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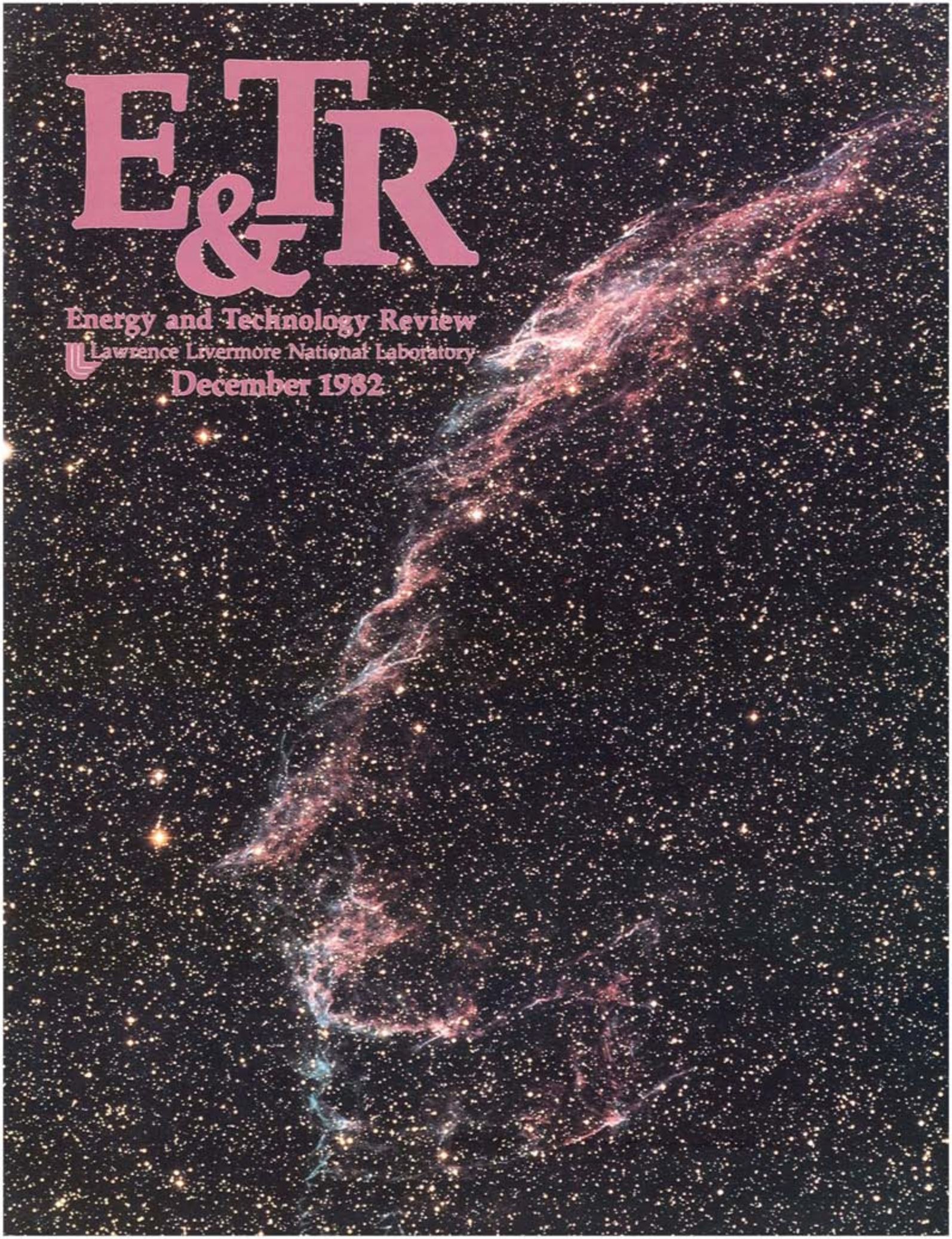
Energy and Technology Review

Lawrence Livermore National Laboratory

December 1982



On October 15, 1981, John H. Nuckolls received the American Physical Society's Maxwell Prize for outstanding contributions to plasma physics. The citation read "For his contributions to the genesis and progress of inertial confinement fusion. His insight into the fundamental physics issues has served to guide and inspire the technical evolution of the field." An article based on his Maxwell lecture to the Society was published in *Physics Today* (September 1982). Because of the interest that readers of the *Energy and Technology Review* could have in John's views on John's views on inertial confinement fusion, the editors of the *Energy and Technology Review* have agreed to reprint the article in this issue. The work described in this article has occurred because of the dedication and inspiration of John Nuckolls and his colleagues at LLNL, as well as of hundreds of researchers elsewhere in the United States and other nations. In addition to their work, consistent support for the U.S. Inertial Fusion Program has been provided by the Department of Energy and its predecessors and by the Congress of the United States. This support has led, in 10 years time, to an effort with the accomplishments and scope described in this article. It should be further noted that, although the thrust of this article is directed toward the long-range power-potential of the Inertial Fusion Program, current support and nearer-term goals are defense related.

Feasibility of Inertial-Confinement Fusion

"We can see no insurmountable roadblocks to the practical achievement of electrical power generated by inertial-confinement fusion ... An ICF reactor may have a relatively small containment volume [and] its operation, maintenance, and repair will be relatively simple."

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So concluded the chairman of a Department of Energy *ad hoc* committee of experts in 1979, after a comprehensive review of the U.S. inertial-confinement fusion program.[1] In spite of this positive evaluation, the role of inertial-confinement fusion in the total U.S. energy program continues to be a subject of disagreement.[2] Before I mention the issues of contention, let me describe inertial-confinement fusion briefly. In a typical reactor scheme, a pea-sized target pellet containing hydrogen isotopes is projected into a reactor chamber, where it is suddenly irradiated with an intense beam of light or ions from a "driver" (Fig. 1). As the surface of the target blasts away, the rocket-like reaction forces implode the target's interior to densities and temperatures sufficient to cause the hydrogen nuclei to fuse, releasing an amount of energy equivalent to that of a barrel of oil (see *Physics Today*, August 1973, page 46).

Those who are skeptical of the inertial-confinement fusion program have a number of objections. They cite the fact that the magnetic-fusion machines—tokamaks and mirrors—are several years ahead, and they note the time and effort that will be required to solve the technical problems, principally the coupling of the laser beam to the target, the confinement of the fusion explosion, and the development of a practical driver.

