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FUSION ENERGY ADVISORY COMMITTEE

Advice and Recommendations to the Department of Energy

In Partial Response to the Charge Letter of September 24, 1991: Part B

March 1992

U.S. Department of Energy
Office of Energy Research
Washington, DC 20585

FUSION ENERGY ADVISORY COMMITTEE
Advice And Recommendations To
The U.S. Department Of Energy.

In Partial Response To The Charge Letter
Of September 24, 1991: Part B

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FEAC/92-I

March 12, 1992

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Preface

This document is a compilation of the written records that relate to the Fusion Energy Advisory Committee's deliberations with regard to the Letter of Charge received from the Director of Energy Research, dated September 24, 1991.

During its second meeting, held in February 1992, FEAC provided a detailed response to that part of the charge that pertained to ITER. In particular, it responded to the paragraph:

> "Then, by January 1992, I would like to have your recommendations on the appropriate scope and mission of ITER and any suggestions you can make to lower its cost or accelerate its schedule. At the same time, I would like your recommendations on the relative importance to the United States of the various ITER technology tasks, on the role and level of U.S. industrial involvement in the ITER engineering design activity, and on the balance between ITER project-specific R&D and the base program."

In order to respond to this charge in a timely manner, FEAC established a working group, designated "Panel I", which reviewed the proposed ITER program in detail and prepared background material, included in this report as Appendix II, to help FEAC in its deliberations.

SEPTEMBER 24, 1991

CHARGE TO FUSION ENERGY ADVISORY COMMITTEE

Introduction

A year ago, the Fusion Policy Advisory Committee (FPAC) reported its findings and recommendations on fusion energy programs of the Department of Energy (DOE). The Secretary of Energy adopted FPAC's recommendations subject to existing budget constraints. This translated to terminating work on alternative confinement concepts and pursuing only the tokamak concept within the magnetic fusion energy program, as a precursor to a Burning Plasma Experiment (BPX) that would be integrated into a larger international fusion energy program. Fusion energy was highlighted in the National Energy Strategy, which mentioned both the International Thermonuclear Experimental Reactor (ITER) and BPX as major elements of the program. The Secretary travelled to Europe earlier this year to conduct personal discussions with the Italian government on their potential interest in a bilateral agreement on BPX.

Since that time, a number of events have led to a reexamination of the strategy being used to pursue an energy-oriented fusion program. The estimated cost of BPX has increased and foreign interest in substantial participation has not materialized. Last week, the Secretary of Energy Advisory Board Task Force on Energy Research Priorities was asked to review the relative priority of the BPX proposal among the programs of the Office of Energy Research and to recommend on the appropriate tasking to the Fusion Energy Advisory Committee (FEAC). The Task Force recommended that the DOE not proceed with BPX, but rather focus on ITER as the key next step after the Tokamak Fusion Test Reactor (TFTR) and the Joint European Torus in developing the physics of burning plasmas, along the lines currently being proposed by the European Community. The Task Force also recommended that the U.S. fusion energy program continue to grow modestly (even in an ER budget that is declining in constant dollars) and suggested that a more diverse program that included a less costly follow-on device to TFTR in the U.S. would be more effective in the long run.

Charge

I would like to explore seriously the programmatic implications of this recommendation under two budget scenarios -- a constant dollar budget for magnetic fusion through FY 1996 and a budget at 5 percent real growth per year through FY 1996. I am therefore charging the FEAC to advise me on the following questions.

1. Identify how available funds now used for BPX, as well as a modest increase (described above) could be used to strengthen the existing base program for magnetic fusion research.

2. Within the above envelope of funding, identify what follow-on experimental devices for the U.S. fusion program might be planned for use after the completion of experiments at TFTR and before the planned start of ITER operation. For such devices, indicate how they would fit into the international fusion program.

3. What should be the U.S. position on the appropriate scope, timing, and mission of ITER if BPX does not go forward?

Although you will need some months to complete the work envisioned in this charge, I would like to have your initial thoughts on the above three topics in a letter report from your meeting of September 24-25, 1991.

Then, by January 1992, I would like to have your recommendations on the appropriate scope and mission of ITER and any suggestions you can make to lower its cost or accelerate its schedule. At the same time, I would like your recommendations on the relative importance to the U.S. of the various ITER technology tasks, on the role and level of U.S. industrial involvement in the ITER engineering design activity, and on the balance between ITER project-specific R&D and the base program.

By March 1992, I would like your views on how to fill the gap in the U.S. magnetic fusion program between the completion of TFTR work and the planned start of ITER operation. In addressing this issue, please include consideration of international collaboration, both here and abroad.

By May 1992, I would like to have your recommendations on a U.S. concept improvement program, including relative priorities and taking into account ongoing and planned work abroad.

William Happer
Director
Office of Energy Research

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February 14, 1992

Dr. William Happer, Director
Office of Energy Research (ER-1)
U.S. Department of Energy
Washington D.C. 20585

Dear Will:

In your charge letter to FEAC in September, you asked for recommendations on the appropriate scope and mission of ITER and any suggestions FEAC can make to lower its cost or accelerate its schedule. At the same time, you asked for FEAC recommendations on the relative importance to the United States of the various ITER technology tasks, on the role and level of U.S. industrial involvement in the ITER engineering design activity, and on the balance between ITER project-specific R&D and the base program.

For these ITER-related questions, FEAC established a panel Co-chaired by Drs. Rulon Linford and Harold Weitzner to provide us with information to help us formulate our advice to you. FEAC received and discussed the Panel report and used it in formulating our recommendations. The Panel did extensive work in a short time and we greatly appreciate their effort.

To begin, you requested recommendations on the appropriate scope and mission of ITER if the Burning Plasma Experiment (BPX) does not go forward. FEAC views ITER and its Engineering Design Activity (EDA) phase as a central element of the U.S. magnetic fusion program. Further, we strongly reaffirm the importance of integrated nuclear testing as a key part of the ITER mission. The cancellation of BPX has, however, compromised the pace and scope of the U.S. program. It will also require an adjustment in the pace of the experimental program of ITER as put forward in the Conceptual Design Activity (CDA) phase just completed.

The absence of BPX increases the technical risk of meeting the goals for fusion energy as stated in the National Energy Strategy (NES). The NES included both BPX and ITER. The necessity of using ITER for the first detailed investigations of high-Q and ignited burning plasmas will extend the phase of ITER dedicated mainly to such physics issues. This first phase is now estimated to take as much as 10 years in which case it would not be completed until about 2015. If an additional 10-12 years of ITER operation is required to obtain the required nuclear testing data, the U.S. program goal of a fusion demonstration reactor (hereafter, DEMO) operating by 2025 will not be achievable.

Additional complementary activities dedicated to acquiring part of the nuclear testing data would permit shortening the ITER test program. FEAC recommends that a study of the feasibility of such a complementary program be undertaken with a view toward making the 2025 DEMO goal more realistic.

You asked for any suggestions we might have to lower the cost of ITER or to accelerate its schedule. As to the timetable, there are both technical and non-technical issues that have long lead times. These preclude a significant shortening of the EDA schedule. Nonetheless, FEAC finds that the timely construction and operation of ITER is critical to the U.S. fusion plan to operate a demonstration reactor. ITER will also serve to demonstrate, in concrete terms to the public, the progress that the fusion program is making toward a practical fusion reactor. FEAC recommends that the U.S. begin the necessary preparations leading to the earliest possible site selection and comitment to the construction of ITER. We believe the U.S. should urge the other parties also to speed the process.

Related to this point, FEAC finds that there will be great benefits both to the fusion effort and to the industry of the country that is selected as the construction location for the ITER project. On the other hand, the host country is likely to incur additional costs. At this time, FEAC recommends that the U.S. move promptly to begin preparation of a proposal to compete in the ITER site selection process. The proposal should take into account the site requirements as defined initially in the CDA phase of ITER, and the revisions to these requirements that may occur during the early phase of the EDA.

The question of cost must be balanced with that of risk. Within the criteria for ITER design adopted during the CDA, the physics requirements of long-pulse ignition set the magnet coil characteristics, and this in turn determines the cost to at least the 80-85% level. The remaining expenditure provides for the nuclear testing mission recommended earlier in this letter and this relatively small increment greatly enhances the cost-effectiveness of ITER. Within this guiding policy, there may be advantages to be realized in staging or phasing the facility capability of the ITER. There could be savings made by accepting greater risk or by assuming more optimistic physics performance than was adopted during the CDA. However, weighing this possibility against the importance that ITER perform to expectations, and recognizing that the European Community CDA review called for somewhat more conservatism in the design, FEAC concurs with the conclusion of our Panel 1 that the level of cost vs. risk in ITER is now about right.

You asked for recommendations on the relative importance to the U.S. of the various ITER technology tasks. The technology tasks identified by the ITER CDA team have been assessed by both the Office of Fusion Energy in DOE and the U.S. ITER Home Team. This assessment was for the purpose of assuring that there will be U.S. strength in areas essential to future fusion construction work.

FEAC finds that the criteria used in this ranking are appropriate to achieve the desired balance among development and technology tasks. The actual tasks themselves may be modified during the forthcoming EDA.

You asked for FEAC recommendations on the role and level of U.S. industrial involvement in the ITER engineering design activity (EDA). The role of industry in the U.S. fusion program should be strengthened in order to prepare industry for the major ITER-construction tasks. The international competition in ITER will require the U.S. to develop a clear strategy for U.S. industry involvement. Such a strategy should take into account the different relationships between government and industry of the different ITER parties. As well, DOE procurement practices should be examined to assure a leadership role for U.S. industry.

To provide U.S. industry with the knowledge of fusion requirements and to secure the maximum benefit from industrial involvement, the DOE should develop a plan that deliberately includes a broader and more integral industrial participation in the fusion program. This plan should encourage the development in industry of both technical and programmatic expertise and should allow for the continuity of this expertise over the long term.

Finally, you asked FEAC for recommendations on the balance between ITER project-specific R&D and the base program. Here, we have interpreted your phrase "the base program" to mean the base Development and Technology program of magnetic fusion. FEAC finds that the R&D activities to be pursued during the EDA will address the physics and technology needs of ITER. Most of these activities will also be important for a fusion demonstration reactor. However, we find that in addition to tasks directly supporting ITER, the U.S. must supplement ITER project-specific R&D with a strong program that addresses other important fusion development and DEMO needs.

The U.S. participation in ITER has up to now been funded primarily out of Development and Technology programs within OFE. FEAC finds that this has severely affected the U.S. base technology program. This program is necessary to ensure the success of our own U.S. fusion program. FEAC recommends that the Development and Technology base program be enhanced beginning with this coming fiscal year.

The fusion materials development program must be enhanced in order to develop the materials needed for DEMO construction and to allow time for testing of these materials in ITER. These materials include those to be used for plasma-facing components, for breeding tritium, and for the basic structure of a fusion machine. FEAC recommends that priority be given to the development of low activation materials for these purposes. In particular, FEAC recommends that DOE initiate a process that will lead to construction of a 14 MeV neutron source to test and qualify such materials. The testing of fusion materials in fission reactors is also an important part of the development program and should be maintained.

Beyond this, the issue of balance between ITER project-specific R&D and the base fusion program is broader than the Development and Technology program alone. There are other important aspects of the magnetic fusion effort which are key to ensuring a strong U.S. program. FEAC is addressing these as part of developing our response to the additional questions in your charge letter. We will report to you again in March and May, per your request.

Sincerely,

Bob Conn

Robert W. Conn
Chairman,
For the Fusion Energy
Advisory Committee

RWC:bw

Appendix I

A letter from the Chairman of FEAC to Panel #1 clarifying the tasks to be undertaken by the panel, dated October 8, 1991.

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October 8, 1991

TO: Dr. Rulon Linford
Dr. Harold Weitzner

FROM: Robert Conn

SUBJECT: Charge To Panel #1, ITER

Thank you for being willing to serve as Chairman and Co-Chairman of the FEAC Panel #1. As members of FEAC, you are aware of the charge given by Dr. Happer on September 24, 1991. Part of that charge requires FEAC to respond to several questions about ITER by January, 1992. Your panel is being charged in this letter to provide FEAC with a report on this topic at the next meeting of FEAC, which is being planned for late January, 1992. The remainder of this letter is devoted to background information, along with specific questions and guidance that I would like your Panel to consider in preparing its report to FEAC.

The questions about ITER in the charge to FEAC can be lumped into two broad questions:

1. What scope and mission should be recommended for ITER, and to what extent could the cost and schedule be reduced from the present estimates?

2. What should be recommended regarding the US involvement in ITER in the following areas:
   a. Prioritization of ITER technology task assignments to be sought by the US.
   b. Role and level of US industry involvement in the EDA.
   c. Balance between ITER specific R&D and the base technology programs.

I would like the Panel to consider the following background and additional questions in your deliberations.

With regard to question 1, the scope and mission for ITER were fairly well defined in the Terms of Reference and by the CDA process. Since ITER has been negotiated at high levels in the governments of the four parties, raising the possibility of modifying the scope and mission of ITER is a delicate issue. However, during the FEAC meeting, Admiral Watkins and Dr. Happer made it clear that budget requirements have made a number of changes necessary. These changes include: 1) at best, only modestly increasing budget projections for the fusion energy program for at least the next five years, instead of the increasing budgets recommended by FPAC; 2) their recommendation that we seek a lower-cost ITER mission that could be implemented more quickly to help fill the gap left by the loss of BPX. Admiral Watkins noted that in his discussions with senior officials in the other parties, he found a similar desire to reduce budget pressures, perhaps by seeking lower-cost approaches for ITER.

In light of this background, I am asking that you work with the ITER Home Team, ISCUS, and DOE/OFE to develop and fill out a matrix of information. The two axes of the matrix should be Mission/Scope and Implications. Four or five cases should be identified for the Mission/Scope of ITER, ranging from a long-pulse burning plasma experiment (no breeding blanket, current drive, etc. and possibly normal coils) to the present scope of CDA design for ITER. The list of Implications should also be carefully developed but should include the implications on the technology R&D needs for the EDA, cost, schedule, the need for other facilities, and the data gap between ITER and a full DEMO. In developing this matrix, only cases that are technically sound should be included. The information in the matrix should provide non-trivial options for FEAC to consider. Based on the matrix, the Panel should provide in their report their ranking of the cases in the form of a suggested recommendation for FEAC's consideration.

