28 November 1980, Volume 210, Number 4473

SCIENCE

The Next Step in Fusion: What It Is and How It Is Being Taken

John F. Clarke

Based on a common hope of the scientific community that it might be possible to utilize the enormous energy resource represented by the deuterium in the world's oceans, attempts to harness the fusion process for the production of energy have been under way in all of the technologically advanced nations of the world since the early 1950's. This effort has been beset with numerous disappointments. However, recent scientific progress indicates that magnetically confined plasmas can be made to produce energy, and we can now look on fusion as an inexhaustible new energy resource. The challenge for the future in fusion lies with learning how to develop this resource. The confluence of recent scientific successes in fusion research with recent perturbations of conventional energy supplies has led to considerable attention being focused on the question of the most effective program strategy as well as the appropriate pace for fusion development.

The Pace of Fusion Development

There has been considerable debate on the appropriate strategy to ensure that the development of fusion is carried out in an optimal fashion. As I will outline below, this debate has largely been resolved by the realization that the points of view of all parties to the debate can be accommodated within one practical strategy. The present controversy lies with the question of pace. It must be emphasized that the scientific success of the fusion program has made this a very concrete issue. At present, magnetic fusion development is not limited by technology, ideas, or personnel.

As long as there was a question of the ability to produce fusion energy, it could be argued that program pace was somewhat arbitrary. This is no longer the case. Fusion has now matured to the point where a realistic development program can be specified in some detail. However, Department of Energy (DOE) program plans show that at a minimum it will still take 15 years to exploit the fusion energy resource and develop it into an economical energy source. The reality of a finite time period for engineering and technological development has replaced the feasibility issue in discussions of fusion program pace. Because of this engineering development period, it is argued that although fusion should be developed for the long term, it cannot affect our near-term energy situation and there is no sense of urgency for its development. The latter position is based on the perception that there are nearer term technologies that can solve our energy problems.

Clearly, a discussion of fusion program pace cannot be divorced from present energy realities. Technically speaking, coal and fission energy are available today to substitute for liquid fossil fuel for applications such as the generation of electricity and heat for residential and commercial use, which account for 48 percent of U.S. petroleum consumption. There is even the possibility that some political solution can be found to ease the supply and cost problem of conventional oil, although questions of national security make this somewhat problematic. In any case, it is becoming clearer that increased combustion of fossil fuels in their liquid or solid forms whatever their source, will have deleterious environmental and social costs (1). Many of the demonstrable difficulties with fossil fuels could be avoided by substitution of fission energy for some applications. For example, a recent analysis of alternatives has shown that in combination with heat pumps, nuclear electricity is more than competitive for space heating in all parts of the United States (2). However, its deployment is restricted by the perception of possible problems associated with safety and waste disposal. Thus, there are clearly practical limitations to increasing energy supplies even with the existing near-term alternatives of coal and nuclear. The problems of these available energy alternatives can certainly be ameliorated by increased near-term research and development, but basic solutions may not be found on a shorter time scale than that for fusion engineering development.

Most recent energy studies show that conservation will have to make up the difference between energy supply and demand in this century. As in the case of coal and nuclear, much can be done with conservation-oriented research. However, when conservation savings have been exhausted and the pressure for adequate energy supply has intensified, the acceptability of the existing fossil and nuclear alternatives may become even more problematic unless there is a radical change in the evolution of environmental and nuclear regulatory practice. In this context, intensifying the near-term development of a technically successful and rapidly developing energy supply option such as fusion would seem to be justified, even in a time of restricted energy research and development. On the time scale required to resolve the present energy problem, it is argued that fusion should be considered as a competitive option for near-term energy development efforts and should be pursued with the same sense of urgency afforded to other energy R & D, whether considered in the near term, as coal and nuclear, or in the long term, as solar (3).

The author is Deputy Associate Director for Fusion Energy, Office of Energy Research, U.S. Department of Energy, Washington, D.C. 20545. This article is based on a paper presented at the All-Union Conference on Controlled Thermonuclear Fusion, Zvenigorod, U.S.S.R., 21 to 27 February 1979.

0036-8075/80/1128-0967$01.50/0

Fusion and Long-Term Energy Supply

In the long term, we know of only three energy supply alternatives: solar energy, the fission breeder, and fusion. The first of these has been used for ages and its basic limitations of low energy density and reliability are well understood (*4*). The hope for its large-scale application lies with the possibility that advanced technology can circumvent these limitations in a manner which overcomes the poor economics and safety problems associated with the large structures needed to recover useful quantities of this dispersed energy source. The fission breeder suffers from the same perceived problems as present-day fission technology. It has additional complications associated with the production of large quantities of plutonium and the requirement for extensive fuel reprocessing—that is, problems of radioactive material transportation, a possible increased risk of proliferation, and the generation of large quantities of low-level waste (*5*).

