Fusion Research With a Future

By Robert L. Hirsch, Gerald Kulcinski, and Ramy Shanny

It's time for the U.S. program to abandon its dead-end focus and to explore alternative paths to practical fusion power.

Major shifts are taking place in the U.S. fusion research program, driven primarily by reductions in federal funding. In the past, the program was dedicated almost completely to developing practical fusion power. Today, the program claims to be devoting roughly two-thirds of its resources to high-temperature plasma physics research and only one-third to fusion power. We believe that a significant shift back to the development of fusion power should be considered. If this shift is to be made, it must be made now, because the United States will soon decide whether or not to participate in the next stage of the International Thermonuclear Experimental Reactor (ITER) project. A commitment to ITER will claim such a large share of U.S. fusion research funds that it will essentially preclude significant exploration of other fusion concepts for at least a decade. To understand what is at stake, it helps to understand the history of the U.S. fusion program.

Fusion research was initiated in earnest in many parts of the world in the early 1950s, and there were high hopes for its early success. Outstanding physicists began to develop the science of high-temperature plasmas, and relatively quickly they conceived some ingenious magnetic bottles aimed at containing hot plasmas. Funds were readily forthcoming, and the quest for practical fusion power began. Enthusiasm and optimism were rampant. The goal was noble: a wholly new, safe, and environmentally benign energy source that would run pretty much forever on an essentially infinite fuel supply.

But in the first decade of fusion research, it became painfully clear that the nonlinear nature of the underlying plasma physics was extraordinarily complex. New plasma instabilities that destroyed plasma confinement were discovered at an alarming rate. It quickly became obvious that researchers needed to learn an enormous amount in order to develop a working fusion power system. As a result, fusion research settled down to what might be called an applied basic science program. It had a clear practical goal, but it needed to acquire a great deal of fundamental understanding before that goal could be realized.

By the late 1960s, researchers were frustrated and disheartened. At that point the Russians reported unusually good results from their tokamak experiments. *[The tokamak is a toroidal (doughnut-shaped) magnetic plasma confinement configuration.]* A special international team verified the Russian results, and laboratories around the world dropped most of their work on other concepts in order to build and develop tokamaks, because they seemed to provide good plasma confinement at last. Today, roughly 85 percent of the U.S. fusion program is devoted to tokamak-related research.

The good news about this stampede to tokamaks was that it led to an explosion of understanding of tokamak plasmas and dramatic increases in their performance. Practical fusion power was still the stated goal in the 1970s, so a group of scientists and engineers dedicated themselves to solving the myriad problems that had to be addressed in order to build a tokamak power reactor.

The bad news associated with this dramatic shift in emphasis was that the goal of practical commercial fusion power became confused with the goal of making fusion power from tokamaks. As we shall see, the two goals are very different.

Market Discipline

Any new electric power source must satisfy a set of criteria dictated by the marketplace. Today's criteria for success have evolved from those in place in the early years of fusion research. Since then, market needs have shifted somewhat, and existing energy sources have improved, some rather dramatically. Fusion technologists must anticipate future market changes as they set and adjust their program goals. Although such goal-setting has been and will continue to be somewhat uncertain, a robust and relatively timeless set of requirements for fusion reactors was developed recently to provide a sound basis for future fusion power R&D. The guide was assembled by a panel of electric utility technologists under the sponsorship of the Electric Power Research Institute (EPRI) in 1994. Their requirements fall into three categories: economics, public acceptance, and regulatory simplicity. We'll describe each of them briefly.

The cost of any new electric power source is of course critical to its acceptance. But as the EPRI report observes, ***"To compensate for the higher economic risks associated with new technologies, fusion plants must have lower life-cycle costs than [the] competing proven technologies available at the time of [fusion] commercialization."***

One important aspect of fusion economics is the system's reliability. Because fusion is likely to involve a large number of new technologies, its initial reliability will be inherently lower than that of its existing commercial competitors, which further increases the challenge of developing practical fusion power.

Note that the EPRI cost requirement came from practical electric utility personnel before the recent deregulation and competitive restructuring of the U.S. electric utility industry began in earnest. Imagine how much more emphatic they would be on the subject of economics in today's environment!

Public acceptance will clearly be essential to fusion's commercial success. According to the EPRI report, "A positive public perception can best be achieved by maximizing fusion power's environmental attractiveness, economics of power production, and safety. Standards must be high: Renewable energy source plants may represent the public's benchmark for environmental cleanliness and safety."