As a matter of procedure, all pages in the Panel's report that contain suggested recommendations should be stamped "draft" to further inhibit improper use of the recommendations.

It is clear that the response to question 1 will have a strong influence on the response to question 2. For example, if the highest priority case for question 1 did not require breeding blankets, that would clearly affect the technology prioritization being considered under question 2. This may also affect industrial involvement and the balance with the base program. Moreover, the impact on industry and the base program are valid factors in determining the response to question 1. Because of this coupling, I recommend that the Panel extend the list of Implications in the matrix to include those affecting question 2.

I would also like to request that the following issues be considered in the Panel's deliberation of question 2. DOE has expressed interest in having industry more involved in the fusion program, but the modest budget projections and the elimination of the BPX have made substantial involvement more difficult. Involving industry under these circumstances will add to the pressure on the base technology programs, particularly in those technologies for which the US is not selected to contribute to ITER. It is also clear that industry's interest in the future of the fusion program will be affected by the type and level of their involvement in ITER. Please keep these factors in mind while responding to the following questions:

- What are the specific technology R&D tasks for the EDA?
- What are the criteria by which FEAC should evaluate the relative importance for the US to be involved in the various technology R&D activities?
- What models for industrial involvement in the EDA should be considered?
- What are the pros and cons for these models?
- What are the present funding levels of the existing base technology programs?
- What is the anticipated funding level in each area if the US were selected by ITER?

- How adequate is the sum of the base funding and the anticipated ITER funding to provide the expected deliverables to the ITER EDA?

- What is the Panel's assessment of the impact of the selection of each case of the matrix on the ability of the US to contribute to the development of fusion power beyond ITER?

Taking the above factors and issues into account, the Panel should respond to the three parts of question 2 by providing in their report suggested recommendations for FEAC's consideration.

Thank you again for accepting this challenging task. I look forward to your report on this important topic.

Sincerely,

RWConn/cc

Robert W. Conn
FEAC Chairman

cc: FEAC Members

Appendix II

The Report to FEAC of Panel #1, dated January 31, 1992.

PANEL #1

REPORT TO FEAC

ON

"...THE APPROPRIATE SCOPE AND MISSION OF ITER..."

January 31, 1992

Chairman R. K. Linford
Co-Chairman H. Weitzner
M. A. Abdou
D. E. Baldwin
K. H. Berkner
L. A. Berry
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N. F. Ness
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R. R. Parker
P. H. Rutherford
H. W. Shaffer
R. E. Siemon
D. Steiner

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.

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 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 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 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, ≤ 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 (≥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 ten to fifteen 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 commercial 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 the 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 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 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.

I. 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. A copy of Dr. Happer's charge letter is in Appendix A. 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 ~1995. In creating Panel 1, the FEAC Chairman, Dr. Robert Conn, elaborated the original charge in a letter dated October 8, 1991, which is also in Appendix A.

During the 1992-7 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 Text 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 as 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 technical 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. I.1.

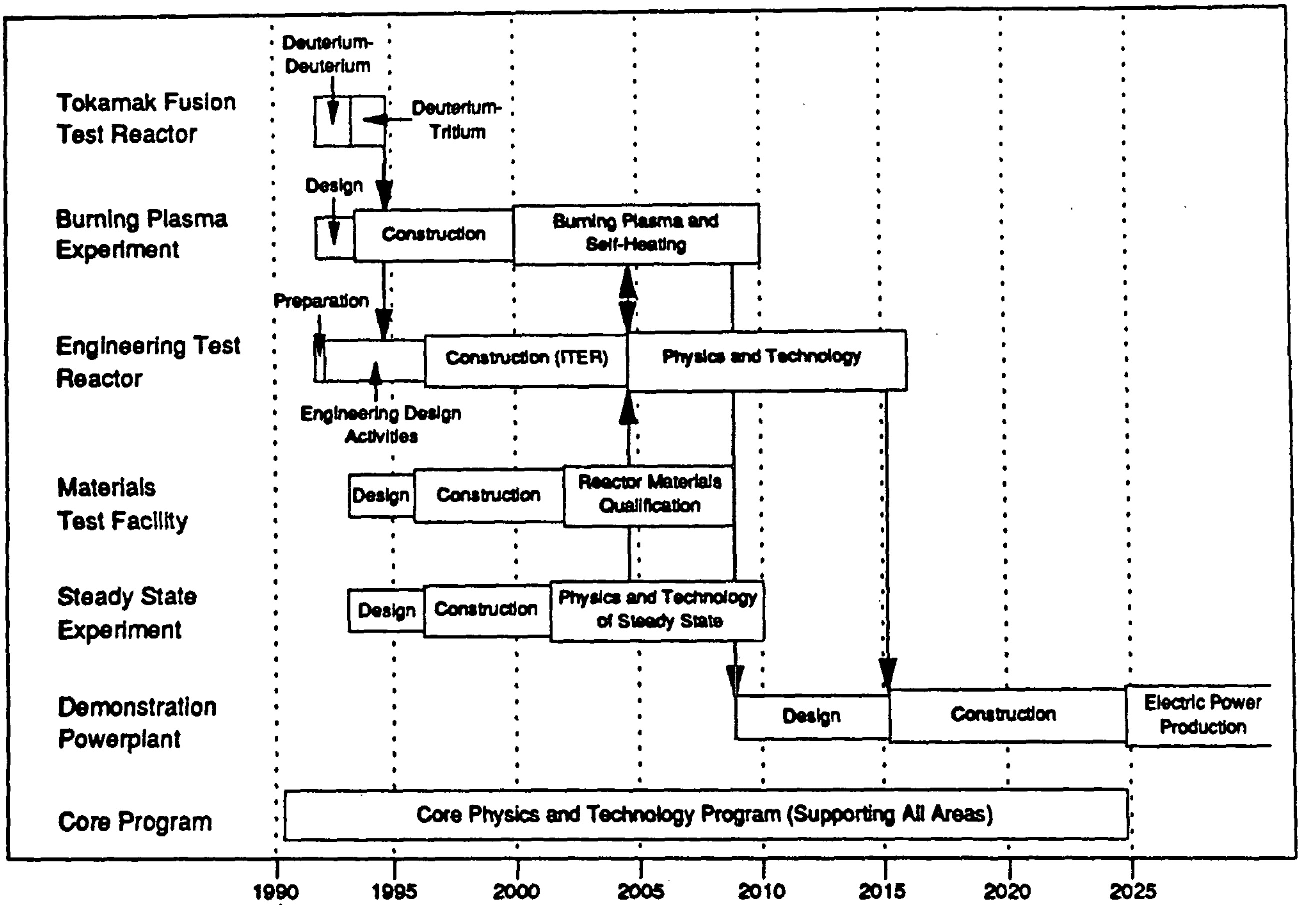

Fig. I.1. FPAC plan for MFE development from the present to the DEMO.

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 Committee-US (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.

II. ITER Development Options

A. Introduction

This section describes three acceptable ITER development scenarios. A fourth option, 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 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 II.1 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 necessary 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.

TABLE II.1
Summary of Scenarios

Scenario Physics & Tech.	Fluence $MW\text{-}yr/m^2$	Approximate Power Level (MW)	Tritium Consumption (kg)	Is Driver Blanket Needed?	DEMO Blanket Integrated Test	Current Drive	End of Mission Begin 2005	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^{++} EQ) (0.4)		
ITER Phase 2	1.0-3.0		165	Yes	Sector	Yes				
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)		

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 ~ \$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 integrated 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 demonstration. The Panel feels that the cost and schedule for the third device is unacceptable, and that without doing the third device, the technical risk transferred 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.

B. 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 current 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-yr/m^2 with an objective of 3 MW-yr/m^2, and quasi-continuous operating periods (with minimum dwell times) of up to two 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/m^2. The CDA plan is 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 satisfactory current drive schemes. The U.S. HARD design improves on the CDA performance, especially 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 ITER-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 physics 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 design. The ability to operate steady state or very long pulse will also demonstrate a more favorable reliability 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 reliability 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 <u>not</u> 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.

C. 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 ignited 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 exhaust 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 dilatory 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 current 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. The impact of more aggressive assumptions regarding availability of tritium from external civilian sources is discussed in Appendix C.

D. 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, <u>except</u> 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 cost 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 ~1 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 operation 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. II.1.

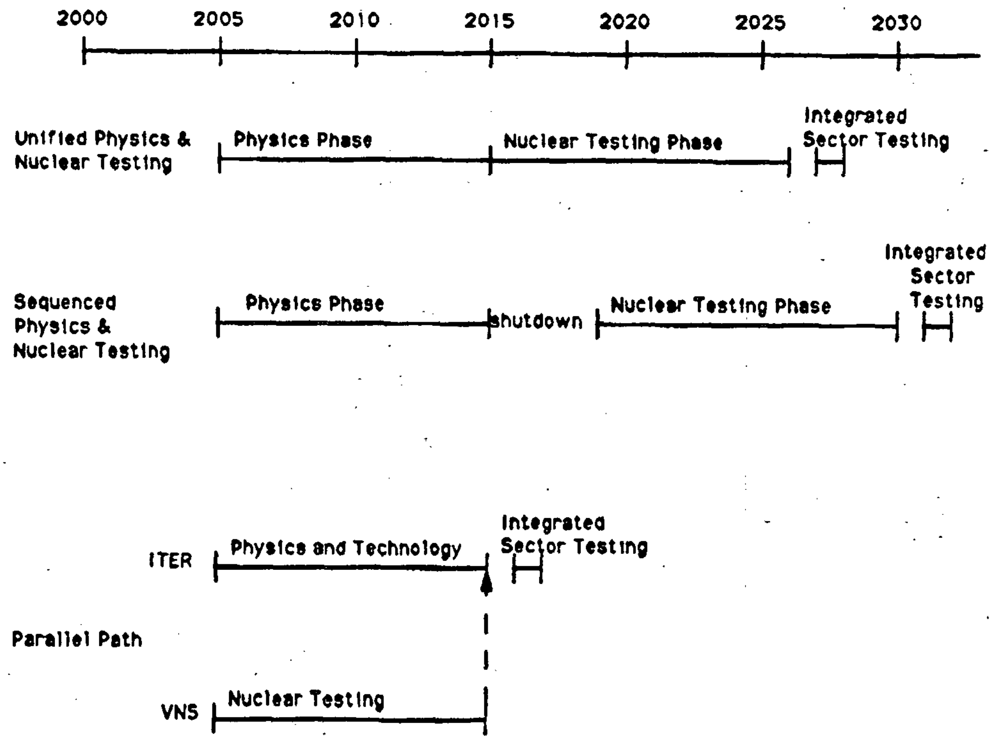

Fig. II.1. Time lines for development scenarios.

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. II.1, 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, in 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 neutron 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 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 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, ≤ 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 (≥ 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 ten to fifteen 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.

III. 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 current 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 operate 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.

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 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 commercial 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.

IV. ITER Cost, Risk, and Schedule

Costs and Advantages for 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 IV.1. In FY 1991 dollars the total cost is approximately $7 billion.

The CDA costs have been established using both system-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.

TABLE IV.1.
The CDA Estimate of Costs From the ITER Conceptual Design Report (ITER Documentation Series No. 18)

Engineering Design Activity	**Cost** $millions $FY89
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**

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 avoid 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% (see Appendix B). In addition, the total cost would include the time and expense of stopping for 2 to 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 four 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 fluence goal implies ITER must operate 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 could 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 magnitude 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 higher access to the core of the machine.

The total cost of the various ITER scenarios is tabulated in Table IV.2. 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 IV.2.
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 ten or more years. At a global fusion cost of, say, $2 B/yr (2015), this savings could amount to $20 to $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 US 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 IV.1 shows a plot of performance vs. cost. Ignition performance is taken as the ratio of fusion heating by alpha particles 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 "ignition margin," the ratio is simply proportional to the product n-Tau-T. Cost in Fig. IV.1 is based on the $6 billion estimate for CDA design, using the simplifying 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.

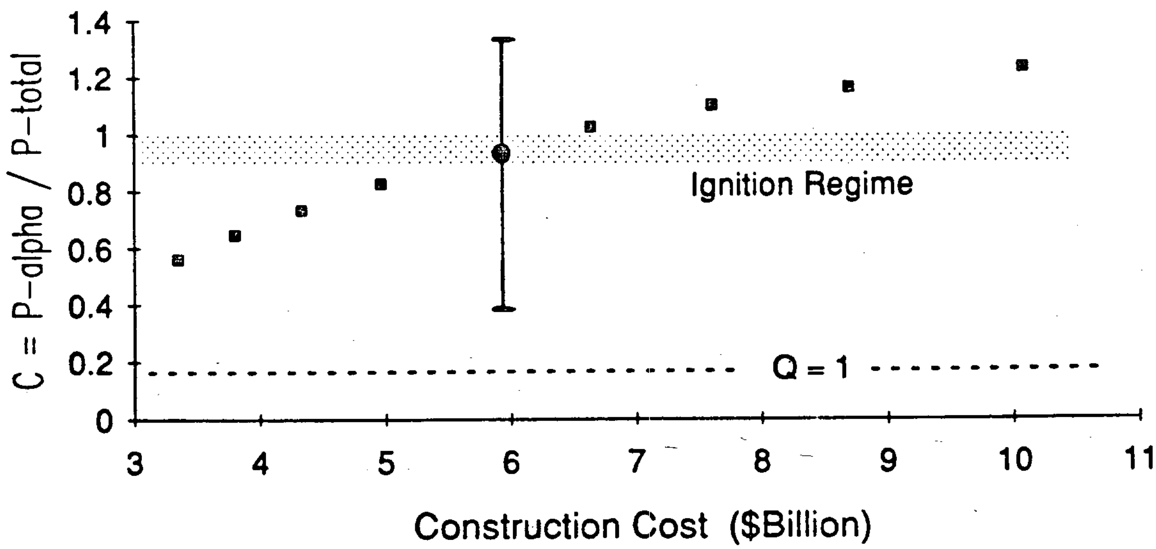

Fig. IV.1. Performance vs. cost for super-conducting ITER designs.

The first conclusion from Fig. IV.1 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 IV.1 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 accumulation in the plasma. For the first 10 to 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 quench 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.

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 the 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 cost for this case, because much of the exploratory testing could be done on the smaller machine where operation would be less expensive.

V. Base Program Support

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.