Fusion, the third long-term energy alternative, seems to offer promise of avoiding the worst problems with both of the other alternatives, if it can be effectively utilized. Recent experimental results from the United States, the U.S.S.R., Europe, and Japan indicate that the tokamak, one of a number of possible fusion approaches, can confine a fusion plasma sufficiently well to produce power. On the basis of current evidence, the Tokamak Fusion Test Reactor (TFTR), now under construction at the Princeton Plasma Physics Laboratory, should demonstrate more than energy breakeven after its completion in 1982 (*6*). Furthermore, extensive technology development programs in the regions mentioned above indicate that there is no fundamental technological obstacle to translating the scientific success of tokamak development to the production of controlled fusion power (*7*). As a result, as we approach the demonstration of scientific feasibility in fusion, confidence in the ultimate useful application of this new technology is growing rapidly and fusion can be taken much more seriously as a possible long-term energy source.

Fusion has a number of potential advantages over the other long-term energy options, and I must emphasize the word "potential." At this stage in the fusion program, one cannot guarantee that they will be realized in a particular fusion reactor concept. It has been pointed out by Holdren (*8*) that it is possible to make a poor fusion reactor which not only does not achieve the full potential of fusion but in fact creates problems worse than those existing in conventional power plants.

An optimally developed fusion reactor could have the flexibility, both diurnal and geographic, which is lacking in the solar and breeder reactor options because of insolation and safety requirements, respectively. Design studies of fusion reactor systems show that their size could be little different from that of conventional fossil or nuclear power plants (*9*), but their intrinsic safety and low environmental impact should allow siting closer to their points of application (*10*). Fusion also has a much smaller radioactivity and waste disposal problem than fission reactors (*11*). Finally, because of the flexibility of design inherent in fusion reactors, wherein the energy recovery region is external to the reaction region, such reactors may be used for other purposes, such as the production of hydrogen and nuclear fuel, as well as electricity production (*12*).

Studies show costs that appear to be reasonably competitive with those of advanced nuclear systems, although these projections are admittedly uncertain at this stage of fusion development. However, studies also show that the novel element of the fusion system, the fusion core, accounts for only 30 to 40 percent of the plant costs. This factor, together with negligible fuel cost (*13*), might allow fusion power costs to asymptomatically approach even light-water reactor power costs as fusion development proceeds. These potential advantages constitute a powerful incentive for carrying out a fusion development program aimed at their full realization.

Fusion Development Policy

The DOE has formulated a policy for fusion which recognizes the need to develop the highest potential of fusion rather than to pursue in an exclusive manner the first fusion concept to reach the energy breakeven milestone (*6*). On an operational level, the Office of Fusion Energy is carrying out this policy by enhancing the scientific and technological base which underlies several types of fusion concepts. We have had to recognize that this policy can result in slowing progress in certain lines of attack if program support is too restricted to allow both full exploitation of technical success and maintenance of a broad scientific base. This is an essential difficulty of the policy, which has led to some controversy.

The urgency imposed on fusion power development by external factors of energy supply and the larger costs resulting from a broad-based approach to developing an optimum fusion system have caused some to question the wisdom of adopting the Olympian goal of the policy outlined above (*14*). This policy is sometimes perceived as aimed at not only developing fusion power but developing it in such a way that it simultaneously solves all the perceived problems of other energy supply systems. The pragmatic argument is frequently made that we should focus our resources on the tokamak, the most promising concept, and dedicate our efforts to building a reactor as soon as possible. It is felt that once fusion produces power, optimization will follow, as it has in all other power systems. This viewpoint has the undeniable appeal of efficiency and economy. It entails the possibility of failure if the approach chosen proves to be unworkable, although this possibility has been somewhat diminished by recent advances in tokamaks. It also entails the risk that, over the long period needed, fusion development will become dedicated to an approach rather than a desirable end product.

It is certainly true that there are practical limitations to the implementation of the more judicious policy. Even with the most diligent effort to foster and develop only the unique and most complementary alternative fusion approaches by means of international cooperative programs, there comes a point at which options must be closed because of lack of resources rather than clear technical failure. The development of a fusion concept through the scientific proof-of-principle stage can usually be done for less than $10 million to $20 million a year for a few years, a reasonable investment for the knowledge gained. However, the step from the proof-of-principle stage to a test of that concept with an energy-producing fusion plasma, which can be used to test the scientific principles and engineering systems of that concept in a fusion reactor environment, can cost hundreds of millions of dollars. If a unique fusion technology must be developed for each concept, the policy of maintaining alternative approaches in order to develop the most optimal fusion reactor becomes unrealistic.