During the past three years, however, inertial-confinement fusion has made substantial progress toward solving its principal scientific and technological problems. The program has:

- Achieved efficient beam-target coupling using short-wavelength lasers,
- Achieved high-density implosions with so-called "hohlraum" targets, which we will discuss later,

- Improved the design of damage-proof fluid walls for reactors with high power densities,
- Designed drivers of higher efficiency and lower cost, and
- Planned to utilize the physical separation of the reactor and the high-technology components—the driver and the target factory—which several reactors can share. These developments make possible fusion reactors with minimum cost and radioactive waste.

The Rise of Fusion

In 1942, Hans Bethe, Enrico Fermi, J. Robert Oppenheimer, Edward Teller, and others began to develop the physics of practical fusion. Later, at Los Alamos, fusion research was driven by the determination and technical leadership of Teller. In 1952, seven years after development of the atomic trigger, seismographs worldwide registered the success of the multimegaton inertial-confinement device. In the 1950s, the extraordinary promise of controlled fusion—safe, clean, low-cost energy with inexhaustible fuel accessible to all nations—led to research programs on magnetic-confinement fusion at many scientific laboratories. The 1960s saw the conception and calculation of inertial approaches to controlled fusion, including schemes based on implosions driven by laser and charged-particle beams.

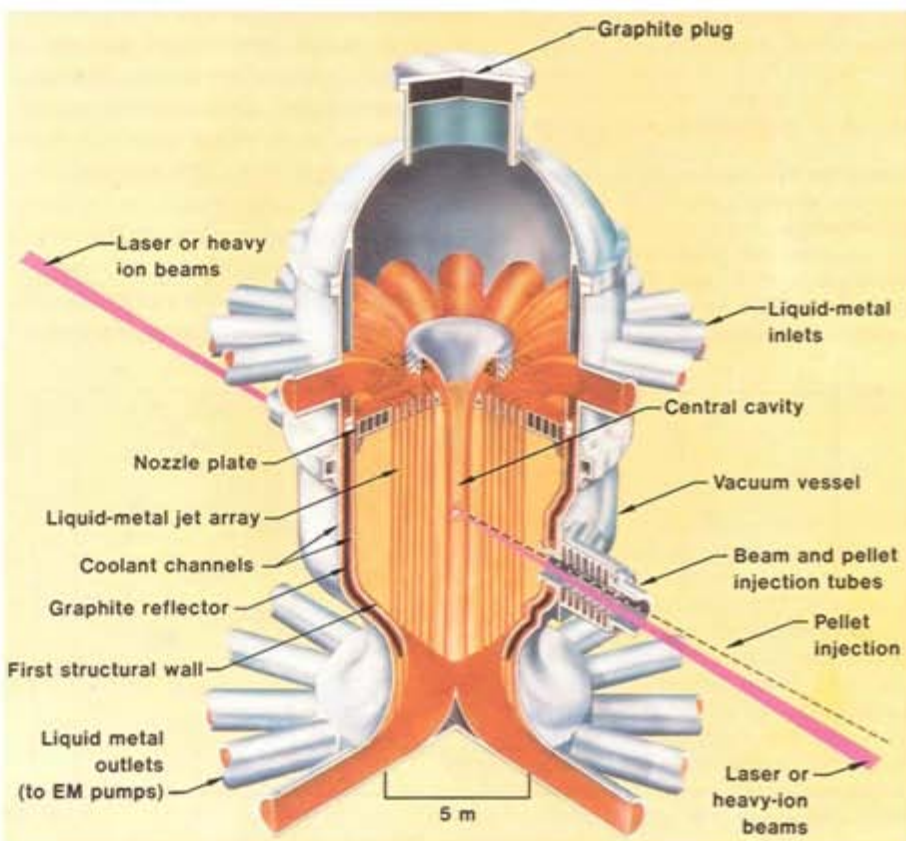
The energy crisis stimulated rapid growth in both the magnetic and inertial-confinement fusion programs in the early 1970s. The inertial-confinement fusion programs at LLNL, the University of Rochester Laboratory for Laser Energies, the Lebedev Institute in the USSR, and Limeil Laboratories in France were joined by the new programs at KMS Fusion Inc., Los Alamos and Sandia National Laboratories, Naval Research Laboratory, Rutherford Laboratory in England, Osaka University in Japan, Max Planck Institute at Garching in West Germany, and laboratories elsewhere. Rapid growth was sustained throughout the 1970s, driven by OPEC policies, fission's political and technical difficulties, and fusion's scientific progress.

In 1980, Congress and the President strongly endorsed accelerated development of magnetic fusion. Although economic difficulties in the United States have postponed this growth, the urgency for fusion continues to increase. The Middle East, source of much of our oil, is unstable. Weaknesses and uncertainties are emerging in the coal-fission strategy for energy independence. Fission energy is increasingly impeded by Murphy's Law (what can go wrong, will) and growing public opposition, while coal energy is generating acid rain and in the 21st century may produce major global climate changes through the CO_2 greenhouse effect.

To contribute to the solution of these energy problems in the next century, inertial fusion must meet several difficult challenges. The DOE experts' committee, which was headed by John Foster, Jr., vice-president of science and technology at TRW Inc., and former DOD Director of Defense Research and Engineering and Director of Lawrence Livermore, noted that "... many problems and unknowns remain, but because of the wealth of promising

Fig. 1

Inertial-confinement-fusion reactor concept known as HYLIFE. A shower of liquid lithium shields the first wall from damaging neutrons, x rays, and hot plasma from exploding fuel pellets. With an average lithium thickness of 50 cm, the neutron damage is sufficiently reduced to increase the lifetime of the first wall tenfold. The neutron-induced radioactivity in the reactor structure is reduced 10- to 100-fold. The average power at the first wall is relatively high, approximately 10 MW/m^2. More than 90% of this power is absorbed by the liquid lithium.



approaches, it seems plausible that with a continued national effort, a prototype experimental ICF reactor will be in operation by the turn of the century."[1] What are these problems, unknowns, and promising approaches?

As we have indicated above, the key idea of inertial-confinement fusion is to ignite small-scale, contained fusion explosions by using high-power beams of energy that implode fuel to superhigh densities and thermonuclear temperatures. For both inertial and magnetic fusion, burn efficiency—the percentage of nuclei that fuse—is proportional to the product of density and confinement time. In magnetic fusion, the fuel density is limited by material properties, so one increases the efficiency of the burn by extending the duration of confinement. In inertial fusion, Newton's laws and thermal velocity limit the confinement time, so one tries to compress the fuel to higher densities. Densities up to 1000 g/cm^3 are energetically accessible via isentropic, or constant-entropy, compression because the Fermi energy—the minimum energy of compressed matter—is much less than the thermal energy at ignition.

