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Report of Panel 1: The Appropriate Scope and Mission of ITER[1]

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SUMMARY OF FINDINGS

This summary of findings is intended to serve as an executive summary. The findings from each section throughout the body of the report are quoted here verbatim.

ITER Development Options (Sec. II)

The Panel endorses the ITER EDA, including commitment to construction, as a pivotal activity in the U.S. fusion program. This activity must be coupled with a strong national program that addresses other DEMO-related tasks in addition to ITER tasks. We emphasize that the U.S. program goals, as stated in the National Energy strategy, would not be achieved if complementary activities to ITER were not carried out.

To accomplish the programmatic objectives of ITER, we find that there basically three scenarios of interest. The first we call the "unified scenario of physics and nuclear testing"; the second we call the "sequenced scenario of physics and nuclear testing." The third we call the "parallel-machine scenario." The Panel finds that while each scenario has particular advantages and elements of risk, all the scenarios provide an acceptable means of meeting the programmatic objectives.

[1] This report was prepared by a panel established by, and reporting to, the Fusion Energy Advisory Committee (FEAC). The report of this panel should not be construed as representing the views, official advice, or recommendations of FEAC.

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A **unified scenario of physics and nuclear testing** is accomplished with either the CDA design or its variant known as the high-aspect-ratio (HARD) design. The CDA design is viewed as not entirely satisfactory by the E.C., Japan, and the U.S. Specifically, the CDA design lacks a self-consistent steady-state operating scenario in which the divertor constraints are satisfied.

The HARD design, as typical of a moderately aggressive design to accomplish unified nuclear testing, makes moderately aggressive physics assumptions with respect to aspect-ratio scaling of confinement times, provides some relief in regard to the still severe divertor design and impurity problems, and improves the prospects for the achievement of most ITER-physics and technology objectives, including blanket studies, nuclear testing, and steady-state opinion.

In the unified scenario of physics and nuclear testing, a strong R&D program will be needed in parallel with ITER design to validate the moderately aggressive technical assumptions and to provide the component reliability needed for a successful and timely nuclear testing program. Otherwise, component failures during ITER operation will lead to increased operating costs because of delayed or extended ITER operations.

A **sequenced scenario of physics and nuclear testing** is represented by the E.C. approach. Based on conservative physics assumptions, The E.C. approach consists of a first stage directed toward the achievement of long-pulse ignition, very limited nuclear testing, and no tritium breeding. The second stage would be devoted to blanket operation, nuclear testing, current drive, and steady-state operation. The fluence in the second stage is moderate, ≤ 1 MW-yr/m^2. The sequenced scenario is

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likely to provide less nuclear experience and entail larger operating costs than the unified scenario. To the extent that conservative confinement scalings are used, the E.C. device will be larger and more expensive in capital cost than the CDA or HARD designs and, therefore, unattractive from the point of view of cost.

A third **parallel-machine scenario** proposes an ITER-class device with moderate (0.1–1.0 MW-yr/m^2) fluence. This superconducting device would carry out an initial phase of operation to explore ignition physics and start nuclear testing. In parallel, nuclear testing would be carried out on a lower power high-fluence ($\leq$1 MW-yr/m^2) nuclear testing machine to provide initial qualification of blanket modules and materials. A tokamak that would serve this purpose as a volumetric neutron source would be much smaller than ITER, non-ignited, and beam-driven. In a briefer second phase of ITER, qualified blanket designs, developed and validated in the smaller machine, would be incorporated for integrated testing, with a need for only low fluence ($<$0.1 MW-yr/m^2). This scenario lowers the risks by providing an alternate path for technology development and fault correction. The initial capital cost is somewhat higher, but the total cost to project completion is likely to be less than the other scenarios because of reduced operating time in the second phase of the larger facility. This scenario also could shorten the time for commercial fusion power developed by 10–15 years, thus reducing the worldwide costs by $20–30 billion.

None of the scenarios address adequately the issue of materials development necessary to achieve the maximum environmental benefit of fusion energy.

The use of copper in an ignited ITER-style device would not reduce cost significantly, nor would it fit within the international ITER consensus.

Data Gap to DEMO (Sec. III)

Physics experimental facilities, using hydrogen/deuterium plasmas, continue to be required in the world mix of facilities to ensure the evolution of an adequate physics basis for a DEMO and for attractive commercial fusion power reactors.

In the absence of a burning plasma experiment, the necessity of using ITER for the first detailed study of high-Q burning plasmas will prolong the physics study phase of ITER and delay the time at which ITER could begin a high-fluence nuclear technology testing phase.

Plasma technologies, such as magnets, heating, high-heat-flux materials, and divertors, are required that are highly reliable and require only infrequent maintenance and replacement. The development of such technologies for DEMO requires specialized facilities and programs.

The construction of a DEMO requires an engineering database on the behavior of materials and components in a fusion nuclear environment over a broad range of operating conditions. ITER is not designed, in any of the scenarios considered, to achieve the high fluence necessary for materials properties measurements at lifetime dpa levels that are needed for the DEMO database for either the low-activation materials or more conventional materials. A 14-MeV neutron source for materials testing remains a necessary, though regularly neglected, element in the world program aiming at DEMO and commercial reactors.

The level of systems analysis currently devoted to fusion commerical requirements is inadequate for a program that is spending roughly a billion dollars a year worldwide and promises to deliver a commercial product on a timetable.

Cost, Risk, and Schedule (Sec. IV)

Given the ITER terms of reference requirement of "demonstrating controlled ignition and extended burn of deuterium tritium plasmas," the Panel has been unable to identify a design or scenario that offers the potential for savings of more than 15% in the initial capital cost relative to the CDA design. The reason is that the size of a superconducting ignition device is set largely by tokamak physics and magnet shielding requirements, independent of fluence goals.

The increase in capital cost associated with providing greater machine capability for a unified program of nuclear testing, as for example in the high-aspect-ratio variant, would be about 9% relative to the CDA. The increased R&D and operating costs associated with providing higher reliability/availability are not included in this estimate.

In view of this Panel, significant non-capital costs specifically for assuring the high-availability, high influence nuclear testing phase of ITER operation have not been adequately included in the CDA cost estimates. These costs, which are difficult to quantify, would be incurred because of the increased R&D needed to ensure a very high level of component reliability, and will arise also from the increased operating costs associated with a lengthy program of technology testing in the ITER combined plasma and nuclear radiation environment. These additional costs would be reduced for the parallel machine scenario, offsetting the increased capital cost for this case, because much of the exploratory testing could be done on the smaller machine where operation would be less expensive.

Base Program Support (Sec. V)

The Panel finds the non-ITER D&T base program to be inadequate for fusion development on the schedule of the DOE National Energy Strategy. The D&T budget was $52 M in FY1987, is $62 M in FY1992, and is projected to be $81 M in FY1993. ITER commitments, however, have reduced the portion devoted to non-ITER R&D in the U.S. Fusion Program from $52 M in FY1987 to $20 M in FY1992 and 1993. This $20 M not committed to ITER must meet domestic program needs, fund present commitments to international collaborations outside of ITER, and support the facilities and base programs that are assumed as existing resources for the ITER estimates.

The panel finds the *balance* of D&T tasks proposed by the U.S. home team generally appropriate.

The panel finds the ITER development funding is inadequate because U.S.-fusion-program estimates for the total ITER R&D package are 40% higher than previously estimated by the international CDA team. In addition, both the U.S. and ITER CDA estimates assumed that ITER would benefit from the existing international D&T effort continuing at about the late 1980s level, e.g., about $50 M/yr within the U.S. Also, many of the costs for developing the high-reliability components needed for nuclear testing are not well understood.

Industrial Participation (Sec. VI)

The U.S. industrial participation in ITER deserves and needs the utmost support from the DOE if it is to succeed. The international competition in ITER requires close attention to and skillful handling of procurement issues to assure a leadership role for U.S. industry.

In view of this Panel, the DOE has been ineffective in implementing a policy that respond to the FPAC recommendations that called for "a substantial involvement of U.S. industry, not only in the hardware phases of the program, but also in the planning, R&D, and analytical phases." A specific plan or process is required to bring about a strong, long-term industry involvement in the fusion program. Other DOE programs have been more effective in developing such industrial participation.

1. INTRODUCTION AND BACKGROUND

At the Fusion Energy Advisory Committee (FEAC) meeting on September 24–25, 1991, Dr. William Happer, Director, Office of Energy Research, DOE, charged FEAC to examine several issues facing the Magnetic Fusion Energy (MFE) program and advise the Department on them. FEAC Panel 1 was created to address those charge questions relating to the U.S. position in the upcoming Engineering Design Activity (EDA) of the International Thermonuclear Experimental Reactor (ITER). The earlier ITER Conceptual Design Activity (CDA) was initiated in 1988 as a cooperative design of an experimental fusion test reactor, with supporting R&D, aimed at joint construction by any combination of the parties, with a construction decision to be made in or about 1995. In creating Panel 1, the FEAC Chairman, Dr. Robert Conn, elaborated the original charge in a letter dated October 8, 1991.

During the 1992–1997 EDA period, the design effort will build on the results of the CDA, which was completed in October 1990. In reviews of the CDA design by the ITER partners, several modifications have emerged that, in addition to addressing known technical issues in the design, offer different mixes of cost, risk, and benefit in meeting the ITER programmatic objective.