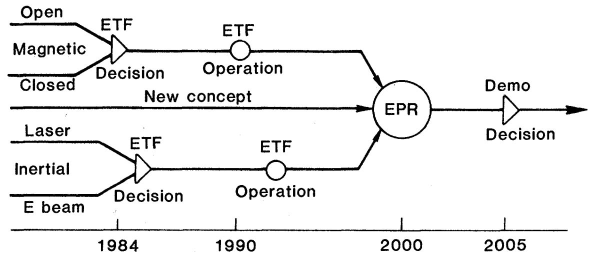
Fortunately, in recent years a great deal of commonality of technology has been developed between different magnetic confinement systems. Even systems as different in basic physical principles as the open and closed magnetic systems share large areas of technology. Superconducting magnets which can exist in the fusion reactor environment, energetic particle beams and radio-frequency techniques for heating the plasma, efficient energy recovery and tritium breeding blanket elements, and fusion reactor remote maintenance techniques are all examples of generic technologies. Furthermore, as the science of plasma physics has developed, we have come to recognize broad common principles which make the different approaches mutually supportive rather than merely complementary. Thus it is possible to conceive of a development plan for magnetic fusion, and for that matter some aspects of inertial confinement fusion, which avoids the pitfalls outlined above. In this plan, which the Office of Fusion Energy has evolved from the basic DOE policy, a number of approaches are developed to the proof-of-principle stage in order to establish the unique properties of each. Simultaneously, generic technologies are advanced to the fusion power stage with one concept. This strategy, as shown schematically in Fig. 1, is consistent with the present DOE policy for fusion and also results in a program which can be paced to available resources.

On the operational level, this strategy means that a fusion system which can provide sufficient quantities of fusion power would be developed into an engineering test facility (ETF), which could be used to develop the generic technology necessary for all approaches. This ETF would provide the technological data base which, together with information from the proof-of-principle physics experiments for each alternative approach and a limited number of specialized technology facilities, would provide the basis for selection of the optimal fusion system. By following such a strategy, the nation will gain the ability to assess the full potential of fusion in the minimum time at minimum cost. By the early 1990's we should be able to understand the basic features and costs of this effectively inexhaustible energy supply.

A strategy utilizing an ETF, to be commissioned as soon as possible after the identification of a suitable candidate concept, allows vigorous development of fusion power without foreclosing options. From this point of view, the only real requirement of the concept chosen for the ETF is that it be able to produce sufficient fusion power in such a way and with sufficient certainty to serve as a tool for developing generic fusion technology.

On the other hand, if the most advanced fusion concept was selected as the basis of the ETF, this strategy would also ensure vigorous development of that concept. On a practical basis, early construction of a power-producing facility based on the most advanced fusion concept is also the proper path of one who wished to ignore the possibility of ever finding a superior fusion system and to proceed most directly to a power reactor. If the most advanced fusion concept is chosen to commence the ETF project at the earliest, technically realistic date, the present DOE policy of developing the optimal fusion system and the alternative policy of developing one fusion system as rapidly as possible can be seen to be identical. The strategy question is thus reduced to the basic question of pace.

Fig. 1. The present DOE strategy involves the use of engineering test facilities to develop the generic technology of fusion. Parallel development of scientific approaches to fusion ensures a broad range of options for the selection of an optimum fusion system. *EPR*, Engineering prototype reactor.



The Next Step in Fusion

Aside from funding limitations, the question of pace is affected by the number of facilities that must be built to carry out the program. The U.S. fusion program finds itself at a critical point in deciding on the precise nature of its next step. In principle, the ETF could be based on any fusion approach now being followed. In practice, the tokamak is the leading ETF concept, simply because the tokamak data base is the most extensive of any fusion approach and the R & D issues related to the tokamak can be sharply focused. The common reference tokamak design adopted by the ETF concept advisory group was based on information generated in four independent studies carried out by Argonne National Laboratory, General Atomic Company, the Princeton Plasma Physics Laboratory working with Grumman Aircraft Engineering Corporation, and Oak Ridge National Laboratory working with Westinghouse Corporation (*15–18*). Each of these studies was aimed at developing what that laboratory or industry considered to be the optimum realization of the next step in the tokamak program. Table 1 shows the main parameters of the four studies and the ETF reference design parameters based on an analysis of these studies. The fact that the four studies came up with systems which were very close in terms of basic size and technological requirements, as reflected in the required magnetic field, burn time, and heating power, reflects the maturity of the tokamak program and indicates that the tokamak R & D uncertainties can be specified with some precision.