There are two basic target designs for inertial-confinement fusion reactors:

- Direct targets absorb the driver beam energy in an outer layer or "shell," which acts directly to produce the implosion.
- Hohlraum targets absorb the driver energy and convert it to x rays, which are contained by a radiation case and are used to drive the implosion.

The hohlraum target designs are classified.

Inertial-confinement fusion must demonstrate that one can achieve a sufficiently high target gain with an affordable driver. Target gain is the ratio of the energy yield of the fusion reaction to the driver energy applied to the target. Assuming driver and implosion efficiencies of 10%, the target gain must be greater than 100. Such a gain is feasible with multimegajoule drivers if three conditions are satisfied: the beam couples efficiently to the target, the fuel is efficiently compressed to 1000 times its liquid density, and a small fraction of the fuel is ignited at the center of the target pellet. The remaining fuel must also ignite by propagation from the central ignited region and burn efficiently.

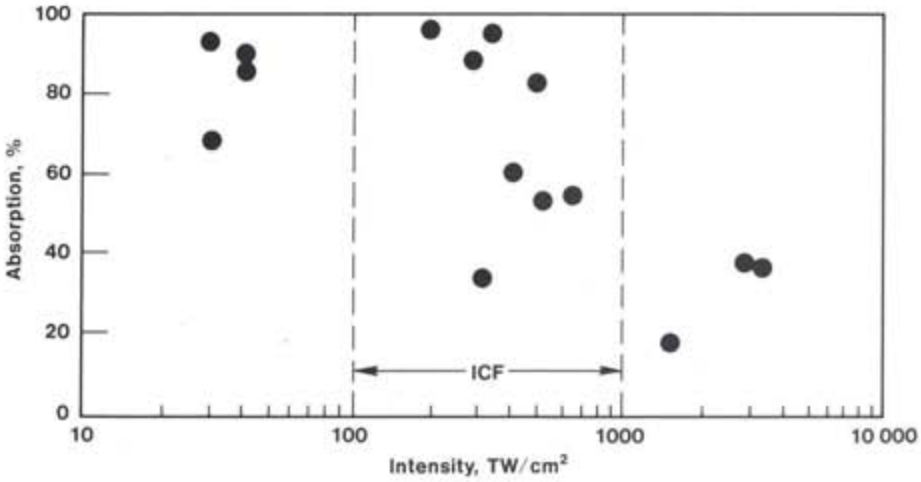
For practical fusion power, we must develop three technologies to harness the high-gain targets: an efficient and affordable driver that couples efficiently to the target, a practical and economically competitive reactor that incorporates solutions to the problems of cyclic fatigue and stress from pulsed radiation, and a practical target factory. We will address these scientific and technological issues.

By using high-intensity short-wavelength laser light, researchers have improved the absorption of energy by targets and reduced the production of hot electrons, which heat the interior of the fuel pellet and make compression less efficient. Figure 2 shows the results of laser-plasma coupling experiments in which absorption was measured[3] as a function of intensity for three laser wavelengths: 1, 1/2, and 1/3 micron. Inertial-confinement fusion targets are designed to operate at intensities of 10^{14} to 10^{15} W/cm^2. At these intensities, there is 90% absorption of short-wavelength light. Also, fewer hot electrons are produced with shorter laser wavelengths.[3] The softer tail of hot x rays in Fig. 3 indicates the reduced production of hot electrons. Experiments at laboratories around the world have confirmed these results.[4]

The efficient coupling of beam and target at short laser wavelengths

Fig. 2

Absorption of laser light by targets increases dramatically with shorter wavelengths. From bottom to top, curves cover data points from experiments at 1, 1/2, and 1/3 micron. (Data from Ecole Polytechnique, University of Rochester, and LLNL.)



confirms our theoretical predictions.[5] Collisional absorption (inverse bremsstrahlung) competes with collective absorption—principally by way of Brillouin and Raman scattering, self-focusing or "filamentation," and the two-plasmon decay instability. At shorter wavelengths, laser light penetrates into denser plasma with higher collision frequency. This raises the threshold for collective instabilities.

Current experiments involve plasmas of millimeter scale length. We must also carry out plasma experiments to test our theoretical predictions at the centimeter scale lengths characteristic of reactor targets. In addition, we need space- and time-resolved measurements of the plasma density profiles and temperatures to confirm our understanding of the details of the laser-plasma interaction.

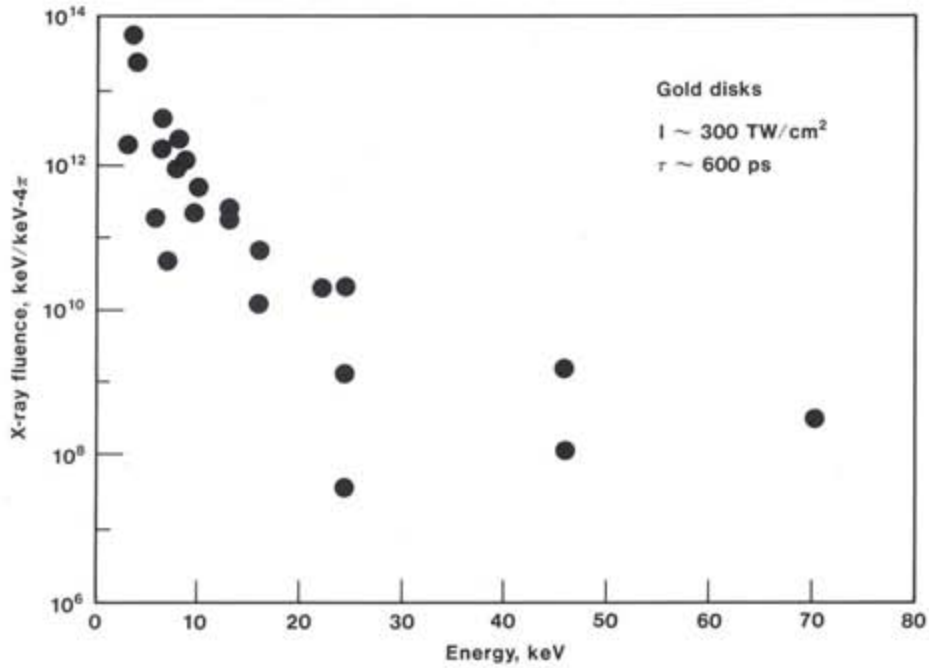


Fig. 3

Superthermal x rays decrease by over two orders of magnitude when shorter-wavelength laser light is used. Such x rays indicate the presence of superthermal electrons capable of heating the target prematurely. The three x-ray energy spectra shown here are from experiments on gold disk targets exposed to laser light focused to about 3×10^{14} W/cm². From top to bottom, curves cover data points from experiments at 1, 1/2, and 1/3 micron.

