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FUSION ENERGY ADVISORY COMMITTEE

Report on Program Strategy for
U.S. Magnetic Fusion Energy Research

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FUSION ENERGY ADVISORY COMMITTEE
REPORT ON PROGRAM STRATEGY FOR U.S.
MAGNETIC FUSION ENERGY RESEARCH

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"Would I had phrases that are unknown, utterances that are strange, in new language that has not been used, free from repetition, not an utterance which has grown stale, which men of old have spoken."

An Egyptian Scribe, Circa 1500 B.C.

I. SUMMARY AND RECOMMENDATIONS

1. INTRODUCTION

Fusion energy offers the world the possibility of a long-term energy supply that is characterized by universally available and virtually inexhaustible fuel, radiological and proliferation hazards much smaller than those of fission, atmospheric impacts negligible compared to those of fossil fuels, impacts on ecological and geophysical processes smaller than those of large-scale use of renewable energy sources, and monetary costs potentially comparable to those of other medium-term and long-term energy options. The goal of the U.S. magnetic fusion energy research and development program is the practical realization of this potential in the second quarter of the next century, with the specific aim, as given in the National Energy Strategy, of starting operation of a Fusion Demonstration Power Plant (DEMO) by the year 2025. Fusion energy research is also the primary avenue for the development of plasma physics as a scientific discipline. The development of fusion and the science of plasmas will provide many practical and scientific challenges with spin-offs in areas as diverse as astrophysics and semiconductor manufacturing.

The Fusion Energy Advisory Committee (FEAC) was charged by the Department of Energy (DOE) with developing recommendations on how best to pursue the goal of a practical magnetic fusion reactor in the context of several budget scenarios covering the period FY 1994 - FY 1998. Four budget scenarios were examined, each anchored to the FY 1993 figure of $337.9 million for fusion energy (less $9 million for inertial fusion energy which is not examined here):

"SEAB Task Force Budget Scenario":
Funding for magnetic fusion energy (MFE) increases in FY 1994 to a level representing 5% real growth above the FY 1993 Presidential request, and increases thereafter at 5% per year in real terms (based on a case recommended previously by the Secretary of Energy Advisory Board Task Force chaired by Professor Charles Townes.)

"Reference Budget Scenario":
MFE funding increases at a steady 5% per year in real terms (above inflation) starting from the FY 1993 level.

"Flat Budget Scenario":
MFE funding remains flat in real terms after FY 1993, meaning only adjustment for inflation.

"Declining Budget Scenario":
MFE funding is constant after FY 1993 in as-spent dollars, hence declining in real terms at an inflation rate assumed to be 3.1% per year.

The comparison of the requirements for developing the potential of fusion with the possibilities offered by these four budget scenarios has led us to the following conclusion:

- **The goal of an operating DEMO by 2025, which is specified in the National Energy Strategy and is the approximate target date required if fusion is to be a significant contributor to U.S. energy supply by the middle of the next century, will be achievable only in the framework of a national commitment to this goal. Of the four budget cases, only the "Reference" and "SEAB Task Force" cases, coupled with additional line item support for ITER construction, are plausibly consistent with this objective.**

A strategy consistent with the fusion DEMO target, and with timely achievement of the commercial potential of fusion, requires both steadily increasing engagement in international collaborative projects and the revitalization of the national efforts in confinement physics and fusion technology. Only with a robust national program will the U.S. be an effective partner in a world fusion program and be able to benefit from its success.

The national program should include three primary elements: (1) a vigorous research base in theoretical, computational, and experimental plasma physics, in fusion technology and materials development, and in reactor systems studies; (2) the operation of existing confinement facilities to extract from them the needed data they were designed to provide; and (3) the design and construction, over the next decade, of additional experimental facilities for confinement-concept improvement research, for studies of long-pulse plasma behavior, and for the testing of candidate reactor materials.

Internationally, the United States will need to provide funds, beyond those required for the national effort, to pay for our participation in the construction of the International Thermonuclear Experimental Reactor (ITER), which has become the key experiment for studying ignition physics and many aspects of fusion engineering. Successful operation of ITER, combined with expected progress in the elements of the national program described in the foregoing paragraph, will provide the basis for DEMO design.

In addition to ITER construction, which as noted will require additional line-item funding, there are other parts of the international program where the U.S. contribution has been included as part of the national program budgets in the cases we discuss in this report. These include: ITER Engineering Design Activities (EDA) and R&D, the international program to develop alternative confinement geometries (which could turn out to be superior to the tokamak as reactors) and, towards the end of the decade, construction of an international 14-MeV neutron source for testing candidate reactor materials.

The FEAC Panel 5 examined the type of magnetic fusion program that should be in place in the years 2000 - 2010 and developed plans and budget requirements for the period FY 1994 - 2000. These plans and budget requirements were used to assure consistency and to develop our recommendations.

2. NEW INITIATIVE COST ASSUMPTIONS

In considering the four budget scenarios we made assumptions about the costs of some major new-initiative programs:

- The reference budget for the ITER Engineering Design Activities, recently agreed to by the international partners, would rise from about $55 million in FY 1993 to about $65 million in FY 1998, in constant FY 1993 dollars. It is assumed that if ITER construction were approved, the required additional funding would be provided separately from the base magnetic fusion program.

- The Total Project Cost for design and construction of the Tokamak Physics Experiment (TPX) is taken to be about $500 M in as-spent dollars. The project as originally planned would have had plasma operation starting in the year FY 1999. Stretching the TPX construction by one year would allow the annual costs of TFTR plus TPX to be approximately constant. It is assumed that TFTR will complete its mission at the end of FY 1994, and that the costs used for TFTR operation and decommissioning are those proposed by PPPL. In the Reference and SEAB Task Force Budget Scenarios, the detailed design of TPX is assumed to begin in FY 1994, and construction begins in FY 1995.

