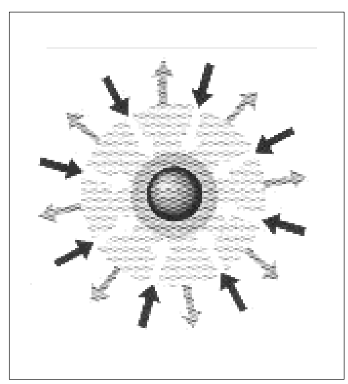
Alternate Concepts for Fusion Energy 4

Over the past several decades, the tokamak has emerged as the most scientifically successful fusion energy concept, and is emphasized in U.S. and world programs. There are, however, a number of alternate concepts (i.e., nontokamak) for fusion energy for which the knowledge base is more limited (see table 4-1). Some of these may have potentially attractive characteristics. In the past several years, alternate concepts have received a declining fraction of the federal fusion energy program budget, leading to the current state in which nearly all emphasis is placed on the tokamak. This chapter addresses the following questions regarding alternate concepts:

- What is the rationale for pursuing alternate concepts as part of a fusion energy program?
- What is the current status of knowledge for alternate concepts?
- What activities are involved in pursuing an alternate concept?
- What is the Department of Energy's (DOE's) current program for alternate concepts?



REASONS TO PURSUE ALTERNATE FUSION CONCEPTS

There is widespread agreement that examination of alternate fusion confinement concepts is an important component of a fusion energy program. The Office of Technology Assessment's 1987 report found that "the characteristics, advantages, and disadvantages of various confinement concepts need further study"[1] for

[1] U.S. Congress, Office of Technology Assessment, *Starpower: The U.S. and the International Quest for Fusion Energy,* OTA-E-336 (Washington, DC: U.S. Government Printing Office, October 1987), p. 11.

TABLE 4-1: Fusion Concepts

Low density magnetic confinement
- Tokamak
- Field reversed configuration
- Spheromak
- Spherical tokamak
- Reversed field pinch
- Stellarator

Inertial fusion energy (I FE)
- Conventional IFE (e.g., heavy-ion, laser)
- Advanced, decoupled-ignition, target systems
- Magnetized-target IFE
- Focused-ion fast ignition
- Z-pinch fast ignition

High density magnetic confinement
- Z-pinch
- Z-Flow-through pinch
- Wall-confined, magnetically insulated

Nonthermonuclear
- Inertial electrostatic confinement
- Colliding beam systems (e.g., MIGMA)

Coulomb barrier reduction
- Muon catalysis
- Others (e.g., antiproton catalysis)

SOURCE: R. Paul Drake et al., Lawrence Livermore National Laboratory, "Advanced Fusion Assessment," Aug. 19, 1994

several reasons, including uncertainty about which concept can form the basis of an attractive fusion powerplant. In 1990, the Secretary of Energy's Fusion Policy Advisory Committee (FPAC) reported:

> . . . there must be an independent program of concept improvement, including study, and where promising, development **of** alternative configurations that may be more suitable for commercialization.[2]

Similarly, in its June 1992 report to the DOE Director of Energy Research, the Fusion Energy Advisory Committee (FEAC) recommended:

> . . . a non-tokamak fusion concept program, at some level, should be supported as a matter of policy. FEAC recommends that DOE retain the flexibility to test some non-tokamak concepts at intermediate scale when warranted by their technical readiness and promise as a reactor.[3]

There are several reasons for supporting alternate concepts as part of a fusion energy program, including reducing risk, identifying more commercially attractive concepts, identifying tokamak enhancements, and promoting competition in research. Reducing risk and identifying potentially more attractive prospects have been most widely cited, including by FPAC, FEAC, and OTA.

Reduce Risk

The tokamak has clearly emerged as the most scientifically successful fusion energy concept. However, while there is widespread agreement that a tokamak powerplant is *likely* to be scientifically and technically feasible, it may ultimately prove not to be, and thus pursuit of alternate concepts reduces the risk of having no fusion energy option should the tokamak prove infeasible. The remaining physics challenges and uncertainties in developing a tokamak fusion energy device are substantial. For example, it is still to be demonstrated that a tokamak plasma can be ignited and that an ignited plasma can be maintained in steady state. There are extensive technology challenges as well, such as developing a divertor (a device to control impurities and remove reaction products)

[2] U.S. Department of Energy, Fusion Policy Advisory Committee, *Report of the Technical Panel on Magnetic Fusion of the Energy Research Advisory Board, Final Report,* DOE/S-0081 (Washington, DC: September 1990), p. 4.

[3] Fusion Energy Advisory Committee, *Advice and Recommendations to the Department of Energy in Partial Response to the Charge Letter of September 24, 1991: Part D,* DOE\ER-0555T (Washington, DC: U.S. Department of Energy, Office of Energy Research, June 1992), June 1992, p. 11.

TABLE 4-2: Challenges to Tokamak as a Commercially Attractive Fusion Energy Concept

Characteristic	Cost/performance implication
Low power density	High capital cost per kW produced
High complexity	Low perceived reliability/maintainability
Large unit sizes of >2 GW (thermal)	Inflexible for power system planning
Deuterion and tritium fuel	Not radioactively benign
Very high development costs	

SOURCE: R Paul Drake et al , Lawrence Livermore National Laboratory, "Advanced Fusion Assessment, " Aug. 19, 1994.

and developing advanced materials well suited for the challenging environment of a magnetic fusion energy (MFE) reactor.[4] In all, tokamak proponents suggest that meeting the existing challenges to making a demonstration fusion powerplant will take a continuous, high-level effort extending more than three decades. This multidecade time horizon for fusion energy development and the substantial challenges ahead suggest the importance of breadth and flexibility in the program.

