Fusion Research (I): What Is the Program Buying the Country?

The fusion program is no longer a level-of-effort research activity with a hazy goal; rather it now is a mission oriented program with detailed near, mid, and long-term goals aimed at achieving a practical product.—ROBERT L. HIRSCH, Assistant Administrator for Solar, Geothermal and Advanced Energy Systems, Energy Research and Development Administration

This country needs to orient its fusion program more toward turning fusion into a real energy option and not just another nuclear reactor.—CLINTON P. ASHWORTH, Pacific Gas and Electric Company

Among the options for plentiful supplies of energy, fusion has a special aura. News accounts invariably note that it would be a way to extract safe, clean energy from seawater, enough to last millions of years. Fringe groups, such as the U.S. Labor Party, see fusion as the keystone to world peace and survival. Opponents of nuclear power rate fusion as one of fission's most attractive alternatives. Even many knowledgeable observers of U.S. energy policy, including members of the National Academy of Sciences, have argued that perhaps the country should skip the breeder reactor, and go straight to an energy economy based on fusion.

The generalizations that fusion would be safer than fission, involve no dangerous radioactive materials, and rely on a cheap and plentiful fuel all are potentially true. But, as many critics now point out, the type of fusion machine that the U.S. program is developing will deliver only some of these advantages—and possibly none of them.

The confusion of well-informed people is understandable, because a gap is developing between what the fusion program appears to promise and what is most likely to deliver. Fusion power would not be infinitely abundant, because the present reactors rely on a fuel cycle that must have lithium, and lithium is not particularly more abundant than the ^{238}U that would fuel the breeder reactor. Fusion power would not be free of radioactive gases because the lithium would be used to breed tritium—a gas more benign than fission products but one that is devilishly hard to contain. The fusion reactor would not be free of waste disposal problems, even though the "spent fuel" would be nonradioactive, because the burning of the fuel would produce approximately four times as many neutrons as a fission reactor, and such a neutron flux would make all the structural materials of the reactor intensely radioactive—one estimate of the waste disposal problem is at least 250 tons of material every year from each reactor. Finally, fusion power would not be cheap. Most of the engineers who have studied the subject agree that the fusion reactors should be expected to be more capital intensive than breeder reactors, and there is no guarantee that they will be any cheaper than the present-day (very expensive) solar electric modules.

The case of fusion power seems to be one in which the dream is so ideal that many careful and judicious people have fixed on it. But the realities of fusion are governed by the laws of physics, which are turning out to be more favorable to the technically less attractive options.

Part of the confusion occurs because the greatest potential advantages of fusion occur with advanced fuel cycles—often referred to as dream cycles (see box). Fusion with these fuels could truly be a clean process with essentially infinite supplies of fuel, but the present research—in Europe, Japan, and Russia as well as the United States—is directed toward fusion with deuterium and tritium as fuel. Some critics call it dirty fusion.

Another part of the confusion about what the fusion program offers the country is due to the way in which it has been aggressively sold to the Congress and the public in the last 5 years. About November 1971, the program managers for the magnetic fusion program stopped saying to Congress "we don't know how to do it," and started saying that with sufficient funds a demonstration fusion reactor could be built by 1995. New experimental machines were requested and approved, considerable money was spent for reactor studies for the first time, and plans were made for extensive test facilities to assess the special materials and engineering problems of fusion. Each year Robert Hirsch, director of the magnetic fusion program during the past 5 years, stressed new improvements in plasma performance, the optimism of the researchers, and the need for more money because of the intrinsic difficulty of the problem.

The selling of fusion has been extremely successful. The magnetic fusion budget has exploded from $38 million in fiscal 1973 to $279 million in the upcoming fiscal year. At the same time the rapid growth of the federal commitment to laser fusion—although the acknowledged primary purpose is a military one—has provided a possible alternative to the magnetic technologies and added considerably to the momentum of the government fusion program. Together laser and magnetic fusion will be allocated $380 million dollars in the coming fiscal year, more than any other energy technologies except coal conversion and the breeder.

The rapid buildup of the fusion program coincided with a great perceived need for alternative solutions to the energy supply problem, and energy analysts have stopped saying "if" fusion can be controlled and started talking about "when" fusion will become available. But no fusion machine has come close to producing more power than it consumes, and questions about how effective various inventions will be at giving the plasma conditions (temperature and longevity) needed for a reactor are still of paramount importance.