Scientific Feasibility

The highest density achieved to date in laser implosion experiments with deuterium-tritium fuel is 100 times that of the liquid. As Fig. 4 shows, this superdense material had a temperature of half a keV and a pressure of 10^{10} atmospheres. These results are in good agreement with theory and with detailed calculations using a complex magnetohydrodynamics-fusion computer program called LASNEX. To ignite reactor-scale targets, the deuterium-tritium must be imploded to a thousand times liquid density and multi-keV temperatures. The ignition pressure is about 10^{12} atmospheres. We expect to achieve 200 to 400 times liquid density with the 10-kJ, 1/3-micron Novette laser by 1984–85, and 1000 times liquid density with the 100-kJ, 1/3-micron Nova I laser by 1986–88.

In these high-density Nova experiments, the fusion energy generated by the deuterium-tritium will approach the deuterium-tritium thermal energy.

Figure 4 shows the status and goals of inertial fusion experiments. The experiments that achieved fuel densities 100 times that of the liquid were performed with the LLNL 10-kJ, 1-micron Shiva laser and had a confinement parameter (the product of density and confinement time) greater than 10^{14} cm^{-3}·s. The Nova I experiments should reach the threshold of ignition—that is, $nt > 3 \times 10^{14}$ cm^{-3}·s and temperatures of about 2 keV. Because of the inefficiencies of the driver and the implosion, inertial-confinement fusion has to push nt a factor of 10 further than magnetic fusion to reach conditions that could lead to practical reactors.

The next major challenge for inertial-confinement fusion is to demonstrate scientific feasibility. Before a reactor

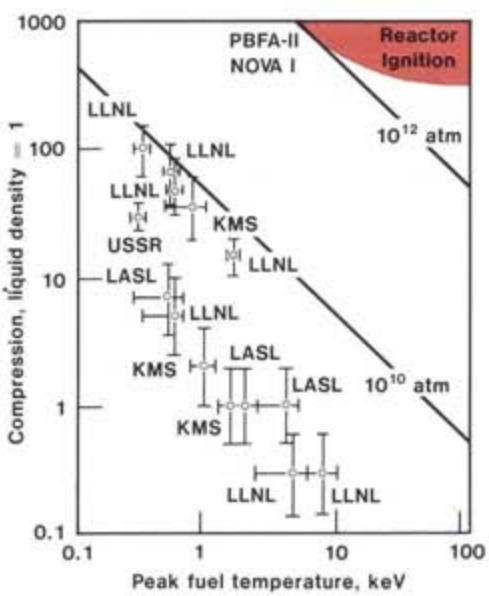


Fig. 4

Results of experiments at various inertial-confinement-fusion research facilties. Point at upper left represents the laser-driven implosion of a deuterium-tritium hohlraum target of classified design. Radiochemical techniques showed the density to be 100 times that of a liquid deuterium-tritium mixture. With the Nova laser, researchers plan to implode deuterium-tritium to a temperature of several keV and a density 1000 times that of the liquid.

driver can be designed, funded, and constructed, we must have have experimental confirmation of theoretical estimates of the required driver parameters—particularly the driver's energy. The reactor driver is so expensive—perhaps a half billion dollars—that it is not feasible to build it twice as large as required, to provide a substantial margin for error. Consequently, we must generate convincing experimental evidence showing that if the reactor-scale driver were constructed, high target gains would result. This convincing evidence may be used as an operational definition of scientific feasibility.

Physicists will use the Nova laser at LLNL and the light-ion facility at Sandia, the Particle Beam Fusion Accelerator II (PBFA II), to explore scientific feasibility. For directly imploded targets, the central core will reach high densities and the threshold of ignition. Then, even though the volume of material at this high density will not be large enough for the burn to propagate in the pellet, it is feasible to diagnose the important parameters: the density distribution, the temperature, and the amount of contamination of the fuel by the imploding shell. A series of implosion experiments will diagnose the imploding shells. Snapshots will be taken with a laser-driven x-ray backlighting source. In addition, researchers will observe the surfaces of the imploding shells with optical and x-ray streaking cameras to measure the amount of preheating, the velocity history, and the symmetry of the implosion. Finally, they will use the Nova to perform laser-plasma coupling experiments at the scale lengths of the reactor target plasma. The results of these measurements will allow us to predict with sufficiently high confidence how large a target and driver will be required to achieve high gain. The Nova and PBFA II facilities will be completed in the mid-eighties.

A Three-Part System

Let us look at each of the key technologies for inertial-confinement fusion power plants: drivers, targets, and reactors. There has been substantial progress in all three areas.

With projected improvements in target gains, the driver would have to deliver an energy of about 2 MJ, a peak power of 100 TW, at an efficiency of 10 to 20%. The beams from the driver must focus to spots a few millimeters in diameter across a 5-m chamber to give a power density of 10^{14} to 10^{15} W/cm^2. The coupling between the beam and the target must be relatively efficient. We will need a repetition rate of ten to twenty implosions per second. The driver should cost less than $400 million.

The candidates for the driver are KrF, CO_2, and free-electron lasers, and beams of heavy and light ions. We are optimistic that light ions will be able to focus to 10^{14} W/cm^2. Researchers have already focused light-ion beams to 10^{12} W/cm^2. We have not yet achieved efficient beam-target coupling with CO_2 lasers. The 40-kJ Antares laser—the world's largest CO_2 laser, now under construction at Los Alamos—will help explore this problem. Imaginative targets are now being tested on the 10-kJ CO_2 Helios laser at Los Alamos.