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 reduced if the information could be provided on a more timely basis.

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 result 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 V.1. 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.

TABLE V.1
D&T Budget (Opex + Equip) $M as Spent

FY	ITER Design/Site	ITER Tech.	Base	Total	Plasma Tech.*	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

*Plasma tech. includes plasma materials interaction (PMI) all years.

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 committed 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.

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.2. FY1992 is a transition year from U. S. to Central Team control of management of 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 V.1.) was smaller, $8-9 M compared to $26 M, and largely emphasized tasks already underway within the base program. The FY1993 and later year 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.

TABLE V.2
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
Assembly/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

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 capability.

The four highest priorities using these criteria were magnetics, plasma facing components, blankets, and heating and current drive. The proposed budgets in Table V.2 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.

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 dollars) 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 affected 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 Chapter IV., 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.

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 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.

VI. 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 the 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 1970s and the early 1980s, 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 constituency 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 commercialization process. To that end, a long-term, broadly-defined teaming relationship best serves the interests of the U.S. and the development of fusion power.

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.

Appendix A

September 1991 Charge to Fusion Energy Advisory Committee

October 1991 Charge to FEAC Panel #1.

CHARGE TO FUSION ENERGY ADVISORY COMMITTEE

Introduction

A year ago, the Fusion Policy Advisory Committee (FPAC) reported its findings and recommendations on fusion energy programs of the Department of Energy (DOE). The Secretary of Energy adopted FPAC's recommendations subject to existing budget constraints. This translated to terminating work on alternative confinement concepts and pursuing only the tokomak concept within the magnetic fusion energy program, as a precursor to a Burning Plasma Experiment (BPX) that would be integrated into a larger international fusion energy program. Fusion energy was highlighted in the National Energy Strategy, which mentioned both the International Thermonuclear Experimental Reactor (ITER) and BPX as major elements of the program. The Secretary travelled to Europe earlier this year to conduct personal discussions with the Italian government on their potential interest in a bilateral agreement on BPX.

Since that time, a number of events have led to a reexamination of the strategy being used to pursue an energy-oriented fusion program. The estimated cost of BPX has increased and foreign interest in substantial participation has not materialized. Last week, the SEAB Task Force on Energy Research Priorities was asked to review the relative priority of the BPX proposal among the programs of the Office of Energy Research and to recommend on the appropriate tasking to the Fusion Energy Advisory Committee. The Task Force recommended that the DOE not proceed with BPX, but rather focus on ITER as the key next step after the Tokomak Fusion Test Reactor (TFTR) and the Joint European Torus in developing the physics of burning plasmas, along the lines currently being proposed by the European Community. The Task Force also recommended that the U.S. fusion energy program continue to grow modestly (even in an ER budget that is declining in constant dollars) and suggested that a more diverse program that included a less costly follow-on device to TFTR in the United States would be more effective in the long run.

Charge

I would like to explore seriously the programmatic implications of this recommendation under two budget scenarios -- a constant dollar budget for magnetic fusion through FY 1996 and a budget at 5 percent real growth per year through FY 1996. I am therefore charging the FEAC to advise me on the following questions.

1. Identify how available funds now used for BPX, as well as a modest increase (described above) could be used to strengthen the existing base program for magnetic fusion research.

2. Within the above envelope of funding, identify what follow-on experimental devices for the U.S. fusion program might be planned for use after the completion of experiments at TFTR and before the planned start of ITER operation. For such devices, indicate how they would fit into the international fusion program.

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October 8, 1991

TO: Dr. Rulon Linford
Dr. Harold Weitzner

FROM: Robert Conn RWConn/cc

SUBJECT: Charge To Panel #1, ITER

Thank you for being willing to serve as Chairman and Co-Chairman of the FEAC Panel #1. As members of FEAC, you are aware of the charge given by Dr. Happer on September 24, 1991. Part of that charge requires FEAC to respond to several questions about ITER by January, 1992. Your panel is being charged in this letter to provide FEAC with a report on this topic at the next meeting of FEAC, which is being planned for late January, 1992. The remainder of this letter is devoted to background information, along with specific questions and guidance that I would like your Panel to consider in preparing its report to FEAC.

The questions about ITER in the charge to FEAC can be lumped into two broad questions:

1. What scope and mission should be recommended for ITER, and to what extent could the cost and schedule be reduced from the present estimates?

2. What should be recommended regarding the US involvement in ITER in the following areas:
   a. Prioritization of ITER technology task assignments to be sought by the US.
   b. Role and level of US industry involvement in the EDA.
   c. Balance between ITER specific R&D and the base technology programs.

I would like the Panel to consider the following background and additional questions in your deliberations.

With regard to question 1, the scope and mission for ITER were fairly well defined in the Terms of Reference and by the CDA process. Since ITER has been negotiated at high levels in the governments of the four parties, raising the possibility of modifying the scope and mission of ITER is a delicate issue. However, during the FEAC meeting, Admiral Watkins and Dr. Happer made it clear that budget requirements have made a number of changes necessary. These changes include: 1) at best, only modestly increasing budget projections for the fusion energy program for at least the next five years, instead of the increasing budgets recommended by FPAC; 2) their recommendation that we seek a lower-cost ITER mission that could be implemented more quickly to help fill the gap left by the loss of BPX. Admiral Watkins noted that in his discussions with senior officials in the other parties, he found a similar desire to reduce budget pressures, perhaps by seeking lower-cost approaches for ITER.

In light of this background, I am asking that you work with the ITER Home Team, ISCUS, and DOE/OFE to develop and fill out a matrix of information. The two axes of the matrix should be Mission/Scope and Implications. Four or five cases should be identified for the Mission/Scope of ITER, ranging from a long-pulse burning plasma experiment (no breeding blanket, current drive, etc. and possibly normal coils) to the present scope of CDA design for ITER. The list of Implications should also be carefully developed but should include the implications on the technology R&D needs for the EDA, cost, schedule, the need for other facilities, and the data gap between ITER and a full DEMO. In developing this matrix, only cases that are technically sound should be included. The information in the matrix should provide non-trivial options for FEAC to consider. Based on the matrix, the Panel should provide in their report their ranking of the cases in the form of a suggested recommendation for FEAC's consideration.

As a matter of procedure, all pages in the Panel's report that contain suggested recommendations should be stamped "draft" to further inhibit improper use of the recommendations.

It is clear that the response to question 1 will have a strong influence on the response to question 2. For example, if the highest priority case for question 1 did not require breeding blankets, that would clearly affect the technology prioritization being considered under question 2. This may also affect industrial involvement and the balance with the base program. Moreover, the impact on industry and the base program are valid factors in determining the response to question 1. Because of this coupling, I recommend that the Panel extend the list of Implications in the matrix to include those affecting question 2.

I would also like to request that the following issues be considered in the Panel's deliberation of question 2. DOE has expressed interest in having industry more involved in the fusion program, but the modest budget projections and the elimination of the BPX have made substantial involvement more difficult. Involving industry under these circumstances will add to the pressure on the base technology programs, particularly in those technologies for which the US is not selected to contribute to ITER. It is also clear that industry's interest in the future of the fusion program will be affected by the type and level of their involvement in ITER. Please keep these factors in mind while responding to the following questions:

- What are the specific technology R&D tasks for the EDA?
- What are the criteria by which FEAC should evaluate the relative importance for the US to be involved in the various technology R&D activities?
- What models for industrial involvement in the EDA should be considered?
- What are the pros and cons for these models?
- What are the present funding levels of the existing base technology programs?
- What is the anticipated funding level in each area if the US were selected by ITER?

- How adequate is the sum of the base funding and the anticipated ITER funding to provide the expected deliverables to the ITER EDA?

- What is the Panel's assessment of the impact of the selection of each case of the matrix on the ability of the US to contribute to the development of fusion power beyond ITER?

Taking the above factors and issues into account, the Panel should respond to the three parts of question 2 by providing in their report suggested recommendations for FEAC's consideration.

Thank you again for accepting this challenging task. I look forward to your report on this important topic.

cc: FEAC Members

Appendix B

Summary Description of the CDA Design Point

Summary Description of Recommended Modifications to the CDA by the E.C. and CDA Team.

CDA Design Point — D. Post

During the Conceptual Design Activities (CDA), from 1988 through 1990, the ITER Team developed a design point with the goal of fulfilling the ITER objectives of demonstrating the scientific and technological feasibility of fusion power by:

1. Demonstrating controlled ignition and extended burn of deuterium-tritium plasmas, with steady-state as an ultimate goal,
2. Demonstrating technologies essential to a reactor in an integrated system, and
3. Performing integrated testing of high heat-flux and nuclear components required to utilize fusion power for practical purposes.

The CDA team analyzed the engineering and physics aspects of that design point in sufficient detail to identify the areas where design solutions exist and where further design and R and D in physics and engineering is needed to make a decision to proceed with construction of an ITER class machine. The final engineering design for construction will be developed during the Engineering Design Activities. The CDA design is described extensively in the various ITER reports and documents published by the IAEA. A summary of the design also appeared in Nuclear Fusion **31** (1991), 1135.

The parameters of the CDA are given in Table 1.

Table 1 ITER Parameters

R	6.0 m	a	2.15 m
$\kappa_{\psi 95\%}$	2	I_p	22 MA
B_0	4.85 T	P_{CD}	145 MW
Superconducting Coils	DN Poloidal Divertor	Breeding Blanket	Full Remote Maintenance

The design parameters of ITER were primarily determined by physics requirements for adequate energy confinement (relying upon long pulse H-mode operation) and adequate MHD stability to minimize disruptions, and by engineering requirements for coil stresses, nuclear shielding, inductive capability, etc. Emphasis was given to addressing safety and environmental issues to the maximum extent possible. The final choice of design parameters was determined by a trade-off between maximizing the engineering and physics margin and minimizing the capital and operating costs.

The major problem areas identified during the CDA were power and particle control and disruption effects. The ability to remove the ~ 200 MW of alpha heating power and ~100 MW of auxiliary heating power without leading to excessive impurity levels in the plasma and to unacceptably short lifetimes for plasma facing components limited the performance of ITER and was the most demanding design problem. The damage and erosion of plasma facing components and stresses in the tokamak structure due to plasma disruptions were also major issues. Both areas are emphasized in the R and D program. Availability in terms of component and system reliability and maintainability (including remote handling) were also identified as areas needing significant advances

The mission calls for two modes of operation: controlled ignition ($\tau_{pulse} \sim 200$ s with inductively driven current) and long pulse technology testing ($\tau_{pulse} \sim 1000$ s to steady-state with some assist from non-inductive current drive). Various operational scenarios were developed for ignition, and long pulse and steady-state testing and operating modes. Representative parameters for these modes are given in Table 2.

The costs for the EDA were estimated by the ITER Management Committee to be: a total of about 1200 professional person-years during 5 years for a total of ~ $ 1.0 B with the engineering design amounting to about $250 M and the Technology R and D amounting to about $ 750 M. The estimated capital costs for construction amounted to a total of about $4.9 B; these cost estimates consist of: Tokamak $ 1.7 B, Tokamak Auxiliaries $ 1.4 B, Buildings and Plant Auxiliaries $ 0.8 B, Costing Contingency $ 0.7 B, and Assembly and Transport $ 0.3 B. Additional costs not included are professional staff during construction of $ 0.8 B and additional Technology R and D of $ 0.3 B. These capital cost estimates do not include purchase and clearing of the site, test blankets and services, plasma diagnostics not needed for control and plasma optimization and taxes and insurance. The operating costs were estimated to be $ 0.27 B/year for machine operations and $ 0.12 B/year for the physics and technology experimental program.The CDA team developed a plan for a five year Engineering Design Phase and a seven year construction phase. Operations were divided into a six year physics and machine commissioning phase followed by a twelve year technology-testing phase.

Table 2 ITER Operational Scenario's

Scenario	A1	B1(optimized)	B6
Description	reference ignition	optimized long pulse	nominal steady state
Plasma current, I (MA)	22	15.6	19
Fusion power, P_{fus} (MW)	1100	1090	750
Avg. neutron wall load (MW/m^2)	1.0	1.01	0.7
Auxiliary heating, P_{aux} (MW)	0	113	115
$Q = P_{fus}/P_{aux}$	ignited	9.76	6.7
Burn time (s)	400	3100	Steady State
Bootstrap current fraction, I_{BS}/I	0.14	0.3	0.3
Non-inductive current, I_{CD}/I	0	0.32	0.7

The data gap to DEMO and the need for additional facilities depends on whether DEMO is steady-state or pulsed. In terms of a pulsed DEMO, long pulse operation appears challenging but probably feasible so that ITER will likely provide the needed data. The ITER CDA design did not resolve the incompatibility of acceptable divertor conditions with a high fraction of non-inductive current drive. Therefore, ITER cannot be relied upon to provide the data needed for a steady-state DEMO, unless this issue is resolved during the ITER EDA. Otherwise, an additional experiment is likely to be needed to establish the credibility of a concept for steady-state operation of a burning tokamak. In all cases, it is expected that a high fluence neutron source (> 10 MWy/m^2) is needed to test materials issues before proceeding to a DEMO.

The CDA team developed a detailed Technology R and D program plan for the Engineering Design Activities. It was planned that each country will do one-quarter of the R and D. The Technology R and D Areas are: Magnets, Plasma Facing Components, Blankets, Current Drive and Heating, Structural Materials, Diagnostics, Fuel cycle, Assembly and maintenance, Safety, and Containment Structures. Although the US has an interest in all of the areas, the major areas of US interest are Magnets, Plasma Facing Components, Blankets, and Current Drive and Heating. It is expected that US industry will play a major role in carrying out the R and D with up to about 40-50% of the funds to be expended in industry depending on the specific tasks and the nature of the tasks assigned to the US. During the EDA, the design will be done by the Joint Central Team with support from the Home Teams. The R and D will be done by the Home Teams. The construction and fabrication will be done during the Construction Phase which is planned to follow the Engineering Design Activities.

The balance between the base technology program and ITER Technology R and D may pose a serious problem for the US. The preliminary approach of DOE has been to re-label much of the base Technology R and D Program as ITER-specific Technology R and D. As ITER becomes a real project and the design team makes definite choices for the various tokamak systems, the ITER R and D program will be focussed to address the issues specific to those systems. Much of the more generic R and D now labeled ITER will either have to be eliminated or put back into the base program.