3. OPERATION OF MODERATE SIZE EXPERIMENTS

At the request of DOE, a panel of FEAC was established (FEAC Panel 4) to review proposals for operation of three moderate-size experiments: the tokamaks Alcator C-MOD at MIT and PBX-M at PPPL, and the stellarator ATF at ORNL. For comparison purposes the panel was also asked to consider proposed upgrades to the DIII-D tokamak at General Atomics. Information about the significance of the technical contributions expected from these programs was presented to FEAC Panel 5 for these deliberations.

4. OTHER PROGRAM AND FEAC INFORMATION

The FEAC has been active since September, 1991. Entering into all considerations in the present report was the information contained in earlier FEAC Panel Reports (Panels 1, 2 and 3) and in the FEAC letters of advice to the DOE of October, 1991, and of February, April, and June, 1992. Copies of earlier charge letters to FEAC from Dr. William Happer, Director of Energy Research, DOE, and the FEAC letters to DOE are included as Appendix E and Appendix F of this report, respectively.

5. HIGHEST PRIORITY RECOMMENDATIONS

Highest priority is given to the following two program elements which retain their priority in all budget cases:

The International Thermonuclear Experimental Reactor (ITER) Engineering Design Activities (EDA)

- RECOMMENDATION: **High priority should be given to strong U.S. participation in the ITER Engineering Design Activities at a level consistent with the international ITER EDA Agreement. (See also FEAC letter to DOE dated February 14, 1992.)**

  COMMENTS: It is critical that the U.S. be a vigorous partner in the ITER Engineering Design Activities leading to construction of ITER. U.S. involvement in the EDA will include technology R&D, supporting activities by the Home Team, support of the San Diego site, and provision of one-fourth of the scientists and engineers for the Joint Central Team. However, only the SEAB Task Force Budget Scenario provides full funding for the selection and qualification of a U.S. candidate site for ITER.

The Tokamak Fusion Test Reactor (TFTR)

- RECOMMENDATION: **High priority should be given to expeditious implementation of the TFTR deuterium-tritium (D-T) program with completion of experimental operation by the end of FY 1994. (See**

also FEAC letter to DOE dated April 1, 1992.) DOE should act to ensure timely progress with the regulatory process in order to start the D-T experiments in FY 1993 to meet the deadline.

COMMENTS: The TFTR tokamak at the Princeton Plasma Physics Laboratory (PPPL) is one of two large tokamaks in the world fusion program that will provide experience with D-T plasmas prior to operation of ITER. (The other is the Joint European Torus — JET). High priority is assigned to the timely execution of D-T experiments on TFTR to provide critical data on alpha particle confinement and collective effects, stability and transport properties of D-T plasmas, operating experience with tritium and with an activated tokamak device, and demonstration of fusion power production. Operating experience with tritium will also provide input for ITER site evaluations in the U.S. All budget scenarios are dependent on the availability of funds beginning in FY 1995 after the envisaged shut-down of TFTR operation.

6. REFERENCE BUDGET SCENARIO

(5% real annual growth, above inflation, from the FY 1993 funding level)

PRINCIPAL RECOMMENDATIONS

The following recommendations are not ordered by priority.

Tokamak Physics Experiment (TPX/SSAT)

- RECOMMENDATION: **Detailed design and construction of the Tokamak Physics Experiment (TPX), a steady-state advanced tokamak to be operated as a national facility at the Princeton Plasma Physics Laboratory (PPPL), should begin in FY 1994 with completion of construction and first plasma operation scheduled for FY 2000. (See also FEAC letter to DOE dated April 1, 1992.)**

COMMENTS: The scientific elements in the mission of TPX are aimed at the study of advanced tokamak operating regimes with high values of beta-poloidal and bootstrap current fraction, and with non-inductive plasma current drive and enhanced confinement modes during pulses lasting at least 1000 seconds. The technologies to be employed in TPX, especially in the areas of plasma power handling, particle exhaust, plasma control, and superconducting magnets are highly supportive of ITER and DEMO reactor technical needs. Operation of TPX around the turn of the century will provide critical physics data on advanced tokamak operation and place the U.S. in a strong position for collaboration in ITER's operational phase. TPX will demonstrate improvements in tokamak performance, reliability, and configurational simplicity which, when coupled with ITER results, will point the way towards to an attractive DEMO.

Intermediate-Scale Confinement Experiments

- RECOMMENDATION: **Upgrades of the DIII-D tokamak at General Atomics to perform divertor development and advanced tokamak experiments should commence in FY 1994.**

  COMMENTS: Information from the DIII-D advanced divertor experiments, including the advanced radiative divertor, will provide critical input for the design and operation of ITER and the TPX tokamak. Subsequent heating and profile control upgrades will permit DIII-D to examine advanced tokamak physics concepts for extended pulse lengths, providing important operating experience in support of TPX.

- RECOMMENDATION: **Preparation for restart of the ATF stellarator at the Oak Ridge National Laboratory should begin in FY 1993, with startup in FY 1994 and the first full year of operation scheduled for FY 1995.**

  COMMENTS: Stellarator configurations offer a reactor concept with size similar to a tokamak, but with the potential for steady-state and disruption-free operation. ATF is a unique facility and an important, complementary part of the world stellarator program. The objective for operation of ATF should be the achievement of average plasma beta values in excess of four percent with enhanced confinement. Operation of ATF will also contribute to research in toroidal confinement physics and will make key near-term contributions to the world stellarator program.

- RECOMMENDATION: **The Alcator C-MOD tokamak at the Massachusetts Institute of Technology should operate through FY 1995 with emphasis on radiative divertor studies, and in FY 1995 the future operation of Alcator C-MOD should be reconsidered.**

  COMMENTS: The high-field Alcator C-MOD tokamak has very high power density and reactor-relevant edge plasma parameters. Near-term emphasis on radiative and gas divertor concepts will provide important input to the ITER divertor design, as well as the advanced radiative divertor program planned on the DIII-D tokamak. Operation of Alcator C-MOD in FY 1996 and beyond would explore advanced tokamak scenarios relevant to TPX, including non-inductive current drive, and current-profile control experiments.