With current technologies, most of the proposed drivers would cost approximately $400/J, or $800 million for 2 MJ. Many of the candidates are making steady progress in reducing their costs. We foresee a factor-of-two cost reduction to about $200/J. Light-ion accelerators and advanced lasers may be significantly cheaper. When the cost of the fraction of the reactor needed to power the driver is included, highly efficient drivers such as heavy-ion accelerators with 25% efficiency may have nearly a twofold cost advantage over marginally efficient drivers such as lasers with 5% efficiency. The table summarizes the status, strengths, and weaknesses of the driver candidates.

Targets. Both hohlraum and direct targets are under development. These target designs include margins of safety to cover uncertainties in the implosion and burn.

Targets for inertial-confinement fusion are fabricated from very small masses of low-cost materials (except for

tritium, which is regenerated in the reactor). Target costs, then, are dominated by the capital and operating costs of the target factory. There are plausible solutions for the principal fabrication problems: surface finish, on-line inspection, and high production rate.

In hohlraum targets, as we mentioned, the energy of the driver beam is absorbed and converted to x rays, which are contained by a radiation case and used to drive the implosion. This gives the hohlraum target one of its important advantages—improved implosion symmetry. One can achieve symmetry without requiring the beams of the driver to be uniform and symmetrically arranged. In some reactor designs, just two beams drive the target. It is more difficult to implode directly driven targets symmetrically with a few beams. Another important advantage of hohlraum targets is that they couple efficiently to several of the driver candidates—possibly including CO_2 lasers. Researchers at LLNL have done many experiments with hohlraum targets using the 1-micron Shiva laser and the 1-, 1/2-, and 1/3-micron Argus laser. The experimental results are in excellent agreement with detailed LASNEX calculations and with theoretical scaling relationships.

Directly imploded targets appear to be advantageous at driver energies under 100-kJ driver energy. Researchers at KMS, NRL, the University of Rochester, and laboratories outside the U.S. are attempting to develop directly imploded reactor targets with both the required implosion symmetry and as high an implosion efficiency as that of the hohlraum targets. Implosion efficiency is the overall energy efficiency with which the beam couples to the target and compresses the fuel. An important problem is how to generate and combine many ultrasmooth laser beams or ion beams.

Significant improvements in targets for inertial-confinement fusion are possible. Compression of the fuel to higher density is theoretically possible. Because the scaling parameter for the burn efficiency of the fuel is the product of the mass of the fuel and the square of its compression, even a 40% increase in density would decrease the fuel mass and driver size by a factor of two.

Driver candidates.

Requirements	CO_2 laser	KrF laser	Free-electron laser	Heavy-ion accelerator	Light-ion accelerator
Efficiency (10-20%)	?	?	+	++	+
Focusing (10^{16}-10^{15} W/cm^2 at 5 m)	+	+	+	+	?
Target coupling (10%)	?	++	++	++	+
Repetition rate (10-20 Hz)	+	+	++	++	+
Cost ($200/J at 2 MJ)	+	+	+	+	++

+: could meet requirements.

It may also be feasible to make significant improvements in the target's implosion efficiency. Nonablative implosion schemes may have a higher implosion efficiency. This could decrease the size of the driver and increase the target gain by a factor of two.

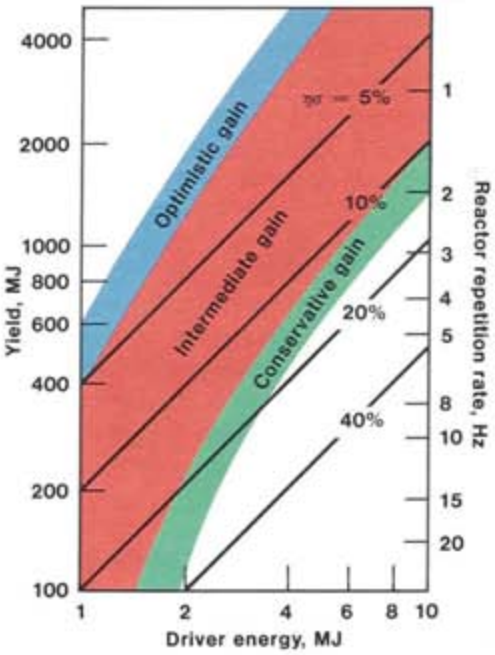
Finally, current target designs provide a substantial margin for error. We may be able to relax these conservative design criteria when we have a greater experimental and theoretical understanding of the implosion and burning of the targets. Such a relaxation could provide another factor-of-two decrease in driver size and increase in target gain.

We hope to achieve a twofold reduction in driver energy requirements and a twofold increase in target gain.

Figure 5 relates the performance of the target to driver and reactor requirements.[6] The lower band in this figure shows the calculated gains of our conservative targets. The optimistic band represents an improvement in target performance by almost a factor of four. The design point is at 2 MJ with a yield of 600 MJ—a gain of 300. For a 1-GW_e reactor and and 10 to 20% efficient driver, the overall energetics would be robust. Driving fusion with a KrF laser of 7% efficiency would give a good overall energy gain for optimistic target performance but only a marginal gain

Fig. 5

Relationships of performance parameters of target, driver, and reactor for a 1-GW$_e$ power plant burning deuterium-tritium fuel. Inertial-confinement-fusion reactors may be economically competitive with light-water fission reactors if they can be designed to have a 2-MJ driver energy (at a direct cost of $200/J), 600-MJ target yield, driver efficiency η_d greater than 15%, 5-Hz reactor repetition rate, and 10- to 20-Hz driver repetition rate. The driver would be shared by two to four reactors.



with projected target performance. The lines of minimum necessary driver efficiency in Fig. 5 are based on the assumption that the product of the efficiency of the driver and the gain of the target is 20. Then the driver uses 10 to 15% of the power the reactor produces.

Reactors. A critical issue for inertial-confinement fusion reactors is the pulsed thermonuclear explosion, which could cause unshielded reactor walls to fail by cyclic fatigue or ablation. In the LLNL HYLIFE fluid-wall design (Fig. 1), there is a shower-like set of fluid jets of liquid lithium between the exploding target and the first wall.[6] The fluid layer, 0.5 to 1 m thick, protects the first wall from the pulse of radiation and hot plasma. It also reduces by a factor of about 10 to 100 the activation and neutron damage in the first wall.