The United States and other fusion nations have begun a program to define the R & D requirements for the tokamak ETF and thereby to define the nature of the next step. A U.S. ETF design center has been established at Oak Ridge National Laboratory with the specific mission of making the design requirements of the ETF more specific. At the same time, an international group under the auspices of the International Atomic Energy Agency has been meeting periodically in Vienna to determine the nature of a possible international project to construct a power-producing tokamak reactor. This group is composed of leading fusion specialists from the U.S.S.R., Euratom, Japan, and the United States, supported by teams of national experts to provide data and information for their evaluation. On the basis of input from more than 200 of the world's leading tokamak specialists, this group concluded that it is scientifically and technologically feasible to begin construction of a power-producing tokamak provided a limited number of R & D tasks are carried out.

Technical Readiness: Preparation of an ETF Data Base

Since the beginning of the U.S. tokamak program, efforts have been made to estimate the requirements of the next generation of tokamaks (*19*). The results of these studies have been used to specify the types of plasma confinement experiments and technology development facilities that would be needed to enable us to satisfy these requirements. Over the last 5 years, a number of large, flexible experimental facilities have been built in the United States to address each of the key physics and technology issues associated with the successful operation of a future tokamak reactor as specified in these studies. Considering the depth of planning in the U.S. fusion program, it should not be surprising that most of the

R & D tasks specified for the International Tokamak Reactor (INTOR) project can be addressed in existing U.S. facilities. Table 2 contains a list of the key physics issues and the facilities which we feel will provide definitive data in each area.

In addition to these scientific programs, the U.S. fusion program has under development the major technological components of a tokamak ETF. These are listed in Table 3. Since each of the plasma physics facilities has been completed and is beginning its experimental life and each of the technological facilities, with the exception of the Fusion Materials Irradiation Testing program, will have begun operation by 1981, the resolution of most of the key issues associated with the successful operation of a tokamak ETF should be in hand by then. Examination of the technological prerequisites for building an ETF shows that the gaps in our present capability can be filled either from the facilities in Table 2 or from focused development activities carried out in parallel with the ETF project. These development activities will challenge the best of our modern technological capabilities, but there does not appear to be any fundamental obstacle threatening the workability of the ETF.

However, it is also true that some scientific issues which can affect the operation of an ETF may not be settled before the ETF decision point shown in Fig. 1. These issues, which are less easy to specify in terms of definitive experiments or more difficult to address short of a reacting plasma, may remain unsettled, because resource limitations have precluded construction of facilities dedicated to their resolution over the past 2 years. They fall into three general categories: long pulse behavior, ignition, and control of an ignited plasma (*20*).

The variety of existing experiments in the U.S. fusion program offers good hope that we can fill the gap in information concerning long pulse operation of tokamaks. Figure 2 shows the pulse length which could be achieved in a number of U.S. experiments (*21*). Some of these devices, such as the Poloidal Divertor Experiment (PDX), Princeton Large Torus (PLT), Doublet III, and Alcator C, are now operational. The TFTR is under construction and will be operational in 1982. Both Doublet III and the TFTR could be upgraded to increase their capability for long pulse operation at higher magnetic fields. Therefore, although these devices were not designed to produce this type of information, they possess the basic capability to do so and, with careful planning and modification, they should fill the information gap in this respect.

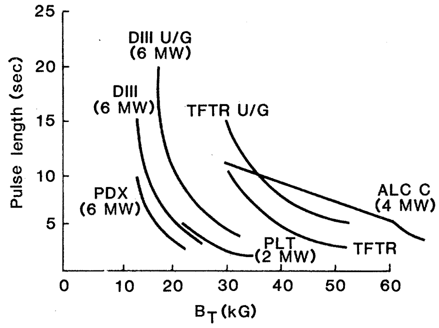


Fig. 2. Several existing devices in the U.S. fusion program have the potential for being upgraded to address unresolved issues of long-pulse operation of tokamaks. Pulses of up to 20 minutes may be obtained at low field on the Doublet III, which is being upgraded under a cooperative fusion agreement between the United States and the Japanese government.