- RECOMMENDATION: **Operation of the PBX-M tokamak at the Princeton Plasma Physics Laboratory (PPPL) should be suspended in FY 1994, with consideration of renewed operation in FY 1996.**

  COMMENTS: PBX-M is an advanced tokamak with capability for strong plasma shaping, non-inductive current drive using lower hybrid waves, current and pressure profile control, kink-mode stabilization, and operation at high beta. In the Reference Budget Scenario, construction of the Tokamak Physics Experiment (TPX) and budget stringencies require the suspension of PBX-M operation in FY 1994. This will reduce the physics data-base in support of the TPX advanced tokamak. Renewed operation in FY 1996 would provide additional operational experience and physics understanding of profile control as a means to access advanced tokamak regimes. Operation of PBX-M would also provide continuity in the experimental program at PPPL during the transition from TFTR to TPX.

Development and Technology

- RECOMMENDATION: **The fusion materials program should be enhanced. The U.S. should participate in an internationally-based 14 MeV neutron source, with detailed design and construction begin-**

ning in the FY 1996 time-frame. (See also FEAC letter to DOE dated February 14, 1992.)

COMMENTS: An enhanced program to develop and characterize reduced/ low-activation materials for fusion is required. Early development of these materials will make them available for testing during the nuclear technology test phase of ITER. An internationally-based 14 MeV neutron source with sufficient flux and irradiation volume will play a pivotal role in materials development for DEMO, including material selection and optimization, measurement of property data for design, and assessment of component lifetimes.

- RECOMMENDATION: **Studies should continue of ways to obtain, in a timely manner, nuclear testing data required to prepare for DEMO. (See also FEAC letter to DOE dated February 14, 1992.)**

  COMMENTS: In its letter to DOE of February 14, 1992, FEAC noted that the absence of BPX from the U.S. program would result in an extended ITER physics phase and a corresponding delay in the acquisition of the nuclear testing data from ITER. FEAC stated that "additional complementary activities dedicated to acquiring part of the nuclear testing data would permit shortening the ITER test program" and recommended that "a study of the feasibility of such a complementary program be undertaken with a view toward making the 2025 DEMO goal more realistic." The need for and role of a complementary blanket testing program will depend upon the timing and scope of the blanket test program of ITER.

- RECOMMENDATION: **There should be a modest enhancement of the fusion development and technology base program. (See also FEAC letter to DOE dated February 14, 1992.)**

  COMMENTS: The R&D tasks undertaken in the U.S. in response to the ITER Engineering Design Activities, together with a modestly enhanced non-ITER base program in Development and Technology, are necessary to proceed to a DEMO reactor. This activity also supports student training in various areas of fusion engineering.

Applied Plasma Physics

- RECOMMENDATION: **Research in the applied plasma physics base program should be maintained at least at the present level of effort.**

  COMMENTS: The Applied Plasma Physics program supports the national fusion effort in the areas of innovative concept development, modest-scale experiments, fusion theory, large-scale computing, and student training. The majority of university efforts in fusion research are supported by the Applied Plasma Physics program.

- RECOMMENDATION: **A concept improvement program that investigates both tokamak refinements and non-tokamak confinement concepts should be maintained as part of the US fusion program as a matter of policy. (See FEAC letter to DOE dated June 12, 1992.)**

  COMMENTS: The tokamak has emerged as the most scientifically successful confinement concept. However, uncertainties remain in the extrapolation of the tokamak to a competitive commercial reactor, and FEAC has recommended in its letter to DOE of June 12, 1992, that both tokamak and non-tokamak concept improvement activities should be supported. The earlier recommendations in this section to develop advanced tokamaks and to restart the ATF stellarator are consistent with this policy, as is a modest effort on less well-developed alternative concepts. Because fusion is a long-term program, FEAC also suggested that a small but formal and highly visible periodic competition be established to foster new concepts and ideas that, if verified, would make a significant improvement in the attractiveness of fusion reactors.

Implications of the Reference Budget Scenario

The Reference Budget Scenario provides adequate funds to construct the Tokamak Physics Experiment (TPX) with initial operation in FY 2000. TPX will demonstrate the integration of advanced tokamak concepts and steady-state technology, impacting both the later phase of ITER operation, and the design of DEMO. Upgrades to the DIII-D tokamak will provide important information for ITER and TPX in both the divertor and advanced tokamak areas. Operation of ATF will allow the U.S. to contribute to the world stellarator program, while the results from Alcator C-MOD will provide information on high-density radiative divertor concepts for ITER. Enhancements to the fusion materials development and testing programs will increase the likelihood of the availability of appropriate materials in the required time frame. While the fusion program plan outlined here entails more technical risk because of the deletion of BPX than that developed by the Fusion Policy Advisory Committee (FPAC), it is plausibly consistent with the operation of a DEMO around 2025.

The budget level in FY 1993 causes a one-year delay in the start of the upgrade of DIII-D and in the TPX program. The Reference Budget Scenario for FY 1994 and beyond does not provide adequate funds to define and develop fully a U.S. candidate site for ITER construction, at least not without further compromising the domestic program or U.S. obligations to the ITER Engineering Design Activities. This jeopardizes the ability of the United States to compete in hosting the site for ITER construction and operation. Also, the base programs are held at levels lower than FEAC believes is appropriate given their importance.

7. SEAB TASK FORCE BUDGET SCENARIO

(5% real growth above inflation beginning in FY 1994 from the FY 1993 Presidential request)

Following its meeting September 20-21, 1991, the DOE Secretary of Energy Advisory Board Task Force on Energy Research Priorities advised the DOE Director of Energy Research that "The Task Force believes that funding for the magnetic fusion program must increase at a modest rate (e.g., 5 percent real growth per year) even at the expense of other programs." On September 24, in his first charge to the newly-formed Fusion Energy Advisory Committee (FEAC), the Director of Energy Research asked the FEAC to consider the program implications of planning for "a budget at 5 percent real growth per year through FY 1996." The starting point for this projected growth was an FY 1993 budget of $360 M (less $9 M for inertial fusion energy), as contained in the budget submission by the President to Congress. This starting point is to be compared to the FY 1993 level of $339.7 M used for the Reference Budget Scenario (less $9 M for inertial fusion energy). We now consider the programmatic implications of the original SEAB Task Force Budget Scenario, which is approximately $20 M higher than the Reference Budget Scenario, assuming that it can be implemented beginning in FY 1994. The following recommendations for the SEAB Task Force Budget Scenario, not ordered by priority, are changes to the Reference Budget Scenario described earlier in Section 6 of this chapter.