As for regulatory simplicity, it is obvious that plant design, operating conditions, and safety will have a significant impact on the purpose and complexity of regulations. Depending on what choices researchers make, fusion power regulation will likely end up somewhere between the extremes of regulation for fossil-fuel plants on the one hand and nuclear power plants on the other. Nuclear power regulations are the most difficult and onerous, so a similar model for fusion power should be avoided as much as possible.

An additional factor that applies in many countries is that huge power plants are no longer as attractive as they used to be. The average size of new power plants ordered in the United States dropped from 550 megawatts in 1977 to 50 megawatts in 1993. In a rapidly deregulating electric power marketplace, it is difficult to project the optimum size of future electric generators. The power industry's previous fixation on economy of scale has yielded to other considerations, such as initial cost and life-cycle costs. Smaller power plants may be more desirable than very large ones in the future.

Tokamak Performance

At present, the tokamak concept is the overwhelming focus of fusion research everywhere in the world, and ITER design is the centerpiece of the effort. ITER is an extremely large, roughly 1.5-gigawatt tokamak designed to ignite a hot deuterium-tritium (DT) plasma and sustain a long-term burn for the first time. It will take about a decade to build and a decade to conduct the research for which it is designed and will cost on the order of $10 billion to build and another $10 billion to operate. It would thereby virtually eliminate the study of other approaches to fusion. The European Union, Japan, Russia, and the United States have cooperated on the ITER design. The participating countries are now considering what commitment they are willing to make to building and operating ITER. The inherent limitations of the tokamak configuration should give them pause.

In 1994, physicists at the Lawrence Livermore National Laboratory compared the cost of the core of the then-existing ITER design to the cost of the core of the Westinghouse AP 600 advanced light-water nuclear fission reactor, an attractive new design that is perhaps the best that nuclear power has to offer for the next few decades. Both systems are designed to produce roughly 1.5 gigawatts of thermal energy. The Livermore researchers concluded that the cost of the ITER core was roughly 30 times more than the cost of the AP 600 core. One expects a first-of-a-kind facility such as ITER to be more expensive than one based on existing industrial experience. But 30 times is an enormous difference, one that is beyond any reasonable hope of being reduced below unity, which common sense as well as the EPRI panel indicate will be necessary.

There are additional problems with tokamaks as practical power reactors. tokamak designs are currently targeted to use DT fuel. This fuel cycle produces 80 percent of its energy in 14-MeV neutrons, which cause considerable damage to construction materials and induce large amounts of short-term (and sometimes long-term) radioactivity. Materials made brittle by neutron irradiation will have to be replaced every few years. This will entail shutting down the facility for several months so that the interior can be completely rebuilt by remote-controlled robots. Such intermittent scheduled repairs will be particularly expensive because while it is shut down, the fusion reactor will be costing money for maintenance but producing no revenue, and replacement power will have to be purchased to offset the power lost while the unit is out of service. Moreover, the damaged radioactive components will have to be disposed of at great expense.

Current federal programs as well as new initiatives should be examined periodically to see if more research could be performed extramurally.

A further complication is the need for tritium breeding in a DT fusion power system. Breeding ratios greater than unity require costly subsystems. The estimated cost of ITER does not include the expense of commercial tritium breeding. Although we know how to produce tritium in a relatively low-temperature fission reactor, breeding it in high-temperature materials in a fusion power plant will be much more difficult. Past production of kilogram-per-year quantities of tritium by batch processing of lithium-aluminum rods outside a fission reactor is far different from processing on the order of 100 kilograms per year of tritium continuously from gases, liquids, or solids in a working fusion power reactor. The true costs and hazards associated with large-scale production of tritium in the hostile environment of a fusion power plant are not yet fully known. Avoiding this complication would clearly be desirable.

This leads us to conclude that the study and development of tokamaks such as ITER is not useful for the development of practical fusion power. Rather, its benefits will be limited to basic high-temperature plasma physics research and some technology testing. This would seem to be what the U.S. fusion program managers mean when they indicate that two-thirds of their program, which is 85 percent tokamak-related, is basic plasma science.