A New Invention Possibly Needed

One of the more important things that the expanded fusion program has done is to clarify the properties of various types of fusion reactors (to be described in a second article). A number of observers—particularly in the utility companies—are deciding that they do not like what they see as the most likely outcome of the Energy Research and Development Administration (ERDA) magnetic fusion program. "It is possible that a fusion reactor anyone in this country is going to want to buy is yet to be invented," says Clinton Ashworth, senior technical adviser for the Pacific Gas and Electric Company and adviser to the fusion study program of the Electric Power Research Institute (EPRI). The problem, in Ashworth's view, is that the national program is heavily tilted toward tokamaks and that they will be too big, too costly, and possibly too unreliable. More than 65 percent of the ERDA magnetic program support goes to this concept, which is a Russian invention with a hollow toroidal chamber in which a plasma is contained by a sheath of toroidal magnets.

Tokamak fusion devices lead all other alternatives in progress toward production of energy, but the conceptual studies of tokamak reactors indicate that they will have to generate at least 2000 and perhaps 5000 megawatts of electricity, be proportionally much larger than fission plants at a given power capacity, and be in many ways more technically complex. "What we don't need from fusion," says Ashworth, is "what we appear to be getting—huge complicated nuclear plants that will probably cost $10 billion each and require restructuring the energy industry to provide and use them." Such mammoth fusion machines would have many of the same siting, licensing, lead time, and perhaps political problems that fission reactors encounter today. Huge tokamak reactors would not, in Ashworth's view, and the view of many others, be a real energy option. Ben McConnell at the Carolina Power and Light Company says, "I think Ashworth is right. I don't think any utility would be interested in buying one."

Commenting on the size of the presently conceptualized tokamak reactors, Robert Hirsch says, "If you built something based on what we know today, you would make it very large to be sure it would meet its objectives. It would be huge and very expensive." But the size of a reactor depends on how the plasma parameters scale upward as larger tokamaks are built, and Hirsch says that it looks as if experiments are moving in the right direction. He visualizes a tokamak reactor that would be 1000 or 1500 megawatts in size. Asked by *Science* how he could be sure of that in the absence of the necessary scaling experiments, he said he could not, but he could see a pathway by which it could be achieved if a number of contingencies were met.

Others in the utility industry think that the rush to build a tokamak reactor will close other good options. Howard Drew of the Texas Electric Service Company finds that the general consensus still seems to be that the country has to go through some very large and expensive tokamak experiments. But, says Drew, "some of us think that before we did that we ought to spend a little more time looking at alternatives."

The biggest tokamak approved by ERDA, the Tokamak Fusion Test Reactor (TFTR) to be built at Princeton for more than $225 million, is already squeezing the rest of the program. The utility representatives, individually and through the Fusion Power Program Committee at EPRI, are trying to alter the ERDA program, to place greater weight on alternative confinement systems that have the potential to become more practical for reactors. In addition, they are trying to find ways to improve upon the presently favored systems.

The alternatives that interest the utility executives are generally inventions that make better use of the magnetic field, producing fusion at higher power densities and making reactors feasible in much smaller sizes. What they would like from fusion would be a modular technology, each unit perhaps 100 or 200 megawatts in size, that could be ordered and installed in 3 or 4 years, with components shipped from a factory. The fact that laser fusion, if it works, could operate in such a modular fashion is one of the reasons the utilities find it attractive. On the other hand, the utilities are concerned that a several-thousand-megawatt tokamak reactor would be not only too costly but also too unreliable to provide base load power for an electrical network.

The fusion advisory committee of EPRI, chaired by Francis Chen at the University of California at Los Angeles, recently prepared a study of novel fusion concepts that—while they have more technical problems—might ultimately lead to more practical reactors than tokamaks. Some of the inventions they identified were the Tormac, a toroidal machine that may offer a lower cost alternative with better plasma stability; the Z-pinch, a machine based on an old idea that has recently been shown capable of extremely high plasma density; the Laser-Heated Solenoid, in which a carbon dioxide laser would heat plasma trapped in a several-hundred-meter magnetic solenoid; various devices that might heat a plasma with Intense Ion Beams produced in devices previously thought only capable of intense electron beams; and the ELMO Bumpy Torus, which is like a tokamak with the magnetic field crimped at intervals around the doughnut.

What almost all of these devices have in common is that they seek to improve the economy and performance of the reactor by increasing the value of beta—the ratio of plasma pressure to magnetic field pressure—in order to make better use of the magnetic fields. Fusion concepts that are capable of reaching high values of beta not only project lower costs for reactors, but they also tend to be cheaper as experimental devices. ERDA supports several of the alternatives identified by the EPRI panel, but only about $5 to $10 million is assigned to "exploratory concepts."