- RECOMMENDATION: **A U.S. fusion nuclear site selection study should be carried out, and the U.S. ITER EDA effort should be enhanced beyond the minimum level required by the International ITER EDA agreement. (See also FEAC letter to DOE dated February 14, 1992.)**

  COMMENTS: Estimates by the U.S. ITER Home Team make it clear that a significant effort will be required to identify and select a U.S. candidate site for ITER or other U.S. fusion nuclear facilities and that the U.S. cannot expect to receive "ITER credit" for these expenses. Furthermore, in the view of the Home Team, funds beyond the $1.2 billion originally agreed to by the four parties will be required in order to properly staff the three co-centers and carry out the EDA on the 6-year schedule.

- RECOMMENDATION: **Programs in Applied Plasma Physics should be moderately enhanced to increase research on concepts that can potentially either improve the tokamak or become superior to it, on fusion theory and computation, and on existing or new scientific experiments. Priority is also given to enhancing operations of the existing mid-sized Confinement Systems experiments.**

  COMMENTS: In the Reference (and Lower) Budget Scenarios, the budget of the Division of Applied Plasma Physics was recommended to be maintained approximately constant. This will cause serious difficulty to the many small-scale efforts that make up that program. FEAC has recommended previously that a modest non-tokamak research program should be maintained in any budget circumstance. FEAC is also concerned that existing modest-size confinement experiments are being inefficiently utilized due to the declining budgets of the past several years.

- RECOMMENDATION: **The base program in Development and Technology should be further enhanced, including materials research, component development research not supported by ITER credits, and design studies of fusion nuclear facilities. (See also FEAC letter to DOE dated February 14, 1992.)**

  COMMENTS: This recommendation is intended to allow a more rapid implementation of the enhancements recommended in the Reference Budget Scenario. It is also intended to permit implementation of previous FEAC recommendations (based on FEAC Panel 1) provided in February, 1992.

Implications of SEAB Task Force Budget Scenario

At the time that the SEAB Task Force recommended a new U.S. tokamak facility, it was assumed that the U.S. would enter into that project from a healthy base of activity, while participating vigorously in ITER. The funding case considered here restores the program balance while proceeding with the TPX and DIII-D upgrades, with the one-year delay caused by the reduction in the FY 1993 funding below the President's budget request.

8. FLAT BUDGET SCENARIO

(Level-of-effort in constant FY 1993 dollars)

Under this scenario the following changes are made to the recommendations for the Reference Budget Scenario. These changes are not ordered by priority.

- RECOMMENDATION: **Despite the severe limitations imposed by the Flat Budget Scenario, a post-TFTR initiative should be retained as a principal program objective. Construction of a TPX would be funded from the roll-off of the costs to operate TFTR and, in subsequent years, of the costs to operate other mid-sized confinement facilities. This will require termination of PBX-M operation in FY 1994 in order to continue the TPX design effort.**

COMMENTS: FEAC notes that in the Flat Budget Scenario, consistent with our other recommendations, the completion date of a TPX with a cost of about $500 million in as-spent dollars would be about 2003. PPPL concurs in this recommendation. The scope and mission of this device can be expected to evolve in this time frame, but the need for a future post-TFTR facility in the U.S. program is certain.

- RECOMMENDATION: **The design of a 14 MeV neutron source should be delayed until at least FY 1996.**

Implications of the Flat Budget Scenario

The operational start of a TPX experiment is delayed by comparison with the Reference Budget Scenario. TPX is designed to integrate steady-state operation with the advanced physics operating scenarios which will be tested in medium-sized U.S. facilities currently operating. These facilities are unique in the world program and have only recently come into operation. Attempting to proceed with TPX on the originally proposed schedule within the Flat Budget Scenario would require shutting down at least two of these medium-sized confinement research facilities, yet these are the very programs providing the physics basis that TPX is intended to exploit. Therefore, if incremental funds are not available starting in FY 1994, it will be necessary to delay any post-TFTR initiative until the operating costs of TFTR and, in subsequent years, of the mid-sized confinement facilities begin to roll off in FY 1995 and beyond. The hiatus in experimental activities at PPPL between the shutdown of TFTR and the start of the new facility would be partially off-set by collaborations on other experimental programs.

The ability of the United States to compete for the ITER construction site while maintaining a vigorous domestic fusion program in support of ITER and DEMO is reduced.

Finally, the continued delay in the development of a 14 MeV neutron source increases the uncertainty and risk in DEMO operation following ITER, and adds to the gap in the time between ITER and a U.S. DEMO.

9. DECLINING BUDGET SCENARIO

(Constant annual budget at FY 1993 level, no adjustment for inflation)

The declining levels of effort under this budget scenario lead to the following changes in the recommendations in the Reference Budget Scenario:

- RECOMMENDATION: **The Tokamak Physics Experiment (TPX) design should be stopped after FY 1993. No significant funding will be available for a post-TFTR initiative.**
- RECOMMENDATION: **Design of the 14 MeV neutron source should be delayed until after FY 1997.**
- RECOMMENDATION: **The upgrade of the DIII-D tokamak should be delayed until FY 1995 and the operation of other intermediate-scale confinement experiments should be reduced and phased out as required.**

Implications of the Declining Budget Scenario

Under this scenario the program would retain its two highest priority goals identified in the Reference Budget Scenario: (1) U.S. participation in the ITER EDA at the level of our international commitment; (2) D-T experiments in TFTR through the end of FY 1994.

As the cumulative effects of a declining level of effort are felt in this budget scenario, the damage to the program as a consequence of no investment in new or upgraded experimental facilities except DIII-D becomes severe. By the end of the ITER EDA in 1998, the fusion program's experimental base will have been reduced to less than half in this budget scenario.