Another issue[2] is the feasibility of containing small thermonuclear explosions, each releasing an energy equal to a few tenths of a ton of TNT. The impulse of an explosion is proportional to the square root of the product of mass and energy. Although the energy, which is mainly radiated into the fluid curtain by neutrons and x rays, corresponds to a significant fraction of a ton of TNT, the mass of the target is a million times smaller because the energy is generated by 10-MeV thermonuclear reactions rather than by 1-eV chemical reactions. Thus, the impulse is reduced a thousandfold because the mass is a million times smaller. In addition, the lithium is sufficiently massive and far from the wall that its interaction with the wall is largely nonimpulsive.

Radioactive target debris is also an issue. Any material in the target besides deuterium and tritium will be activated by the intense flux of neutrons. Debris is also activated further when it is recirculated in the liquid lithium inside the reactor. In targets designed to minimize radioactive debris, the most significant radioactive material is lead. The dominant radioactive isotope of lead is lead-203, with a half-life of 53 hours. Other radioactive isotopes are negligible. A few months after the lead has been separated from the lithium, the radioactivity has decayed by a large factor, and the lead may be recycled in the target factory. The total mass of radioactive target debris generated during the 30-year lifetime of the reactor is a few tons.

In the HYLIFE scheme (Fig. 1), beams come to the target from drivers that are of about 100 m from the reaction chamber, which has a characteristic dimension about 5 m. The average product of density and thickness through the pattern of lithium jets is about 40 g/cm^2. Figure 6 shows the lifetime of a first wall made of low-chromium, nickel-free steel as a function of the thickness of the lithium.[6] In fusion reactors, at least two neutronic effects reduce the lifetime of the first wall: atomic displacements and the generation of helium. For a lithium thickness of 0.5 m, the first wall's lifetime is about 30 years, so it would not have to be changed during the lifetime of the reactor. Without the fluid shield, the wall would last about five years and would have to be replaced by remote control several times during the life of the reactor.

Because the time required for a splattered droplet to fall 5 m is about 1 s, the current HYLIFE design is limited to a repetition rate of about 1 Hz. To increase the repetition rate to 5 Hz so that we can fully exploit both high-repetition-rate drivers and the heat

capacity of the flowing lithium, which is several times larger than required, we must improve the design. With evolutionary improvements, 3 Hz may be feasible. Several radical variations would take us beyond that. In the HIBALL scheme,[7] for example, porous silicon carbide tubes would confine the streams of liquid lithium-lead alloys. These tubes would also reduce the liquid flow rate.

With respect to feasibility, the conclusions of the Foster Committee are valid: the number of promising approaches is in reasonable balance with the problems and unknowns, and no insurmountable roadblocks are evident. Beyond scientific feasibility, a major challenge for the inertial-confinement fusion program is to realize the potential advances: higher gain targets with reduced driver requirements; cheaper, high-efficiency drivers; and fluid-wall reactors able to withstand high repetition rates.

Utility

We now turn our attention to the two crucial questions of economic and environmental utility for all approaches to fusion:

- In the limit of its best performance, is fusion economically feasible?
- Do fusion's economic and environmental potentials justify accelerated development, particularly in light of the possible limitations of coal and fission energy? Unfortunately, fusion's economic and environmental potentials are not very well known. Nevertheless, I will attempt to answer these questions assuming we can achieve scientific feasibility and the potential advances that we have discussed.

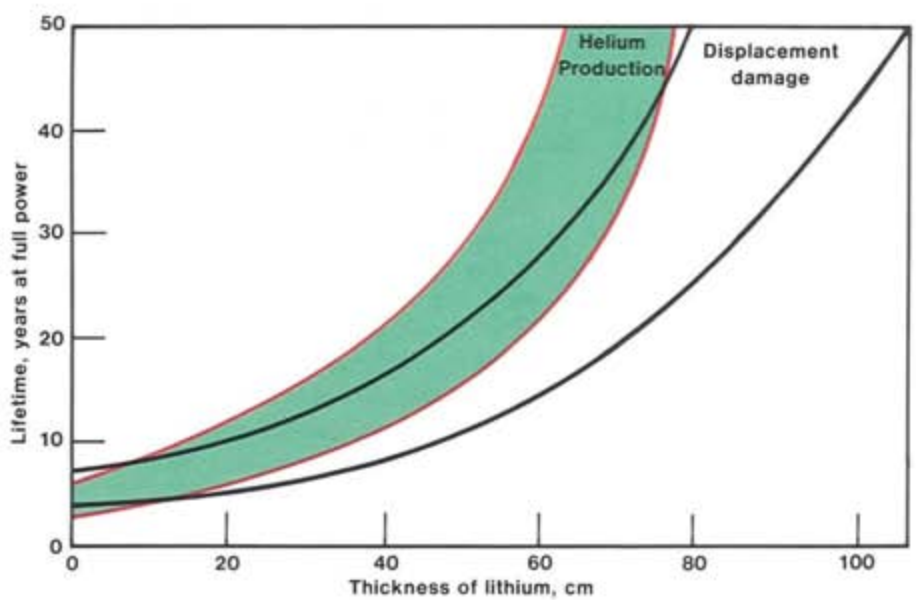
Were it not for the potential existence of the "fusion hybrid" reactor—and the liquid-metal fast breeder reactor—the long-term escalation of fossil and fission fuel costs and the cost of externalities such as radioactive waste and safety hazards might make fusion economically competitive even at twice the cost of current fossil and fission energy systems. Fusion hybrids produce fuel for fission reactors. Most first-generation inertial and magnetic fusion systems are estimated to be roughly a factor of two more expensive than light-water fission reactors. However, fusion hybrids would cap any increase in the cost of fission energy due to escalating fission-fuel costs at only 20% of the total cost of the light-water reactors themselves, because a hybrid costing as much as three light-water reactors could supply fuel for 15 light-water reactors. Furthermore, the National Academy of Sciences Committee on Nuclear and Alternative Energy Systems estimates that externalities are not significant. Liquid-metal fast breeder reactors would also limit the escalation of fission fuel costs.[8]

Nonetheless, because U.S. energy costs are less than 10% of the GNP, our society could decide to buy an environmentally superior energy system costing twice as much as a minimum-cost energy system. However, U.S. industry powered by a doubly expensive energy system, employing expensive labor, would be less competitive with foreign industry powered by minimum-cost energy systems. This would be particularly true if foreign industry invested its energy system cost savings in automation, advanced technology, and research and development.