Information on ignition and post-ignition dynamics will be more difficult to acquire. In essence, we need information on the process of alpha-particle heating of a reacting fusion plasma, and there are only two devices under construction from which we can obtain this information in a direct way, the TFTR at Princeton Plasma Physics Laboratory (*22*) and the Joint European Tokamak (JET) at Culham Laboratory in Great Britain (*23*). As in the case of the experiments which may give us long pulse data, these devices were designed with a specific mission and not specifically focused on ignition issues. The mission of TFTR is to achieve energy breakeven with a burning deuterium/tritium plasma. That of JET is to obtain and study a plasma in conditions and with dimensions which approach those needed in a nuclear fusion reactor. From its inception, each project had as one of its subsidiary goals the study of alpha-particle production and plasma heating. However, the questions of plasma energy confinement scaling, heating, and maintaining a clean fusion-quality plasma were prerequisites for these alpha-particle studies and therefore took precedence as project goals.

More recently, with the encouraging results on plasma energy confinement, heating, and maintenance of plasma cleanliness from the PLT experiment (*24*), together with the increasing importance of alpha-particle information as part of the data base necessary for the ETF, the goal of investigating the physics of a burning fusion plasma has received renewed emphasis. In the United States, a thorough review of the information which might be obtained, based on the latest plasma modeling projections for TFTR operation, was conducted to determine the extent to which relevant alpha-particle heating information could be obtained from that device (*25*). The review indicated that, with some modifications, a sufficiently high energy gain (Q) can probably be achieved in the TFTR to provide significant information on alpha-particle containment, thermalization, and possibly heating in the PLT type of hot ion operation. The necessary modifications are now being made in parallel with the project's completion.

Even with these modifications, the total information obtained from the TFTR facility will be limited by the pulse length, the plasma pressure, and the induced radioactivity of the machine structure. To supplement this information, the United States is supporting European efforts to improve the information flow from the JET facility through sharing of our neutral beam heating technology. We are also supporting the design of a West German ignition experiment being proposed by the Max-Planck-Institut. In addition, much information is being obtained on the hot ion mode of ETF operation from existing hydrogen and deuterium experiments such as PLT, ISX-B (Impurities Studies Experiment-B), Doublet III, and PDX. For example,

Table 1. Parameters for an engineering test facility determined in independent studies by Oak Ridge National Laboratory (ORNL), Princeton Plasma Physics Laboratory (PPL), General Atomic Company (GA), and Argonne National Laboratory (ANL) and the ETF reference design parameters based on these studies.

Parameter	ORNL*	PPL†	GA	ANL	ETF
Major radius of torus (m)	5	4.5	3.6	4.7	4.5
Minor radius of torus/vertical elongation	1.2/1.6	1.2/1.6	1/2.7	1.3/1.6	1.2/1.6
Magnetic field (teslas)	5.3	6	5	4.5	5
Ion temperature, T_i (keV)	12	13	12	8	10
Energy confinement time, τ (sec)	1.2	1.4	1.4	2.5	2
Magnetic safety factor, q	3.8	3	2.5	3	3
Plasma pressure/magnetic field pressure, β	7	3.7	6 to 9	7	6
Burn time (sec)	500	86	30	60	30 to 120
Beam energy (keV)	150	150	150	180	150
Beam power (MW)	50	35	60	40	50

*With Westinghouse Corporation. †With Grumman Aircraft Engineering Corporation.

Table 2. Major scientific issues for a tokamak engineering test facility being explored by existing U.S. tokamaks.

Issue	Tokamak
Beta limits	ISX-B
Plasma shaping	Doublet III, PDX, ISX-B
High T_i scaling	PLT, PDX
Impurity control	ISX-B, PDX
Radio-frequency heating	Alcator C, PLT, ISX-B
High $n\tau$* scaling	Alcator C

*$n\tau$, product of plasma number density and confinement time and one measure of how close a device is to energy breakeven.

experiments on ISX-B have shown that intensive neutral beam heating at high plasma pressure can produce the physical conditions that occur with alpha particles during ignition. It is now clear that it will be possible to simulate many, if not all, of the phenomena associated with ignition in existing beam-heated experiments. It should be noted that the hot ion mode of operation would allow ETF to produce substantial amounts of power to fulfill its engineering development function even in the absence of full ignition (*26*).

Fusion Strategy and Pace: Choice of a Timetable

On the basis of present rate of progress, it is likely that the scientific and technical base for an aggressive strategy to explore fusion power generation can be provided in a timely way. Recognizing this, the House science and technology subcommittee asked the DOE to provide them with program plans which would bring fusion to the demonstration phase by the years 1995 and 2000, well ahead of the department's nominal planning case of 2010.