■ Identify More Commercially Attractive Concepts

Even if a tokamak energy device ultimately proves scientifically and technically feasible (which most observers believe is likely), it may not be commercially attractive. There are several tokamak concept characteristics that may lead to a commercially unattractive reactor product. Without significant technical breakthroughs, these characteristics could cause tokamak energy devices to have inherently high capital costs, difficult maintenance, large unit sizes, and other unattractive features, as shown in table 4-2.[5] Recent reactor studies performed for the fusion energy program indicated that the cost of electricity from a fusion powerplant based on the tokamak concept would be somewhat in excess of today's best fission powerplants, assuming all scientific and technical feasibility challenges are met over the next several decades.[6] Table 4-3 summarizes criteria identified by electric utility industry personnel as important for practical fusion power systems.

Pursuing alternate concepts, including novel ones, may provide a breakthrough for an ultimately more economic fusion energy device. There are several alternate concepts that in theory address some of the challenges associated with the tokamak. However, their scientific and technical development remains inadequate to determine likely feasibility. It should be noted that there is at present no alternate concept that appears superior to the tokamak. Rather, there is insufficient information to determine the long-term prospects of many alternate concepts. While an alternate concept may appear promising, the relative lack of information and technical development for most

[4] Many technology challenges facing the tokamak would also have to be addressed by some alternate concepts, but there are many exceptions. For example, by using a liquid wall of materials not subject to neutron activation or degradation, by its very nature, the inertial fusion energy concept need not require the same advanced materials. Similarly, alternate concepts involving fusion of certain fuels other than deuterium and tritium such as helium-3 would result in less extensive production of high-energy neutrons, and thus may not require the same developments in advanced materials as needed for the tokamak.

[5] L.J. Perkins et al., Lawrence Livermore National Laboratory, "Fusion, the Competition and the Need for Advanced Fusion Concepts," paper prepared for OTA Workshop on Fusion Energy, June 8, 1994.

[6] F. Najmabadi et al., "The ARIES-I Tokamak Reactor Study," UCLA-PPG-1323, 1991.

TABLE 4-3: Criteria for Practical Fusion Power Systems

Economics-lower lifecycle costs than competitors
Plant size flexibility
Short, simple construction schedule
Design simplicity
High reliability, availability
Low fuel costs
Long life
Low end-of-life costs
Public acceptance
Environmental attractiveness, minimal radioactive wastes
Low costs
Maximum safety
Licensing simplicity

SOURCE: Office of Technology Assessment, adapted from Jack Kaslow, Electric Power Research Institute, "Criteria for Practical Fusion Power Systems, " presentation to the Fusion Energy Advisory Committee, Dec. 1-2, 1994.

makes that promise speculative. In contrast, the advanced state of development of the tokamak makes it relatively easy to identify its likely shortcomings—less well developed alternate concepts may well have shortcomings that will not be identified without further development efforts.

■ Identify Tokamak Enhancements

Even if the tokamak proves to be the most commercially attractive fusion concept, research on alternate concepts can support tokamak improvement and technology development. A current example is the field reversed configuration (FRC) concept, a toroidal MFE concept at a relatively low level of development. The largest FRC device, the Large S Experiment (LSX) was built by Spectrum Technologies, Inc. between 1986 and 1990 at a cost of $14 million with a planned yearly operating budget of about $3 million. Although DOE decided in late 1990 to terminate funding for LSX experiments examining the feasibility of the FRC fusion concept (see below), LSX received partial funding to explore its use as a technology for refueling of tokamaks.[7]

■ Promote Competition in Research and Development

Finally, pursuing more than one fusion concept may provide the discipline that comes with competition. Providing a competitor for the tokamak was one of the reasons for supporting the now-abandoned magnetic mirror concept during the 1970s and early 1980s.[8] Similarly, in the late 1980s, then-Energy Secretary Watkins proposed a head-to-head competition between the tokamak and inertial fusion energy (IFE).

STATUS AND PROSPECTS OF ALTERNATE CONCEPTS

There are several alternate fusion concepts with a wide range of maturity levels or development of the information base. Over the past decades, the primary focus of the fusion energy program has been on several MFE concepts.[9] Extensive research relevant to IFE has also been performed, largely for its potential defense applications. As a result, many MFE and IFE concepts generally enjoy afar more advanced knowledge base than other fusion concepts such as the colliding beam and inertial electrostatic concepts. Past efforts have been much less extensive both in theory and experiment, and knowledge about the prospects is far more speculative.

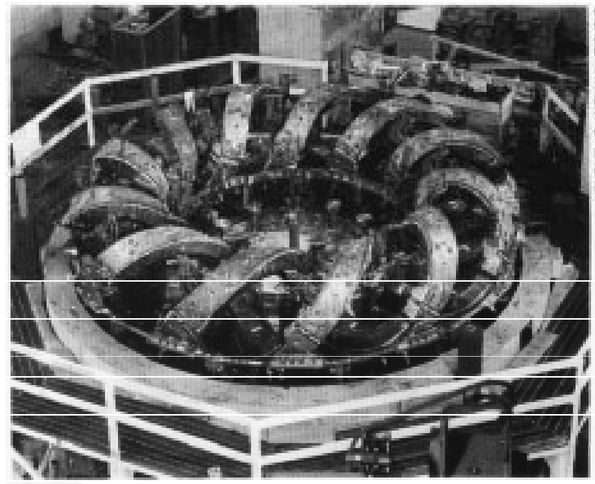
The likelihood that some alternate concept may attain and exceed the expected technical and economic performance of the tokamak remains speculative. Developing comparative information judging the relative strengths and weakness of a broad range of alternate concepts and assessing

[7] Alan L. Hoffman, University of Washington, letter to OTA, May 9, 1994.

[8] See, e.g., "Fusion's $372-Million Mothball," Science, vol. 238, Oct. 9, 1987, p. 153.

[9] For a primer on various magnetic confinement fusion concepts, see Office of Technology Assessment, op. cit., footnote 1.

the information base has not been a priority of the fusion energy program. In particular, there is no current, published DOE-sponsored analysis of the comparative technical prospects and challenges of the broad array of fusion concepts including novel ones or those previously examined and no longer pursued. DOE has sponsored and published, however, reviews of alternate MFE concepts that discuss their relative level of development and likely prospects,[10] and has supported some analyses of the relative prospects of IFE.[11] The lack of comparative assessment of non-MFE or IFE concepts is consistent with the fusion energy program's primary focus on MFE concepts rather than a broader array of fusion concepts.