The ITER programmatic objectives were established as part of the Terms of Reference for the CDA, and they have recently been reaffirmed by all of the four ITER partners (the U.S., Japan, the European Community, and the Soviet Union) in their individual national reviews of the ITER CDA activity. The ITER programmatic objective, taken from the Test of the ITER EDA Agreement and Protocol One (July 1991), is as follows:

> The overall programmatic objective of ITER, which shall guide the EDA, is to demonstrate the scientific and technological feasibility of fusion for peaceful purposes. ITER would accomplish this objective by demonstrating controlled ignition and extended burn of deuterium tritium plasmas, with steady-state as an ultimate goal, by demonstrating technologies essential to a reactor in an integrated system, and by performing integrated testing of the high-heat-flux and nuclear components required to utilize fusion energy for practical purposes.

This programmatic objective will be supported by technical objectives to be negotiated early in the EDA with tecnical support provided by the ITER-EDA Special Working Group 1 (SWG 1). Dr. Happer's request to FEAC is in the context of developing the position to be taken by the U.S. in these important negotiations. This report provides background information for the FEAC's deliberations.

The importance of the ITER cooperation to the U.S. fusion program was underscored in 1990 by the Secretary of Energy's Fusion Policy Advisory Committee (FPAC). The FPAC recommended U.S. participation in the ITER EDA as an important step in preparing for an ITER construction decision. As a second part of preparation for ITER construction, the FPAC also recommended proceeding with the U.S. Burning Plasma Experiment (BPX) at Princeton, which was designed to provide the first laboratory experience in plasmas having a majority of their heating arising from self-generated alpha particles. Data from BPX was seen by the FPAC, as well as by the subsequent U.S. National Review of the ITER CDA Design, as important for reducing the risk and duration of the physics phase of ITER operations. The FPAC Plan for MFE Development from the present to the Demonstration Reactor (DEMO) is shown in Fig. 1.

Part of the need for reevaluating the U.S. position regarding the ITER technical objectives stem from the recent DOE decision not to proceed with BPX construction. The absence of BPX will eliminate an important stepping stone between today's machines and ITER, so that ITER's burning-plasma physics objective assumes increased significance.

In preparing this background document, FEAC Panel 1 used material from the U.S. ITER Home Team, the U.S. SWG 1 Team, and independent work by U.S. fusion community members, as well as, earlier studies by the ITER Steering Commitee-U.S. (ISCUS), the ITER Scientific and Technical Advisory Committee (ISTAC), the U.S. National Review of the ITER CDA, and the ITER Conceptual Design Report. Also, on January 16, 1992, a meeting was held with P. Rebut and M. Yoshikawa to discuss the issues being considered by this Panel.

This report is organized as follows: Section II describes several scenarios that can be interpreted as meeting the programmatic objective in different ways, while permitting different mixes of aggressiveness, risk, and cost. Section III assesses the data gap between today's machines and a DEMO. Section IV describes cost, schedule, and risk associated with the scenarios presented in Section II. Section V deals with the base program support. Finally, Section VI addresses U.S. industrial involvement.

2. ITER DEVELOPMENT OPTIONS

2.1. Introduction

This section describes three acceptable ITER development scenarios. A fourth section, which we rejected, consists of a *copper-conductor* ITER device for long-pulse ignition physics plus a smaller, copper low-Q nuclear and technology testing device. The three ITER

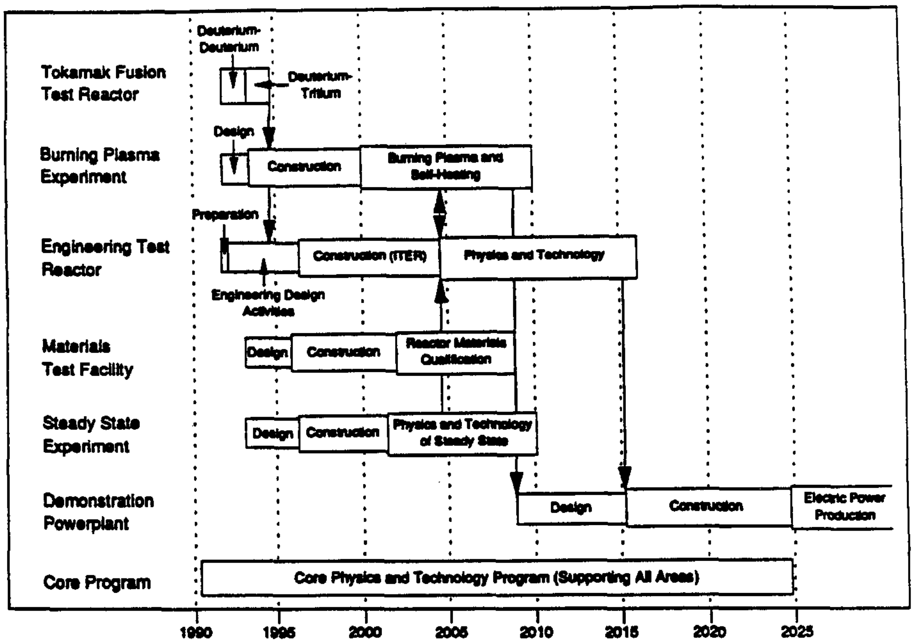

Fig. 1. FPAC plan for MFE development from the present to the DEMO.

development scenarios all plan to carry out the "technologies essential to a reactor in an integrated system," as well as the "integrated testing of high-heat flux and nuclear components."

During the nuclear testing phase planned for ITER, high fluence (1–3 MW-yr/m^2) is desired for material and blanket development. A full blanket testing program would start with scoping studies using 0.5–1.0 m^2 modules and end with a validated DEMO concept after about 3 MW-yr/m^2. The selected DEMO blanket concept, including high-grade heat extraction, would then be tested in one or more full sectors (a sector is 1/32 of the ITER torus) for a few months (low-fluence, $\leq$ 0.1 MW-yr/m^2) near the end of the ITER operational lifetime.

The **unified physics and nuclear testing scenario** contemplates using ITER for nuclear and blanket testing from the earliest feasible time. The present embodiment of this somewhat aggressive scenario includes the original CDA design, a high-aspect-ratio modification (U.S. HARD design), and other possible variations.

The **sequenced physics and nuclear testing scenario** emphasizes beginning with a low-to-moderate fluence ignition-physics phase, and later proceeds to a testing phase when suitable plasma conditions are well established. The E.C. modification of the CDA design is typical of the more conservative sequenced scenario.

The **parallel-machine scenario** consists of an ITER-like device, which would ultimately do integrated blanket tests for a DEMO at low fluence; plus, a low power non-ignited nuclear technology test machine that would serve as a volumetric neutron source (VNS) providing moderate-to-high fluence. Blanket concepts would be validated in the second machine and then receive integrated low-fluence tests in the ITER machine.

For any of these scenarios, a 14-MeV neutron source for materials testing, including low-activation material development, would be separately necessary in addition to facilities for concept improvement. Table I summarizes many of the properties of interest of these scenarios, and rates the three ITER scenarios for reliability against classes of risks.

A fourth, non-ITER, scenario was examined to evaluate the possibility of significant cost reduction of the ITER activity by using a copper-coil design for the long-pulse ignition machine. To accomplish the ITER mission, it would be necesssary to add a second, non-ignited nuclear technology machine. This pair has only a modest reduction in cost and falls short of the ITER systems integration goal. As a consequence, this option is not discussed elsewhere in the report after the next three paragraphs.

As an option with the goal of reducing costs, a copper-coil, long-pulse ignition experiment could certainly be designed and constructed. For short pulses and low-neutron fluence, one can build a high-field, compact smaller device, which could be liquid nitrogen or water cooled. The cost, based on BPX work, might be \$2–3 billion.

There is significant risk that the pulse length would be inadequate to investigate He accumulation and particle control issues. A long-pulse Cu ignition machine would necessarily be larger, of lower field and actively cooled. Long-pulse He ash accumulation and particle control issues would be addressed and the cost would be about \$4–5 B. Neither device would have non-inductive current drive or the ability to handle large neutron fluence.

A small copper-driven device would be constructed to perform nuclear technology and materials testing. This device would be capable of producing a fluence of ~ 1 MW-yr/m^2 and would test neutron properties of nuclear materials and technologies.

A significant deficiency arises in that neither device is capable of performing the steady-state integerated tests of nuclear fusion technologies and components in a burning-plasma environment. A third device to perform this integration would be required to verify the technologies for future DEMO use, or one accepts the significant extrapolation to the DEMO without prior demonstraton. The Panel feels that the cost and schedule for the third device is unacceptable, and that without doing the third device, the technical risk transfered to the DEMO is too great. We therefore conclude that a multiple machine approach based on copper devices for both the ignited plasma and nuclear testing are not credible for our National Energy Strategy goal of a DEMO by 2025.