The chief alternative to the tokamak in the ERDA program is a high-beta machine being developed at the Lawrence Livermore Laboratory. It is an open-ended machine which is named the mirror because its magnetic fields are configured in such a way that they are at least partially effective at reflecting plasma that is escaping at the ends back into the machine. The fusion power coordinating committee of ERDA recently recommended that a mirror machine comparable to the TFTR be built, but at $100 million it will cost only half as

Dream Cycles

The cleanest and most plentiful type of fusion power cannot be produced from deuterium and tritium—the fuels research programs around the world are now trying to prove feasible. On a laboratory scale, fusion can be derived from other combinations of fuels, or fuel cycles. In order of the increasing difficulty of igniting the reaction, some of the other fuel cycles are

• Deuterium with deuterium, which would be the most plentiful resource, since deuterium is found in relatively high concentrations in seawater.
• Deuterium with helium-3, which would only produce charged particles (helium and hydrogen). Neutrons copiously produced in the deuterium-deuterium and deuterium-tritium fuel cycles would be absent.
• Helium-3 with helium-3, which would also burn cleanly. However helium-3 is not a primary fuel. It must be made by a fusion of lithium and hydrogen, and thus the fuel resource would be limited by the supplies of lithium, just as the deuterium-tritium cycle [*Science* **191**, 1037 (1976)].
• Hydrogen with boron, which would produce only charged particles. Both fuels are abundant, although perhaps less so than deuterium.

Proving the feasibility of any of the advanced fuel cycles is undeniably more difficult than the considerable challenges posed by the present programs aimed at achieving deutrium-tritium fusion. But one might ask, according to Robert W. B. Best at the FOM Institute for Plasmaphysics in the Netherlands, who has studied the subject extensively for EURATOM, "Why are the radiation hazards of tritium-based fusion reactors, as measured by the large R & D effort, considered more manageable than the physical problems of clean fusion?"—W.D.M.

much as the TFTR. The third-running project in the ERDA program is also a high-beta machine, based on the theta-pinch concept. The plasma in the theta-pinch has so far defied efforts to stabilize it, and if there is not a dramatic turnaround in performance of the project, at Los Alamos Scientific Laboratory, it will be phased out at the end of next year.

Clearly one of the factors that has discouraged the utilities about the prospect of tokamak reactors is the cost. At the University of Wisconsin, Gerald Kulcinski and his colleagues have designed three conceptual tokamak reactors. The first two were estimated to cost about \$1000 per kilowatt in 1974, and the third was found to cost \$3000 per kilowatt in late 1975. The costing of the third study was done under subcontract by the Bechtel Corporation. The jump in costs was partly attributable to the design, which sought to improve on the poor thermal efficiency of the first two, but even with reduced thermal efficiency the price would have come out to be high. Kulcinski characterizes these costs as 30 to 50 percent greater than costs of a fission breeder but not out of range of the cost of many future sources.

Fusion Costs Estimated to Be High

At Princeton in 1974, Robert Mills directed the design of a large tokamak reactor, which was estimated to cost \$600 per kilowatt in 1973 dollars and would be closer to \$1000 today with inflation. Russian researchers working at the High Temperature Institute in Moscow also estimate the cost of a tokamak reactor between \$600 and \$1000 per kilowatt, according to Kulcinski, but these numbers are generally thought to be too low, perhaps by a factor of 2. The issue of costs for a tokamak fusion reactor is very controversial right now, but the general consensus of U.S. researchers is that it will be at least as expensive as the fission breeder. This is not the official position of the ERDA management, however. Hirsch says that fusion has a range of possible costs that go from levels below that projected for the breeder to levels above it.

Acknowledging that the different degrees of detail available for fusion and the fast breeder make comparisons difficult, R. Hancox at the Culham Laboratory in the United Kingdom reached an even stronger conclusion. Hancox, who directs reactor studies at what is perhaps the strongest European fusion research center, compiled cost estimates for most of the reactor designs in the United States and Europe since 1970, and found that they were three to five times as much as the capital cost of a breeder reactor. Noting that most studies conclude that the first wall of the fusion reactor (the wall of the plasma chamber) will have to be changed several times during the reactor's life-span, he found little comfort in the hope that operating and fuel costs for fusion would be low enough to offset the higher capital costs. Hancox concluded that to make a tokamak reactor competitive, ways would have to be found to increase the beta value from the currently assumed values, between 1.4 and 5.6 percent, to at least 10 percent. The beta values in actual tokamak experiments today are generally well below 1 percent.