U.S. DEPARTMENT OF ENERGY

The Advanced Torodidal Facility Stellaratorat Oak Ridge National Laboratory in Tennessee.

■ MFE Concepts

Prior to the 1990s, DOE pursued a variety of MFE concepts that use magnetic fields to control the range of motion of the plasma. This research effort included construction of several small and intermediate facilities to examine such diverse MFE concepts as stellarators, mirrors, reversed field pinch; and FRC. Notably, only the stellarator has come close to attaining the plasma conditions (e.g., confinement times, temperatures, and densities) attained by tokamaks. The lower levels of performance, however, may be due to a lack of follow-through rather than a lack of potential. Many major alternate concept experiments have been either canceled prior to completion of construction, or kept to a limited experimental effort primarily for budgetary reasons rather than poor technical promise. As noted by DOE in its fiscal year (FY) 1993 budget request:

> . . . fiscal constraints have required the program to prematurely narrow its focus to the tokamak concept, including tokamak improvement activities, and to eliminate major alternate magnetic confinement program elements.

Table 4-4 shows the status of several experimental facilities for alternate magnetic fusion concepts that were under development but were canceled, mothballed, or operated minimally since the mid- 1980s. The FRC case provides one example of a technically successful alternate concept with a limited knowledge base that DOE largely discontinued due to budgetary considerations. FRCs have highly complex effects that are not well understood, requiring experimental work to determine the physics of stability and confinement. If the physics turnout to be favorable, however, FRC may present an attractive reactor concept, with high output power densities and the potential for relatively simple engineering compared to the tokamak (e.g., a natural divertor to exhaust reaction products and heat, based on the device's linear geometry). Work on small FRCs at Los Alamos National Laboratory and Spectra Technology, Inc. in the late 1970s and 1980s was promising, leading to a DOE decision to build a larger device--the $14 mi1lion LSX to explore the physics in a regime more relevant to reactors.

---

[10] For example, see Fusion Energy Advisory Committee, op. cit., footnote 3; and Argonne National Laboratory, Fusion Power Program, "Technical Planning Activity: Final Report," prepared for the U.S. Department of Energy, Office of Fusion Energy, January 1987.

[11] For example, see Fusion Policy Advisory Committee, op. cit., footnote 2.

TABLE 4-4: Major U.S. Alternate MFE Concept Experiments Since the Mid-1980s[1]

Concept	Facility	Construction cost ($ in millions)	Status
Mirror	MFTF-B	$372	Closed in 1986, upon completion of construction.
Stellarator	ATF	$19	Operated intermittently since opening in 1990, mothballed 1994.
Field reversed configuration	LSX	$14	Operated minimally upon completion in 1992; being relocated since 1993 to be used for tokamak fueling experiments.
Reversed field pinch	CPRF	$58 (unfinished)	Canceled during construction, 1992.
Reversed field pinch	MST	$4	Operated at reduced budget since opening in 1988.
Spheromak	MS	$4	Maryland Spheromak was phased out in 1992 without attaining anticipated performance.
Spheromak	S-l	$9	Constructed at the Princeton Plasma Physics Laboratory in 1983, operated until 1987, demonstrating some fundamental physics of the concept.

[1] There are a number of alternate concepts that have been pursued in other countries in addition to the U.S. facilities listed here

SOURCE: Office of Technology Assessment, 1995.

However, the anticipated $3 million annual funding to conduct experiments on the LSX to explore the prospects of FRC for a potential fusion energy device was dropped in 1991, the year after construction was completed. A more limited experimental course was continued at about one-quarter the planned budget, examining the use of the FRC concept for tokamak refueling.

The reversed field pinch (RFP) concept has a limited knowledge base and has been greatly cut back due to budgetary considerations. As with FRC, RFP has physics challenges (primarily, poor energy confinement) requiring experimental work. However, if techniques can be developed to improve confinement, RFP offers some potentially attractive features. A key benefit is that the magnetic field required is about one-tenth that of the tokamak, which could lead to a more compact, high-power density fusion powerplant. In the early 1990s, DOE canceled construction of a $75-million RFP device, the ZT-H, that was about 75 percent complete, again for budgetary reasons. A much smaller RFP device, MST, continues partial operation at the University of Wisconsin. Operation of an Italian RFP device called the RFX of similar size to the ZT-H began in 1991.12

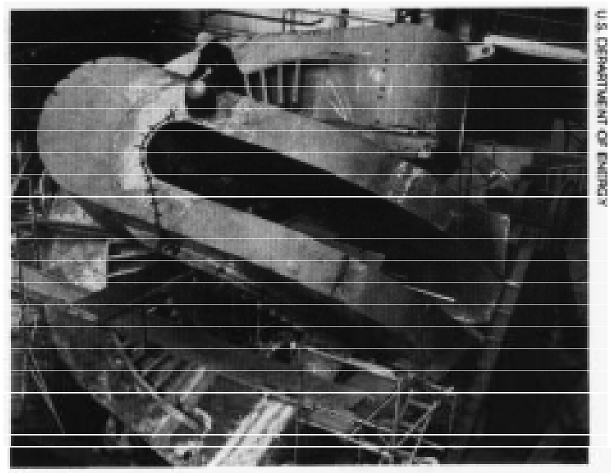
The largest fusion energy project cancellation is the Mirror Fusion Test Facility-B (MFTF-B), a $372-million (as spent) alternate concept device that was mothballed due to budget constraints the day after completing construction in 1986, but prior to its commissioning.[13] MFTF-B did face considerable technical challenges identified during the last two years of its construction, as experiments at much smaller mirror facilities gave

[12] Fusion Energy Advisory Committee, op. cit., footnote 3, p. 7.

