 24 Nov., p. 1281). Unless the primary mission is fulfilled, all of the touted side benefits are but academic.

Can the fusion reactor concept *ever* be a practical, cost-effective method for production of central station electricity? Unfortunately, nature has interposed not one, but three, unsurmountable obstacles. (i) The required temperature and other plasma conditions for even the easiest fusion reactions (deuterium-tritium or D-T and the deuterium-deuterium or D-D) seem to be unattainable. (ii) These reactions release energetic damaging neutrons that change the physical properties of the reaction vessel and make it radioactive. (iii) Most devastating of all, power cannot be extracted from within the reacting plasma. It can be gathered only at the peripheral wall.

Each of these obstacles bears on the practicality and cost-effectiveness, and thereby the future, of fusion power. Efforts to achieve the necessary plasma conditions are leading to a reactor system design of monumental complexity. Effects of the neutrons dictate that the operating utility be prepared to periodically replace the highly radioactive and almost inaccessible vacuum vessel—an unacceptable requirement. And inability to extract power as heat within the reacting region (as is done in fossil-fueled boilers and fission reactors) forces the engineering to physical dimensions that are much too costly. When the utility executive finally figures the capital investment charges and operating costs that would apply to each kilowatt-hour generated, he will close the book on another broken dream.

But all need not be lost. There is no lack of scientific and technical frontiers, and there will be new ideas. The greater benefit will come from applying our resources, including the efforts of talented scientists and engineers, in directions that can make a difference in the future. When large-scale usage is intended, however, one must be sure that the development is guided by a practical and cost-effective concept.

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Comparing Student Test Scores

In the Policy Forum "Myths about test score comparisons" (1 Dec., p. 1446), Iris C. Rot-