Let there be no misunderstanding. Basic plasma physics research is a noble effort that most of the world's fusion physicists believe is worth doing. For that reason, DT tokamak research to understand the physics of burning plasmas can certainly be justified, but only up to a point. Although ITER would produce some interesting plasma physics insights, the enormous cost and the diversion of talent from the goal of developing a practical fusion power concept would be tragic. DT tokamaks, as we understand or envision them today, simply do not offer a workable approach to commercial fusion power.

The Road Not Taken

So where does one look for the concepts that could lead to practical, marketable fusion power? Perhaps not in DT-fueled systems. Their inherently high neutron fluxes create serious concerns about radiation damage, large inventories of radioactive materials, and significant radioactive waste problems that will be expensive to manage, unpopular with the public, and very complicated to regulate. This means that researchers should devote much more effort to developing so-called advanced fusion fuel cycles with low or zero neutron fluxes. We should also remember that smaller fusion systems are likely to be much more acceptable in the marketplace than the gigawatt-sized systems of many tokamak reactor conceptual designs.

What fuel cycles and plasma containment concepts should be studied for practical fusion power? We have already suggested that fuel cycles with low neutron yield should be pursued more aggressively. As to the most appropriate plasma containment concept, no one can say for sure because so little effort has been expended to find out. Certainly it must be small in size, low in power level, economical, and very attractive to a wide range of buyers. A small but determined group of scientists were developing potentially attractive confinement concepts in the mid-1980s, but a reorientation of nearly the entire U.S. fusion program to tokamaks cut short that promising research prematurely. Still, there now exists a solid foundation of knowledge of basic high-temperature plasma physics and advanced fusion technology, plus a core of highly trained technical personnel. Therefore, the pursuit of concepts other than the tokamak could move relatively quickly, certainly much faster than the more than 40 years it took to reach today's state of knowledge.

One might even ask whether any fusion concept is capable of meeting our practical requirements. That question will remain unanswered until researchers study the most attractive options seriously and possibly develop new ones. We have faith that one or more concepts will ultimately prove viable.

The United States has invested, at a conservative estimate, more than 12 billion 1996 dollars on plasma confinement approaches for eventual DT fueling, principally the tokamak. The level of effort invested in concepts designed to burn advanced fuels is probably much less than 1 percent of this total. Such concepts have not been given a chance-certainly not with the benefit of today's advanced understanding of the science and technology involved.

There is serious question whether the United States should stay involved in the ITER construction project. Because tokamaks and the DT fuel cycle are extremely unlikely to become commercially viable, it is far more prudent to shift a large fraction of current funding to concepts, technologies, and fuel cycles of greater promise. In an era of tight federal R&D budgets and escalating ITER costs, continued investment in ITER construction could squeeze out all opportunity to pursue new approaches, let alone other plasma applications. Indeed, if viable or potentially viable commercial fusion confinement concepts have not been identified, how can we possibly know whether the physics knowledge we gain from tokamak experiments and ITER will have any relevance to practical fusion power?

Other Applications

Finally, let us briefly consider near-term commercial applications of plasma science and technology, as well as applications involving the products from fusion reactions such as neutrons, protons, and alpha particles. Not many people realize the enormous practical applications that have resulted from fusion research and development. Stephen O. Dean, president of the Fusion Power Associates educational group, has found that "Plasma and other technologies developed in part by fusion energy research programs are being used [widely]. Applications include efficient production of advanced semiconductor chips and integrated circuits; deposition of anti-corrosion and other types of coatings; improvements in materials for a wide variety of applications; new techniques for cleaning up and detoxifying waste; plasma flat-panel displays; high current switches for the power industry; medical and biological applications; improvements in a wide variety of related technologies, such as isotope separation, microwave sources, cryogenics, superconductivity, and optics; new technologies, such as light sources and digital radar; and contributions to many areas of basic science, such as space physics and supercomputing."

It may also be possible to use some of the products from fusion reactions in small fusion devices for near-term commercial applications before the problems of generating power are solved. Applications such as the production of radioisotopes for medical use, as well as protons and neutrons for the process industry and defense, appear potentially attractive if the fusion source is small and relatively inexpensive. Ideally, this approach would involve devices with the potential for using advanced fuels but that at present have low Q (energy out/energy in) ratios. The construction and operation of such small low-Q devices could also provide insights on how to build higher-Q systems that might lead eventually to commercial electric power. Even if that does not happen, at least there would be some financial profit to offset the research costs. We believe that tokamaks have no such practical, near-term commercial applications.