The primary consequence of this budget scenario is to severely undermine the capability of the U.S. fusion program to effectively participate in ITER and to contribute to an improved design of DEMO. Under this budget scenario it is highly unlikely that the U.S. could be an effective participant in ITER construction and operation. Also, with the shutdown of TFTR in FY 1994 and in the absence of resources to begin a major post-TFTR initiative as a national facility at the Princeton Plasma Physics Laboratory, the U.S. position as a world leader in experimental confinement physics and the role of PPPL as a key national resource for fusion work in the U.S. will be seriously eroded. The loss of critical personnel with fusion expertise nationwide will be severe.

In the declining budget scenario, serious re-examination of the program strategy will be needed to determine the best means to continue progress towards a DEMO reactor, albeit on a considerably delayed time scale. It is the opinion of FEAC that this scenario is inconsistent with the spirit of the ITER EDA agreement to which the U.S. is committed, and leads to the indefinite delay of the DEMO.

II. BACKGROUND, CHARGE, AND SCHEDULE

1. Background

At the first meeting of the Fusion Energy Advisory Committee (FEAC) on September 25, 1991, Dr. William Happer, Director of the Office of Energy Research of DOE presented a broad charge to FEAC to provide advice on major but not all elements of the magnetic fusion energy program. These elements included the International Thermonuclear Experimental Reactor (ITER), a new U.S. Tokamak Physics Experiment (TPX), the near term tokamak experimental program, and the concept improvement program. In response, the FEAC has sent to DOE four letter-reports dated October 7, 1991; February 14, 1992; April 1, 1992; and June 12, 1992. The early charge letter from Dr. Happer and the letters from FEAC to DOE are included as Appendix E and Appendix F of this report, respectively.

In the FEAC letter to DOE dated April 1, 1992, it was pointed out that as the committee addressed the issues of Dr. Happer's original charge, FEAC found the need to examine the program in its entirety. Our concern was to be sure that our recommendations took proper account of all the elements of a sound U.S. magnetic fusion program. In particular, we were concerned that the implementation of certain FEAC recommendations would require reductions in other important program areas that had not been reviewed or discussed. As a result, we suggested the need to conduct a more complete assessment of the U.S. fusion program strategy and priorities.

2. Charge and Schedule

On June 22, 1992, Dr. Happer wrote to Professor Robert Conn, Chairman of FEAC, requesting that FEAC provide recommendations on strategic program planning for magnetic fusion. Dr. Happer suggested that FEAC consider several budget scenarios. In all cases the primary goal is to make maximum progress towards a Demonstration Power Plant (DEMO), with full consideration of the role of international collaboration.

FEAC appointed Panel 5 to address the specifics of the June 22, 1992 charge and to provide a written report to FEAC by September 15, 1992. The objective was to review, discuss, and adopt a version of the report as its own. This report constitutes that version. Many program elements had been reviewed earlier by FEAC and reports of FEAC Panels 1, 2, and 3 were available to Panel 5. In addition, Panel 4 has been active in reviewing three mid-size confinement programs, the Alcator C-Mod and PBX-M tokamaks and the ATF stellarator. Participant members of Panel 5 who are not members of FEAC brought special expertise to the panel that assured proper coverage of all important program topics. The memberships of FEAC and of FEAC Panel 5 are given in Appendix B and Appendix C, respectively.

Panel 5 met for a one-week meeting July 26-31 at Crested Butte, Colorado. Personnel from the Office of Fusion Energy, DOE, attended to provide background information and clarify policy questions. During the meeting, the various programs and budget scenarios were reviewed, and preliminary information from FEAC Panel 4 was presented. During this meeting, Panel 5 deliberated the issues, reached conclusions and recommendations, and developed an outline for this report. The report was written over the period from August 15, 1992 through September 18, 1992, with interim versions being sent to all members of FEAC as well as all members of Panel 5. This process resulted in all members of FEAC being well-briefed on the matter. The report was debated and this final version was adopted by FEAC at its meeting in Washington, D.C. on September 22 and 23, 1992.

III. MAGNETIC FUSION ENERGY

A. THE RATIONALE FOR DEVELOPING FUSION ENERGY

The fusion energy program seeks to develop a safe, environmentally attractive, and economically competitive energy supply for the long-term. A variety of key experimental advances over the past two decades have brought the program to the point where it is ready to proceed to the next level of development, namely, an experimental fusion reactor. Fusion energy offers the potential of a long-term energy supply for the world characterized by:

1. a fuel supply extractable from sea water (thus available to all countries) in sufficient quantity for millions of years;

2. safety and environmental advantages in the areas of radiological hazards and nuclear materials proliferation when compared to fission energy systems; advantages with respect to emissions to the atmosphere when compared to fossil fuel options; and advantages with respect to impacts on ecological and geophysical processes when compared to renewable energy forms;

3. monetary costs comparable to those of long-term energy options.

Although it might be possible for the United States and other countries to meet their long-term energy needs at acceptable costs using a mix of energy sources that does not include fusion, uncertainties about the size of those needs and about the liabilities of the non-fusion energy-supply alternatives make it prudent to add fusion to the mix. The continuing R&D investments required to establish whether fusion's promise can be realized in practical form are modest compared to the immense potential costs of not having enough energy that is economically affordable, environmentally tolerable, and politically secure.

1. Fuel Supply

"First generation" fusion reactors are assumed to be based upon the deuterium-tritium (D-T) fusion reaction, and the breeding of tritium by the capture in a lithium blanket of neutrons produced by the D-T reaction itself. Extracting lithium from sea water until its concentration falls to one-half of today's value would yield at least 150 million terawatt-years of thermal energy in "first generation" fusion reactors. This compares to world energy use of about 13 terawatt-years per year in 1990.

Extracting deuterium from sea water until its concentration falls to one-half of today's value would yield 250 billion terawatt-years of thermal energy in "second generation" fusion reactors based on the deuterium-deuterium (D-D) reaction. For comparison, world coal supplies are estimated at 5 to 10 thousand terawatt-years.