2.2. Scenario with Unified Physics and Nuclear Testing

This moderately aggressive scenario proposes one device capable of addressing most of the physics and technology issues. Such a device would plan for both tritium breeding and nuclear testing, and it would contemplate steady-state operation through the implementation of non-inductive drive. Both the CDA device and the U.S.-proposed high-aspect-ratio (HARD) design fall within this category. Other variations could be generated as a result of the EDA phase. This approach is characterized by the introduction of a breeding blanket initially and the intention to develop a machine of high reliability capable of achieving, at a minimum, long-pulse operation on the order of 1000s, fluences of at least 1 MW-

Table I. Summary of Scenarios

Scenario physics and tech.	Fluence MW-yr/m²	Approximate power level (MW)	Tritium consumption (kg)	Is driver blanket needed?	DEMO blanket integrated test	Current drive	End of mission begin 2005	Risks: Cost capitol (oper.) $B	Risks: Min. tech. risk	Risks: Timely Info. for DEMO
Unified	3.0	1000	165	Yes	Sector	Yes	2028	6 (0.4)	3	2
Sequenced									2	3
ITER Phase 1	0.3	1000	17	No	No	No	2032	6^- (6^{++} EC)		
ITER Phase 2	1.0–3.0		165	Yes	Sector	Yes		(0.4)		
Parallel-path									1	1
ITER	0.3	1000	17	No	Sector	Maybe	2017	6^-		
VNS	1.0–3.0	50	8	No	Module testing	Yes	2015	>2 (0.2)		

yr/m² with an objective of 3 MW-yr/m², and quasi-continuous operating periods (with minimum dwell times) of up to 2 weeks. The operation schedule would consist of about 10 years for ignition physics followed by another 10 years of nuclear testing. Tritium consumption would be about 165 kg, for 3 MW-yr/m². The CDA plans to install a cold-breeding blanket at the outset to produce the necessary tritium. The breeding or "driver" blanket is not reactor relevant because it uses low-temperature water coolant and a stainless-steel structure to minimize risk. Such devices are clearly moderately aggressive in view of the probable impact of unresolved technical issues. Most likely, a high reliability/availability machine would require substantial research and development addressed to reliability issues in the EDA phase.

The issue of confinement capability is somewhat distinct from that of the approach to nuclear testing. Although the E.C. considers the CDA ignition capability marginal and opts for a higher ignition margin, the U.S. review considers the CDA ignition capability more than adequate for short-pulse ignition and adequate for long-pulse ignition. In any case, driven operation at high Q would be a satisfactory mode of operation for the nuclear testing program.

In both reviews, minor engineering weaknesses have been found and substantial problems have been noted in divertor design, helium ash build-up, and the development of staisfactory current drive schemes. The U.S. HARD design improves on the CDA performance, specially for long-pulse operation, and relies on increased aspect ratio to maintain confinement properties as the plasma current is reduced. In addition, the driven current is reduced, the bootstrap contribution is higher, and the toroidal field is increased.

In recent years, most large tokamaks have operated with an aspect ratio of about 3, although there is some experience at larger aspect ratio. If the INTER-89P scaling accurately represents the dependence of energy confinement time on aspect ratio, as recent results strongly indicate, then a significant improvement on the CDA design is possible at the somewhat larger aspect ratio of $A = 4$. The HARD design takes advantage of this improvement and proposes a device that can encompass the phyics and testing objectives of ITER. It should be able to achieve ignition, demonstrate steady-state operation, and use the steady-state operating mode to achieve breeding and other nuclear testing objectives. If the ITER-89P scaling were to fail, then long-pulse operation at substantially reduced Q would be likely. The outstanding issue is the reliability of the confinement extrapolation to high values of A, although some engineering design issues also need to be resolved. The principal advantage of the HARD design is that it provides a steady-state (or at least very long-pulse) mode of plasma operation at high neutron wall load, thereby satisfying the requirements for nuclear testing better than the CDA deisgn. The ability to operate steady-state or very long pulse will also demonstrate a more favorable reliablity and availability potential for fusion.

If machines of this class were successful, then much of the technology and physics needed for a DEMO would be achieved. If one could not carry out the entire ITER program because physics or technology limitations prevented full nuclear testing while still allowing some long-pulse operation, then the excess cost over a minimum machine to accomplish goals similar to the E.C. first-phase operation is probably no more than 10–15% of initial cost. Partial initial failure of the nuclear mission might require substantial retrofitting, as in the E.C. plan, in order to conclude the nuclear mission successfully. With a unified scenario of physics and nuclear testing, ITER is firmly committed to the central goal of timely nuclear technology development.

Aggressive nuclear testing goals advocated in the unified scenario of physics and nuclear testing obviously imply greater risk of failure, mainly because of hardware unreliability, than in more conservative scenarios. In addition, a somewhat greater investment is at risk in the event of serious hardware failure. On the other hand, the additional machine hardware (such as the driver blanket and current-drive systems) introduced in pursuit of the more aggressive objectives are not themselves considered to be significant sources of unreliability or failure potential. Indeed, increased attention to reliablity issues would obviously be advantageous whatever are the nuclear testing objectives.

The ITER project will be the largest and most visible activity in the world fusion program. A possible criticism of the scenario in which ITER pursues aggressive nuclear-testing objectives and is viewed as a full Engineering Test Reactor is the implication that the DEMO must then have the same economic and environmental characteristics as ITER. To avoid this, compensating emphasis must be placed on tokamak concept improvement and on a broad program of nuclear development involving advanced materials and attractive environment/safety features. On the other hand, there is a significant public-perception risk in *not* pursuing aggressive nuclear-testing objectives, in that any superconducting, high-duty factor machine of the ITER class has the intrinsic capability for achieving such objectives, so that the setting of relatively low availability/reliability goals will be seen as implying lack of confidence in the practical potential of fusion systems.

2.3. Scenario with Sequenced Physics and Nuclear Testing

The E.C. assessment of the CDA is that the ignition margin is inadequate, because of uncertainty in the presence of substantial helium ash concentrations, and that installation of a driver blanket from the beginning is an unnecessary and costly complication. They have proposed a larger, and more costly, device that would increase the probability of successful ignition. Self-sufficiency in tritium, possible steady-state operation, and much nuclear testing would be deferred until a second phase in which major modifications of the device would be considered. The strong emphasis on a program of burning plasma and other physics experimentation at modest neutron fluence in the first phase was dictated by their wish to defer some costs to the second phase, and by some skepticism as to the availability, at construction time of a satisfactory driver blanket design and steady-state mode of plasma operation consistent with satisfactory divertor performance. However, the longer inductive pulse length and the relatively high neutron wall load obtainable in the larger device advocated by the E.C. satisfy the basic requirement for the nuclear testing program. The dependence on external tritium supplies will limit the amount of nuclear testing that can be accomplished in the first phase of operation. In the E.C. plan the fluence would be limited to about 0.3 MW-yr/m^2 and periods of quasi-continuous operation (with minimum dwell times) would be limited to about 40 hours. It is likely that the ITER activity would be extended by some years in this scenario, partly because of increased physics experimentation and partly because of the 3–4 years needed for driver blanket installation. Further, the possibility of relatively easy modifications into a second phase, with the addition of a blanket and current drive, is far from sure. In addition, several studies (including in the E.C.) have indicated that a nuclear testing program corresponding to a fluence in the range 1–3 MW-yr/m^2 will be needed to provide the database for selecting a DEMO blanket. It is likely that the integrated cost of this scenario would be somewhat higher than the first, although this scenario would have a higher likelihood of initial physics success if the increased confinement margin is implemented as advocated by the E.C.

The E.C. approach adopts a goal of moderate-fluence and defers full-scale nuclear testing until more is known about ignitied plasma behavior and blanket design. Similarly, the commitment to current-drive and steady-state operation is delayed until there is better physics knowledge of steady-state plasma operation with effective power exhause and impurity control. In addition, the E.C. questions whether there is yet a definitive understanding that a DEMO must be steady state. Clearly, such a strategy is desirable if major modifications in our concept of a fusion reactor appear. It is highly cost ineffective and dliatory if the level of machine availability/reliability needed for the more aggressive approach turns out to be achievable.

The main purpose of a conservative strategy is, obviously, to minimize the technical risk that minimum objectives will not be achieved. Certainly, provision of increased confinement margin, as the E.C. advocates, would increase the assurance that ignition will be attained even in the face of modest shortfalls in plasma performance. On the other hand, provision of increased confinement margin requires a significantly larger device, with a correspondingly significant increase in capital cost (estimated at 15% over the CDA by the E.C. and 20–25% by the U.S. ITER home team).

A nominally "conservative" approach introduces its own set of risks. Reliance on a single plasma heating system without current-drive capability, as the E.C. also advocates, will introduce a new element of physics risk in that an effective means of controlling the plasma profile will be lacking. However, the main risk associated with an approach that defers moderate-fluence nuclear testing to a second phase of ITER, after major machine modifications, is the programmatic risk that the second phase will be unacceptably delayed or may never be implemented at all. This risk is serious, both of itself and because the uncertainty whether or not the second phase of ITER will actually be implemented will tend to inhibit effective program planning in the area of nuclear and blanket testing. There is also a technical risk that the minimal, low-fluence nuclear testing program that will be possible in the first phase of ITER will be inadequate to provide the data needed for development of a DEMO-relevant blanket in the second-phase. Finally, there could be a public-perception risk in not operating ITER up to the reliability/availability levels of which it would be intrinsically capable because of an enforced reliance on external tritium supplies. Public perception of fusion practicality could be adversely affected by the inability of ITER to demonstrate levels of machine availability exceeding about 5%.

On the basis of analysis carried out during the CDA, the fluence achievable in the first phase of this "sequenced" scenario has been assumed to be limited by external tritium supplies to about 0.3 MW-yr/m^2.

2.4. Parallel Path Scenario

The Panel has also explored a third scenario that, if adopted, could avoid some of the potential problems identified for the above scenarios. This alternative, which would contain two parallel, coordinated facilities, would be designed to achieve the full ITER objectives with reduced technical risk on an accelerated timescale. The second of the two facilities could be incorporated within the ITER agreements only after negotiations with our partners. Alternatively, it could be done under other international agreements or as a national initiative.