There is every reason to believe that these and other applications of fusion science and technology will continue to evolve and be important. Accordingly, a national fusion program might profitably include a component aimed at nearer-term applications. Such an effort would have a number of advantages, such as helping to maintain program support, helping to keep researchers oriented to the practical, and providing employment opportunities for researchers who wish to work on nearer-term technology.

The study and development of tokamaks such as ITER is not useful for the development of practical fusion power.

We believe that the U.S. national fusion program should emphasize concepts that can lead to practical fusion power, along with smaller efforts on high-temperature basic plasma physics and plasma applications research. The tokamak concept as we know it today is unlikely to lead to practical fusion power, but related research at a modest level could be justified as interesting high-temperature basic plasma physics research. Continuing tokamak research to the ITER construction stage is not justifiable in the present federal budget environment, because that commitment would surely starve funding for concepts with a higher likelihood of producing a commercially viable electric power system.

Because the federal government has justified decades of fusion research funding on the grounds that it would lead to practical fusion power, we believe that a reorientation away from tokamaks toward more promising, smaller, advanced fuel concepts is in order. The highly trained fusion researchers that are now in the field, combined with today's advanced knowledge of plasma physics and previous small but significant investigations into advanced confinement concepts, should greatly facilitate this effort.

The current U.S. fusion budget is roughly one-quarter of its 1977 peak in real terms. Although research on inherently lower-cost fusion concepts should be cheaper than expensive tokamak research, we believe that the present annual budget of somewhat more than $200 million would be required to develop fusion — the ultimate power source for modern civilization.

Recommended Reading

Stephen O. Dean, "Application of Plasma and Fusion Research," Journal of Fusion Energy, Volume 14, No. 2, 1995, p. 251.

Electric Power Research Institute, Criteria for Practical Fusion Power Plants. Palo Alto, California: EPRI, 1994.

Gerald Kulcinski, "Near Term Commercial Opportunities from Long Range Fusion Research," Fusion Technology, Volume 30, 1996, p. 411.

L. John Perkins et al. "Fusion, the Competition, and the Need for Advanced Fusion Concepts." Livermore, California: Lawrence Livermore National Laboratory, March 30, 1994.

U.S. Government Accounting Office, Federal Research: Changes in Electricity-Related R&D Funding. Washington, D.C.: Government Printing Office, August 1996, p. 5.

About the Authors

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Letters — Wishful Thinking

Republican of California Chairman, House Energy and Environment Subcommittee Weston M. Stacey's article is an enthusiastic recapitulation of all the promises of the fusion concept and stresses the value of U.S. participation in the proposed ITER program. The article by Robert L. Hirsch, Gerald Kulcinski, and Ramy Shanny is a more realistic recognition of the uncertainty of fusion technology and a plea for more scientific creativity in developing alternatives to the ITER tokamak concept.

Unfortunately, Stacey's premises for justifying federal support are factually misleading.

(1) The fuel supply is not "virtually unlimited" because the availability of lithium, which is essential in the deuterium tritium (D-T) fuel cycle, is similar to the availability of uranium ample now, but finite.

(2) The contention that the tokamak concept might eventually compete with advanced nuclear fission and fossil plants is wishful thinking that ignores the reality of the tokamak's complexity and size, arising from its plasma and engineering requirements, as compared with those of a fission or fossil plant. Today's estimate by fusion enthusiasts of the capital cost of the ARIES tokamak plant is at least three times that of a nuclear fission plant, and experience suggests that it is likely to be much greater when the real costs of fabricating the complicated magnet, heat transfer, containment, and maintenance systems are included.

(3) Finally, the environmental benignity of fusion is a matter of degree, only slightly better than fission, and neither is as environmentally attractive as solar sources. The radioactive wastes from both need similar custodial attention during the initial century. Fusion does not produce fission products or plutonium, but it does produce tritium, and both are hazardous materials, although plutonium is of more concern in the weapons area.

It is unfortunate that the fusion community has perpetuated the myth that fusion is a foreseeably practical end-game for our energy resources. With the present concept, it certainly is not. It is, of course, a fascinating scientific experiment and should be evaluated and supported in that light. Stacey presents ITER as a test facility and thus a step toward the successful development of fusion. ITER might test some parts of the tokamak concept, but this will not be sufficient for a practical plant design. U.S. participation in such an international facility is a political as well as technical matter. Hirsch, Kulcinski, and Shanny recognize the uncertainty of tokamak fusion as a national energy source. It is time for the fusion community to acknowledge this reality, so that the public is not further misled and the politicization of this area of science is not continued. The public and Congress have become increasingly cynical about the intellectual integrity of the physics community, and fusion is a case in point. In this regard, the Hirsch, Kulcinski, and Shanny article is a step toward reevaluating the appropriate role of fusion research in our national science programs.