Potential Modifications of the ITER CDA Design and Mission Suggested by the EC and CDA Team — D. Post, J. Galambos, J. Perkins

During the CDA, the central design team identified a number of desirable design modifications and the portions of the ITER mission which posed the most difficulty given the present state of tokamak physics and engineering. The national reviews of the CDA design by the EC and the US recommended similar modifications to the CDA design and, in the case of the EC, recommended modifications of the ITER mission. All parties agreed that a number of detailed design adjustments would need to be implemented during the beginning of the EDA. The other major conclusions and recommendations were:

1. The CDA design was generally capable of meeting the goal of ignited operation. In addition, the EC review recommended a modest increase in the energy confinement margin (~ 10 % in IA).
2. The CDA design was, at best, only marginally capable of meeting the technology testing mission due to the difficulty of achieving acceptable divertor conditions simultaneously with substantial levels of non-inductive current drive. In addition, the EC review panel concluded that the availability goals were too ambitious, and that a breeding blanket was unnecessary both because adequate neutron fluence could be achieved with externally supplied tritium and because a breeding blanket introduced needless uncertainties and additional costs into the design.
3. Additional adjustments in the engineering design would be desirable to increase the engineering margin. In particular, the EC recommended that the coil stresses be reduced by about 15% and that additional neutron shielding and inductive capability be provided.

The EC review recommended a two phased approach to machine operation with the first phase consisting of about 2500 hours of integral burn in about 10 years with an external tritium supply and reduced current drive power. The goals of the first phase would be to achieve controlled ignition and burn for 1000 s, demonstrate the feasibility of essential technologies (e.g. superconducting magnets and remote maintenance), test reactor components (especially power and particle control systems), and test blankets by the use of test modules. The EC review panel argued that 2500 hours of integral burn is adequate to address all of the crucial blanket performance issues, and that testing blanket materials cannot be done in ITER under any circumstances because that will require fluences in the 10 MWy/m^2 range and can only be done using a dedicated 14 MeV neutron testing facility. In operational terms, the proposed schedule basically amounts to following the CDA schedule (a six year physics and machine commissioning phase followed by a high flux testing phase) until the external supply of tritium is exhausted after 3—4 years of high power and high availability D/T operation. At that point (roughly ten years after first plasma), a second "Extended Performance Phase" would be considered which could potentially include a breeding blanket (preferably high temperature) and possibly advanced features such as a full current drive system and would address the high fluence questions. Sufficient inductive capability for 1000 s of operation without non-inductive current drive should be incorporated in the machine from the start.

The EC review panel stated that the proposed changes of postponing part of the current drive system and all of the breeding blanket, and simplifying the design by such steps as replacing neutral beams with ICRF would off-set the cost increases incurred by increasing the machine size to increase the confinement and engineering margins so that the net cost would be similar to the CDA costs. An important ingredient in their strategy is the emphasis on the use of existing technology and the minimization of the need to conduct extensive Technology R and D programs.

A set of parameters under consideration by the EC ITER team (NET team) are listed in Table 1.

Table 1 EC Proposed EDA ITER Parameters

R	7.0 m	a	2.33 m
$\kappa_{\psi 95\%}$	2	I_p	23 MA
B_0	5.1 T	$P_{auxiliary}$	70 MW

This modified design incorporates the recommended changes to decrease the magnet stresses, to increase the inductive capability and to increase the confinement margin (~ 20 % increase in $n\tau_E T$).

Our analysis indicates that relative increases in cost for each design modification are:

Confinement (~ 20 % increase in $n\tau_E T$):	$\Delta \$ \approx 14\ \%$
Increase in inductive pulse from 400 s to 1000 s:	$\Delta \$ \approx 2\ \%$
Increase in inductive pulse from 1000 s to 2000 s:	$\Delta \$ \approx 6\ \%$
Increase in engineering margin:	$\Delta \$ \approx 4\ \%$

The increases in the inductive pulse length were computed using the CDA rules which were judged to be optimistic, so that a 2000 s pulse with the CDA rules is roughly equivalent to a 1000 s pulse with the recommended EC rules ($l_{i3} \sim 0.75$, $C_{Ejima} \sim 0.5$, instead of 0.65 and 0.4 respectively). Analyses using the US systems code TETRA are roughly consistent with the EC Proposed EDA ITER Parameters. The EC estimate for the cost increase is ~ 15-20 % for their design, while the TETRA analysis indicates it would be ~ 20—25 %. Such a design would have similar ignited and hybrid performance to the CDA, but would still have poor steady-state performance compared to the high aspect ratio designs being considered by the US and other parties.

As is the case for the CDA, the data gap to DEMO depends on whether DEMO is pulsed or steady state. If pulse length of 1000 to 10,000 s is adequate, then the EC ITER without the Extended Performance Phase could potentially provide much of the information needed for DEMO. The major unresolved issues would be the need for current profile control using non-inductive current drive and the need for demonstration of high availability and blanket tests for more than 2500 hours of integral burn. The Extended Performance Phase would be needed for these issues. If DEMO is to be very long pulse (≥ 10,000 s) or steady state, then another ETR experiment will likely be needed. With or without the Extended Performance Phase, a 14 MeV neutron test facility will be needed.

A Technology R and D Program for the EC proposed machine would obviously require no R and D for a "cold" breeding blanket. There would also be little or no R and D needed for current drive systems. The EC emphasis on simplifying the design, and reducing the need for new R and D facilities and shortening the time required to carry out the R and D will likely reduce the scope, time, and cost of the Technology R and D program needed for ITER. The major R and D areas are then likely to be magnets and plasma facing components.

In terms of the role of the US and US industrial involvement, there would obviously be less participation in facilities because there would be fewer facilities. The impact of emphasizing present technology is unclear. While there would be less R and D, development, design, and construction could occur much sooner with a likely result that large scale industrial involvement could begin at an earlier time. In terms, of the effect on the balance between ITER and the base Technology R and D programs, a more focussed, simpler approach will further decrease the portion of the base program that will count for ITER credit.

The trade-offs can be categorized in terms of risk(Table 2). The EC emphasizes a "roll forward" strategy by relying on what is available now in physics and technology. It recommends minimizing the risk by reducing the mission and building more margin in the machine to achieve the reduced mission. If the reduced mission for ITER together with the rest of international fusion program is adequate in terms of what is needed to proceed with a DEMO, this is a sound strategy. On the other hand, if the reduced mission is inadequate, an increase in the risk may be required.

Table 2 Schematic Illustration of ITER Risk

	Minimum Risk	Increased Risk
$P_{auxiliary}$	~ 50 MW (RF, eg. ICRF,..)	100-150 MW of non-inductive CD
Availability	Low (~ .01—0.03)	High (~ 0.1—0.3)
Nuclear Mission	Minimal (≤ 0.25 MWy/m^2)	Aggressive (1—3 MWy/m^2)

Appendix C

Impact of External Tritium Supplies on "Sequenced" ITER Scenarios

Impact of External Tritium Supplies on "Sequenced" ITER Scenarios

This report's analysis of the "sequenced" physics and nuclear testing scenario, in which ITER does not have a breeding blanket in its first phase of operations, has made use of the conclusion developed during the CDA that the neutron fluence achievable during this phase would be limited by external tritium supplies to a maximum of about 0.3 MW-yr/m^2. This conclusion was based on a relatively conservative assessment of civilian tritium supplies and an assumed early start of ITER operations. Specifically, it was projected that about 20 kg of tritium would be available at the start of D-T operations, assumed to be in 2002, and that the subsequent supply rate would be about 3 kg/hr. Attention was also drawn to the degree of uncertainty in projections of future tritium supplies and to the possible development of new civilian applications of tritium, which presently consume only very small fractions of the available supply.

On the other hand, if a more realistic schedule for the start-up of ITER operations is assumed and if the full stockpile and production of tritium from Ontario Hydro's CANDU reactors is assumed to be available for ITER, it could be possible to achieve a neutron fluence approaching 1 MW-yr/m^2 in a somewhat extended first phase of ITER operations, without requiring a breeding blanket.

A recent assessment by Ontario Hydro (OH) as part of Fusion Power Associates' "Pilot Plant" study has projected the future civilian tritium inventory as given in Table C.1.

Table C.1
Projected Civilian Tritium Inventory (kg)

Year	Total in OH CANDUs	Total including non-OH CANDUs
2005	35	50
2011	42	66
2017	52	80

These projections assume, of course, that there is no consumption from fusion applications, nor any significant increase in other non-civilian uses. Through 2005, the projected inventory in Ontario Hydro's CANDU reactors is also entirely in reactors presently in operation; by 2017, a part of the inventory is in projected new reactors not yet in operation. The additional inventory in CANDU reactors not under the control of Ontario Hydro ("non-OH CANDUs") is in various parts of the world, including some in Canadian reactors operated by other utilities. For present purposes, we assume that only the tritium in Ontario Hydro reactors is available to ITER.

To take a particular example, we suppose that the first phase of the sequenced physics and nuclear testing scenario shown in Fig. II-1 (called the "physics phase") is extended by two years, i.e., to 2017, in order to accommodate a program of nuclear and blanket-module testing in the latter part of this phase. We divide this extended first phase into two halves--a six-year "physics phase" (2005-2011), followed by a six-year "nuclear/blanket-module testing phase" (2011-2017). The six-year physics phase is assumed to accumulate an insignificant neutron fluence and to consume only a very small amount of tritium specifically about 3 kg total. The basic machine configuration remains the same as the program progresses from this physics phase into the subsequent testing phase; the testing program is accomplished with blanket modules installed through ports. Although the transition from "physics" to "nuclear/blanket-module testing" is supposed, for simplicity of analysis, to be abrupt, the transition will in practice be gradual, as the emphasis of the program changes, allowing ports to be reassigned from diagnostics to blanket test-modules. In the example chosen for analysis, the physics phase has been limited to six years, as in the CDA, since this provides the most severe case to consider from the viewpoint of providing tritium for the subsequent testing phase. If the physics phase extends beyond six years, in order to provide the detailed studies of high-Q burning plasmas discussed in Sec. III of this report, then the start of the testing phase will be correspondingly delayed. However, because of the increase in accumulated inventory, the external tritium supply will be even greater. Thus, the most aggressive scenario, in which the physics phase is the shortest possible (i.e., six years), is the most demanding case to consider in assessing the adequacy of external tritium supplies for the subsequent testing phase.

Allowing for the exponential decay of tritium with a time constant of 18 years, the projected inventory given in Table I implies an average production rate of 4.3 kg/yr during the period 2011-2017 (OH CANDUs only). Assuming that 3 kg has been burned in the preceding six-year physics phase, there will be available for the nuclear/blanket-module testing phase (2011-2017) the initial inventory of 39 kg plus the 4.3 kg/yr supply. Allowing for a final unburned tritium inventory in ITER of 4 kg, a total of approximately 54 kg will be available for burning in ITER during this six-year nuclear/blanket-module testing phase. Assuming that ITER operates with a total fusion power of 1 GW and an average neutron wall load of 1 MW/m^2 (as in the CDA design), this will allow for a total fluence of about 0.95 MW-yr/m^2 to be accumulated during this six-year testing phase (2011-2017).

Thus, provided the needed reliability and availability can be obtained, the neutron fluence achievable in an extended first phase of ITER operations could approach 1 MW-yr/m^2, without requiring a breeding blanket. Relative to the "sequenced" physics and nuclear testing scenario described in Sec. II.C (See Table II.1) and depicted in Fig. II.1, a part of the nuclear testing program envisioned for the second phase (up to a fluence of about 1 MW-yr/m^2) could be accomplished with the machine in its first-phase configuration, i.e., without a breeding blanket. Whether this would serve to accelerate the overall schedule depends on (i) the operating time needed for the physics phase alone, which may well take as much as the entire ten years shown in Fig. II.1, rather than the six years assumed in the example analyzed here, and (ii) whether the testing program requires a fluence approaching 3 MW-yr/m^2, rather than the 1 MW-yr/m^2 available in the present example, in order to develop a database sufficient for the construction of an integrated full-blanket or blanket-sector test. Relative to the "unified" physics and nuclear testing scenario described in Sec. II.B (see again Table II.1 and Fig. II.1), the scope of the nuclear testing phase would be reduced below that possible if a breeding blanket is installed at the outset, which is envisioned to allow a fluence of up to 3 MW-yr/m^2.

Although the cost of purchasing essentially the entire Ontario Hydro inventory and production of tritium at present quoted prices ($29K per gram) would be substantial, it would seem reasonable to suppose that agreement could be reached to acquire these large quantities at a price much below that charged today for very small quantities.

In regard to the reliability and availability needed for the fairly aggressive example considered here, in which a neutron fluence approaching 1 MW-yr/m^2 is accumulated in a six-year nuclear/blanket-module testing phase (2011-2017), it should be noted that this corresponds to an average availability in the range 15-20% during this period.

Equivalently, about 9,000 integrated burn-hours must be accumulated, corresponding to 1500 burn-hours per year. Although ITER operation can be expected to be fully mature by the beginning of this phase, this availability requirement will impose severe demands on machine and component reliability, as discussed in Sec. IV. In addition, the (assumed inductive) pulse length must be increased to about 2,000 seconds to keep the implied total number of pulses in this phase (approximately 16,000) below the design limit on PF/OH magnet cycles (20,000 in the CDA design).

Appendix III

Minutes of FEAC Meeting of February 5/6, 1992.

MINUTES

Meeting of Fusion Energy Advisory Committee
Sheraton Pleasanton
5115 Hopyard Road
Pleasanton, CA 94588

February 5-6, 1992

Present:
Dr. Robert W. Conn, Chairman, UCLA
Dr. David E. Baldwin, LLNL
Dr. Klaus H. Berkner, LBL
Dr. Ronald C. Davidson, PPPL
Dr. Stephen O. Dean, Fusion Power Associates
Dr. Rulon K. Linford, LANL
Dr. Robert L. McCrory, Jr., University of Rochester
Dr. Norman F. Ness, University of Delaware
Dr. David O. Overskei, General Atomics
Dr. Ronald R. Parker, MIT
Dr. Barrett H. Ripin, NRL
Dr. John Sheffield, ORNL
Dr. Harold Weitzner, NYU

Wednesday, February 5, 1992

Welcome and Opening Remarks

Dr. Conn called the meeting to order and welcomed the members to the meeting. He expressed his thanks to the persons at Lawrence Livermore National Laboratory who had worked hard in organizing the meeting at Pleasanton. He pointed out that a very full agenda had been drawn up for the meeting.