[13] "Fusion's $372-Million Mothball," op. cit., footnote 8, pp. 152-155.

disappointing results for the mirror concept generally. MFTF-B would also have been expensive to operate, costing tens of millions of dollars annually. However, as it was never operated, MFTF-B did not provide experimental evidence either supporting or rejecting the mirror concept. As shown in table 4-4, several other major facilities were built during the 1980s to test a variety of alternate concepts, most of which were retired early or pursued a limited course of experimental studies.

Some alternate MFE concepts previously investigated and found less promising than the tokamak may warrant reconsideration, based on improvements in technology and theoretical understanding. For example, one of the major challenges with the stellarator concept was designing and fabricating the relatively intricate magnets required. However, advanced computer-based analytical capabilities continue to improve the ability to design and manufacture magnets. Some of these techniques were developed and used in producing the now prematurely retired Advanced Toroidal Facility (ATF), the most recent stellarator.[14] While the stellarator may not ultimately prove more attractive than the tokamak, improving magnet technology continues to reduce one of its principal drawbacks. Advantages relative to the tokamak include that they are inherently steady state, have no plasma current, and thus do not suffer from disruptions and instabilities of the plasma. The approximately $1-billion Large Helical Device (LHD), under construction in Japan, is a superconducting stellarator similar to ATF in concept, but closer to TPX in scope and cost. A similar scale stellarator has been proposed in Germany. A much smaller stellarator with a cost of about $3 million is under construction at the University of Wisconsin as part of DOE's small program for alternate fusion concepts.



End magnets for the Mirror Fusion Test Facility (MFTF-B) Lawrence Livermore National Laboratory

DOE last sponsored a detailed examination of the prospects for tokamaks and alternate magnetic confinement concepts in the mid- 1980s, which resulted in a January 1987 report, "Technical Planning Activity: Final Report" (TPA).[15] While that document remains a useful source of information, there has been considerable change since it was produced. For example, there have been major advances in tokamak performance, some limited experimental efforts on some alternate MFE concepts, and a continuing improvement in the broad base of physics and technology related to fusion. Thus, the TPA does not provide an entirely up-to-date foundation for evaluating the current merits of alternate fusion research efforts. More recently, DOE's FEAC panel on concept improvement (FEAC panel #3) has provided a substantially less detailed review of alternate concepts, which makes note of the advances in MFE.

Reviews of MFE concepts have classified the concepts according to their status or level of development.[16] For example, FEAC panel #3 di-

[14] Following completion of construction in 1988, ATF was held to a limited operational schedule and retired prematurely for budgetary reasons rather than poor technical performance.

[15] Argonne National Laboratory, op. cit., footnote 10.

[16] Ibid.; Office of Technology Assessment, op. cit., footnote 1; and Fusion Energy Advisory Committee, op. cit.. footnote 3.

TABLE 4-5: Level of Development of Alternate Concepts

FEAC Panel #3-concept improvement	OTA *Starpower* report
Highly developed concepts	**Well-developed knowledge base**
Tokamaks	Conventional tokamak
Stellarators	
Developing concepts	**Moderately developed knowledge base**
Reversed field pinch	Advanced tokamak
Field reversed configuration	Tandem mirror
	Stellarator
	Reversed field pinch
Small scale innovative concepts	**Developing knowledge base**
Unspecified	Spheromak
	Field reversed configuration
	Dense Z-pinch

SOURCES: Fusion Energy Advisory Committee, *Advice and Recommendations to the Department of Energy in Partial Response to the Charge Letter of September 24, 1997: Part D,* DOE/ER-0555T (Washington, DC: U.S. Department of Energy, Office of Energy Research, 1992), p 11, and U S. Congress, Office of Technology Assessment,*Starpower: The U.S. and the International Quest for Fusion Energy, OTA-E-336*(Washington, DC: U.S. Government Printing Office, October 1987), table 1-1, p. 12.

vialed MFE concepts into three categories of development, as shown in table 4-5. The panel did not explicitly investigate the prospects for potential fusion powerplants, but rather commented on the current state of scientific understanding of alternate concepts. Similarly, OTA's 1987 *Starpower* report included a listing of magnetic confinement concepts then under investigation in the United States, and their level of development based on DOE's TPA. The lists of concepts in the earlier documents (i.e., OTA and TPA) are longer, reflecting the greater variety of alternate MFE concept research then being pursued.

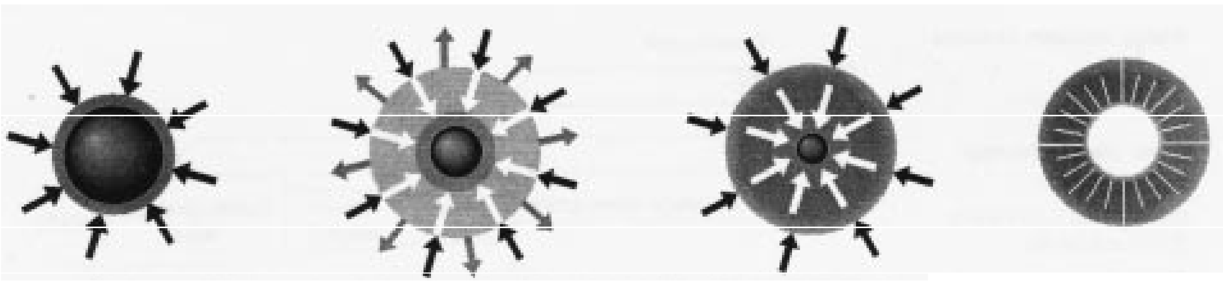
■ IFE Concepts

Considerable effort has been devoted to understanding inertial confinement, in which a pellet of fusion fuel is heated and compressed by intense lasers or heavy-ion drivers to such high densities that the fuel's own inertia is sufficient to contain it for the very short time needed for fusion to occur (see figure 4-l). Numerous reviews have concluded that the IFE concept using a heavy-ion driver is a promising approach to an eventual fusion powerplant.[17] DOE has sponsored reactor studies of conceptual designs of IFE power-Plants.[18] There is, however, considerable scientific and technical uncertainty with IFE. Overall, IFE proponents envision a $4-billion civilian effort (supplemented with about $4 billion in DOE Defense Program research) over the next 30 years involving several new facilities to address the scientific and technical challenges, culminating in a demonstration powerplant. Although much

[17] FFAC Panel #7 Report, "Inertial Fusion Energy," in U.S. Department of Energy Fusion Energy Advisory Committee, *Advice and Recoinmendations to the U.S. DOE in Response to the Charge Letter of Sept. 18, 1992* (Washington, DC: June 1993); and Fusion Policy Advisory Committee, op. cit., footnote 2.