2. Safety and Environment

If priority is given in the development of fusion to achieving its potential for reduced radiological hazards, it is estimated that even in "worst case" accidents, population exposures to radiation will be about 100 times smaller than exposures from "worst case" fission reactor accidents. (Use of fusion fuels not involving lithium would give even larger improvements over fission.) The radioactive-waste hazards of fusion — based on the most meaningful indices combining volume, radiotoxicity, and longevity — can be expected to be at least 100 times smaller than those of fission. Fusion-energy systems will be less likely than fission-energy systems to contribute to the acquisition of nuclear-weapons capabilities by subnational groups, and it will be easier to safeguard fusion systems against clandestine production and use of fissile materials by governments.

Fusion energy will have no counterpart to the problems of mining, air pollution, acid rain, and greenhouse-gas-induced climate change associated with the use of coal. It will have no counterpart to the ecological problems associated with large-scale production of biomass for energy (heavy use of land, water, fertilizers, and pesticides, and the loss of natural biodiversity), and it will have smaller ecological and geophysical impacts than hydropower, ocean thermal energy, and (probably) geothermal energy. The land-use requirements of fusion energy will be smaller than those of solar electricity generation.

3. Economics

The costs of the raw fuel for fusion — lithium and deuterium — would be well below one-tenth of a cent per kilowatt-hour of electricity. The total costs of fusion-generated electricity will be dominated instead by the construction cost of a power plant. The potentially higher costs of fusion plants compared to fission plants that may arise as a result of the complexity of fusion technology could be substantially offset for the safest designs by savings resulting from easier siting, easier licensing, and reduced requirements for "nuclear-grade" certification of plant components. The availability and costs of energy from fusion would not depend on regional topographic and climatic differences, as do the availability and costs of electricity from sunlight, wind power, hydropower, and ocean thermal energy; and fusion energy would not have the costly problems of intermittency and energy-storage associated with large-scale use of photovoltaic and wind technologies.

4. Role of Fusion in a Prudent Energy Mix

A variety of energy options, other than fusion, may prove to be attractive for the long term, including photovoltaic and solar-thermal electricity generation, fission breeder reactors, direct production of hydrogen using sunlight, ocean thermal energy, hot-rock geothermal energy, and selected biomass energy technologies. But there are significant uncertainties about the monetary and environmental costs of most of these options. Many are likely to become costlier as the scale of exploitation increases, and as it becomes necessary to resort to lower-quality sites. It must be remembered that all renewable energy options, since they are based on geophysical or biological energy flows, involve environmental effects that may be negligible when employed at small scale but which become increasingly problematic when employed at large scale.

It is imprudent to assume, based on today's understanding of these matters, that any one of the renewable energy options or even any combination of them will suffice to meet civilization's future energy needs. After all, there is little chance that the world's population can be stabilized below 10 billion people. Even if it is assumed rather optimistically that improvements in energy efficiency can permit a high standard of living for everyone in the long run at an average energy use rate of 3 kilowatts per person (which is half the figure for Germany today and barely more than a fourth of the figure for the United States), the implied global rate of energy use is 30 terawatts. This level is almost two and half times the 1990 world energy use rate of 13 terawatts, some 10 terawatts of which are coming from fossil fuels that will have to be replaced with sustainable supplies even if energy use does not grow at all.

The problems of scaling up renewable energy sources to meet demands of this magnitude would be substantial. For example, to make 30 terawatts using photovoltaics would require covering more than 1 percent of the land area of the planet with photovoltaic cells capable of converting 10 percent of the sunlight falling on this area to electricity. (One percent of the land area is equal to the area that is urbanized today.) To make 30 terawatts based on biomass would require increasing by 20-fold the 1.5 terawatt biomass-fuel enterprise (fuel, wood, charcoal, crop wastes, and dung) that supplies energy for cooking and space and water heating for more than 2 billion people in rural regions today. (This activity is already associated with severe environmental impacts on land, water, and air.) Furthermore, a biomass energy operation of the required size would need to be between 5 and 10 times bigger than that of world agriculture in 1990. World agriculture now uses more than 10 percent of the world's total land area and a much larger fraction of the land that is suitable for high-yield plant growth.

Meeting the long-term energy needs of a large and diverse civilization is likely to require the contributions of a variety of different energy sources. The more sources that are available, the better are the chances that the total need can be met without putting so large a burden on any one source that its problems become unmanageable. Since the number of long-term energy sources is very limited — consisting only of geothermal energy, sunlight and its derivatives, fission energy based on breeder reactors, and fusion energy — it is only prudent to explore the potential of all of these as thoroughly as possible. In the event that the large-scale use of fission breeders cannot be made publicly acceptable because of concerns about safety, waste management and/or linkages to nuclear weapons — an outcome that cannot be ruled out — it will be even more crucial that fusion energy succeed in a form that reduces these concerns to an acceptable level. The annual worldwide R&D investments required to proceed with fusion development in a timely manner are in the range of a thousandth of current sales of fossil fuels.

B. THE REQUIREMENTS OF FUSION DEVELOPMENT

The National Energy Strategy has proposed a target date of 2025 for initial operation of a Magnetic Fusion Demonstration Power Plant (DEMO). To design a realistic DEMO, a number of scientific and engineering accomplishments are required, which we outline here.

1. Physics Requirements

Fundamental to magnetic fusion, it must be demonstrated that magnetic confinement of plasma is adequate to burn the fuel with sufficient energy gain. Almost all past fusion research has been devoted to this extremely complex scientific problem. Confinement as measured by the product of density, n, energy confinement time, τ, and temperature, T, (the triple product $n\tau T$), has improved by a factor of about one million as a result of the past decades of effort. Further improvement by a factor of about 7 is required for a reactor. We list a set of representative scientific milestones on this path:

1. Scientific Breakeven - Plasma energy confinement in deuterium plasma sufficient that, with D-T fuel, the fusion power that would be produced is equal to the power used to heat the plasma. This milestone has now been essentially met in the Joint European Torus (JET) and has been approached in the Tokamak Fusion Test Reactor (TFTR). These achievements are a measure of the state-of-the-art.