Up-Date from DOE

Dr. J. F. Decker presented to FEAC the President's budget request for fusion for FY93. Overall the requested budget for the Department of Energy had increased from $19.0 to 19.3 billion to yield a 2.1% increase. The budget was broken down into non-defense, defense and environmental management sectors. The defense sector showed a decrease from $8.3 down to 7.5 billion, which represented a decrease of 12.3% in real terms when inflation was taken into account. The environmental management sector was the one growing most rapidly, showing a 24% increase from $4.3 to 5.3 billion, much of it earmarked for supersite clean up. This sector was assumed to grow in the future at 9% per year.

Dr. Decker outlined the financial situation concerning the technology programs in some detail, and provided the following table:

Program	FY92 Estimate	FY93 Request
Increasing Energy Efficiency/ Conservation	$283.2	$350.7
Fossil Energy	$886.3	$825.2
Nuclear Energy	$365.8	$344.7
Fusion Energy	$337.1	$359.7
Renewable Energy	$205.6	$209.8
Electric Energy Systems and Storage	$ 38.0	$ 40.1
TOTALS	$ 2,096.0	$ 2,130.2

Dr. Decker presented budget data for the Office of Energy Research as a whole, where very real growth is being achieved. The actual figures were:

FY91 Actual	FY92 Enacted	FY93 Request
$2,615.5	$3,031.2	$3,370.6

Dr. Weitzner pointed out that he saw no provision for the construction of ITER in the figures that had been projected for the future. Dr. Decker responded that none of the "out-year" figures (beyond 1993) were meaningful.

Dr. Decker continued that the FY93 budget for New Weapons Research will keep employment in that area at the FY91 level. However, in the nuclear physics program, two major facilities will be closed: One at Lawrence Berkeley Laboratory and one at Los Alamos National Laboratory.

Dr. Conn pointed out that the expenditure figures that had been presented indicated that the fusion program was the only one that really had been affected by the recommendations of the Townes SEAB Task Force. He reminded the committee that in September 1991, the Director of Energy Research had asked all the scientific communities to "help their country". Only one community, fusion, had responded and "helped its country". The others had resisted fiercely. Dr. Decker agreed that the up-grade to the main injector at Fermilab had moved ahead despite the recommendation of the Townes task force: The other projects reviewed by that task force were still not sufficiently advanced to warrant budgetary consideration.

Dr. Baldwin pointed out that from the figures that had been projected, it appeared that the defense budget was still growing while the non-defense sector was shrinking. Dr. Decker reiterated that the numbers for the out-years, beyond FY93, should be ignored.

Dr. Anne Davies reviewed the fusion energy budget situation in detail. First, she wanted to let FEAC know that the 1992 Reprogramming Letter had not yet been sent to Congress. Second, she pointed out that one of the largest percentage increases in the budget went to fusion for FY93. She provided copies of the budget for the committee. In response to a question by Dr. Ripin, Dr. Davies indicated that 75-80% of university research could be viewed as supporting tokamak technology as opposed to supporting alternative technologies.

Dr. Davies reviewed the D-T Program. A 1,000 Curie test has been planned in TFTR for this summer. She presented a detailed R&D schedule and agreed that it was "tight". Dr. Baldwin asked what would happen if the program failed to meet this schedule. Dr. Davies replied that there were no specific contingency plans. Rather, scheduling problems would be looked at as they occurred and reviewed in light of what the actual problem was. The decontamination and decommissioning phase is being planned now.

Referring to ITER, Dr. Davies indicated that Dr. Mike Roberts was hoping to get the actual Phase II agreement signed in April. Russia had agreed to pick up the commitment of the former Soviet Union. Meanwhile the site and building in San Diego were progressing well and the Program Director was preparing to move there. The U.S. Home Team had received a strong response to the request for U.S. members of the Central Team. Dr. Tom James was heading up the ITER program in the Office of Fusion Energy at present, where a large effort was currently in progress. Dr. Parker pointed out that the U.S. was not making a financial commitment to staff the joint Central Team until FY93. Dr. Davies responded that if the people were ready to go before then, OFE would attempt to find the funding required for the balance of FY92.

Dr. Davies presented the projected future funding chart for fusion energy. She pointed out that it contained real growth, at the rate of 5% per year through FY97, to be followed by adjustments for cost-of-living increases; however, this was still well below the FPAC "constrained" budget scenario. Dr. Parker pointed out that in reality the budget actually depends upon when work upon the new facility, intended to replace BPX, starts. Since that date is uncertain, he questioned whether in fact there wasn't some flexibility in the budgeting and planning. Dr. Davies agreed that this was so, but cautioned that $500 million was the figure that was more or less set in the minds of Admiral Watkins and Dr. Happer. The projected program should not exceed this. While she was concerned over the length of the gap between the shut down of TFTR and the start up of the new project (TPX), since any long delay would result in the lay-off of personnel and the interruption of training programs, she stressed that the fusion community should not rush to build a $500 million device just for the sake of it. Rather, a device should be proposed that makes a real, much needed contribution to the fusion program. The project must be agreed to by the international partners in ITER.

Dr. Conn pointed out that the removal of BPX from the program had placed a $27 million budget line in jeopardy. He asked for DOE's view on how best to protect that funding and if Dr. Davies would explain the strategy that DOE intended to use. Dr. Davies responded the OFE would try to put some of the $27 million under the TPX project. She emphasized that the early availability of areas of consensus from FEAC would be of great help since she realized that FEAC's full recommendations would not be available in time.

Progress Report from Panel II

Dr. Baldwin provided committee members with a brief written report of Panel II's progress to date. He presented a verbal summary to the committee to initiate discussion. The "charge letter" had asked the panel to review how best to fill the gap between the completion of work on TFTR and the planned start of ITER operation. In particular, the panel had been asked to review four intertwined areas of justifiable need within the present fusion program. These were:

- The need for a new premier U.S. facility to operate during the "gap"

- The need to make more productive use of existing facilities
- The need to up-grade existing facilities and initiate modest-size special purpose ones
- The need to prepare for ITER construction and operation

Dr. Baldwin presented his own chart illustrating fusion energy funding projections. He pointed out that funding for the new project would be most difficult during the first year or two.

Dr. Sheffield, co-chairman of Panel II, explained that while the panel had agreed to concentrate heavily upon steady-state tokamak technology, there had also been agreement not to totally abandon the stellarator. Whereas the panel would not suggest proposing a major stellarator machine as an alternative to TPX, it saw a need to keep abreast of advances in stellarator technology.

Dr. Parker re-emphasized that the ITER budget will not be as shown in the projections, so the out-years are meaningless. He stressed that ITER will have to be treated in some special way, especially when it starts growing rapidly. He also objected to the acronym TPX and what it stands for: It implies that the facility has a physics mission only; devices of a technology nature would appear to be excluded. Dr. Davies responded that TPX will have a long-pulse physics mission and a long-pulse technology mission.

Dr. Baldwin mentioned a suggestion that had been made to the panel for reducing overall program costs. This concerned looking at how the Advanced Photon Source had been funded, organized and managed. The program comprised one "on-line" device only. Potential users developed special add-on equipment "off-line" and brought that equipment to the device when they were ready to conduct their experiments. This arrangement resulted in a reduction in the total overhead of the program since it was only necessary to operate and maintain one machine at one site. He agreed that the situation would be more difficult in the case of a tokamak but concluded that the fusion community did need to develop a new logic for total project cost that would fit in with the logic of a national site for fusion. Dr. Decker responded that there was another significant difference between the APS and a national tokamak. The APS program is funded by many agencies and so lends itself naturally to an "add-on" approach. The DOE would pick up the entire bill for the national tokamak and consequently would review the program as an entity. Dr. Davies added that it was the intention to build a complete machine in this instance. No account would be taken of later add-ons since the nature of these would not be known at the outset. Dr. McCrory commented that the budgetary problems associated with the national machine all occurred in the early years of its proposed development. He suggested that the current matrix of missing technology and physics data would not be completely fleshed out. Choices would be made that would increase the funding available to the new machine through curtailment of existing programs aimed at completing this matrix. As an example, he quoted possibly reducing the D-T phase of TFTR.

Dr. Berkner asked for information on how the panel worked. Dr. Sheffield responded that the panel would review many possible alternative programs. The advocates for each program had been asked to prepare "White Papers", by the end of February, that described in detail the program that they were proposing, that outlined very clearly the mission for the machine, and that explained the need for that mission. Dr. Baldwin added that the discussions that took place on the panel produced background information for transmission to FEAC. He stressed that the panel would not, and could not, make recommendations to the U.S. government, and indicated that he preferred panel reports that did not make recommendations. Rather he considered that what was needed from the panels were fairly concise distillations of the panel proceedings.

Referring back to an earlier statement of Dr. Sheffield's, Dr. Weitzner asked if he had meant long-pulse or steady-state when he had referred to the new machine. Dr. Sheffield responded that he had meant steady-state since the initial pulse length would start at 1000 seconds and, for all intents and purposes, that was steady-state. Dr. Parker asked if the panel was excluding everything but a steady-state machine. Dr. Sheffield responded that they were not.

Dr. Parker indicated that he felt the panel was focusing excessively on the near term and not on the out-years. He urged the panel to look closely at operating budgets, to look carefully at possible up-grades, and in every instance to consider the total cost of the project, from beginning to end.

Review of Charge to FEAC

Dr. Conn indicated that he would take a few minutes to review the charge to FEAC in order to set the stage for the Panel I report that was to follow. The Townes task force had recommended that there be no BPX but that the fusion program should still continue to grow. 5% real growth in the budget had been the subsequent selection.

Specifically, the charge had asked what the U.S. position should be on the appropriate scope, timing and mission of ITER if BPX did not go forward. It also solicited suggestions that might lower the cost of ITER or accelerate its schedule. It requested recommendations on the relative importance to the U.S. of the various ITER technology tasks, on the role and level of U.S. industrial involvement in the ITER engineering design activity, and on the balance between ITER project-specific R&D and the base program.

Panel I Report

Dr. Rulon Linford introduced the report that had been prepared by Panel I and provided FEAC members with written copies of it. He thanked all the participants for their time and help. He made special mention of the ITER Home Team and of its leader, Alex Glass. He explained that in order to cover all aspects of the charge, the panel had worked as five teams, led respectively by Harold Weitzner, Dave Baldwin, Lee Berry, Wil Gauster and Ron Parker. Presentations representing the collective findings would follow.

- ITER Development Options

Dr. Harold Weitzner made the first presentation which was concerned with ITER development options. The issues that the international parties had agreed that ITER should address were seen as being absolutely critical. The panel had accepted them and thus did not attempt to reorient the world program. The development options that had been considered included:

1. The CDA Design, and modifications to it.
2. The subsequent EC design that had in essence been endorsed by the Japanese.
3. A parallel path option which comprised ITER operating in parallel with a smaller machine dedicated to the development of technology.

A "copper" machine was reviewed briefly but the panel saw no real merit in pursuing such a device and it was eliminated from further consideration.

The real difference between the development options concerned the timings of the testing programs. The U.S. consensus was that testing of the technology issues should proceed along with that of the physics issues. This contrasted with the EC view which was that physics testing should be complete before technology testing commenced.

Dr. Conn pointed out that the real difference between the EC and U.S. viewpoints revolved around the amount of ignition margin that was required for ITER since this affected the economics. Japan and U.S. did not differ too much on ignition margin. Referring to the Unified Scenario of the CDA approach, Dr. Conn asked if the panel viewed that approach without BPX as now being more risky, and whether the panel would recommend swinging nearer to the EC approach. Dr. Weitzner replied that the loss of BPX would not materially change the panel's views. He emphasized that the big differences between what the EC and the U.S. were advocating lay firstly in the fact that the EC was not committed to the second (technology) phase of ITER, and secondly in the EC's lack of current drive for ITER. Referring to the parallel approach, Dr. Conn asked if the test phase that was planned for ITER was identical to Phase I of the EC approach. Upon receiving a positive reply, Dr. Conn indicated that the real issue therefore lay in the second (technology) phase.

Dr. Berkner explained that in the EC approach, the cost savings arising from declining to undertake the second phase would be fed back into the initial machine to ensure the success of the first phase, whereas in the U.S. approach, the funds intended for the technology phase would be used for the construction of a small new technology machine.

Dr. Conn then asked if the panel had reviewed the cost implications of using tritium if it had to be purchased. Dr. Weitzner replied that this was most difficult to deal with since the cost of purchasing tritium in quantity was unknown. However, the panel had agreed that the program must limit its demands for tritium. Dr. Overskei explained that the concept of a small machine to be operated in parallel with ITER arose in the context of tritium availability. It was assumed that tritium would either be bred in ITER or purchased from the Canadians.

Dr. Baldwin indicated that their terms of reference had in essence constrained the panel's thinking. Nevertheless, the result was re-affirmation of the importance of the technical mission. The time and cost to complete the mission were the real issues that had to be faced.

Dr. Davidson asked if the panel had discussed putting forward a candidate site for the construction of ITER. Dr. Weitzner replied that it had not. However, the point had been made that a commitment to construction should be made as soon as possible. Dr. Linford pointed out that Dr. Rebut, director designate of the Engineering Design Activity of ITER, was on record as having indicated that the site selection process should be complete within four years. This would mean that the site selection process would have to begin now in order to adhere to this timetable.

Dr. Ripin asked if the ITER machine was the same in all three options. He wished to be sure that the machine had not been downsized in any scenario. Dr. Weitzner replied that the panel had not seen much difference in any of the machines. Dr. Parker indicated that the EC version would appear to be the least expensive , especially at the outset, since it lacked any heating requirements.

Dr. Baldwin commented that the panel had taken into consideration a letter written by Dr. Rosenbluth to Dr. Linford, co-chairman of the panel. The letter outlined new, aggressive physics options for ITER. The panel did not ignore these: Rather, members felt that the EC would not accept changed physics rules for the design of ITER and so did not pursue the matter in great depth. Dr. Weitzner indicated that the CDA rules had been taken into the EDA brief "as is": The panel felt that the rules were reasonable and saw no reason to ignore or change them.