[18] See, e.g., R.W. Moir et al., "HYLIFE-II: A Molten-Salt Inertial Fusion Energy Power Plant Design-Final Report," *Fusion Technology,* vol. *25,* January *1994, pp. 5-25.*

FIGURE 4-1: Inertial Confinement Fusion Process



Heating

Laser or particle beams rapidly heat the surface of the fusion target, forming the plasma envelope.

Compression

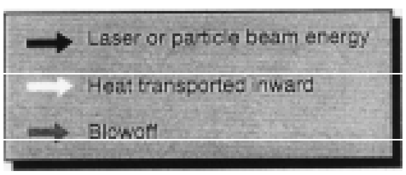
Fuel is compressed by rocket-like blowoff of the surface material.

Ignition

The fuel core reaches high density and ignites.

Bum

Thermonuclear burn spreads rapidly through the compressed fuel, yielding many times the input energy.



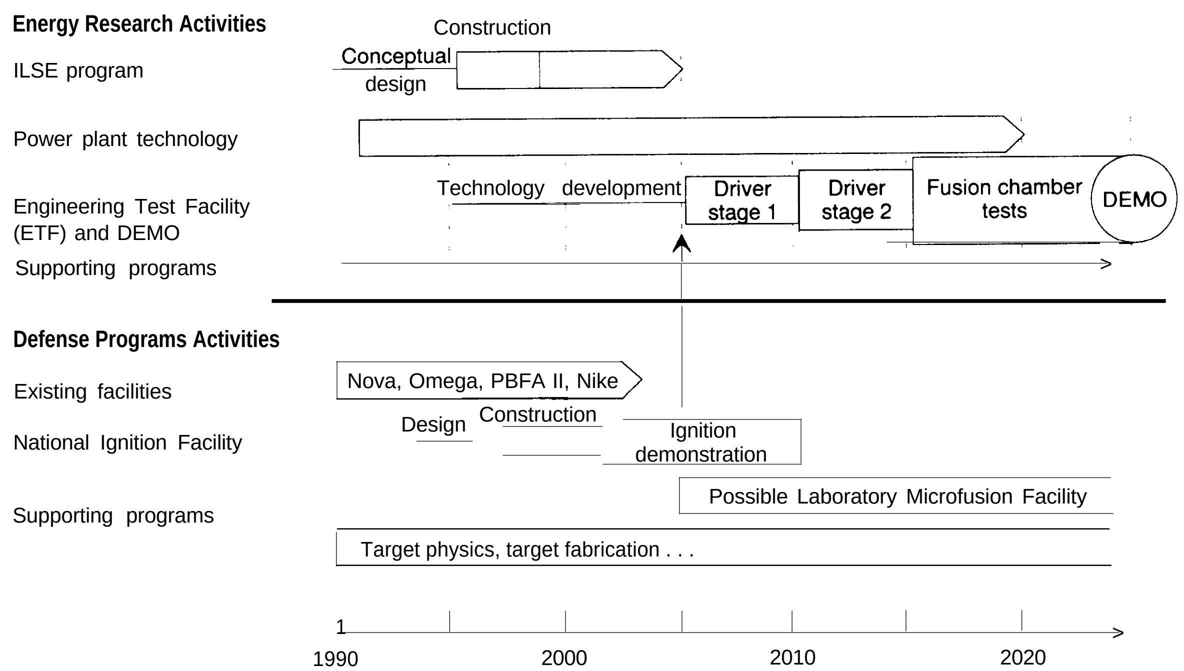
SOURCE: **Lawrence Livermore National Laboratory.**

scientific and technical work remains to be done (see figure 4-2), the information base for IFE is moderately well established, as are the next research and development steps.[19]

Inertial confinement research mimics, on a very small scale, some processes in the hydrogen bomb, and most of the research relevant to IFE has been performed by DOE's Office of Defense Programs for its applications to nuclear weapons and stockpile stewardship responsibilities. The scientific feasibility of achieving high gain in an inertial confinement fusion target has been demonstrated in underground nuclear explosion experiments at the Nevada Test Site in a program called Halite/Centurion. The next step in examining the science of target physics and ignition depends on the National Ignition Facility (NIF), another effort planned for the DOE Defense Program. NIF is a proposed $1-billion research facility being considered as part of the stockpile stewardship program to maintain expertise for nuclear weapons. The scientific results that NIF or something like it would produce are essential to demonstrating ignition and propagating burn of high-gain targets, and to establishing the requirements that an IFE driver would have to meet. However, whether NIF is pursued will depend more on weapons-related reasons, including its role in stockpile stewardship and the potential effects on weapons proliferation rather than its benefits for the fusion energy program. DOE announced plans to proceed with NIF in October 1994, but is also performing a detailed study of the

[19] See, e.g., B. Grant Logan et al., "The Inertial Confinement Fusion Pathway,"paper presented at the Forum on Pathways to Fusion Power, American Nuclear Society Annual Meeting and Fusion Topical, New Orleans, LA, June 22, 1994.

FIGURE 4-2: One Proposed U.S. Inertial Fusion Strategy



SOURCE: Roger O Bangerter, Head, Fusion Energy Research Program, Lawrence Berkeley Laboratory, "Heavy Ion Inertial Fusion, " testimony at hearings before the House Committee on Science, Space, and Technology, Subcommittee on Energy, Aug. 2, 1994

effects of the program on nuclear nonproliferation, expected to be completed in 1995.