This scenario would contain a large superconducting tokamak, much like the current vision of ITER. In a first phase of operation, it would address the physics of long-pulse ignition with steady state as an ultimate objective, and would carry out a program of testing blanket modules at low-to-moderate fluence. In its second phase, which would last only a few years or less, this machine would address integrated testing of DEMO-relevant blanket sector(s) and other nuclear technologies.

As described, this machine's objectives would be very much those of the ITER CDA technical objectives, *except* that it would not need to operate in its technology phase for sufficient duration to accumulate the 1–3 MW-yr/m^2 target fluence for ITER's nuclear testing. It is an important point that the desired nuclear testing at moderate-to-high fluence does not require the full 1000-MW power level of ITER. In fact, all that is required is some 20 m^2 of testing surface, or 20 MW of fusion power at the ITER's wall loading. Using the full ITER for this purpose is very inefficient in both operating costs and tritium consumption.

If the large machine did not have the requirement to operate to the full fluence level and if it were to be used in its second phase only for integrated demonstration of blankets and technologies that had been developed elsewhere, there could occur a savings in capital costs of 15% relative to the CDA design (a savings also realized in the E.C. approach), and a more significant savings in operating cost resulting from the reduced operating lifetime. Also, the reduced demand for tritium, a factor of 10 less than for the other scenarios, would eliminate the need for a driver blanket.

A second, much smaller and less expensive, driven (not ignited), steady-state machine producing neutrons at ~ MW/m^2 would complement the larger facility in important ways as suggested above. It would be used to preselect blanket and other nuclear technologies, and it would need to operate for sufficient duration to fulfill the ITER fluence requirements, i.e., 1–3 MW-yr/m^2. By starting operating well in advance of the larger machine's second phase, the smaller machine could complete the high fluence earlier than could a testing program using the larger machine, thereby better matching the planned schedule for the DEMO. A comparison of the time lines for the three scenarios is shown in Fig. 2.

In order for the two-machine approach to be economically competitive in terms of overall costs, the capital cost of the smaller machine must be of the order of the savings in costs realized by the reduction in operation of the larger machine. It could be more, as shown in

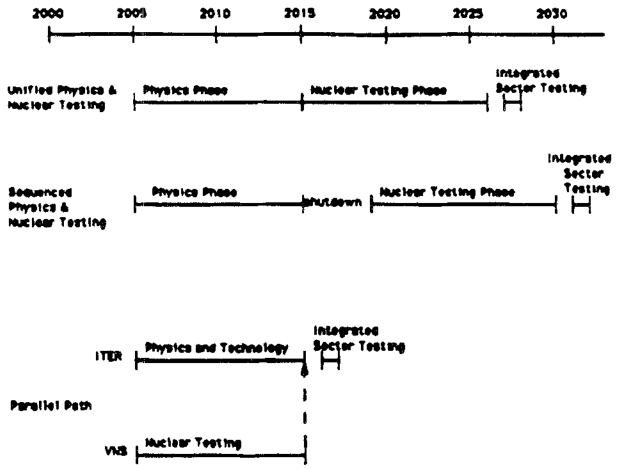

Fig. 2. Time lines for development scenarios.

Fig. 2, but if this reduction were taken as 5–6 years (one-half the currently estimated 10–12-yr technology phase) at an annual budget of $350–400 M/yr, one obtains a target of up to $2 billion for the construction costs of the smaller machine. Designing a technically achievable machine to meet this mission at this budget would be a challenge owing to the costs associated with achieving high fluence. Preliminary estimates suggest that this should be possible, but this cost question needs careful examination.

There is a second way by which this two-machine strategy could be cost effective, although it is a manner that is hard to quantify. Use of the large machine to obtain high-fluence data in the planned 10-yr technology phase has been widely recognized to require a technically very demanding level of availability, 10–30% *averaged* over a 10-yr period. A similar reliability would, of course, be required in use of the smaller machine for this purpose. However, there it is expected that necessary high availability could be developed in a less costly manner.

For the smaller machine to complement the larger in the way described, the two machines would need to be constructed as nearly as possible at the same time. Unacceptably large annual budgets during the construction time could be avoided by omitting the cost of the driver blanket, delaying the introduction of the current drive power, and (possibly) stretching out somewhat the construction of the large machine—emphasizing again that completion of the entire ITER mission would thereby be accelerated in comparison with the single-machine scenarios.

In the foregoing, it has been implied that the smaller machine would be a driven tokamak. Although the tokamak might indeed prove the most cost effective and useful device, other technologies should also be considered. If, if addition, the universally agreed-upon need for an intense 14-MeV neutron source is considered, then this scenario has the advantage that it would be possible to site ITER, the nuclear technology test facility, and the 14-MeV neturon source in different countries. This might facilitate the site-selection process for ITER.

In view of the potential advantages that this variant of the ITER program might provide, the Panel believes that it warrants further consideration but recognizes that many important questions remain to be examined.

ITER Development Options Findings

The Panel endorses the ITER EDA, including commitment to construction, as a pivotal activity in the U.S. fusion program. This activity must be coupled with a strong national program that addresses other DEMO-related tasks in addition to ITER tasks. We emphasize that the U.S. program goals, as stated in the National Energy Strategy, would not be achieved if complementary activities to ITER were not carried out.

To accomplish the programmatic objectives of ITER, we find that there are basically three scenarios of interest. The first we call the "unified scenario of physics and nuclear testing"; the second we call the "sequenced scenario of physics and nuclear testing." The third we call the "parallel-machine scenario." The Panel finds that while each scenario has particular advantages and elements of risk, all the scenarios provide an acceptable means of meeting the programmatic objectives.

A unified scenario of physics and nuclear testing is accomplished with either the CDA design or its variant knonwn as the high-aspect-ratio (HARD) design. The CDA design is viewed as not entirely satisfactory by the E.C., Japan and the U.S. Specifically, the CDA design lacks a self-consistent steady-state operating scenario in which the divertor constraints are satisfied.

The HARD design, as typical of a moderately aggressive design to accomplish unified nuclear testing, making moderately aggressive physics assumptions with respect to aspect-ratio scaling of confinement times, provides some relief in regard to the still severe divertor design and impurity problems, and improves the prospects for the achievement of most ITER physics and technology objectives, including blanket studies, nuclear testing, and steady-state operation.

In the unified scenario of physics and nuclear testing, a strong R&D program will be needed in parallel with ITER design to validate the moderately aggressive technical assumptions and to provide the component reliability needed for a successful and timely nuclear testing program. Otherwise, component failures during ITER operation will lead to increased operating costs because of delayed or extended ITER operations.

A **sequenced scenario of physics and nuclear testing** is represented by the E.C. approach. Based on conservative physics assumptions, the E.C. approach consists of a first stage directed toward the achievement of long-pulse ignition, very limited nuclear testing, and no tritium breeding. The second stage would be devoted to blanket operation, nuclear testing, current drive, and steady-state operation. The fluence in the second stage is moderate, $\leqslant 1$ MW-yr/m^2. The sequenced scenario is likely to provide less nuclear experience and entail larger operating costs than the unified scenario. To the extent that conservative confinement scalings are used, the E.C. device will be larger and more expensive in capital cost than the CDA or HARD designs and, therefore, unattractive from the point of view of cost.

A third **parallel-machine scenario** proposes an ITER-class device with moderate (0.1–1.0 MW-yr/m^2) fluence. This superconducting device would carry out an initial phase of operation to explore ignition physics and start nuclear testing. In parallel, nuclear testing would be carried out on a lower power high-fluence ($\geqslant 1$ MW-yr/m^2) nuclear testing machine to provide initial qualification of blanket modules and materials. A tokamak that would serve this purpose as a volumetric neutron source would be much smaller than ITER, non-ignited, and beam-driven. In a briefer second phase of ITER, qualified blanket designs, developed and validated in the smaller machine, would be incorporated for integrated testing, with a need for only low fluence (<0.1 MW-yr/m^2). This scenario lowers the risks by providing an alternate path for technology development and fault correction. The initial capital cost is somewhat higher, but the total cost to project completion is likely to be less than the other scenarios because of reduced operating time in the second phase of the larger facility. This scenario also could shorten the time for commercial fusion power development by 10–15 years, thus reducing the worldwide costs by $20–30 billion.

None of the scenarios address adequately the issue of materials development necessary to achieve the maximum environmental benefit of fusion energy.

The use of copper in an ignited ITER-style device would not reduce cost significantly, nor would it fit within the international ITER consensus.

3. DATA GAP TO DEMO

The purpose of a demonstration reactor is to demonstrate all the features of the first generation of commercial power reactors. However, some modest degree of extrapolation from the DEMO to the first commercial plant is permitted. For example, the cost of electricity from a DEMO may not be competitive with other power sources, but the extrapolation to competitive cost must be evident from DEMO experience. Likewise, the safety and environmental advantages of fusion must be evident from the DEMO experience even though the "ultimate" low activation material might not be qualified in time for the DEMO. The DEMO must produce net power and deliver a reasonable amount of electricity to the grid.

To provide the database for constructing a DEMO, adequate programs must be expanded in the following general areas, as has been discussed in detail in many reports (e.g., "Technical Planning Activity," ANL/FPP-87-1).