- Reduced Cost/Accelerated Schedule Considerations

Dr. Dick Siemon outlined the body of the report in more detail. He indicated that the panel had used the CDA cost estimate data. Here, the machine itself had been estimated to cost $4.9 billion in 1989 dollars, and the infrastructure another $1.1 billion, giving a total of $6 billion. In 1992 dolars, this total inflated to $7 billion. The annual operating costs had been estimated at $400 million in 1989 dollars.

The panel had reviewed more aggressive engineering scenarios for ITER but had concluded that these were not reasonable. The trade-off of lower performance at smaller size and reduced cost was also considered but the panel had concluded that the proposed size was "about right". A "copper" machine had been investigated as opposed to the proposed super-conducting machine but no real savings had been found: The machine itself might prove to be a little less expensive to construct but the copper magnets would consume a lot more power than super-conducting ones and the higher operating costs would offset the savings in construction. The cost impact of less aggressive nuclear testing had been analysed: Small savings could be expected for ITER; possibly, long-term savings could be expected if the parallel path option were pursued.

The lack of clearly identifiable cost savings in the above analyses led the panel to consider if there was a change in mission that could affect cost in a beneficial manner. In a reactor regime, it is foreseen that materials must possess a useful lifetime in excess of 10 MW.yr/m^2. Hence there is a need, within the ITER timeframe, to develop a DEMO-relevant blanket. There is also a need to develop low-activation materials. These requirements drive the need for high fluence in the test facility. The panel felt that the provision of high fluence in ITER would establish overly aggressive goals for the program. The perceived alternative was to construct a second machine for high fluence testing: This new machine would be run in parallel with ITER. One major benefit of the parallel approach lay in the fact that it eliminated the risk that high fluence testing may never be conducted in ITER. The overall cost of the parallel path program was estimated to be close to that of the CDA "unified" program. The annual rate of "spend" would, however, be higher since the parallel path program would be completed ten years earlier than the unified program.

Dr. Baldwin indicated that the parallel path scenario was an aggressive approach intended to ensure the operation of DEMO in 2025. He pointed out that this start date represented a U.S. goal rather than an EC goal. Dr. Davies interjected that this was no longer so: A start date for DEMO of 2025 had now been adopted by EC as their goal also. Dr. Conn commented that there appeared to be quite a risk of not achieving everything that was needed from ITER. He continued that if the risk was as high as the panel thought it was, why had the unified approach been considered at all? Dr. Siemon responded that setting the necessary goals for ITER was not unreasonble in itself since the machine could be made to respond to the requirements: The real issue was: "How much does the U.S want to rely upon that machine actually being used to achieve the goals?"

Dr. Conn commented that the fundamental driver for the parallel path approach was the desired start up of DEMO in 2025. The other two approaches do not permit a 2025 start. This highlighted an inconsistency between the timetable contained in the National Energy Strategy and where the program actually is in time. He continued that FEAC must resolve whether to develop its recommendations based upon achieving the stated goals of the National Energy Strategy or upon pursuing the most expedient course for the fusion program. He pointed out that if the parallel path approach suggested by the panel was adopted, in reality all that would change was the time at which the money was spent, rather than the amount of money to be spent. The plan still called for spending $2 billion, the difference being that the money would be spent later rather than for BPX. Dr. Parker added that the panel would also have changed the mission: The original machine was intended to support the physics phase of ITER.

Dr. Linford stated that the first two alternatives, viz. the unified and EC approaches, were fraught with danger. If either approach was adopted and failed to complete the technical mission, then what would be done? Would the world fusion community retrofit ITER and delay DEMO? Or would the decision be made to go ahead with DEMO without the required information? The approach offered by two parallel machines would reduce the risks.

Dr. Ripen questioned the real difference between sequenced testing and parallel testing since even the parallel testing approach involved a second "sequenced" phase in ITER. Dr. Siemon responded that this second test phase was to be a less rigorous one, mainly related to blanket technology. Dr. Conn pointed out that this was to be a fully-integrated sector test intended to eliminate the issue of scaling between ITER and DEMO. The fully-integrated sector test was to be the final check in ITER: It had always been in the program.

Dr. McCrory stated that he felt it would be a mistake for FEAC to place too much emphasis upon the National Energy Strategy and its requirements when formulating its recommendations. The conclusion that he had reached while listening to the panel's report and the subsequent discussion was that a more-comprehensive ITER was needed. He supported this by questioning the distribution of funding within the magnetic fusion energy budget: "If the fusion community sees the technical mission as being so important, why is this not reflected in the present expenditures?" Dr. McCrory continued that it will be difficult to maintain enthusiasm for the fusion program unless there was clearly visible progress. He stated that FEAC should take a position on which of the options would be the best for the U.S.

Dr. Overskei cautioned that Dr. Rebut planned to do something different with ITER than the program contained in the EC document. He said that the physics phase stretched for 10 years in all three scenarios presented by the panel, but that Dr. Rebut thought that 6 years would be more reasonable. However, Dr. Rebut was basing his conclusion upon the assumption that the hardware in the EC scenario would work flawlessly from the outset. Dr. Overskei pointed out that the panel had made the assumption that smaller size would result in higher reliability and early availability of the parallel machine. The danger is that this second machine will not be operational from the outset either: Thus the parallel path scenario is also subject to real vulnerability. He warned that FEAC would find it very difficult to "sell" the immediate availability of the small machine, particularly since many of the problems that must be solved for it were that same ones faced by ITER; for example, the divertor. Dr. Parker concurred that Dr. Rebut will address only "his" physics issues in ITER: Dr. Rebut's program does not include much of what others wish to investigate.

Public Comment

Dr. Paul Rutherford, Princeton Plasma Physics Laboratory, presented FEAC members with copies of a paper entitled: "Parallel Machine Scenario: A Dissenting View". He emphasized that he did not wish to detract from the efforts of the members of Panel II but he was concerned over the technical credibility of the smaller machine and its ability to accomplish its nuclear testing mission, and over the impact of undue U.S. reliance on such a machine in forthcoming international discussions concerning the technical objectives of ITER. He pointed out that the second "small" machine was very ambitious since its proposed performance much exceeds that of JET.

Dr. Rutherford continued that ITER could be used for technology testing and that sufficient civilian supplies of tritium would be available during the life of the machine to enable it to accumulate neutron wall loads of up to 1MW.yr/m^2. He postulated that this would be less costly than anticipated since the selling price of tritium, in volume, would be 5-to-10 times less than current prices for small samples. He postulated further that ITER would, in fact, be capable of accumulating such a neutron wall load since it would be operating reliably and continuously in the later stages of the program.

Dr. Sheffield disagreed with Dr. Rutherford's views. He pointed out that the cost of running ITER for the extra ten years needed to complete the technology program, at $400 million per year, meant that the U.S. should look very carefully at alternatives. The objectives of ITER should be set for ITER alone, and not in conjunction with objectives set for any other machine. ITER should be made to do what it can do: While the second machine may be useful, it is not needed for ITER.

Dr. Parker indicated that the real difference between what Dr. Rutherford was proposing and the CDA scenario was that Dr. Rutherford's scenario did not need a driver-blanket. He questioned the proposed length of the physics phase which was only 6 years; in all the other scenarios this phase lasted from 10-to-15 years. Dr. Rutherford interjected that his program required just 2,000 seconds-long pulses, not steady-state continuous operation. Dr. Parker objected that the transition time between the initial configuration and completion of the conversion to the later configuration needed for technology testing could be as long as 10 years, when account was taken of all the potential changes that would be required.

Dr. Conn pointed out that Phase I of ITER operation has a technology mission mixed in with the physics mission. Dr. Rutherford indicated that his proposal used the same machine as the EC scenario with the exception of the 2,000 seconds pulse length. However, he agreed with Dr. Parker that the objectives of the machine had changed.

Dr. Balwin stated that if the flux proposed for the small machine was only half of that proposed for ITER, then the small machine must run for twice as long to accumulate the same wall loading and hence must be that much more reliable. He asked how anyone could guarantee that when the design did not exist? The alternative was to seek ways of obtaining the needed information off-line: It was not necessary for every test to be performed in a tokamak. Dr. Rutherford concurred saying that 3 MW.yr/m^2 can not be obtained from ITER and he very much doubted whether the proposed small machine could achieve it either. He stated that the real questions were whether 3 MW.yr/m^2 was really needed and, if so, how to achieve it.

Dr. McCrory stated that while it would be wrong to rely solely upon ITER to accomplish the U.S. nuclear mission, he saw a very real problem arising with Congress if an attempt was made to push for the construction of two machines simultaneously. Dr. Dean pointed out that the panel did not receive Dr. Rutherford's dissent in time to consider it in depth. However, he felt that if it could be shown that ITER was capable of fulfilling both missions, then it would be impossible to persuade Congress that a second machine was needed. On the other hand, he was concerned that the U.S. could end up placing all its eggs in the ITER basket and stressed that the U.S. should not be totally reliant upon ITER.

Dr. Conn summarized the salient points of Dr. Rutherford's case in comparison with the EC/Japanese case. Dr. Rutherford saw no inconsistency between his view and the EC and Japanese views, which he felt would be modified by the price and availability of tritium.

Continuation of Panel I Report

- Compact Steady-State Tokamaks for Nuclear Testing

Dr. Parker provided committee members with a written presentation that indicated what the small parallel machine might look like, and what its mission might be. He described it as a Steady Burn Experiment (SBX). The goal of the machine was to provide high-performance steady-state plasmas that were suitable for the investigation of all the alpha physics issues at Q = 1. These issues included investigation of divertor heat loads, of current drive and of advanced confinement regimes. The technology phase would include blanket development and thermomechanical testing, the qualification of plasma-facing components, materials development, the development of remote assembly and maintenance in a fusion reactor environment, the investigation of safe D-T operation in an integrated fusion system, and the development of curent drive and divertor technologies for DEMO. He pointed out that the economy of scale is good in a small machine.

Dr. McCrory asked how such a machine would be decommissioned. Dr. Parker replied that one would bury it. Dr. Conn explained that the point Dr. Parker was making was that such a machine would have to be constructed and operated on a national site. Dr. Parker continued that by using the same algorithms that were used to estimate the cost of TPX, the cost of the SBX machine had been calculated at $573 million.

Dr. Conn asked if Dr. Parker saw a machine of the type proposed having two operating phases. Dr. Parker answered affirmatively saying that the first phase would not involve the use of tritium. The second phase, during which tritium would be used, would require that the machine be fitted with remote handling facilities.

Dr. Conn pointed out that if FEAC looked carefully at the three scenarios that the panel had presented to them, not much difference would be found between the desires of the various international parties as far as Phase I - the physics phase - was concerned. It was in Phase II - the technology phase - where major differences occurred. Dr. Parker stated that if the proposed technology machine was built as a back-up to ITER, then the U.S. could readily reconcile its view of the first phase of ITER with those of the other parties. He stressed that the possibility of the existence of his machine should not influence what the U.S. did at the negotiating table. Dr. Conn said that the EC position must be pushed more towards satisfying the technology requirements. Dr. Parker agreed stating that the U.S. needed to ensure that ITER had a higher wall loading than presently planned.

Dr. Weitzner stated that the U.S. position must be self-consistent and that fusion must remain an energy program. The sequential approaches advocated by the international partners would push DEMO so far out into the future that adopting them would put the entire fusion program in jeopardy. He felt that a strong study of current drive was needed; this could not be undertaken in ITER as presently configured. Dr. Parker responded that since the U.S. did not have the investigation of current drive in its program, it would be difficult to force such upon the EC.

Dr. Sheffield said that the reasons for constructing the small machine were easy to define:

1). To demonstrate the divertor
2). To demonstrate optimized plasma
3). And then to add D-T and use the machine for testing.

He continued that, logically, these tasks would be taken in sequence and work would stop immediately if it were sensed that the direction was wrong. In the present circumstances, the program did not have that luxury. Dr. McCrory commented that if this approach was the one taken by the U.S., then a very heavy investment would be made in technology that ultimately may not be needed. Dr. Parker countered that a start needed to be made with other technologies now if the program was going to meet the 2025 time-frame for DEMO. He stressed that it was not just the blanket technology that was needed: In particular, remote handling techniques needed to be developed on working tokamaks, by actually performing the tasks required of them, as opposed to being developed in some laboratory simulation program.

- Base Program Support for ITER

Dr. Lee Berry provided the committee members with a written presentation that he summarized verbally. Referring to the findings and recommendations of the panel, Dr. Conn commented that on the surface it appeared that the panel was advocating doubling the non-ITER D&T base program simply to cause it to recover to its former level. However, he conceded that the arguments that had been made for the increase in the body of the report were most persuasive.

Dr. Baldwin remarked that while the graph that illustrated the comparisons between the U.S. and CDA ITER R&D cost estimates showed agreement in many areas, there were very large discrepancies in two areas and a significant difference in another. He suggested that the areas of difference be reviewed again. Dr. Berry replied that the U.S. ITER Technology Task Group, under the chairmanship of Dr. C. C. Baker, had looked at re-estimates of all the costs. He pointed out, however, that in many of the instances where the U.S. value and the CDA value had shown good agreement, both cost estimates had in fact been made by the same person or group of persons; hence he expected close agreement. Since in the areas of major disagreement the U.S. estimates were invariably higher, Dr. Baldwin suggested that these might reflect the cost of performing the work in the U.S. He postulated that estimates from other nations, and especially Russia, could genuinely be a lot lower.

- Industrial Participation

Dr. Wil Gauster presented the panel's findings concerning industry's role in fusion development: A written version of the presentation was given to the members of FEAC. One of the suggestions by the panel to help remove the obstacles that industry claimed hindered its participation in the fusion program was the appointment of a point-of-contact person who would report directly to the Director of Energy Research. Dr. Baldwin questioned this, indicating that since it was the fusion program that was involved, the person should report to OFE. Dr. Weitzner responded that industry's problems were not at OFE but were occurring elsewhere in DOE where OFE was unable to help. Dr. Conn indicated that the ombudsman approach might present a better way of overcoming any difficulties that were being experienced. He made the point that budget constraints were such that it was difficult to provide enough money to spark industry's interest. He suggested that teaming, where industry adopted a small role initially, represented the only way of involving industry in the fusion program. Dr. Gauster concurred that teaming would make a good starting point, and was something that could be implemented immediately, but added that he would also like to see more meaningful leadership roles being developed for industry.