The Soviet Union is in the final stages of planning for a tokamak that will be four times larger than the American TFTR, and already the Soviet researchers are running into hard economic realities enumerated in Hancox's analysis. The Russian machine, named the T-20, would have a plasma cross section 4 meters in diameter and an overall diameter of 10 meters. Many researchers estimate that it will be at least a billion-dollar experiment, and extrapolating from the TFTR with the generally accepted law of cost scaling (the cost going as the square of the minor radius of the torus) would put it over \$3 billion. The T-20 project is not officially approved yet, and many observers think that the Soviet Union has given up on the prospect of an economically viable pure fusion reactor.

Instead, the Soviets are now planning to build the T-20 as a hybrid reactor—a fusion core surrounded by a blanket of uranium designed with the goal of producing plutonium. The fission-fusion combination buys an energy multiplication factor of approximately 10, since 17-Mev fusion neutrons induce a fission reaction that releases 200 Mev in the blanket. Therefore, the transformation of a fusion machine into a hybrid allows the condition of energy production to be reached at plasma conditions that are much less stringent than those of a pure fusion reactor. The Soviets estimate that the T-20 will be large enough to yield the plasma parameters needed for a hybrid reactor, and they want to test its ability to compete with the fast breeder in fissile fuel production. The ERDA program also has a small component and the EPRI program may soon have a larger component devoted to hybrid reactor studies—a subject that will be covered more thoroughly in a later article.

Tokamak fusion research is not an endeavor that is suited to pessimistic people, nor is it an energy research program that is suited to a country with limited funds. ERDA has just completed a long-range planning guide that outlines the pathway, in PERT chart fashion, to a tokamak fusion demonstration reactor before 2000. The most favored pathway at the present time (called Logic 3) goes from the big Princeton tokamak to a prototype (PEPR) to show that the plasma will reach a temperature where it will be self-igniting (\$400 million), an engineering reactor to do materials tests (\$500 million), and at least one experimental power reactor (EPR) to generate tens of megawatts of electricity (\$800 million). All are seen as necessary steps before a commercial-sized demonstration reactor can be built. According to the plan, the total program cost to get to a machine that generates about 10 megawatts would be about \$9.2 billion, in addition to the \$800 million that has already been spent.

The breeder was able to reach the same milestone with an expenditure nearer a hundred million dollars, and solar electric technologies are expected to do the same. In 1964 the prototype breeder EBR-II produced 20 megawatts of electricity for a cost of \$32 million, and the cost for a 10-megawatt solar electric plant now being solicited by ERDA is estimated (in the fiscal 1977 budget hearings) at about \$90 million.

None of the critics of the fusion program are opposed to the expenditures of money required. Most of the utility representatives characterized themselves as very "pro-fusion." But many find an unrealistic degree of optimism in the program, and some are worried that the program is being too much "master-planned." If the answer to fusion has not been found in the tokamak, as many observers obviously think, then a program that puts more emphasis on innovation and discovery is called for.

The next few years will likely be crucial ones for the magnetic fusion program. Since future experiments seem certain to cost at least \$500 million each, the processes of public analysis and political scrutiny will require more and more tangible benefits. Already scientists influential in public policy are suggesting that the country must soon ask for a hard comparison, when the construction of a half-billion-dollar fusion experiment may mean the displacement of a near-demonstration plant for an alternative fission process or a major solar energy option. The consideration of the hybrid reactor as a possible goal may further cloud the issue of the desirability of fusion. According to Hirsch, it is too early to know whether or not the hybrid is a suitable goal, however.

The advantages of fusion over other energy options are probably not economic ones, and do not lie with the superiority of the fuel resource either, unless dream cycles are realized. The advantages are chiefly environmental and social ones, but those are not guaranteed. "Several ways that fusion reactors could work would not be cleaner or safer than fission," says John Holdren at the University of California at Berkeley. "My fear is that in the haste to get a machine at all, we will throw away the potential advantages as a matter of engineering expediency."

The national energy plan of ERDA emphasizes that fission, solar energy, and fusion are three long-range energy *options*, which should not all be expected to work. What is not clear at the present time is whether the efforts on behalf of fusion will be directed toward making it the most attractive option, or whether the program planners will settle for an unattractive technology likely to be implemented only if the other options fail.