An important factor that may make possible inertial-confinement fusion power plants that are economically competitive with fission is driver

Fig. 6

First-wall lifetime, as a function of the thickness of the protective liquid lithium curtain. These estimates are for a 2.25 Cr-1.0 Mo-steel wall in a 10-MW/m² fusion reactor. A half meter of shielding extends the lifetime from a few years to 30 years. Uncertainties are indicated in the effects of neutron-induced helium production and atomic displacements. The upper and lower curves for helium production shown here represent 1000 and 500 atomic parts per million of helium, respectively. The upper and lower displacement damage curves represent 330 and 165 displacements per atom, respectively. (From Ref. 6.)



separability, which the Foster Committee discussed: "One of the chief advantages of ICF is that ... the driver is separate from the reactor vessel itself, and can be removed some distance. This means that an ICF reactor may have a relatively small containment volume; that its operation, maintenance, and repair will be relatively simple; and that the most expensive components will not be subject to neutron bombardment and activation. The potential also exists for a great variety of power plant sizes and configurations." Furthermore, a separated driver can drive several reactors, and time-shared target factories are feasible as well.

For inertial and magnetic confinement approaches where time sharing and fluid walls are not feasible, one can reduce costs and radioactivity by other means, such as developing cheaper superconducting materials that sustain higher magnetic fields (to increase the power density), and developing first-wall materials with longer lifetimes and less neutron-induced radioactivity.

The high cost of first-generation fusion reactors relative to light-water fission reactors is principally due to three factors:

- The costs of fusion technology equipment—inertial confinement's driver and target factory, or magnetic confinement's superconducting magnet, beam heater, and high-vacuum system.
- Fusion's lower power density.
- Fusion's more complex first-wall and heat-transfer structures.

Reducing Costs

Even if fusion-technology equipment were free, some approaches to fusion power would be more expensive than light-water reactor power because of the second two factors. To be economically competitive, the cost premium due to these three factors must be less than about 50%—20% for fission fuel escalation and 25% for unescalated fission fuel-cycle costs. In light-water reactors, fuel-cycle costs are approximately 25% of the total and are not included in capital costs. To a first approximation, fusion reactors could compete economically with light-water reactors even if they were almost 50% more expensive to build. Here is one possible accounting of the extra costs:

- The cost of the driver is less than 20% of the cost of a light-water reactor. This would compensate for the 20% increase in the cost of fission energy due to the escalation of fuel costs.
- The cost of the targets and target factory is less than 25% of the cost of a light-water reactor. This compensates for the light-water reactor's 25% fuel-cycle expense. The capital cost of the reactor and the rest of the power plant must not be more than that of a light-water reactor.

The cost of the driver may be reduced to 10 to 20% of the cost of a 1-GW_e light-water reactor—approximately $150 million not including interest charges. For multimegajoule short-wavelength lasers and heavy-ion accelerators, one can achieve such a reduction by reducing the driver costs twofold to $200/J at 2 MJ, improving the targets twofold (to reduce the required driver energy to 2 MJ) and by using a single driver for two or more reactors. We have already discussed these driver and target improvements.

Multireactor systems with time-shared drivers are apparently feasible. Laser and heavy-ion beams can be switched rapidly and propagated over 100-m distances; these drivers are capable of 10- to 20-Hz repetition rates with relatively small increases in cost. Some high-energy accelerators, from which heavy-ion technology is derived, show high capacity factors. If a few additional beams are designed into multibeam fusion lasers, then individual beams could be shut down for repair without affecting the operation of the reactor. A four-reactor site with 4-GW_e total electrical output would generate roughly 1% of the projected U.S. electrical energy in 2010 and less than 0.5% in 2050, assuming a slow 2% energy growth rate.

Driver technologies such as light-ion accelerators and advanced lasers may be cheap enough so that with an additional factor-of-two improvement in target performance, fusion reactors as small as 0.5 GW_e would be

economically competitive with light-water reactors. These small reactors would be highly advantageous for initial commercialization.

The estimated capital and operating cost of the target factory is 10 to 30% of the cost of a light-water reactor.[7] Several factors limit the cost. These include the factory's small size, complete automation, and long-term high production rates, as well as its location at the power plant and the automated transport of pellets to the reactor chamber.

The capital cost of a HIBALL fluid reactor and the rest of the power plant without the driver and target factory is estimated to be approximately equal to the capital cost of a light-water reactor.[7]

Inertial-confinement fusion reactors may have an exceptionally high capacity for optimization. The target is small and cheap and may be improved during the lifetime of the reactor. The driver and target factory, being separate from the reactor, are not strongly activated by the fusion neutrons. Utility-bankrupting meltdown accidents are not possible when the reactor is switched off. Based on our experience with fission reactors, this potential for incorporating technological advances and responding gracefully to Murphy's Law is important for the development of practical high-technology power plants.

Deuterium-burning reactors would have significant environmental advantages over deuterium-tritium reactors and might also be cheaper. The radioactivity of the first wall would be reduced because fusing deuterium produces fewer and less energetic neutrons. To eliminate the tritium generated in the liquid-lithium curtain, one would protect the first wall with a material such as liquid lead. By eliminating the lithium and minimizing the tritium hazard and first-wall radioactivity, fusion would approach its full environmental potential. Because the fuel burn occurs inside the target and because fewer neutrons are produced, deuterium-burning inertial-confinement fusion reactors would have a higher power density than deuterium-tritium-burning reactors. Such a deuterium-burning inertial-confinement fusion system may be significantly cheaper than a light-water reactor, depending on the size of the driver required to achieve high gains with deuterium-burning targets.

Attractive fusion-fission hybrids and hydrogen synfuel producers are also feasible—hybrids, because neutron and tritium breeding is possible within the first wall, and synfuel producers, because reactors with walls at very high temperatures may be feasible.[9]

Options for the Future

There are major uncertainties in our understanding of the CO_2 greenhouse problem. However, if calculations are correct, before the middle of the 21st century it may be highly desirable to rely on nonfossil fuels to generate more than 50% of the total energy. U.S. fission energy could not be accelerated rapidly enough to sustain historic economic growth rates, and is now in the process of losing its competitive advantage.

One option would be to accept slower economic growth and the associated loss in GNP.

A second option would be to import uranium and zero-defect, low-cost "Toyota" and "VW" fission reactors. These two options would be extraordinarily expensive, costing many trillions of dollars over several decades.

A third option would be to counteract the warming caused by the greenhouse effect with global climate control—seeding the stratosphere with aerosals to increase Earth's albedo, for example.[10] The greenhouse warming due to the projected doubling of the CO_2 in the atmosphere might be counteracted by increasing the albedo by 2%. Global climate control would require an unprecedented degree of international cooperation. The best strategy might be to intensify energy conservation, revitalize the U.S. fission industry, and accelerate the development of economically competitive fusion and solar energy.