Dr. Ness stated that in order to get industry involved in the fusion program, it would be necessary to provide money to industry. But since funds were limited, that money could only be taken from existing programs. He continued that when eventually it came to bidding on projects, the process would not be a fair one because of the different relationships between industry and government in the U.S. and in Japan. Dr. Conn responded that the DOE must ensure that U.S. industry is in a position to bid effectively: In essence, the lowest bidder with demonstrated capability would win. U.S. industry must develop that capability.

Dr. Sheffield agreed with Dr. Ness concerning the funding of industry. He added that the problem is not one of just getting money into industry but of continuing to get money into those same industries year after year, and maintaining the budget for industry year after year. This was the only way to develop capability. Dr. Overskei commented that continuity is very important to industry. He pointed out that the national laboratories and universities enjoy continuity: The national laboratories needed continuity in order to establish and maintain facilities, and universities needed it to ensure that students were able to complete multi-year research programs. He continued that ITER will be a driving force for industry and wanted to know whether funding of the ITER construction phase would remain the prerogative of OFE or whether, while remaining in Energy Research, it would be administered differently. If so, it would make sense to have a point-of-contact person in ER.

Dr. Dean commented that OFE does not believe in establishing core roles for industry since it causes too much trouble for DOE. Rather, OFE prefers to provide funding to the national laboratories and to have them interface with industry. A further impediment lay in the fact that OFE would not give industry a fee, because of the extra paperwork involved. Dr. Conn responded that an ombudsman could change the attitude in OFE. Dr. Dean stated that the problem was not confined to OFE but that it existed at the ER level. Dr. Conn concluded that if DOE's dealings with industry were really found wanting, then there would be a need to correct the situation.

- Acceptance and Publication of the Panel's Report

Dr. Weitzner stated that he wished to acknowledge Dr. Dick Siemon's major contribution to the panel's report. Dr. Conn responded that FEAC had received the report of the panel with appreciation but pointed out that that did not mean that FEAC had endorsed it. Rather, FEAC would view the report as providing background material that would help it to evaluate the situation for itself.

A lengthy discussion ensued concerning when and how the document should be published, whether the document should have an existence of its own, and who should be able to circulate it. Considerable emphasis was placed upon the manner in which dissenting viewpoints should be treated and, in particular, if different treatments should be given to those viewpoints depending upon their source of origin; from FEAC members, from panel members who were not members of FEAC, or from the general fusion community. It was realized that if it was agreed to include such views, they would inevitably become part of the record.

It was finally agreed that the report should be included in a package that would comprise:

- FEAC's letter in response to the charge
- The report itself, to which would be added a statement indicating its original intended purpose
- An appendix entitled "Alternative Viewpoints", which would contain contrary opinions arising from all sources.

Public Comment

Dr. Rob Goldston, Princeton Plasma Physics Laboratory, presented a graph of neutron flux versus heat flux, and from it concluded that the proposed SBX machine was unrealistic and unnecessary. He indicated that this second machine was not needed for nuclear testing. Dr. McCrory concurred, stating that the mission proposed for SBX was misguided and that a variety of ways of tackling nuclear testing existed without resort to the construction of another tokamak. Dr. Goldston pointed out that when "port" testing of materials was complete in ITER, and when the blanket was fully in place, the machine coulkd be driven a lot harder that the CDA specification called for.

Mr. Tony Chargin, Lawrence Livermore National Laboratory, expressed concern over potentially poor industrial participation in ITER. He stated that it was not too soon to involve industry in the program but that "bureaucracy" was an impediment to such involvement. He pointed out that the national laboratories were not set up to transfer technology to industry and that the present DOE policies would result in Japanese industry pulling well ahead of their U.S. counterparts.

FEAC Deliberations

Dr. Conn summarized what FEAC must accomplish during the remainder of the meeting. First, however, it needed to look at the recommendations that had been made during the panel presentations and determine which could be adopted readily and which were likely to need thorough discussion. Then, FEAC should look at how important the technology phase really is. The committee should also look at the importance of the 2025 start date for DEMO, and at the balance between ITER R&D and Base Program R&D. Also, it should review the involvement of industry in the fusion program. He emphasized that FEAC needed to get the letter containing its recommendations to Dr. Happer by no later than February 18 in order to provide him time to study it before he testified before the Energy Subcommittee of the House Science, Space, and Technology Committee on February 20.

FEAC also needed to establish Panel III. Dr. Conn suggested that Dr. Dean be appointed chairman of that panel, and that Dr. Ripin be appointed vice chairman. He indicated that Dr. Anne Davies would discuss the charge with the committee in the morning.

Dr. Conn then asked that the committee provide him with an indication of how it viewed each recommendation of the panel. He drew the attention of the committee to the recommendations tabulated in the presentations and raised each one in turn. Of the twenty five recommendations that were reviewed, the committee agreed that it could adopt only three without thorough discussion. Dr. Conn concluded that it would take the entire second day of the meeting to review the panel's findings and that consequently, and with regret, the presentations that were to have been made to FEAC concerning programs of interest at Lawrence Livermore National Laboratory and at Lawrence Berkeley Laboratory would have to be cancelled.

Thursday, February 6, 1992

FEAC Deliberations and Report Preparation

Dr. Conn outlined the revised agenda for the day. He indicated that, overnight, he had prepared viewgraphs that he felt would assist the committee in their review of the panel's recommendations and sentiments. These are presented below together with the discussion that they evoked:

- "The ITER EDA should be a (the) central element in the U.S. Magnetic fusion program."

The committee voted 8 to 4 in favor of "a" as opposed to "the".

- "The activity of the ITER EDA must be supplemented by a strong national program to address DEMO related tasks in addition to tasks directly supporting ITER."
- "The U.S. should urge the parties to move the process forward as quickly as possible to a commitment to site selection for ITER and to construction of ITER."
- "The U.S. should move promptly to prepare a U.S. site proposal which will compete during the ITER site selection process."

Dr. Conn commented that this sentiment needed to have added to it the idea of incorporating the site requirements coming from the EDA.

Dr. Ness stated that he did not see how a site proposal could be prepared before the site requirements were known. Dr. Baldwin responded that a good foundation had already been laid by the CDA. Dr. Berkner commented that the statement assumed that the U.S. wanted to provide the site for ITER. Dr. Parker said that whether or not the U.S. is interested will depend upon the conditions that will be imposed upon the site, especially the financial ones; therefore there might be circumstances that were unacceptable. Dr. Conn stated that the proposal should be ready in case the U.S. did want to host the activity. If subsequently the conditions were not acceptable, then the U.S. need not pursue it. Dr. Davidson stressed that the fusion community would not get the support of Congress for ITER construction if the U.S. did not show a willingness to compete for the site. He reminded the meeting that very large expenditures were involved in the construction phase. While acknowledging that unfavorable terms and conditions might subsequently emerge, Dr. Parker and Dr. Conn saw no reason not to proceed with the proposal now.

- "The FEAC recommends and supports the importance and commitment to the nuclear technology mission of ITER",
  or
  "The FEAC reaffirms the importance of the timely aquisition of nuclear technology testing data."

Dr. Parker pointed out that the nuclear technology mission was not yet defined. Dr. Conn indicated that ultimately it was intended that ITER should be used for the integrated testing that would lead to a successful demonstration reactor. The committee preferred the first of the two statements, amended as below:

> "The FEAC strongly reaffirms the importance and commitment to an integrated nuclear technology mission for ITER."

- "In the absence of BPX the first phase of ITER will spend significant time (up to 10 years) in a physics-dominated phase relating to ignition and long-pulse. A phase emphasizing nuclear technology and qualification tests for DEMO may also take 10-to-12 years."

Dr. McCrory asked if BPX was really such a loss since the Europeans did not think so. Dr. Parker responded that this was a U.S. letter and should reflect the U.S. viewpoint. Dr. Conn added that FEAC should make the point that the U.S. fusion community is upset by the loss of BPX. Dr. Davidson pointed out that the real issue was that the 2025 date for DEMO is in jeopardy because of the loss of BPX. Dr. Dean added that, irrespective of any technology requirements, ITER is now going to have to undertake burning plasma experiments. Dr. Parker pointed out that a recent detailed analysis of the ITER program had projected a physics phase of from 9-to-15 years. He therefore considered that the wording "up to 10 years", that tentatively had been placed in parentheses, was in fact valid and that there was no need for the parentheses.

Dr. Conn explained that this discussion had led up to what FEAC perhaps should say concerning the possibility of fusion energy being almost a practical reality by 2025. Without other activities, for example those projected for the parallel machine that had been suggested by the panel, it was unlikely that the 2025 date could be met. Hence:

- "Without significant complementary activities, the U.S. program goal of beginning operation of a fusion DEMO in 2025 is unlikely to be achieved."

Dr. Sheffield concurred with this statement but added that the real point was that the U.S. fusion budget was too low, by a factor of two, to achieve DEMO in 2025.

Dr. Overskei objected to the wording: "In the absence of BPX . . . " and suggested that it was irrelevant to the ITER team. BPX did not exist and should be forgotten. FEAC had to ensure that ITER did not meet with the same fate. Dr. Parker countered that the charge specifically mentioned the impact on ITER of the loss of BPX. Hence reference must be made to BPX. Dr. McCrory proposed that FEAC's opinion of the penalty that the program was paying for the loss of BPX should stand alone. He continued that, later in the letter, FEAC could indicate that the loss would adversely affect the timing of DEMO, but that the letter should not tie BPX and ITER together. Dr. Weitzner concurred; ITER should not be coupled with the demise of BPX. Dr. Conn stated that the committee must refer to the loss of BPX somewhere in the letter. Dr. Ripin suggested that BPX should be treated as a separate issue: The consequences of its loss, the expanded timescale that would result, and the increase in technical risk should be explained in a paragraph that stood alone.

Dr. Parker pointed out that the committee was dealing with two intertwined issues. From the beginning, DEMO could not have been started up on time if it was relying on getting all the necessary information from ITER. Now, ITER had been delayed by at least another two years, one due to the delay in the start of the EDA, and a second to the increase in the length of the EDA. The loss of BPX had exacerbated the situation. Dr. McCrory suggested that a stronger statement, such as "The cancellation of BPX has compromised . . . ", should be used in the letter. Dr. Linford added that it should be made clear that if the technology phase had to be undertaken in ITER, it would lead to 20-to-25 years of ITER operation. Dr. Parker indicated that the overall time-frame could be even longer than that. He pointed out that eventually a driver blanket would be needed for ITER. Since the physics phase was due to last for 10 years and would not require a blanket, he felt it unlikely that the international partners would agree to include one from the outset. Rather, the blanket would only be added when it was needed for the start of the second phase. However, it would be necessary to break ITER down in order to install the driver blanket and that process would take four years, further adding to the delay of DEMO. He stressed that this was the real price that the fusion program would pay for the cancellation of BPX.

Dr. Conn summarized the discussion concerning BPX, saying that the penalty arising from its demise was an increase in the length of the physics phase of ITER, from 4-to-6 years to 10-to15 years. He suggested that FEAC write a paragraph on the loss of BPX, a second paragraph describing possible activities complementary to ITER, and a third paragraph outlining the consequences upon ITER and DEMO of cancelling BPX. With regard to the complementary activities, he suggested:

- "To develop the materials needed for DEMO construction and for testing in ITER, especially low-activation materials, a strongly enhanced materials development program (for structures, breeders and plasma facing components) must begin now. In particular, DOE should take the lead in initiating an international effort leading to the construction of a 14 MeV neutron source for fusion materials development."

Dr. Parker expressed concern over whether the 14 MeV source should be international. He considered that the U.S. should have the source in its national plan. Dr. Conn indicated that suggesting that the source be international now did not preclude establishing it as a national facility at a later date. Dr. Parker responded that if the effort to establish the source internationally failed, then it would be very difficult to gain acceptance for it within the national program. Dr. Weitzner pointed out that while the panel had agreed in principle to the source, it had not dealt with the matter in any depth and so appropriate background material was unavailable. Dr. Berkner suggested that the word "international" should be omitted from the statement.

Dr. McCrory pointed out that a financial conflict was likely to develop between the 14 MeV source and a blanket testing facility if the fusion community were to attempt to go ahead with the construction of both. He asked what the nuclear science community's view was concerning the relative merits of the two facilities. Dr. Conn responded that there were two distinctly different viewpoints. Those who would not go ahead with the construction of a reactor without low-activation materials would opt for the construction of a 14 MeV source. Those who consider that a machine made from steel would be satisfactory would opt to construct a blanket testing facility. But, the real question that should be asked is: "Why develop a blanket that will not do what will lead to safe fusion?" He emphasized that he supported very strongly the need for low-activation materials.

Dr. Sheffield pointed out that FEAC had neither the need nor the mandate to make a choice between testing facilities now. That would emerge from the investigation being undertaken by Panel II. Both choices should be left in the letter. Dr. Overskei commented that the committee's "wish list" kept getting longer. He felt that FEAC should simply identify what ITER will do and what ITER will not do. Dr. McCrory pointed out that the list of things that ITER would not do would be infinitely long. Dr. Overskei agreed but made the point that the committee was instead putting together an infinitely long list of other items that must be investigated. He reminded the committee that ITER had never been viewed as an engineering test bed. While it was true that ITER would be used for certain technology tasks, it was never intended that it should perform them all.

Dr. Anne Davies interjected that the four international parties had already decided that, once the formal agreement to proceed with the EDA had been signed, a meeting would be held to review everything that still needs to be accomplished in order to reach the goal of fusion energy. Funding of the "scoping" activities that would be involved had already been included in the FY93 budget.

Dr. Berkner said that since the panel had not dealt with the issue of the 14 MeV source in any depth, FEAC should vote on the matter. Dr. Conn concurred, indicating that since FEAC was the senior body it should not be limited to matters that had been reviewed by its panels but could include matters not considered by panels. The committee voted 8 to 4 in favor of omitting the word "international" from the statement. The committee then voted 9 to 3 in favor of including the statement in its letter.

Formation of, and FEAC Charge to Panel III

Dr. Conn reminded the committee that the charge to Panel III was contained in the last paragraph of the original letter of charge to the Fusion Energy Advisory Committee:

> "By May 1992, I would like to have your recommendations on a U.S. concept improvement program, including priorities and taking into account ongoing and planned work abroad."