—WILLIAM D. METZ

Laser Spectroscopy: Illuminating the Dynamics of Collisions

Laser light is characterized by its high intensity, monochromaticity, temporal and spatial coherence, and directionality. In the first half of this decade researchers exploited these properties to develop a panoply of ultrahigh resolution techniques for resolving the details of atomic and molecular spectra. If this collection of techniques constitutes the first wave of laser spectroscopy, then the second wave may comprise ways to apply lasers to the study of atomic and molecular collisions.

The use of lasers to study collisional phenomena conveniently breaks into two categories: steady-state and transient methods. The steady-state techniques are, for the most part, variations of the highly successful "Doppler-free" laser spectroscopies, which depend on the high intensity and monochromaticity of lasers (*Science*, 24 October 1975, p. 344). In addition to these features, the transient techniques also take advantage of the coherence properties of laser light and are optical analogs of Fourier transform nuclear magnetic resonance (NMR) (*Science*, 20 October 1972, p. 247).

Collisions are of fundamental interest to researchers in many fields. For chemists, the study of chemical reactions reduces ultimately to the study of collisions that break the chemical bonds of the reactants and allow product bonds to form. Astrophysicists measuring the spectra of radiation emitted from stars or large interstellar gas clouds need to understand collisional effects in order to extract the physical parameters of the gas from their data. And spectroscopists trying to unravel the details of atomic and molecular structure find that collisions determine the shape of spectral lines.

The latter effect on line shapes has, in fact, provided the principal means of studying collisions since the work of A. A. Michelson, 80 years ago. The width of a spectral line is determined (by way of the uncertainty principle) by the lifetimes of the quantum states involved, which can be limited by several varieties of collisions. In particular, by measuring the detailed dependence of the widths of spectral lines on gas pressure (pressure broadening), spectroscopists have obtained information about the forces between colliding particles.

One problem with this approach is that any measurement is an average over atoms or molecules with a thermal distribution of velocities and quantum states, which are constantly colliding. With the new laser methods, scientists can select specific quantum states and velocities by changing the laser frequency, but a given particle will, in general, suffer many collisions and the resulting spectrum will be averaged over these. The transient optical methods have the additional advantage of being able to distinguish between different collision mechanisms that are simultaneously operative.

A major limitation of spectroscopic methods has been the Doppler effect, whereby gas particles have a distribution of resonant frequencies for absorption or emission of radiation that corresponds to the thermal distribution of particle velocities. The Doppler effect is a considerable nuisance to spectroscopists because a broad band due to the superposition of narrow lines from each velocity group obscures the intrinsic spectrum.

Exploiting the Doppler Effect

At the Massachusetts Institute of Technology (MIT), Ali Javan, Thomas Mattick (now at the University of Washington), and their colleagues took advantage of the Doppler effect to measure the temperature dependence of pressure-broadening of absorption by ammonia molecules. Particles absorbing laser light at frequencies near the center of the Doppler band move slowly in the direction of the laser beam, whereas those in the wings of the band move rapidly. Since the temperature increases with the square of the velocity, the effective absolute temperature of particles in the wings of the Doppler profile may be more than ten times that of the apparatus.

The MIT investigators used a variation of a Doppler-free method known as saturation spectroscopy. The researchers directed two beams from a nitrous oxide infrared laser in opposite directions through a gas-filled cell. They tuned the frequencies of the two beams symmetrically about the center of the Doppler profile, one to a higher frequency and one to a lower frequency. Under these circumstances, the velocity group that absorbs light from both beams must have a nonzero velocity along the direction of the laser, and the greater the frequency shift, or detuning, the greater the velocity selected.

The experiment consists of measuring the difference in the absorption of a weak probe beam with and without a strong counterpropagating beam. The latter, termed a saturating beam, excites a large fraction of the particles in the selected velocity group, so that the excited particles do not absorb light from the probe beam. As the laser frequencies are tuned through the resonance condition (symmetric detunings), the difference in the probe absorption displays the pressure-broadened line shape.

By comparing their results with certain theorectical models for pressure-broadening, the MIT group determined that collisions between ammonia molecules are predominantly inelastic collisions—that is, collisions that change the quantum states of one or more or the colliding particles. Collisions between ammonia and xenon, however, were found to be governed by elastic interactions that changed the velocities of the particles, but not their quantum states. The MIT group was also able to estimate the forms of the force laws governing these collisions.

Charles Rhodes and William Bischel of the Lawrence Livermore Laboratory, Livermore, California, using a technique called optical double resonance, were able to measure velocity changes due to collisions. (Both researchers are now at the Stanford Research Institute, Menlo Park, California.) Optical double resonance is another Doppler-free laser spectroscopy. Two lasers of different frequencies excite two different transitions in an atom or molecule; usually both