There are important scientific and technical challenges for IFE that go beyond the target physics research needs shared with the Defense Program. The most important of these is development of a driver that is both efficient and can be operated at a high repetition rate (e.g., several times per second) for use in an eventual IFE powerplant. In contrast, while lasers can be highly effective for target physics research, which requires a repetition rate of one burst every several hours, they lack the efficiency and repetition rate needed by IFE powerplant drivers. Numerous reviews have supported development of a heavy-ion driver, which is the most advanced concept. The heavy-ion driver concept builds on the considerable investment in science and technology developed for the accelerators used in high-energy physics. The next step in heavy-ion driver development is called the Induction Linac Systems Experiments (ILSE), with an estimated construction cost of about $50 million. While heavy-ion drivers appear to be the most advanced concepts for IFE, there are other approaches that may eventually prove attractive as well, including light-ion drivers and advanced lasers.[20]

Budget constraints have caused a continued deferral in the development of key research efforts for IFE, including ILSE. Despite favorable recommendations from review committees for proceeding with ILSE, the IFE budget was reduced from $9 million in FY 1992 to $4 million in FY

[20] Charles D. Orth et al., Larence Livermore National Laboratory, "Diode-Pumped Solid-State-Laser Driver for Inertial Fusion Energy Power Plants," *ICF Quarterly Report, vol. 3, No. 4,* July-September 1993, pp. 145-154.

1993. In commenting on the lack of progress in the IFE effort, one review body found the following:

> The Department of Energy has not established an IFE program that resembles remotely the one envisioned by FPAC. Ostensibly this has been due to stringent funding allocations for fusion as a whole.[21]

In general, IFE proponents suggest a development path with inherently less dependence on extremely expensive individual facilities than the tokamak by virtue of greater modularity in experimental facilities. For example, while an ignition facility is an expensive component of an IFE development path, that one facility could service the research needs of several drivers. An overview of the research needs for IFE development and a simplified development path as developed by proponents is shown in figure 4-1. In total, IFE proponents project budget needs of about $4 billion over the next three decades to develop a demonstration powerplant (DEMO).[22] This cost estimate includes neither the anticipated $1.8 billion to build and operate NIF, nor other efforts paid for under DOE's Defense Program. Counting all defense research also relevant to IFE would add about $4 billion to the costs. Further, it must be noted that the cost estimates are highly uncertain, and depend on such unresolved physics issues as the gain achievable with a given driver.

Other Novel Concepts

A number of novel fusion energy concepts have been suggested that take fundamentally different approaches from those used in either MFE or IFE.[23] Relative to inertial and magnetic confinement fusion, these approaches have generally received very limited attention in the fusion energy program, and are at an embryonic development stage, with far less well understood and demonstrated scientific concepts. While the lack of scientific understanding and demonstration can be a notable shortcoming of novel concepts, some proponents find this to be the essence of their potential benefit and justification for support. For example, one physicist long associated with certain novel concepts notes:

> If there is a route to dramatically more attractive fusion systems, it will be in the investigation of new or relatively unexplained physics rather than in engineering refinements of present or recently terminated programs.[24]

Just as the scientific aspects can be highly speculative, the broader technology issues that would have to be addressed leading to a fusion energy powerplant based on any of these concepts have typically not been examined in detail. However, proponents of these concepts suggest a variety of possible advantages relative to the tokamak, ranging from ability to use advanced fuels (e.g., helium-3 and deuterium, which produces less neutron radiation than results from the deuterium-tritium reactions of tokamak and IFE) to smaller, more flexible powerplant sizes, to lower construction and operating costs. As noted earlier, DOE has not published an analysis of the comparative technical prospects and challenges of novel alternate concepts.

One example of the many novel concepts is muon catalysis, which involves using a subatomic particle called a muon to shield the electric charge of one of the nuclei in a fusion reaction from the other. This shielding mitigates the repulsive forces

---

[21] FEAC Panel #7 Report, op. cit., footnote 17.

[22] Donald Correll, Deputy Program Leader, Laser Programs—Inertial Confinement Fusion Lawrence Livermore National Laboratory, fax to OTA, July 22, 1994; and Roger O. Bangerter, Lawrence Berkeley Laboratory, "Heavy Ion Inertial Fusion" testimony at hearings before the House Committee on Science, Space, and Technology, Subcommittee on Energy, Aug. 2, 1994.

[23] For brief descriptors of a number of novel concepts, see for example, Global Foundation, Inc., "1st International Symposium on Evaluation of Current Trends in Fusion Research: Book of Abstracts," Washington, DC, Nov. 14-18, 1994.

[24] Normal Rostoker, "Alternate Fusion Concepts," paper presented at the 1st International Symposium: Evaluation of Current Trends in Fusion Research, Washington, DC, Nov. 14-18, 1994.

and allows the nuclei to approach closely enough to fuse without the need for extreme temperature. Muon-catalyzed fusion reactions have been observed in high-energy physics experiments dating back several decades, although the number of fusion reactions produced per muon before it decays was lower than would be necessary to make the process worthwhile.

Inertial electrostatic confinement fusion is a more developed, but still novel approach that has received limited attention from the fusion energy program. The concept involves confining the highest energy fuel ions electrostatically, leading to greater reactivity than found in an MFE plasma. While some work has been performed examining the scientific basis of the concept including at the University of Wisconsin and the University of Illinois, the theoretical studies remain at a relatively preliminary stage. A related concept, the colliding beam, was largely discarded decades ago based on theoretical and experimental results using the migma reactor approach that indicated an inability to develop a sufficient ion density. However, proponents of the concept suggest that developments in the field of high-energy physics and in the accompanying technology of linear accelerators may provide solutions to this drawback of the colliding beam concept.[25]

Perhaps the most widely debated and controversial novel concept has been cold fusion. In 1989, two researchers, Stanley Pons and Martin Fleischmann, announced that they had discovered a method of producing nuclear fusion at room temperature using a simple electrochemical apparatus. Although some researchers reported results supporting the claims, many of those findings were subsequently retracted or could not be confirmed by other researchers. A 1989 DOE advisory committee of nuclear physicists and chemists concluded that "evidence for the discovery of a new nuclear process termed cold fusion is not persuasive."[26] Today, a handful of researchers continue to report that electrolysis of heavy water can lead to the production of excess power. Some investigators theorize that unusual and unexplained chemical or nuclear processes may in fact be at work. The inability to routinely reproduce experimental findings has proven to be a continuing challenge, and the results are still questioned by a majority of the scientific community. However, the Japanese agency MITI has an ongoing program examining the phenomena, with funding of about $5 million in 1994.[27]

STEPS IN EXAMINING ALTERNATE CONCEPTS

The next step that would be required in development of any alternate concept depends on its level of maturity. While immature concepts may be well suited to a great deal of relatively inexpensive theoretical analysis for screening purposes, some such as IFE are at a point where major facilities such as ILSE and NIF are required to continue development.