Figure 7 shows the results of a recent calculation of the CO_2 greenhouse effect by NASA scientists.[11] Their

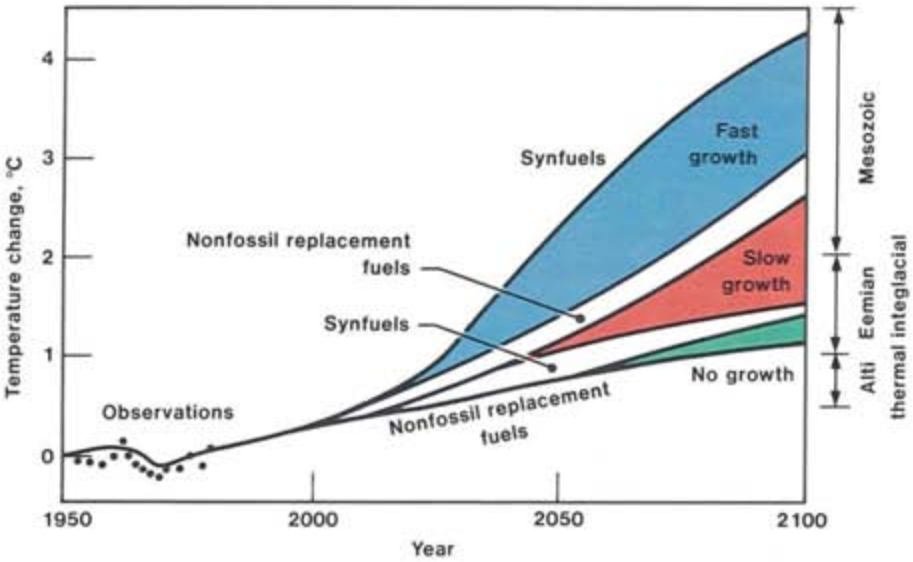


Fig. 7

Projected average global temperature change due to CO_2 produced in the combustion of fossil fuels. The temperature is expected to rise above the background noise in the time period 1990 to 2000. The graph shows the results of various economic growth rates and strategies of fuel use. The upper and lower slow-growth curves represent results of calculations assuming no coal phaseout and a coal phaseout beginning in the year 2020, respectively. "Business as usual" scenarios generate average temperatures approaching those of the dinosaur age. In the slow-energy-growth scenario, the temperature increase may be limited to 1.5°C by starting to phase out coal burning fairly early in the 21st century. (From Ref. 11.)

model predicts the average increase in global temperature due to CO_2 from the burning of fossil fuel. The calculations indicate that the temperature increase will become observable in the period 1990 to 2000. With slow (2%) energy growth rates and no reduction in the use of coal, the temperature increases 2.5°C by 2100. If coal burning is phased out in the period 2020 to 2060, the temperature increase is only 1.5°C. This is not much more than the 1°C temperature increase that is predicted to occur because of the CO_2 already added to the atmosphere. When we take into account the warming due to both CO_2 and trace gases, the time to begin phasing out the burning of coal may advance 20 years to the early 21st century.

Optimists argue that the net greenhouse effect may be beneficial. Part of the USSR might become viable farmland, although part of the U.S. prime middle western farmbelt may become drought-prone. The growing season would lengthen and plants might grow faster. Melting of the West Antarctic ice cap and flooding of the world's coastal cities probably would not occur in the 21st century. However, while the rate of change of climate may be difficult even for affluent civilizations, it could be disastrous for underdeveloped nations with populations that already border on starvation.

Coal burning will also cause serious environmental problems such as acid rain.

Because it takes 40 years to install a new energy technology, the candidates to replace fossil fuels should be demonstrated in this century. The most highly developed candidate is fission. An important question is, could fission meet U.S. energy needs by 2030? The U.S. fission industry is being crippled by Murphy's Law. No new domestic orders for reactors are projected for 5 to 10 years. The probability of a reactor-disabling (and utility-bankrupting?) accident is estimated to be as high as one in thirty per reactor lifetime. Alvin Weinberg has called for the development of a second-generation light-water reactor, 10 to 100 times more reliable than current designs.[12]

A recent study[13] by TRW and LLNL scientists—including Teller, a leading proponent of the coal-fission energy strategy—concluded that with moderate energy growth rates fission could generate only about 15% of the total U.S. energy by 2040. Inspection of these calculations shows that with either slow (2%) energy growth rates and limited energy conservation or with historic energy growth rates and continuing improvements in conservation, fission could generate only about 30% of the U.S. energy by 2040 to 2050. With both slow growth and strong conservation, fission energy would be adequate. Fission energy has a limited rate of acceleration because of the 10- to 20-year doubling time of liquid-metal fast breeder reactors, the finite U.S. supply of fuel for light-water reactors, and realistic deployment rates for a newly developed fast-breeder-reactor technology. Because one fusion-fission hybrid could support 15 light-water reactors, an accelerated hybrid program in conjunction with a revitalized, competitive U.S. fission industry could generate 50% of the total U.S. energy by 2030, even assuming historic growth rates.

The projected international energy problem is much more severe than the U.S. problem. In the next 50 years, rapid international energy growth will

be driven by a doubling of the population and steady per capita energy growth. Energy demand is projected[14] to increase three- to fourfold. This would require a tenfold increase in coal production and/or 10 000 nuclear reactors. The market for practical fusion reactors would be on the order of $10 trillion. In comparison to the multitrillion-dollar costs associated with these various options, the estimated $20 billion cost of fusion development is relatively small.

In the 1980s, the magnetic and inertial-confinement fusion programs will develop an improved understanding of fusion's economic and environmental potentials. There should be a coordinated management of these programs with a common basis for program planning and evalution. Based on our present incomplete understanding, I believe the answers to the two crucial questions of utility are positive: economically competitive fusion systems are potentially feasible, and the accelerated development of fusion is justified by fusion's economic and environmental potentials, especially in light of the significant limitations of coal and fission energy.

Because of the limitations of the coal-fission strategy, we may need fusion and other alternative energy sources by the early 21st century. The U.S. fusion program should be accelerated to develop practical power plants on this timescale.

The development of fusion energy merits high national priority and a major commitment of our scientific and technological resources.

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