2. Demonstrated Breakeven - Actual use of D-T fuel to produce an amount of fusion power equal to the heating power. This implies that plasma stability and confinement are not degraded by the energetic fusion products under approximate breakeven conditions. These issues will be addressed on TFTR and JET in the next three years.

3. Plasma Ignition - Requiring a factor of about 7 better triple product than milestones 1 and 2 would allow the fusion reaction to be self-sustained and permit the study of thermal stability and the efficiency of plasma heating by the self-generated alpha particles. This was to have been

the mission of the Burning Plasma Experiment (BPX), but with the cancellation of BPX due to budgetary reasons, ignition will now be investigated for the first time on ITER. Fusion strategy must consequently be based on a delayed and more ambitious step directly to long-pulse ignition in ITER. If Italy proceeds with Ignitor, this might provide earlier results on ignition that would be useful as a guide to ITER operation.

4. Long-Pulse/Steady-State Physics - Study of a number of physics issues — long-time current-profile evolution and control, plasma-wall equilibrium, and fusion ash and impurity removal — requires pulses of many minutes, rather than seconds, of plasma duration. Very long pulses would also require non-inductive current drive. Key issues are current self-sustainment via the bootstrap effect and disruption control in high-performance regimes. The proposed Tokamak Physics Experiment (TPX) would address these issues under relevant physical conditions but without D-T fuel. Such results would be useful in guiding ITER operations, and for designing an attractive DEMO.

5. Long-Pulse Ignition - Combining the accomplishments of milestones 3 and 4, long pulse ignition would constitute a major step towards the realization of fusion power. This is the first main objective of the proposed International Thermonuclear Experimental Reactor (ITER). ITER is now in the Engineering Design Activities (EDA) phase and is the largest pre-DEMO step envisaged in fusion energy development.

6. Scientific Understanding and Concept Improvement - At present, the macroscopic properties of tokamak plasmas are well understood, while small-scale microturbulence processes which govern anomalous heat and particle transport are only qualitatively understood. Tokamak design now depends upon empirical "scaling laws." While the empirical confinement information achieved in today's tokamaks may be adequate for ITER design, modest improvement via better operating regimes or improved configurations would enhance the eventual economic attractiveness of tokamak reactors. Such improvements should be incorporated into a DEMO. Exploring these

opportunities for tokamak improvement is a primary mission of the existing short pulse-length mid-sized machines. A primary goal of the Tokamak Physics Experiment (TPX) is to explore advanced tokamak confinement in steady-state. Theoretical and computational physics as well as small-scale basic physics experiments are key to gaining the scientific understanding required for concept improvement.

There are important reasons to pursue the study of concepts other than tokamaks, such as the stellarators, which do not require a sustained plasma current and thus are not likely to be subject to disruptions. Stellarators also do not require plasma current drive. Confinement in stellarators has not been studied nearly as extensively as in tokamaks. The ATF stellarator is among the world's leading experiments and the major U.S. effort in this area. There is a larger stellarator now under construction in Japan and another that is proposed for construction in Europe. The earliest operation of either of these stellarators is the late 1990s. We believe it is important to sustain U.S. participation in stellarator research and to develop, at lower levels of effort, novel confinement concepts which can improve the attractiveness of a fusion reactor. (See FEAC letter to DOE dated June 22, 1992.)

2. Engineering/Technology Requirements

In addition to the aforementioned scientific milestones, there is an equally challenging list of engineering and technology issues that must be addressed in order to make fusion a reliable, economically competitive, safe, and environmentally desirable energy source. Some of these engineering advances are naturally made in order to build and operate today's experiments, but many more must be developed for ITER, for DEMO, and for a commercial fusion power station.

One key area is the development of advanced materials and energy-converting blankets that will not be undertaken in connection with ITER (which must be designed to use existing materials). Most important is the development of low-activation structural, blanket and first-wall materials. Such materials must meet demanding thermomechanical requirements and must retain integrity when subjected to high neutron fluence. But unlike today's structural steels, low-activation materials will have much lower levels of neutron-induced radioactivity. Proposed candidate materials include low-activation ferritic steels, vanadium alloys, and non-metallic materials such as the composites used in aerospace applications. Silicon carbide composites are nearly ideal from the point of view of achieving low levels of neutron activation and afterheat.

A greatly enhanced development program is required to perform materials development, thermomechanical testing, and irradiation testing using fission reactors. Such an effort is essential for a fusion energy DEMO. Starting construction of a 14-MeV neutron source, presumably as an international project, will be required before the end of the decade. Blanket development with a focus on the needs of a DEMO may require, in addition, a plasma-based volume-neutron-source facility. Whether such a facility is required and what its role should be will depend upon the mission, scope and timing of the technology phase of ITER, as agreed to by the ITER partners.

To design ITER and ultimately an attractive DEMO reactor, other key engineering accomplishments are needed and these include:

1. Impurity Control and Handling of the High Heat Loads - This is the issue generally agreed to be the most difficult design issue for ITER. Key contributions are expected from the operation of divertors in the Alcator C-Mod and DIII-D tokamaks, from research in non-tokamak testing facilities such as the PISCES experiments at the University of California, Los Angeles and the PMTF facility at Sandia National Laboratories, and later from the operation of the long-pulse TPX tokamak.

2. First Wall Design - This is a crucial area with the goal of assuring the longevity of the first-wall under repeated pulse loading and high neutron fluence.

3. Development of large superconducting magnets to produce both

steady and pulsed magnetic fields - The magnets to generate the toroidal and poloidal fields will be the largest ever built.

4. Tritium processing and breeding - A major blanket test program will be part of ITER, and R&D is required to use ITER with maximum efficiency. Further, the use of tritium as the plasma fuel will entail large-scale tritium handling related to fueling, exhaust, and blanket operation and will be a major challenge for safety and environmental acceptability. Many aspects of this technology are being developed in collaboration with Japan at the Tritium System Test Assembly at Los Alamos National Laboratory.

5. Heating and current drive technologies - This is a key requirement for steady-state tokamak fusion reactors. Reasonable efficiency is needed in order to keep the recirculating power low in a practical system.