Theoretical research, modeling, and analysis can be useful tools for examining the likely merits of an alternate concept. These theoretical efforts can include a wide range of expertise from detailed physics (e.g., modeling of radiation/ magneto-hydrodynamics for high-density plasmas; modeling of particle orbits and collisional effects) to reactor design and economic analysis assuming favorable physics (e.g., commercial reactor evaluations and systems modeling[28]). Computational abilities continue to improve, making theoretical studies increasingly feasible. Even for relatively more advanced concepts, theoretical analysis can be useful for estimating the potential long-term attractiveness, and thus

[25] B.C. Maglich et al., "Modern Magnetic Fusion," Advanced Physics Corp. Report # SAFE-94-104, May 5, 1994.

[26] Energy Research Advisory Board, "Cold Fusion Research," a report to the U.S. Department of Energy, November 1989.

[27] *Nature,* vol. 367, Feb. 24, 1994, p. 670.

[28] These include, for example, the ARIES series of studies for tokamaks and HYLIFE-II for heavy-ion inertial fusion.

help set priorities for the next, more costly experimental steps.

One team of fusion researchers at Lawrence Livermore National Laboratory (LLNL) has proposed an "Advanced Fusion Assessment Program" intended to perform objective evaluation and development of alternate concepts. They intend for the effort to become an effective tool for DOE in managing the longer term fusion program, by taking good ideas far enough that DOE can choose an appropriate organization to pursue an experimental program.[29] As envisioned by the LLNL team, this program would encompass the following:

- Seek out good ideas for fusion systems that offer improvements over present concepts that approach an order-of-magnitude.
- Build appropriate teams of LLNL, U.S. scientists, and U.S. industry to evaluate both the physics *and* reactor potential. Make scientific and engineering evaluation tools available to people with new ideas.
- Provide neutral, objective evaluation rather than advocacy of specific ideas.
- Provide physics support as needed as such programs get underway.

The LLNL proposal emphasizes theoretical, rather than experimental, studies. These would be integrated studies, including a full range of analysis from basic physics to examining the likely reactor characteristics and economics, assuming the physics is found promising after experimental efforts. The effort could be useful as an integrated screening tool and may be able to sort out the truly promising but undeveloped concepts from less promising ones. According to LLNL team members, an initial evaluation of an undeveloped concept, including basic physics and reactor potential, could be performed for a few hundred thousand dollars. A full theoretical, computational, and reactor potential study would probably require a few million dollars.[30] Overall, the LLNL proposal suggests a one-year budget of about $3.5 million, or less than one percent of the fusion energy program budget.

Understanding, evaluating, and developing a fusion concept cannot be accomplished with theoretical work alone, however. In some areas of fusion physics, theory and modeling capabilities are not currently adequate for exploring fusion energy concepts. For example, existing theoretical tools are better suited to analyzing high-density plasmas than low-density plasmas such as tokamak. Thus, for alternate concepts involving low-density plasmas, experimental devices are essential for examining the physics prospects. Even in those cases for which analytical capabilities are well suited, the complexity of the physics and technology requires extensive experimental work as a concept is developed to validate the predictions of theory. The evolution of scientific and technological understanding has typically proceeded in stages using increasingly capable, and often larger, facilities. This evolution builds on the empirical results from operation of previous facilities, extrapolating the existing knowledge base to design a more capable facility.

The necessary dependence on experimental facilities and research to verify theory can make concept development expensive. One aspect of the reliance on empirical results is that advanced studies require increasingly capable and expensive facilities as a concept is developed, which can lead to substantial budget requirements. However, examination of a wide range of alternate concepts does not necessarily entail an extensive series of facilities reaching into several billions of dollars. There are two main reasons: first, as information is gained about a concept during earlier stages of development, only some will be found to merit promotion to subsequent stages of development. Criteria for promoting a concept to a subsequent stage (and development of more and costlier experimental facilities) may include development

[29] R. Paul Drake et al., Lawrence Livermore National Laboratory, "Advanced Fusion Assessment," Aug. 19, 1994.

[30] D.E. Baldwin and John Perkins, personal communications, Aug. 11, 1994 and Nov. 17, 1994.

cost, likelihood of technical success, and likelihood that the concept, if successful, will provide a substantial cost or performance advantage over the tokamak. Budgetary considerations can also be an important criterion for determining whether the prospects of a concept justify the additional spending for further development work.

Second, while tokamak development has involved a series of larger, more capable, and more expensive facilities reaching on the order of $10 billion, some alternate concepts may not require as extensive a succession. For example, a concept with inherently higher power densities such as FRC, if found to be technically promising based on theoretical reviews and small experimental efforts, may require smaller and less costly facilities relative to the tokamak. While pursuing FRC would still require a series of theoretical and experimental efforts, including development of larger facilities if current results so warrant, its proponents suggest that an engineering test reactor could be far smaller and less costly than ITER.[31] As noted in the previous section, IFE provides another example of a potentially less costly and more flexible development path for a fusion powerplant.