The cases illustrated in Fig. 3 were provided by drawing on the planning activities which have been an integral part of the magnetic fusion program since the first oil crisis in 1973. The basic DOE strategy outlined above was maintained in developing three program paces which would lead to the desired end points. The base case is close to the program now being followed in fusion development. The overriding consideration in this case is limited funding, and so facilities are brought on line in a sequential mode. As indicated, this would lead to the first major fusion milestone after the turn of the century. This milestone is the assessment of fusion's full potential in the light of a broad scientific and technological base following from our basic strategy of parallel physics and technological development.

Table 3. Major components of engineering test facility under development by the U.S. fusion program.

Component	Operational date
Tritium system test assembly (500 moles per day)	1981
Long pulse efficient neutral beams (120 keV, 2 MW, 5 seconds)	1981
Pellet fueling system	1979
Large superconducting coil test project (2.5 by 3.5 m bore coils)	1981
Fusion materials irradiation testing program	1985

The second case reaches this milestone in 1993. The necessary breadth is provided by adding new facilities to explore alternative physics and technology

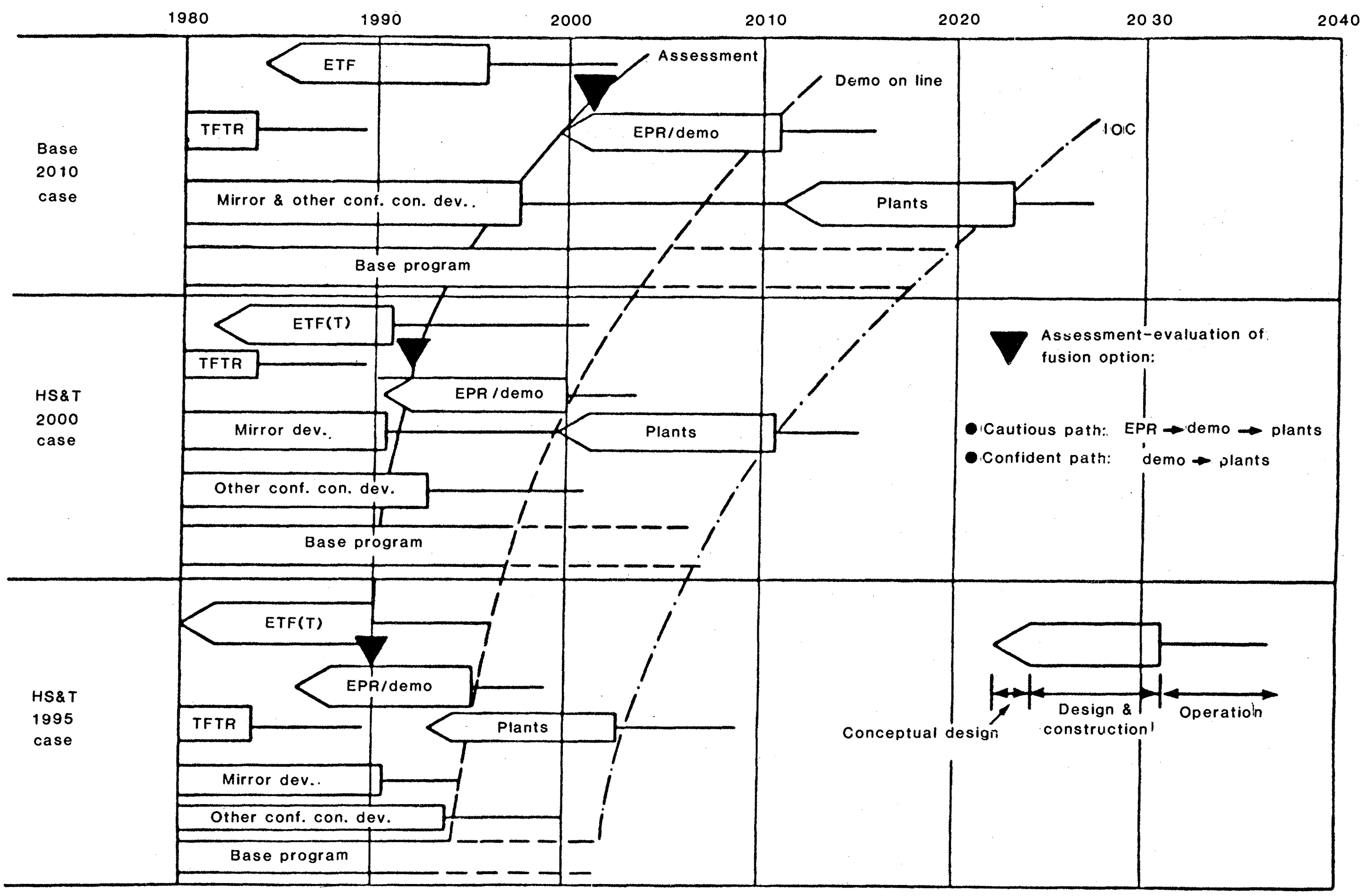


Fig. 3. In the present DOE strategy for magnetic fusion development (base case) an engineering test facility is based on operation of devices now under construction. More aggressive strategies are possible in which the ETF is based on information from existing tokamaks in order to deal with fusion technology problems at the earliest date. These strategies would require parallel development of optimized confinement concepts which, together with ETF technology, would allow an earlier assessment of fusion's actual potential and an earlier start on commercialization of an optimized fusion concept. The latter strategies were provided in response to a request from the House science and technology (HS&T) subcommittee.

options in parallel with an accelerated ETF program. These additional parallel facilities also serve to keep the risk at the same level in this accelerated program as in the nominal case by providing multiple backup options. We consider this to be the technically paced case, as opposed to the financially limited base case.

The final case uses maximum project overlap and additional backup work plus a heavier reliance on the tokamak option through the demonstration phase to minimize the time to commercial fusion power. It should be noted that this provides the fusion assessment only 2 years earlier than in the previous case, even though the demonstration phase is reached 5 years earlier and the initial operational capability 8 years earlier. This is because, in spite of parallel efforts, the engineering development phase of fusion leading to an ETF will take a minimum of 10 years to prepare the basic facilities. Once this base is established, more rapid acceleration can be attempted in the deployment of fusion power.