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Letters — Tokaturkey

Once a Tokaturkey, always a Tokaturkey. The Gothic cathedral builders built to the glory of God; these technological cathedral builders build to the god of Mammon (as revealed through research and retirement, using science as pork). But members of Congress are not as stupid as DOE bureaucrats and fusion physicists (and their managers) think them to be, as evidenced by Congress' continuing reduction of the program budget for the past 17-plus years. It is now at a level less than twice that (in real dollars) at which it started in 1972, when Drs. Hirsch, Alvin Trivelpiece, and Stephen Dean, and I sold its 20-fold escalation to a Congress driven by the Arab oil crisis.

Sic transit gloria mundi. Kill the present program and start over.

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Letters — Insurmountable Effort?

The two articles by Weston M. Stacey and Robert L. Hirsch, Gerald Kulcinski, and Ramy Shanny provide an informative overview of the serious concerns about fusion R&D as the momentous decision approaches on whether to proceed with construction of ITER. Stacey is an avid supporter of ITER; Hirsch and his colleagues believe it is a waste of time and money. In our view, Hirsch, Kulcinski, and Shanny's article is the more reasonable of the two.

It is certainly true that DOE's fusion R&D program has become narrowly and inappropriately fixated on tokamak reactors. From what is known to date, tokamaks are extremely expensive, scientifically unproved, technologically challenging, and would generate significant amounts of radioactive waste. After 40 years and $14 billion of taxpayer funded research, DOE has no idea when or if commercial fusion power will be available.

Moreover, some scientists believe that the problems related to tokamak technology are virtually insurmountable. William Dorland and Michael Kotschenreuther of the Institute of Fusion Studies at Austin have developed a physics based model that suggests that plasma turbulence will prevent ignition and the sustainable reaction needed to create fusion power. Indeed, DOE's Fusion Energy Science Committee released an assessment in April 1997 acknowledging that the difficulty of confining plasma may prevent ITER from achieving its design goals.

Hirsch, Kulcinski, and Shanny are correct - the United States should not allocate any additional money for ITER. The project is losing support throughout Europe; and Japan, the only country interested in providing a site for ITER, is in a severe budget crisis that is forcing a delay in any large scientific project for the next three years. In addition, because the United States and other international partners are not willing to contribute sufficient resources to build the estimated $10-billion facility, Japan would have to provide the majority of the funding; an unlikely prospect.

The large amount of R&D money spent on magnetic fusion, primarily related to tokamaks, competes with funding for renewable energy resources that are more cost-effective and have a much greater chance of providing energy in the near term. Last year, Congress appropriated $232 million for magnetic fusion (primarily tokamak-oriented) and $240 million for initial confinement fusion for weapons stockpile stewardship activities; a total of $472 million for FY 1997. In contrast, the entire renewable energy budget (including solar, wind, hydrogen, geothermal, and biomass) for FY 1997 was $266 million.

The excessive funding for tokamak-based fusion is disproportionately high in comparison to the numerous and diverse renewable sources available and creates competition between the two programs for scarce federal dollars within the energy R&D budget. Magnetic fusion should be funded as a basic science program, not as energy-supply R&D. And Hirsch, Kulcinski, and Shanny are mistaken in their belief that fusion research, even if oriented toward alternative concepts and fuels, requires more than $200 million a year.

DOE should phase out its tokamak reactors and fund a modest alternative program oriented toward basic science research. It should abandon the ITER project to those countries, if any, that are willing to pay an exorbitant cost for a high-stakes gamble that may never pay off. The United States and its international partners should increase their commitment to sustainable energy resources, which can provide a greater proportion of the world's energy needs. Lawrence Lidsky of the Massachusetts Institute of Technology, a former fusion researcher, expressed our conviction when he noted that "It is hard to make an economically based argument for fusion. You can't justify it, especially as other sources of energy look better and better. The only fusion reactor we need is already working marvelously-it's conveniently located a comfortable ninety-three million miles away"

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Investing in R&D