Dr. Conn indicated that Dr. Dean had agreed to act as Chairman and Dr. Ripin as Vice Chairman of the panel. He suggested Dr. Norman Ness, Dr. Harold Weitzner and Dr. Klaus Berkner as the other FEAC members of the panel. Suggestions for panel members who were not members of FEAC included Dr. Charles F. Kennel (UCLA), Dr. Noah Hershkowitz (University of Wisconsin), Dr. A. Boozer (College of William and Mary), and Dr. Dick Siemon (LANL).

Dr. Anne Davies provided an explanation of the charge. In the light of the budget cuts that had occurred in FY91, OFE had elected to concentrate its program on tokamak improvement. She asked the advisory committee to provide, in particular, recommendations on how best to utilize the equipment and personnel at facilities such as PBX and TPX. Also, OFE would like a broader policy statement on the alternative concepts issue in the fusion program. The present feeling in DOE was that the U.S. should not passively give up the pursuit of alternative concepts. She asked that the committee determine the criteria that DOE should use in making decisions concerning alternative concepts and emphasized that OFE would not be satisfied with a statement that simply indicated that DOE should set aside "$X million" for an alternative concepts program.

Dr. Conn pointed out that FEAC should avoid appointing a panel of project advocates who held very strong views. Rather, the fusion community would be better served if such advocates presented their views to an impartial panel. For this reason, he felt it better that Dr. Boozer and Dr. Hershkowitz play the role of advocate rather than that of panel member.

Dr. Dean asked if it was intended that all aspects of IFE be omitted from review by the panel. Dr. Davies indicated that this was inded the case. Inertial fusion energy should be treated as a separate charge to FEAC at some later date.

Dr. Balwin pointed out that the charge encompassed two levels of review. The first related to the overall determination of policy and the second to the establishment of priorities. Dr. Dean indicated that he was concerned that unless everyone with an alternative concept heard of the panel in a timely manner, litigation could result.

Dr. Baldwin asked Dr. Davies what was meant by the range of alternatives to which she had referred. In particular, could it include the Z-pinch? Dr. Davies responded that it could be included but that specific recommendations concerning particular processes were not being sought. Rather, the issue was one of recommending the criteria by which DOE could evaluate the competing processes.

There was general agreement among the committee members that the charge was confusing and needed redefining. Dr. Davies agreed to redefine it. Dr. Ripin suggested that the charge should include the evaluation of concept improvements and up-grades that stem from technology as well as from physics. Dr. Dean countered that this would broaden the field considerably. Dr. Davies stated that DOE would like to restrict the charge to advice on "confinement" concept improvements and up-grades.

Dr. Sheffield and Dr. Baldwin pointed out that Panel II was already reviewing ATF, PBX-M and DIII-D as competitors in an advanced tokamak improvement scenario. There would therefore appear to be some confusion over the respective charges to the two panels. Dr. Conn suggested that the charge to Panel III should be reformulated by DOE. He indicated that Panel II should continue in its present direction and should not be influenced by the apparent conflict with Panel III. Dr. Davies suggested that Panel III should perhaps concentrate more on policy issues. Further discussion failed to clarify the issue and it was decided to adopt the solution that Dr. Conn had suggested: Panel II should continue with no limitation being placed upon its charge, and Dr. Davies would arrange for DOE to reformulate the charge to Panel III.

FEAC Deliberations and Report Preparation

The committee once more turned to the task of writing their letter of recommendation.

- "An active, on-going program using fission reactors and other techniques should be maintained to aid development of materials for DEMO (and for testing in ITER)."

The committee agreed that this sentiment should be incorporated in the paragraph that discussed low-activation materials.

- "The balance now between the base program, particularly the D&T base program, and the ITER project-specific R&D, is not appropriate. Specifically, we recommend that DOE enhance, over a period of 3-to-4 years (beginning in FY93), U.S. D&T base-program activities in the three key areas of: 1) Plasma facing components and blankets; 2) plasma technologies; 3) materials development. Over a 3-to-4 year period, the effort should be enhanced by about $20 million."

Dr. Overskei said that this statement assumed that the new level of effort was correct and, by inference, that the old level was correct also. He felt it would be more appropriate for FEAC to specify the tasks that should be undertaken rather than recommending a specific dollar amount. Dr. Dean countered that "balance" was a part of the charge and must be addressed. Dr. Conn pointed out that the next few months would be very important to the fusion program; if FEAC did not make recommendations then decisions would be made without the committee's input. He summarized that FEAC, generally, was in favor of indicating that a imbalance existed between the programs, and was also in favor of enhancing the D&T program over a period of time, without specifying either the degree of enhancement or the exact period of time: Nor should the actual activities that would benefit from the enhancement be specified.

Dr. Weitzner reiterated that the ITER process was pulling resources away from the D&T base-program activities and that these needed to be restored. Dr. Conn suggested that including a phrase such as "Because the ITER process is consuming the base D&T R&D funds, and only a fraction of these funds is spent on technology, the balance between . . . " might be an appropriate way of dealing with the situation.

Dr. Overskei pointed out that, even as things stood, many areas of technology were not being addressed for lack of funding anyway. Hence, the D&T program should have more funds available to it irrespective of what was happening with the funding of ITER-related projects. Dr. Baldwin and Dr. Dean expressed support for Dr. Overskei's position. Dr. Dean pointed out that the U.S. would win some of the contracts awarded for ITER R&D and would lose others. He emphasized that the U.S. needed to keep supporting the technology in those areas where ITER contracts were not won, or that technology would be lost and the U.S. would not be in a postion to compete for future work in such areas.

- "The balance of D&T tasks proposed for the U.S. is about right for what was reported in the CDA."

Dr. Weitzner said that the panel did not review this in any depth and he would be happy to omit it. Dr. Berkner countered that the four criteria that had been used by the ITER Home Team were reviewed. The panel had agreed with the criteria for the balance of the program. He felt that the sentiment should be included but that it should be reworded to more accurately reflect what had been reviewed. Dr. Ripin queried what would happen to the U.S. program when ITER construction costs became a reality. He suggested that FEAC raise this issue in its letter.

- "Industry should be brought into the U.S. fusion program in order to prepare it for the major ITER-construction tasks. This can be done by having industry-laboratory-university partnerships formed to build and operate (any new facility in the U.S. program) . . or . . (one or more facilities needed in the U.S. program) . . or . . ?"

Dr. Decker explained that the DOE was currently undertaking many different programs with industry. Industry was sharing the cost of these programs and was providing a large amount of the total funding. However, the programs all promised pay-offs to industry that were much nearer-term than anything envisioned for fusion. Dr. Parker agreed that industrial involvement in the fusion program would be difficult to deal with. Dr. Sheffield drew the committee's attention to a recent report on fusion, prepared for the Library of Congress, which had reviewed the potential for industrial participation in the program. He pointed out that there was a very definite need to develop a strategy to involve industry. Dr. Overskei added that the DOE should develop a policy covering how industry should participate in the entire ITER program. Dr. Linford supported this statement.

Dr. Ness stated that, despite all the money that the U.S. government had poured into "space" industries, there was still no commercial vehicle available for space activities. He indicated that, because of the constant need to improve quarterly results, until it was clearly seen that profits could be made from fusion activities, it would be equally dificult to persuade industry to invest in fusion.

Dr. Conn suggested that the recommendation concerning industrial involvement should perhaps be modified to read:

- "The role of industry in the U.S. fusion program should now be strengthened in order to prepare industry for the major ITER construction tasks. The international competition in ITER will require the U.S. to develop a clear strategy for U.S. industry involvement. As well, there needs to be a skillful handling by DOE of procurement issues to assure a leadership role of U.S. industry."

Dr. Baldwin complained that the statement was too soft. It implied that a problem existed in the way that DOE was handling procurement matters but failed to emphasize it. Dr. Conn added that the strategy that was developed should also take into account the different relationships that existed between government and industry in other countries; Japan and those in Europe.

Dr. Overskei emphasized that the mechanisms for involving U.S. industry are already in place. What was needed was for the DOE to treat the matter of industrial participation seriously rather than casually as had been the manner to date. He continued that there were ways around the difficulties that industry was experiencing at DOE. The current laws permitted the government to do whatever it wished concerning the awarding of contracts. Contracts could even be awarded non-competitively and sole-source if necessary. Examples of the government's flexibility included awards made in the national interest, awards made because of expediency, and awards made to small/minority-owned/women-owned businesses.

The committee was undecided over whether to limit the statement concerning industrial participation to ITER, or to broaden it to include fusion as a whole. The final vote was 6-to-6 with one abstention.

Dr. Conn indicated that it was time for the committee to review the big questions that it faced:

- "The U.S. should examine the scenario of a second machine (in parallel with [or complementary to] ITER) which is focused primarily on nuclear technology testing. The aim is to shorten the time for integrated blanket tests in ITER, to shorten the time to DEMO (to make achievable a 2025 DEMO operating date), and to save total costs through 2020 (start of DEMO construction)."

Dr. Baldwin indicated that this was a soft approach to the subject and that he approved of it. He cautioned, however, that it raised the implication that the U.S. wanted this machine as a competitor to ITER. Dr. Weitzner requested that the committee be asked to indicate whether or not it wished to include this sentiment in its letter. After some general discussion the committee indicated:

1) That the sentiment should be included;
2) That the order of the sentences be reversed;
3) That an introduction describing the scenario within which the panel had made its recommendation be written and added to the sentiment; and
4) That the sentence which now came second should read: "FEAC recommends that the scenario involving a second machine in parallel with ITER, and which is focused primarily on nuclear technology testing, be examined."

Dr. Berkner protested that the committee should not select one option and ignore the other two. Rather the committee should provide the rationale that supported its selection of this option and its rejection of those favored by the CDA and the EC. Dr. Dean once again pointed out the budgetary problems that the program faced. As presently envisaged, the budget would not even cover the United States' share of ITER when construction started. He reiterated that the fusion program needed a large boost in its budget.

Dr. Overskei, Dr. Conn and Dr. Davidson all agreed that while the budget did present a major problem, there was an additional problem associated with the timing of the fusion program that must be addressed also. The committee would have to weigh the risk of hurting ITER, through the diversion of funds to alternative projects, against the risk of having fusion progress so slowly that the ultimate goals were moved so far out into the future that everyone would lose interest in the program. Dr. Weitzner said the FEAC must emphasize that the parallel machine does not compete with nor replace ITER. Rather its purpose was to accelerate the program and shorten the time to DEMO.

Dr. Decker was asked for his opinion on the parallel machine. He replied that he was hesitant to comment. He stated that the fusion program was passing through a particularly difficult phase and that DOE was currently looking carefully at a replacement for BPX. He indicated that FEAC should ask itself whether the construction of the proposed parallel machine was the most important thing that needed to be undertaken in the program. He cautioned that the letter must be worded carefully so that it did not damage U.S. credibility nor weaken ITER.

Dr. Parker stated that it was time to reformulate the U.S. program for fusion, but asked if it would be wise to accept slippage from 2025 as the target date for the initial operation of DEMO. Dr. Overskei stated that an acceptable tactic leading to the introdution of the parallel machine might be to raise it in the context of what ITER would not do, while lauding what ITER could do. Dr. McCrory cautioned FEAC against being "machine happy". He said that the letter that was being planned looked like an over-rapid response to the demise of BPX and would not be viewed favorably. Dr. Parker responded that it was the committee's charge to indicate how the fusion program might recover from the loss of BPX and that the option of the parallel machine should not be ignored. Dr. Dean summarized that the loss of BPX had affected both the scope and mission of ITER, but that the situation could be recovered through the use of a parallel machine: The total expenditure would end up being the same as for BPX but it would have been deferred for a while. He recommended including the proposed statement in the letter.

Dr. Parker commented that while there had been a large response to the request for U.S. nominations to the ITER central team, many of the nominations appeared to be persons who are relatively inexperienced or not particularly well-known in the fusion program. He indicated that he would like to see the laboratory directors put up much better candidates since that would show real U.S. commitment to ITER.

Dr. Conn turned to the request in the charge that FEAC look for ways to reduce the cost of the ITER program or to shorten its duration. He summarized that the panel had concluded that it was not possible to reduce cost significantly. FEAC had already agreed to push for early commitment to construct the machine and had agreed that the U.S. should prepare a site proposal: Hence the matter of program acceleration had already been dealt with. Dr. Overskei pointed out that FEAC had not considered the impact of hard versus soft risks in the physics of the machine. Dr. Linford concurred that a more aggressive approach could be taken to ITER that would perhaps yield cost savings. But, there was increased risk associated with the pursuit of this path that the panel had determined would be unacceptable to the international partners. The possibility was reviewed by the panel but this process was not accorded much prominence in the report. He emphasized that the analysis was performed but the panel had not been in favor of recommending that more risk be taken. Dr. Linford agreed that FEAC's letter should contain a paragraph showing that if the ITER program was to stay within the physics rules that had been established for it, then no cost savings could be identified. But, if the physics rules were to be changed, then the savings could be significant.

- "A high priority should be given to obtaining maximum D-T plasma information from TFTR (and JET) prior to the construction of ITER."

It was agreed not to include this statement in the letter since it had already been included in the previous letter report of October 7, 1991.

- "The level of systems studies should be increased (by approximately $3 million) and used to bring in industries which can in the end lead to the design of DEMO."

Dr. McCrory stated that this issue, at $3 million, was a small one: It should not be included since it would detract from the overall impact of the large issues. Dr. Davidson agreed that this level of detail should not be in the letter. Dr. Dean pointed out that while he had no particular liking for the $3 million figure, nevertheless the thought should not be lost.

Dr. Ripin emphasized that when ITER entered the construction phase, it should be removed from the fusion program budget and become a separate line item. He suggested that the letter should contain a recommendation concerning how the ITER budget should be handled once construction begins. Dr.

Linford suggested that any such sentiment should be included immediately following the sentences that dealt with moving ahead quickly to secure agreement to construct ITER and to select the site for the facility.

Dr. McCrory protested that by pursuing this course, FEAC would be raising the visibility of future "big" budgets. He suggested omitting all reference to the ITER construction budget at this time. After some brief discussion, Dr. Conn concluded that the committee seemed to have reached agreement to omit the item. Dr. McCrory added that it was important to have a well established national fusion strategy. He emphasized that while the letter report itself clearly should not be viewed as providing that strategy, nevertheless the points contained in it should be recognized as being very important to it.

The letter report that was eventually presented to Dr. Happer is given as Appendix I to these minutes.

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