DOE'S PROGRAM FOR ALTERNATE CONCEPTS

In the Energy Policy Act of 1992 (EPACT), Congress set a goal for DOE of pursuing a broad-based fusion energy program that would, by 2010, verify the practicability of commercial electric power production. EPACT further directed the department to develop a comprehensive plan for the program that would "include specific program objectives, milestones and schedules for technology development, and cost estimates and program management resource requirements."[32] However, DOE has not yet developed that overall plan. Nor has it explicitly examined and justified a level of effort and a process for identifying, evaluating, and, where appropriate, pursuing alternate concepts, which arguably are one aspect of a broad-based fusion energy program. That is, there is no explicit DOE analysis of the relationship between alternate concepts and the overall fusion energy program objective—developing a technically and economically attractive method of electric power production.

Although DOE has not published a strategic plan for the fusion energy program, it has pursued a course of greatly reducing emphasis on alternate concepts in the past several years. With substantial cutbacks in alternate concept work in the past several years, many fusion researchers (including those not identified with any particular alternate concept) perceived indifference or worse on the part of DOE for alternate concepts. The FEAC panel #3 on concept improvement noted the following:

> . . . statements and communications by the Department [of Energy] led to the perception in the fusion community that proposals for research on non-tokamak concepts would not be supported by OFE, and should not be submitted. . . . The rationale given was that research on competing concepts could not be supported, since, even if the research were successful, no funds would be available to develop the concept to its next, more expensive state; thus it would be best not to begin.[33]

Similarly, LLNL researchers have recently noted: "There is now little focus on seeking, generating, and objectively examining advanced ideas" and "in fact, the current environment is rather hostile

[31] Hoffman, op. cit., footnote 7.

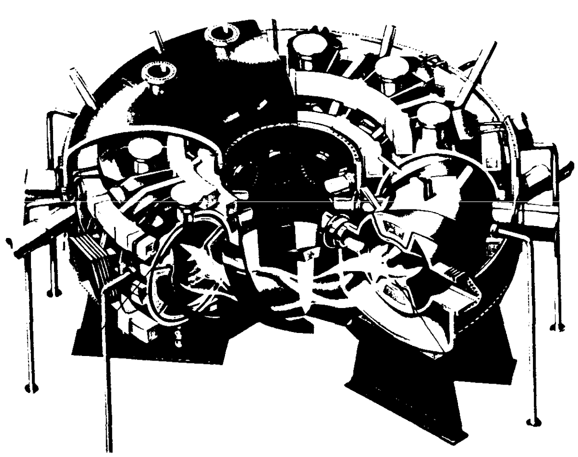
[32] Energy Policy Act of 1992, Public Law 102-486, Oct. 24, 1992, Sec. 2114.

[33] FEAC Panel #3, "Concept Improvement: A Report to the Fusion Energy Advisory Committee," May 11, 1992, p. 2, in Fusion Energy Advisory Committee, op. cit., footnote 3, app. I.

to new ideas for fusion and inventors have trouble finding support."[34]

In 1992, FEAC recommended "that a small but formal and highly visible periodic competition be established to foster new concepts and ideas that if verified would make a significant improvement in the attractiveness of fusion reactors."[35] In response, DOE announced an "Innovative Concepts Initiative" and a request for proposals "to support innovations in tokamak improvements and new fusion confinement systems."[36] The announcement anticipated awarding a total of $1 million to be divided among no more than three grants. DOE judged 15 of the 24 applications to be eligible and provided those to a non-DOE peer review committee. A total of $1.2 million annually in fiscal years 1993 through 1995 was provided to the three winning applicants. Among these was a concept closely related to FRC, called the Ion Ring.

The current level of effort devoted to alternate concepts is widely viewed as inadequate relative to the overall fusion energy program. While pursuit of alternate concepts is widely agreed on by fusion proponents as one aspect of a balanced fusion energy program, the appropriate level of effort devoted to alternate concepts is less clear. In FY 1994, about $1.2 million, less than 1/2 percent of the total fusion energy budget, was dedicated to the Innovative Concepts Initiative. About $4 million was devoted to inertial fusion energy, the most developed and promising alternate concept, an amount insufficient to proceed to the next development step, a heavy-ion driver experiment. In fact, FEAC had in 1993 reported to DOE that "there is no credible program for the development of a heavy-ion fusion energy option" at an annual funding rate of $5 million.[37]



JAPAN MINISTRY OF EDUCATION

The Large Helical Device (a stellarator) under construction in Japan is estimated to cost about $1 billion.

DOE suggests that a "healthy, but constrained" alternate concepts program would require about $100 million per year.[38] However, a substantial amount of information could be developed with a far more modest program that provides a freer basis for making future alternate concept decisions. For example, pursuing an advanced fusion assessment proposal of the type suggested by LLNL researchers, supporting the civilian portion of the IFE budget, repeating the DOE Innovative Concepts Initiative, and restarting or accelerating confinement concept experiments at existing but underused or idled facilities such as LSX and the ATF stellarator could cost under $20 million or about five percent of the current fusion energy program budget. Increased international collabo-

[34] Perkins et al., op. cit., foootnote 5.

[35] Fusion Energy Advisory Committee, Op. Cit., footnote 3, p. 11.

[36] *Federal Register,* vo1. 57, No. 244, Dec. 18, 1992, pp. 60197-60198.

[37] U.S. Department of Energy, Fusion Energy Advisory Committee, *Advice and Recommendations to the Department of Energy in Partial Response to the Charge Letter of September 18, 1992,* DOWER-0594T (Washington, DC: June 1993), p. 11.

[38] U.S. Department of Energy, "Fusion Energy Program," briefing package presented by N. Anne Davies to OTA, Apr. 28, 1994.

ration making use of existing alternate concept research facilities in other countries may also be a lower cost alternative to sole U.S. funding of new intermediate-scale facilities.

CONCLUSION

In summary, while alternate concepts provide no panacea for fusion energy development, there is merit in examining them as part of a broad fusion program. Relative to the expected costs of the tokamak effort, a great deal of exploratory work can be conducted at modest cost. Assuming some of the concepts prove technically promising, however, further development may require larger budgets for construction of expensive facilities. As with the tokamak effort, the potential role of the overall fusion energy program in meeting long-term energy needs, and the level of research effort justified by that potential role, are critical issues for the direction of alternate concepts research.