- optimization of the magnetic confinement configuration
- study of the properties of burning plasmas
- development of required plasma and nuclear technologies
- development of required materials
- systems analysis of commercial reactor requirements

As these programs are expanded and new facilities and facility upgrades are considered to advance the state-of-the-art in the above areas, it is important to keep in mind the two primary attributes that will characterize a successful commercial fusion system: (1) competitive economics, and (2) safety, environmental, and licensing advantages.

Planning studies that have been performed in the past have always identified the need for one or more large fusion test reactors, prior to the DEMO, having the integrated plasma and technology performance necessary to permit confident extrapolation to a DEMO. ITER is the latest embodiment of what has been called, generically, an engineering test reactor.

Although an engineering test reactor has been viewed as an essential element along the fusion development path, it is still only one of a set of complementary, specialized facilities necessary to provide the data and experience base for the DEMO.

Optimization of the magnetic configuration can be studied in less complex facilities than those required for an engineering test reactor. Furthermore, studying the physics of magnetic confinement in sufficient depth to be able to optimize the configuration requires dedicated facilities. The importance of optimization is due to the fact that a straightforward extrapolation of today's physics leads to very large devices that are unlikely to produce power at a competitive price. Additional data are required on issues such as steady state, divertors, disruptions, and current drive. Improvements are desired in such areas as better energy confinement, higher plasma pressure, more efficient durrent drive, and less costly heating methods. Study of these issues does not require a burning plasma. Fusion science has not yet reached the stage where the plasma core for ITER can be based on a physics basis that would be satisfactory for the core of an economic commercial fusion reactor. Also, the DEMO requires a better physics basis than that currently used for the design of ITER.

The properties of burning plasmas is a new regime for which there is almost no data. For this reason, the U.S. had proposed a relatively small facility (BPX) designed to study the physics of burning plasmas. Although ITER must necessarily operating in the burning physics regime, it did not appear to be cost-effective or timely to use that facility as a test bed for the study of burning plasma physics. With the demise of BPX, and in the absence of any agreed upon alternative, ITER has become, by default, the first opportunity to study burning plasmas in detail.

Plasma technologies, such as magnets, heating, plasma-facing components, and divertors, require further development for DEMO. The development planned for ITER will be helpful but not adequate for DEMO. Much of this technology can be accomplished in a non-radiation environment in specialized test facilities.

An engineering test reactor is an ideal facility in which to test **nuclear technologies** for the DEMO. However, before an engineering test reactor can be used for this purpose, it must already have nuclear-qualified materials and components sufficiently reliable that the test reactor itself can run at high availability. Also, as noted previously, the need to transfer the BPX program of burning plasma physics to ITER will result in a delay of several years in the time at which ITER will be available for nuclear testing. The parallel-path scenario, discussed in the previous section, fills this programmatic need.

Commercial fusion reactors ultimately should be built using low activation **materials**. The most promising materials from this standpoint, such as Vanadium alloys and SIC, are not currently commonly used as construction materials. Furthermore, commercial reactor and DEMO materials must maintain adequate properties in a radiation environment for an extended period of time.

Systems studies of the commercial requirements for fusion may identify a variety of specialized test facilities that are needed to complement an engineering test facility. For example, a recent on-going study indicates that it may be desirable to build a low power, driven fusion "pilot plant" to permit utility and industrial engineers to gain operational experience prior to the initiation of a DEMO. The issues to be addressed in such a plant include the production of high grade heat; operation and maintenance technologies; power plant instrumentation, control and protection; power plant safety, environment, and licensing; and waste management and decommissioning.

The various alternative design approaches being discussed for ITER have a ripple effect on all other aspects of the fusion development plan. In some cases, these effects are a matter of degree, but in other cases, such as a case in which the ITER mission were restricted to burning plasma physics, the impact on other elements of the program could be profound.

In the case where ITER maintains its original objectives as an engineering test reactor, it is essential either that it proceed rapidly through any burning plasma physics study phase and into a mode of reliable, high availability operation as a technology test bed or that a separate, smaller technology test reactor be constructed in parallel.

In the cases where ITER emphasizes its burning plasma physics phase and postpones or eliminates its technology testing mission, the separate nuclear technology test facilities become essential if the DEMO is to operate in the 2025 time frame.

In all cases, it is important that the international program plan for fusion development include an appropriate mix of complementary facilities and programs necessary for construction of the DEMO and follow-on commercial reactors.

Finally, it is important to remember that ITER, in any form, could be significantly delayed, or even cancelled, for reasons beyond the control of U.S. fusion program managers. Thus, the U.S. and world program should contain a mix of physics and technology test facilities that allows continued progress on critical issues in the absence of ITER, so that a revised engineering test reactor concept could evolve and be implemented.

3.1. Data Gap to DEMO Findings

Physics experimental facilities, using hydrogen/deuterium plasmas, continue to be required in the world mix of facilities to ensure the evolution of an adequate physics basis for a DEMO and for attractive commercial fusion power reactors.

In the absence of a burning plasma experiment, the necessity of using ITER for the first detailed study of high-Q burning plasmas will prolong the physics study phase of ITER and delay the time at which ITER could begin a high-fluence nuclear technology testing phase.

Plasma technologies, such as magnets, heating, high-heat-flux materials, and divertors, are required that are highly reliable and require only infrequent maintenance and replacement. The development of such technologies for DEMO requires specialized facilities and programs.

The construction of a DEMO requires an engineering database on the behavior of materials and components in a fusion nuclear environment over a broad range of operating conditions. ITER is not designed, in any of the scenarios considered, to achieve the high fluence necessary for materials properties measurements at lifetime dpa levels that are needed for the DEMO database for either the low-activation materials or more concentional materials. A 14-MeV neturon source for materials testing remains a necessary, though regularly neglected, element in the world program aiming at DEMO and commercial reactors.

The level of systems analysis currently devoted to fusion commercial requirements is inadequate for a program that is spending roughly a billion dollars a year worldwide and promises to deliver a commercial production on a timetable.

Table II. The CDA Estimate of Costs from the ITER Conceptual Design Report[a]

	Cost ($ millions FY89)
Engineering Design Activity	
Design work	250
Engineering R&D	385
Prototype testing	397
Total	1032
ITER construction phase	Cost
Tokamak	1700
Tokamak auxiliaries	1400
Buildings and plant auxiliaries	800
Assembly and transport	300
Construction cost contingency	700
ITER construction cost subtotal	4900
Professional manpower during construction phase	800
Additional technology R&D during construction	300
Total project cost	6000
Annual operating expense	Cost
Tokamak operation	270
Nuclear testing program	120
Total operating budget	390

[a] (ITER documentation series no. 18).

4. ITER COST, RISK, AND SCHEDULE

Costs and Advantages for an Integrated Testing Scenario. The cost of the CDA integrated nuclear testing scenario provides a basis to which other designs and scenarios can be compared. The CDA device in FY 1989 dollars has a nominal cost of $6 billion for construction and $400 million per year for about 18 years of operation as summarized in Table II. In FY 1991 dollars the total cost is approximately $7 billion.

The CDA costs have been established using both systen-code type analysis and a "bottoms-up" work breakdown analysis by engineers. In the absence of a detailed design the estimates are obviously subject to some uncertainty.

The HARD design (high-aspect ratio design) by the U.S. home team provides the same ignition-mode performance as the CDA with improved capabilities for steady-state operation. The design has been examined at the systems analysis level and in recent more detailed studies. The cost is about 9% greater than the CDA mainly because the toroidal field coils are more massive and expensive.

The significant advantage of this moderately aggressive scenario is that much of the technology and physics needed for a DEMO would be achieved by meeting the technical objectives, thus providing a demonstration of fusion's engineering practicality. Providing the level of reliability and availability needed for some reasonable nuclear testing program, would allow ITER to realize its full potential in the fusion program. Installing a blanket at the outset and purchasing power for current drive would be consistent with commitment to a central goal of timely nuclear technology development. A possible criticism of the scenario in which ITER is viewed as a full Engineering Test Reactor is the implication that the DEMO must then have the same economic and environmental characteristics as ITER. To void this, compensating emphasis must be placed on concept improvement and on a broad program of nuclear development involving advanced materials and attractive environmental/safety features.

Costs for the Sequenced Nuclear Testing Scenario. To be more certain of achieving controlled ignition performance, the E.C. review recommends increasing the cost of ITER by 14%. About 2/3 of the cost increase is for improved performance capability and 1/3 for increased engineering margins. At the same time a two-stage or sequenced nuclear testing scenario is recommended. The two-stage approach allows *initial* savings, which would offset the proposed cost increases by means of the following: (1) installing a shield instead of a blanket, (2) installing 70 MW of heating/current drive power instead of 145 MW, (3) installing reduced fuel cycle systems, given the absence of a blanket, the reduced operational requirements, and lower rate of fuel consumption, and (4) a reduction in the plant. These actions will result in costs at a later time. Also, the U.S. home team finds a larger cost for the recommended design changes: about 20–25%. In addition, the total cost would include the time and expense of stopping for 2–4 years to install a breeding blanket before a high-fluence testing phase could begin. Thus, the total cost of this scenario is seen to be larger than for the integrated nuclear-testing scenario.