The DOE strategy places great emphasis on the comprehensive fusion assessment. The decision about the subsequent pace of fusion development after the basic assessment milestone is reached will be determined by the need for energy and the state of competitive alternatives at that time. Our ability to consider the application of fusion power in a rational way depends on the completion of this physics and engineering assessment milestone. One key point established by the studies leading to the paces of Fig. 3 is that the date of this assessment can be moved by up to 12 years.

The basic DOE strategy for the development of fusion lends itself to different paces. In developing some of these paces in response to Congress, we have found it possible to keep the risk within bounds even for extremely rapid programs by broadening and adding redundancy as the pace accelerates. We have tried to ensure that if an aggressive pace similar to those in Fig. 3 is unsuccessful, it will be so because of sound technical facts uncovered by experiments, not because of an insecure scientific foundation or prematurely foreclosed options. Considering the potential benefits of the fusion process, failure to develop a viable fusion reactor would be a tragedy, but early knowledge that it is indeed a false dream would prevent the waste of scarce energy development resources and technical expertise in a more drawn-out effort. In fusion, as in other energy areas, development of a definite near-term knowledge base, even if negative, is in itself in the national interest (*27*).

Conclusion

The evolution of the world energy situation and further work within U.S. and world fusion programs in the next 2 years will reveal the best pace for fusion development. The present magnetic fusion strategy lends itself to several paces without sacrificing scientific breadth, foreclosing options prematurely, or unreasonably delaying the advent of practical fusion power. In developing the key element in this strategy, an acceptable ETF design concept compatible with a nominal or aggressive fusion strategy, our ETF Design Center is using the best talents of our fusion laboratories and U.S. industry. We are assessing R & D requirements to ensure that the ETF concept could, in fact, be implemented on a sound scientific base. Finally, we are focusing our experimental physics and technology development programs to provide the critical data specified by R & D requirements assessment.

Recognizing that international efforts to advance fusion to the power production stage are also necessary to provide maximum assurance in the next step, we are supporting the INTOR project of the International Atomic Energy Agency, wherein the four major blocs in fusion power development, the United States, U.S.S.R., Japan, and Euratom, are seeking to come to a common agreement on the nature of an international tokamak power reactor project. We are also supporting an effort at the Institut für Plasmaphysik of the Max-Planck-Institut, in the Federal Republic of Germany, to develop a flexible precursor ignition experiment which might provide early information for optimizing an experimental power reactor. Finally, we are encouraging joint planning of research on the world class tokamaks now under construction, the T-15 in the U.S.S.R., JT-60 in Japan, JET in Europe, and TFTR in the United States, in order to maximize the data base for the next step. This program should prepare the ground for the next step—taking the fusion program into the engineering development phase—perhaps as early as 1981.