Failure to achieve full performance (fusion output power, availability, etc.) can be characterized as a "soft" failure of investment to the extent that reduced performance is achieved that is still useful. In contrast, a "hard" failure of investment would follow from the class of events that cause the project to be terminated. For example, the time to replace a toroidal coil is estimated to be about 4 years. This may be an unacceptable delay and cost leading to the termination of ITER. Failure of safety systems leading to a large release of tritium is another event that might lead to program termination. The E.C. sequenced nuclear testing scenario emphasizes a "roll forward" approach with maximum reliance on what is available now in physics and technology. By concentrating resources on a design using available technology to the greatest possible extent, the risk of "hard" failure as a result of hardware problems is minimized, and this is an important advantage of the E.C. scenario.

Additional Costs and Risk of Single-Machine Scenarios. A fluence goal of 1–3 MW-yr/m^2 has been established for blanket and materials development. Fluence at this level is consistent with the view that ITER is an Engineering Test Reactor in preparation for a DEMO. For the available flux in ITER of 1 MW/m^2, which is difficult to increase much because of beta and magnetic field limitations, meeting the influence goal implies ITER must operation between 10% and 30% of the time averaged over a 10-year period. This represents an extremely demanding requirement for availability.

Maximizing integrated plasma burn-time has not yet become an objective in the operation of large tokamaks, and how the program should go about achieving this objective deserves careful thought. Present-day large tokamaks can operate reliably for extended run-periods of repetitive short pulses. With the same repetition rate using long-pulses, ITER provides a much higher duty-factor than that of today's copper coil tokamaks, and therefore ITER has the intrinsic capability to achieve substantial levels of availability and integrated plasma burn time. However, realizing this capability depends on hardware reliability in a very large first-of-a-kind system that must operate with high heat fluxes and an intense 14-MeV neutron flux.

There is considerable uncertainty in the prospect that ITER will reach the availability objectives because of plasma and subsystem reliability issues. This will translate either into higher cost to improve the reliability or increased risk of failure to meet the goals.

Regarding cost, an intensive effort in component testing and quality assurance would appear to be needed for meeting the objective of high availability. In addition, ITER operations need a large contingency of time and expense for the retrofitting of equipment as experience accumulates. These costs are not clearly included in the CDA cost estimates, no doubt because they are intrinsically difficult to quantify.

Regarding risk, this Panel has serious concerns about whether the high-fluence nuclear testing goal of 1–3 MW-yr/m^2 would be met with budget resources likely to be available. The March 1991 U.S. national ITER review and the E.C. review had similar concerns. To quote the E.C. review:

> When planning endurance tests in ITER the uncertainties and limitations in availability as well as the operation cost/benefit should be the main considerations in deciding what testing can reasonably be accomplished. An endurance test mission of ITER would be a very ambitious goal, and the final decision to implement it can only be taken on the basis of experience gained in a previous phase concentrating on performance tests. As such, an endurance test mission should be considered an option to be examined in detail during the EDA, but not as an essential component of the ITER testing programme at the outset.

Costs and Advantages of the Parallel-Path Scenario. Without question, any ITER design capable of meeting the ignited-plasma objectives, and thus operating at about 1 gigawatt, will represent a facility of enormous value for advancing the technology of fusion. What is at issue is the desirability and feasibility of relying primarily on the large ITER-class device for the high-fluence nuclear testing needed for blanket development, materials testing, and other plasma and nuclear technology development.

The cost of a two-machine scenario is difficult to estimate because designs for the second machine have not been adequately studied. Design studies in past years, recent consideration in the fusion community of a "pilot plant" design, and ongoing examination of possible next-generation experiments in the U.S. make it reasonable to expect that this issue will be resolved. The cost estimate in this report of $2 billion for the second machine is a factor of two larger than estimates prepared by advocates of a two-machine scenario around the community. Also, the estimate is comparable to what this Panel believes could be saved in operating costs on the ITER-class device by transferring much of the nuclear testing mission over to the second machine.

The ITER-class long-pulse ignition machine could be built initially as in the E.C. two-stage scenario with less current drive, reduced fluence requirements, and no driver blanket. The up-front savings of about $0.9 billion could be used for the nuclear technology machine instead of increased confinement margin, while still preserving the ultimate capability of the ITER-class machine for eventual integrated testing.

The technology testing machine would not operate in an ignited mode, so the size and cost of the machine would be reduced significantly compared with ITER. Assuming the machine were a tokamak, the major radius might be $R = 2.5$ m, which corresponds to a plasma volume of about 7% of that in the large machine. Among the ramifications of small size are the safety advantages that follow from having an order of magntitude lower radioactivity inventory. The small machine would operate as a low-Q steady-state or very-long-pulse driven device, with fusion power of perhaps 50 MW and flux of about 1.0 MW/m^2. Both copper and superconducting options are possible, although our Panel discussion has tended to favor the copper approach because of lower cost and high access to the core of the machine.

The total cost of the various ITER scenarios is tabulated in Table III. The possible up-front savings is not a factor because the money is presumed to be spent at a later time. Also not included is the lower cost of R&D and operations expected for the parallel-path scenario in the achievement of high-availability. Apart from this parallel-path advantage, the conclusion of this comparison is that the scenarios do not differ enough in cost to distinguish them given the uncertainties in the projections.

Table III. Total Capital and Operating Costs of ITER Scenarios

Scenario		Capital $B	Operating $B/yr	Yrs	Integrated cost $B
Unified	ITER	6	0.4	23	15.2
Sequenced	ITER	6	0.4	27	16.8
Parallel-path	ITER	6^-	0.4	12	10.8
	VNS	2	0.2	10	4.0

The main advantages of the parallel-path scenario are the reduced technical risk for achieving the nuclear testing mission needed for a DEMO and the earlier time at which such data would be available. This scenario is seen by advocates as placing a more equal emphasis on the importance of fusion technology and plasma physics than do the other scenarios. It avoids the risk that fusion technology, delayed until later phases of ITER, may never actually be done. The smaller machine provides an independent path for technology development and a less expensive means for learning and correcting mistakes. The cost for capital equipment is initially larger, although the rate of spending during construction could be adjusted for the two devices to prevent any increase in the annual budgets compared with the single-machine scenarios.

Finally, the parallel machine scenario could significantly reduce the overall global fusion programmatic costs to and through DEMO simply because the fusion development enterprise would be shorter by 10 or more years. At a global fusion cost of, say, $2 B/yr (2015), this savings could amount to $20–30 billion.

Possible Tradeoff Between Performance and Cost. The cost of an ITER-class device is largely determined by the goal of studying long-pulse ignition physics. Therefore, we have investigated how much money might be saved by taking increased risk with respect to achieving the physics objectives. The results are relevant to any of the scenarios. The U.S. ITER home team systems code was used to examine a set of super-conducting machines with various sizes, which largely determines the cost and performance. The size was varied from 4 to 8 meters while making no changes in the ITER CDA "physics" (impurity and helium ash concentration, aspect ratio, enhancement factor on energy confinement scaling, stability in terms of small q, Troyon limit, density limit, etc.). The pulse length was held fixed at 1000 seconds for each machine, which provides equivalent capability for studying the long-pulse issues. The smaller machines generate less wall loading and are thus less capable of the nuclear testing mission (the smallest machine at $R = 4$m generates 0.2 MW/m^2.

Figure 3 shows a plot of performance vs. cost. Ignition performance is taken as the ratio of fusion heating by alpha paticles to the total heating needed to sustain the discharge. This ratio, called C and used as a figure of merit in the E.C. review of ITER, has the advantage compared with the "Q value" of being well behaved in the regime of interest instead of becoming infinite. Algebraically, the ratio is $C = Q/(Q+5)$. Sometimes called

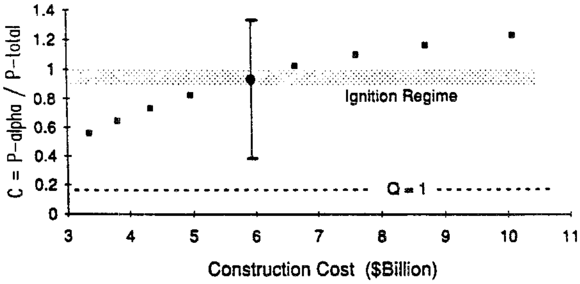

Fig. 3. Performance vs. cost for super-conducting ITER designs.

"ignition margin, " the ratio is simply proportional to the product *n-Tau-T*. Cost in Fig. 3 is based on the $6 billion estimate for CDA design, using the simplying assumption that the manpower and R&D costs scale with the system-code estimate of hardware cost.

The error bars were estimated using the same error analysis for performance that was used for BPX. The main contribution to the error-bar in the figure is, as in the case of BPX, the multiplier of L-mode confinement. In the case of ITER, an additional contribution arises from uncertainty in the helium concentration.

The first conclusion from Fig. 3 is that the CDA design point is indeed a reasonable choice. The projected ITER C value is about 0.95, and the expected value for C is between 0.9 and 1.0 in a reactor. The value of C must exceed about 0.5 in order to have the physics of heating dominated by alpha particles. Figure 3 also shows that a finite range of choices is available, and if a "design-to-cost" approach were adopted, one might choose to save perhaps $1 or $2 billion by accepting increased risk with respect to physics performance. A case for doing so might be strengthened by noting that the performance indicated on the graphs has assumed 10% helium concentration (CDA "rules") because of ash accumulaton in the plasma. For the first 10–20 seconds, the ignition performance will be considerably better before the helium ash accumulates, which allows study of short-pulse full ignition physics. If helium ash buildup were to quency the discharge, the ITER program could be directed towards development of improved ash removal techniques.