Note added in proof: On 23 September 1980, in a nearly unanimous vote, Congress passed the Magnetic Fusion Energy Engineering Act of 1980, which established as a national goal the demonstration of the engineering feasibility of magnetic fusion in the early 1990's and operation of a fusion demonstration plant by the turn of the century. The act authorizes a program and sequence of devices similar to the HS&T 2000 case of Fig. 3. A major review of the magnetic fusion program by the Energy Research Advisory Board recommended an almost identical program of development for magnetic fusion to the DOE in August 1980. On the basis of this review and the recommendation of the DOE, President Carter signed the bill into law on 7 October. At the signing, the President echoed the sentiments of the congressional debate on the issue by noting that fusion power offers the potential for a limitless energy source with manageable environmental effects.

References

1. S. H. Schurr, *Energy in America's Future* (Johns Hopkins Press, Baltimore, 1979).
2. R. H. Bezdek, A. S. Hirshberg, W. H. Babcock, *Science* **203**, 1214 (1979).
3. Hearing Before the Subcommittee on Energy Research and Production of the Committee on Science and Technology, U.S. House of Representatives, *The Magnetic Fusion Energy Program—Its Objectives and Pace* (86th Congress, 1st Session, 11 December 1979).
4. F. Hoyle, *Energy or Extinction? The Case for Nuclear Energy* (Heinemann, London, 1977).
5. S. M. Keeney, chairman, *Nuclear Power—Issues and Choices* (Nuclear Energy Policy Study Group, Ford Foundation; Ballinger, Cambridge, Mass., 1977).
6. J. M. Deutch, *Congr. Rec.* **124**, 145 (1978).
7. W. M. Stacey, Jr., *et al.*, *The U.S. Contribution to the International Tokamak Reactor Workshop* (IAEA, Vienna, 1979).
8. J. P. Holdren, *Science* **200**, 168 (1978).
9. D. Steiner and J. F. Clarke, *ibid.* **199**, 1395 (1978).
10. J. R. Young, *Siting Commercial Fusion Power Plants* (BNWL-2015, Battelle Pacific Northwest Laboratories, Richland, Wash., 1976).
11. R. G. Clark, *Safety Review of Conceptual Fusion Power Plants* (BNWL-2024, Battelle Pacific Northwest Laboratories, Richland, Wash., 1976).
12. F. Williams, R. Airey, L. Bogart, A. Callingford, paper presented at the International Conference on Alternate Energy Resources, Miami, 6 December 1977.
13. W. Hafele, J. P. Holdren, G. Kersler, G. L. Kulcinski, *Fusion and Fast Breeder Reactors* (International Institute for Advanced System Analysis, Laxenburg, Austria, 1977).
14. M. McCormack, *Congr. Rec.* **125**, 22 (1979).
15. D. Steiner *et al.*, *Oak Ridge TNS Program 1978* (ORNL/TM-6720, Oak Ridge National Laboratory, Oak Ridge, Tenn., 1979).
16. J. Rawls *et al.*, *GA TNS Project*, vol. 3, *Summary, Status Report for FY-78* (General Atomic Company, San Diego, Calif., 1978).
17. D. L. Jassby, Ed., *SLPX, Superconducting Long-Pulse Experiment*, vol. 1, *Executive Summary* (Princeton Plasma Physics Laboratory, Princeton, N.J., 1978).
18. *Tokamak Experimental Power Reactor Conceptual Design* (ANL/CTR-76-3, Argonne National Laboratory, Argonne, Ill., 1976), vols. 1 and 2.
19. S. O. Dean, Ed., *Status and Objectives of Tokamak Systems for Fusion Research* (US-20, U.S. Atomic Energy Commission, Washington, D.C., 1973).
20. J. M. Rawls, Ed., *Status of Tokamak Research* (DOE/ER-0034, Department of Energy, Washington, D.C., 1979).
21. N. A. Davies, chairperson, *Long Pulse Tokamak Operation* (DOE/ET-0084, Department of Energy, Washington, D.C., 1979).
22. *TFTR Final Design Report* (PPPL-1475, Princeton Plasma Physics Laboratory, Princeton, N.J., 1978).
23. J. Booth, Ed., *The JET Project* (Commission of European Communities, Luxembourg, 1976).
24. H. Eubank *et al.*, in *Plasma Physics and Controlled Nuclear Fusion Research*, Proceedings of the 7th International Conference, Innsbruck, 1978 (International Atomic Energy Agency, Vienna, 1979), vol. 1.
25. J. F. Clarke, chairman, *Final Report, Ad Hoc Committee on TFTR Physics* (DOE/ET-0096, Department of Energy, Washington, D.C., 1979).
26. J. F. Clarke, *Nucl. Fusion* **20**, 5 (1980).
27. T. C. Schelling, *Thinking Through the Energy Problem* (Committee for Economic Developments, New York, 1979).