Schedule. The Panel understands and supports the desire expressed in the FEAC charge to accelerate the EDA schedule if at all possible. The U.S. ITER home team presented their views of the schedule constraints, and the subject was discussed with P. Rebut and M. Yoshikawa during their interactions with the Panel. The schedule has two important constraints: the magnet R&D needed before the ITER design is finished, and the process of selecting a site for construction. By starting immediately on the site selection work and placing high priority on the magnet R&D in the EDA, it appears possible to begin construction as early as 1997, which unfortunately only recaptures the approximately 1-year delay since the CDA ended.

4.1. ITER Cost, Risk, and Schedule Findings

Given the ITER terms of reference requirement of "demonstrating controlled ignition and extended burn of deuterium–tritium plasmas," the Panel has been unable to identify a design or scenario that offers the potential for savings of more than 15% in the initial capital cost relative to the CDA design. The reason is that the size of a superconducting ignition device is set largely by tokamak physics and magnet shielding requirements, independent of fluence goals.

The increase in capital cost associated with providing greater machine capability for a unified program of nuclear testing, as for example in the high-aspect-ratio variant, would be about 9% relative to the CDA. The increased R&D and operating costs associated with providing higher reliability/availability are not included in this estimate.

In view of this Panel, significant non-capital costs specifically for assuring the high-availability, high-fluence nuclear testing phase of ITER operation have not been adequately included in the CDA cost estimates. These costs, which are difficult to quantify, would be incurred because of the increased R&D needed to ensure a very high level of component reliability, and will arise also from the increased operating costs associated with a lengthy program of technology testing in the ITER combined plasma and nuclear radiation environment. These additional costs would be reduced for the parallel machine scenario, offsetting the increased capital costs for this case, because much of the exploratory testing could be done on the smaller machine where operation would be less expensive.

5. BASE PROGRAM SUPPORT

5.1. Introduction

The ITER EDA is supported primarily by the Development and Technology (D&T) Program within the U.S. Office of Fusion Energy. Confinement tasks are conducted within a framework of "voluntary R&D" within the U.S. Base Program (Divisions of Confinement and Applied Plasma Physics), while the ITER technology development tasks are a part of the EDA. Issues associated with these two areas will be discussed in the following two sections.

5.2. Confinement

Current ITER physics design guidelines are based on an assessment of the physics database by the ITER physics group using international experts to provide input. In many areas, additional data could be provided by confinement experiments. The physics team has identified these needs and the four ITER parties have responded with voluntary programs to provide the needed data. These activities are not funded by the ITER EDA organization. There is no "ITER-credit" for ITER-related physics R&D activities. In some cases, such as the divertor, the ITER design could be improved and risks induced if the information could be provided on a more timely basis.

5.3. Development and Technology

Background. Historically, D&T has had three major roles. The first is as a developer and supplier of the advanced technology needed to confine, heat and fuel, and exhaust heat and particles from confinement devices. This technology is critical to the Physics Program. A common perception is that the fusion program is paced by our physics understanding of basic plasma properties. However, the fundamental theories often exist years before they can be verified in experiments. This delay in implementation is often the results of the vital technology not being available when needed. Conversely, new technology applied to fusion devices is more often responsible for improved plasma performance than is an increased understanding of fundamental plasma physics.

The second role is to develop those long-range, reactor-related technologies, such as materials, reactor blankets, safety, and tritium handling, which are critical to the overall attractiveness of fusion power. Some areas, such as tritium processing, are beginning to be utilized in present experiments, while others, such as low activation alloys and hot breeding blankets, are long lead items and/or will only be needed at the demonstration reactor phase of fusion development. While the time scales may be long, the engineering environmental, and economic characteristics of fusion depend as much or more on these technologies as on the development of improved confinement systems.

The last role is future planning through systems studies. This activity helps define the potential of fusion energy, as well as pointing out its weaknesses. These studies allow comparison with other potential contributors to the long-term energy future, as well as giving an important perspective on those areas of fusion physics and technology which have the greatest leverage in the development of an attractive fusion power system. This activity has, at times, also supported preconceptual design activities for next-step fusion facilities.

D&T Funding. The funding profile for D&T from FY1984 through FY1993 (projected) is shown in Table IV. Budgets for the remainder of the EDA are projected to be similar to FY1993. The roughly $20 M/year not committed to ITER must meet domestic program needs, fund present commitments to international collaborations outside of ITER, and support the facilities and base programs (discussed below) that are assumed as existing resources for the ITER estimates.

The ITER projections have uncertainties. The amount designated for development is based on a 1:3 split between design and development. If more effort is commited to design in an effort to accelerate the project, then less funds will be available to support ITER technology development. Additional demands on funds not considered in the ITER EDA cost estimate include increased support of the U.S. site and high costs for sending staff to the German and Japanese sites.

Many of the ITER tasks prepare industry to effectively compete for fabrication tasks during ITER construction. If effort in these areas is cut back because of reduced ITER development funding as described above or because the U.S. is not selected by the ITER central team to participate, it would be in the U.S. interest to support some level of effort in order to maintain a competitive position and to prepare for the DEMO. In either case, there would be additional needs that are not in the present plan.

5.4. ITER Development Funding by Area

The FY1992 breakdown of the D&T budget by area for both the base program and ITER is shown in Table V. FY1992 is a transition year from U.S. to Central Team control of management tasks. At the present time, the FY1992 ITER distribution is a proposal based on the CDA R&D plan and is subject to negotiation with the ITER Central Team and approval by the ITER Council. U.S. funding for ITER development in FY1988–1991 (shown in Table IV) was smaller , $8–9 M compared to $26 M, and largely emphasized tasks already underway within the base program. The FY1993 and later year

Table IV. D & T Budget (Opex + Equip) $M as Spent

FY	ITER design/site	ITER tech.	Base	Total	Plasma tech.[a]	Fusion tech.	Systems studies
84			85	85	41	31	13
85			73	73	38	21	14
86			62	62	30	20	12
87			52	52	25	16	11
88	8	8	42	58	30	17	13
89	8	8	42	58	30	17	3
90	8	8	33	49	24	14	3
91	8	9	33	50	23	16	3
92	16	26	20	62	26	18	2
93	18	40	23	81	37	24	2

[a] Plasma tech. includes plasma materials interaction (PMI) all years.

Table V. D & T Technology Funding for FY 1992 $M

	ITER technology	Base technology
Magnets	5.9	1.4
Beams	3.2	0.4
ECH	1.7	1.9
ICH	0.0	2.4
Asssembly/maintenance/ containment	0.4	0.0
Plasma facing component	6.5	0.8
Pellets	0.5	1.2
TSTA	0.5	1.6
Blankets	4.0	0.4
Materials	2.5	5.4
Environment/safety/economics	0.7	1.5
Diagnostics	0.4	0.0
Systems studies	0.0	2.1
Total	26.3	19.1

funding by area will depend on how the ITER R&D plan is modified for the EDA and which U.S. proposals are accepted by the Central Team.

5.5. U.S. ITER Task Selection

The criteria for U.S. ITER task selection include (Summary by C. C. Baker, ISCUS, October 1991):

1. The tasks should prepare U.S. Industry to compete effectively in future fusion construction work.
2. The tasks should involve critical technology that has a major impact on ITER as well as U.S. development of fusion energy.
3. The tasks should involve all of the technology areas.
4. The tasks should be primarily in areas where the U.S. already has a demonstrated ability.

The four highest priorities using these criteria were magnetics, plasma facing components, blankets, and heating and current drive. The proposed budgets in Table V reflect these priorities, taking into account the size of the task as estimated during the CDA. The Panel did not review either the criteria or the proposed tasks except at the most general level. The Panel was generally supportive of both the criteria and the resultant priorities.

5.6. Adequacy of ITER Development Funding

The U.S. home team, with support of the broader fusion community, has reviewed the cost estimates that were generated by the central team during the CDA (Baker *et al*, June 1991). Both the CDA and U.S. estimates assumed that the ITER tasks are increments to existing international D&T programs. The U.S. estimate (in 1991) was higher, $973 M vs. $690 M from the CDA, with the major increases being in the areas of containment structure (vacuum vessel), plasma facing components, and blankets.

Impact of ITER Strategy Selection on Post-ITER U.S. Fusion Development Capability. The ability of the U.S. to contribute to post-ITER fusion development depends on the overall technical progress of the international fusion effort (not just ITER) and on the extent to which the U.S. has the scientific and industrial resources to build on this progress. These resources are measured by the existence of a critical number of experienced scientists and engineers and the ready availability of needed technology.

The three scenarios evaluated by this Panel can all reach ITER objectives, although on different schedules and with different levels of risk. Assuming all approaches would be successful, the overall technical progress of fusion would be roughly equivalent for any choice. The U.S. competitive position depends more upon the size of the base program than which scenario is followed.

Since implementation of any of the strategies requires substantially the same technology and engineering, U.S. capability is far more effective by the nature of its participation than the choice of the strategy. The particular technology development tasks assigned to the U.S., the extent and type of fabrication and construction tasks awarded to U.S. industry, and the amount and scope of technology development (including industrial involvement) outside of ITER are critical factors.

Impact of Strategy Choice on Balance Between ITER and Base Technology. The level of funding for the base D&T program, the schedule for the base D&T program, and the overlap between ITER development tasks and those planned by the U.S. independent of ITER are characteristics that impact the balance between ITER and the base program. As discussed earlier, currently planned funding of the base program, while analyzed in most detail for the unified scenario of physics and nuclear testing (which corresponds most closely to the CDA plan), is inadequate for the other two scenarios as well.

Over the term of the ITER program, the needed development for any of the scenarios is substantially the same. However, as discussed in Section 4, the schedule for substantial nuclear testing is significantly different for each scenario. It is likely that the pace of nuclear technology development, correctly or incorrectly, would be matched to the ITER schedule. Overall costs would be increased for the stretched scenarios, but reduced in the near term. Thus, the more slowly paced scenarios may allow a "more balanced program," but only with the expense of stretched schedules.

The task overlap between the U.S. base and ITER technology depends on both the needed technology and the particular tasks in which the U.S. participates. While likely to be significant, the impact of overlap is difficult to evaluate because technology needs have not been defined for all strategies and U.S. participation has significant uncertainty. As a result, a meaningful assessment in this dimension was not possible.

In all cases, D&T base program funding is inadequate and, consistent with the present goals and budgets for fusion development in the U.S., should increase by about \$20 M. These incremental funds should be distributed (roughly) along the following lines:

1. Plasma technology (heating, current drive, and fueling—\$5 M. This would allow adequate support of present experiments and the development of improved next-generation components that would be used to better realize the objectives of present and future confinement facilities and support the operation of future domestic D–T facilities.
2. Plasma facing components and blankets—\$7 M. Improved divertor concepts and materials would be developed and the necessary R&D for hot breeding blanket development would be performed.
3. Materials—\$5 M. Significant development of reduced activation materials would be started and planning (as well as some initial design) would be carried out for a 14-MeV neutron source.
4. System studies and safety—\$3 M. Fusion power plant designs would be updated with substantial industrial support. Additional evaluations and studies to understand the environmental characteristics of fusion would also be performed.

This breakdown is generally appropriate but will have to be reassessed as the needs of the Confinement program are better defined and as the ITER R&D task list and U.S. task assignments are established.

5.7. Base Program Support Findings

The Panel finds the non-ITER D&T base program to be inadequate for fusion development on the schedule of the DOE National Energy Strategy. The D&T budget was \$52 M in FY1987, is \$62 M in FY1992, and is projected to be \$81 M in FY1993. ITER commitments, however, have reduced the portion devoted to non-ITER R&D in the U.S. Fusion Program from \$52 M in FY1987 to \$20 M in FY1992 and 1993. This \$20 M not committed to ITER must meet domestic program needs, fund present commitments to international collaborations outside of ITER, and support the facilities and base programs that are assumed as existing resources for the ITER estimates.

The Panel finds the *balance* of D&T tasks proposed by the U.S. home team generally appropriate.

The Panel finds the ITER development funding is inadequate because U.S.-fusion-program estimates for the total ITER R&D package are 40% higher than previously estimated by the international CDA team. In addition, both the U.S. and ITER CDA estimates assumed that ITER would benefit from the existing international D&T effort continuing at about the late 1980's level, e.g., about $50 M/yr within the U.S. Also, many of the costs for developing the high-reliability components needed for nuclear testing are not well understood.

6. INDUSTRIAL PARTICIPATION

In recognition of the fact that industry will build the ITER device, Panel 1 was asked to recommend a proper role and level of U.S. industry involvement during the Engineering Design Activities. A very significant role will be necessary if U.S. industry is to compete internationally for fabrication and construction contracts. In addition, strong participation during the EDA, as well as in construction and operation phases of ITER, will be needed to put U.S. industry into a favorable position for subsequent activities leading to the commercialization of fusion and will bring important benefits to that process. Attention to U.S. industry's place in fusion development is particularly important in times of both increasing international scientific collaboration and increasing economic competition.

Throughout the 1970's and the early 1980's, U.S. industry involvement in fusion R&D was significant and valuable: industry participated extensively in the design and fabrication of the large confinement experiments constructed during this period. Since that time, however, industry's role has diminished significantly because of declining budgets and the need to maintain core scientific capabilities at the laboratories. In order to prepare U.S. industry to compete successfully for ITER fabrication and construction contracts, as well as to maintain the domestic contstituency needed to support an R&D effort of the required magnitude, a new approach is necessary.

An important start has been made by the U.S. ITER Home Team, which together with the Department of Energy has developed an industrial participation plan for the Engineering Design Activities. In this plan, opportunities are provided for individuals from U.S. industries to be assigned to the Joint Central Team and to be Task Area Leaders on the U.S. Home Team. Work packages pertaining to U.S. Home Team design tasks, as well as to the technology R&D tasks assigned to the U.S. by the Central Team, are to be awarded competitively to U.S. industries. These tasks include the development, design, and fabrication of prototypes or "scalable models" of critical technologies required for the successful construction of the ITER facility; the design and construction (or modification) of test facilities; and prototype testing in these facilities. In all these areas, U.S. industry is expected to participate extensively, either in a prime role for a given task or as part of teams formed with other industries, laboratories, and universities. The plan is structured to encourage early formation of industry-laboratory teams, with emphasis on technology transfer to the industry partner. The policy goal is to provide to U.S. industries the experience needed to bid successfully on the construction of the ITER and its components.

It is unlikely, however, that the plan described above will be sufficient to achieve that goal. The U.S. will not be assigned tasks in all areas of technology that are important for ITER; R&D tasks affecting some key components and subsystems will be the responsibility of the other partners. Therefore, U.S. industry participation in the areas assigned by the Central team to the U.S. will not be sufficiently broad for successful competition in the construction phase. Industrial programs in addition to ITER are needed to develop and maintain a strong competitive position for U.S. industry during the EDA period and beyond.

Ample opportunities for such additional industrial programs exist in the portion of the U.S. program that is not part of ITER, since the non-ITER U.S. program is currently budgeted at approximately six times the current annual U.S. contribution to ITER.

A proper concern is, then, the role of industry in the fusion program as a whole, of which the activities specifically performed for ITER are only one portion. This broader issue has been the subject of numerous studies and reviews, most recently by the Fusion Policy Advisory Committee (FPac) in 1990, whose recommendations were incorporated into the Department of Energy's National Energy Strategy (1991). The FPAC recommendations pointed out that attaining the ultimate objective of the program, the commercialization of a new source of electrical energy, "would be expedited by substantial involvement of U.S. industry, not only in the hardware phases of the program, but also in the planning, R&D, and analytical phases." The recommendation proposed specific "steps to bring industry into the planning and R&D activities already under way," which include teaming laboratory, industry, and university resources, establishing a formal industrial participation program, and encouraging personnel exchanges.

The benefits derived from an industrial participation program are broad. The R&D process gains from the proven ability of industry in the manufacturing sector to develop, design, and manufacture equipment with high operational reliability in an economical manner. However, in order to fill this role, industry must be involved from a project's initial planning stages, through R&D and preliminary design, into final design, manufacture, and device operation. These activities extend clearly beyond the usual function as a supplier of materials, equipment, and services. Participation in the operating phases of devices is critical in order to obtain feedback on the performance of components and systems and to incorporate future improvements. In addition, there must be a steady funding base and level of activity, which can be provided by a core industrial program that augments specific projects.

A strong candidate for a continuing core activity is the area of reactor designs for devices parallel to and beyond ITER, including fusion engineering reactors, possible demonstration reactors, and commercial power plants. Benefits would include an increased industrial awareness of the issues concerning fusion and the provision of a useful mechanism for the flow of ideas and concepts from industry into the fusion program.

An industrial participation program will allow the U.S. to expand its industrial fusion infrastructure and to develop a broad constituency for fusion power. To prepare for the eventual demonstration and commercialization of fusion, industries who will ultimately design, build, and service fusion reactors, must participate in ITER and in other program elements in a significant way. Their first-hand experience with factors such as capital costs, licensability, unit availabilities, plant safety, and financial liabilities, as well as the projected cost of power production, will be important in determining the acceptability of fusion power plants to utilities.

Industry will best fill its role in ITER and in the domestic fusion program through teaming among industries, universities, and laboratories in all portions of the fusion program. The advantage of teaming lies in the synergistic strengths of the participants. To work effectively, such arrangements must be long term and based on realistic assessments of mutual capabilities and commitment. The national laboratories can build on their competence in applied science. The strength of industry lies in its engineering, design, and fabrication skills, program management, and its thorough understanding of the demands of commerce and the market. The strength of universities lies in their focus on basic research and their mission to provide trained individuals to industry. Where there is overlap or similarity in capabilities, emphasis needs to be placed on the differentiating strengths of a given institution and the ultimate objective of strengthening the competitiveness of U.S. industry. Each partner must give up elements represented more strongly by others in return for effectiveness and competitiveness in the total fusion R&D and commercializaton process. To that end, a long-term, broadly-defined teaming relationship best serves the interest of the U.S. and the development of fusion power.

6.1. Industrial Participation Findings

The U.S. industrial participation in ITER deserves and needs the utmost support from the DOE if it is to succeed. The international competition in ITER requires close attention to and skillful handling of procurement issues to assure a leadership role for U.S. industry.

In the view of this Panel, the DOE has been ineffective in implementing a policy that responds to the FPAC recommendations that called for "a substantial involvement of U.S. industry, not only in the hardware phases of the program, but also in the planning, R&D, and analytical phases." A specific plan or process is required to bring about a strong long-term industry involvement in the fusion program. Other DOE programs have been more effective in developing such industrial participation.