6. Control and diagnostics - The challenge is to perform ever-more complex diagnostics and control in the environment of an operating reactor.

7. Remote maintenance capability and achievement of high system availability - This will greatly influence the ultimate maintainability and economics of a reactor.

8. Fueling - Fueling systems are needed to inject deuterium and tritium in a manner which minimizes unfavorable wall interactions and helps control the density profile.

The systems integration and demonstration of these engineering accomplishments with due regard for environmental, safety, and health goals is a key mission of ITER. Successful operation of ITER plus collateral progress in concept improvement and lower-activation materials and blanket development will provide the basis for construction of a DEMO.

IV. PROGRAM ELEMENTS

1. Deuterium-Tritium Operation in TFTR

The demonstration of significant fusion power production comparable to the externally applied plasma heating power is a long-standing goal of the fusion program. To meet this goal the Tokamak Fusion Test Reactor (TFTR) experiment at the Princeton Plasma Physics Laboratory (PPPL) will be operated with tritium in FY 1994. The experiment should generate in excess of ten megawatts of fusion power with the ratio of fusion energy output to heating power (called Q), exceeding 0.3. Operation with tritium will also provide important engineering and safety experience with a large neutron-emitting fusion system. Meeting these objectives will significantly boost public awareness of the potential and practicality of fusion power.

Important scientific advances will result from operating TFTR with tritium. The physics of a D-T burning plasma containing a significant population of 3.5 million electron volt (MeV) alpha particles generated by fusion reactions represents a key technical issue for the fusion program. For example, possible instabilities caused by collective alpha-particle effects will be studied in TFTR and the results combined with theory to provide guidance to ITER. Under optimum conditions with Q ~ 0.5, the first signs of alpha heating in the center of TFTR plasmas should be detectable. A further issue is whether the high-confinement enhancement, large bootstrap fraction conditions achieved in deuterium in TFTR will be altered in D-T plasmas.

The only approved experiments prior to ITER capable of using tritium and studying alpha-particle physics are the TFTR in this country and the Joint European Torus (JET) in Europe. A further contribution of these experiments will be the demonstration of tritium technology in a fusion experiment that meets the full ES&H requirements. Both because alpha-particle physics is largely unexplored experimentally and because many technical issues must be addressed in operating with tritium (e.g., tritium handling, neutron emissions that interfere with diagnostics, induced radioactivity that interferes with maintenance), these two facilities represent a reasonable world-wide effort to advance our understanding of D-T physics and to prepare for operation of ITER. TFTR operation with tritium remains the highest near-term priority for the U.S. fusion experimental program. (See also the FEAC letter to DOE dated April 1, 1992.)

2. International Thermonuclear Experimental Reactor (ITER)

ITER was catalyzed by the call of government leaders in Summit Meetings held in 1985 for more substantial international cooperation in order to increase the efficiency and minimize the cost of fusion power development. There followed a three-year Conceptual Design Activities (CDA) phase in which experts from the United States, the European Community, Japan and the Soviet Union developed a conceptual design for a large tokamak (roughly twice the size of JET in linear dimension) capable of producing a substantial amount of fusion power (about 1000 megawatts) for prolonged periods of time (ranging from a pulse of a few hundred seconds duration to steady-state operation.)

At the conclusion of the CDA, the parties agreed to pursue further the design of such a device through a detailed engineering design, and a formal agreement was signed in July, 1992 committing the four parties (with Russia replacing the Soviet Union) to the six-year Engineering Design Activities (EDA) phase. Support of the EDA, which includes a substantial R&D program necessary to validate the design, will be a cornerstone of the U.S. fusion effort during the next six years. (See FEAC letter to DOE dated February 14, 1992.) The actual construction of the ITER device, although not committed to at this time, will play a pivotal role in enabling the U.S. program to undertake a demonstration reactor step in the second quarter of the next century.

The programmatic objective of ITER is to demonstrate scientific and technological feasibility of fusion power by initially demonstrating controlled ignition and extended burn in D-T plasmas. ITER would then proceed to use the neutrons produced as a result of D-T fusion reactions to test fusion power components. The testing of blanket modules would come first, followed possibly by a second stage involving the introduction of a full or partial blanket where useful heat and new tritium fuel is produced. The ITER machine will use large superconducting magnets and require high-heat-flux plasma-facing components that will be typical of the systems required for a DEMO. ITER will thus provide the first opportunity for integrated testing of key fusion technologies.

In the absence of the Burning Plasma Experiment (BPX) that was canceled by DOE in FY 1992, ITER will be the first facility capable of addressing the scientific issues of vigorously burning plasmas. ITER will also be able to study critical questions that arise in burning plasmas on longer time scales. Examples of such key issues include burning plasma thermal stability, and long-pulse divertor operation including helium ash removal. Thus ITER will permit a more comprehensive study of burning plasma behavior than would have been possible in BPX, but such studies will come later in time and in a more expensive device. (See FEAC letter to DOE dated February 14, 1992.)

While the technical challenges that confront the design of ITER during the EDA are substantial (especially in the areas of superconducting magnets and high heat flux components), the political challenge to reach a construction agreement may be even greater. A key element in the decision to construct will be site selection, a process that must begin soon. In order to be prepared for this, the U.S. and its ITER partners must begin early in the EDA to identify and evaluate candidate sites. It is in the best interests of the U.S. fusion program, particularly from the viewpoint of preparing industry to become more significant participants, to make every effort to site ITER in the U.S. Consequently, a site selection activity aimed at identifying an attractive U.S. site for ITER and for fusion development should begin as soon as practicable. (See FEAC letter to DOE dated February 14, 1992.) It should be borne in mind that the costs of being host to ITER may be substantially higher than those of U.S. participation at a foreign site and that ITER construction will require substantial line-item funding.

3. Materials Development

For a D-T burning reactor to demonstrate that fusion power can be safe, environmentally attractive and economically competitive, new advanced materials with low neutron-activation properties must be developed. Inside the shield of a fusion reactor will be many materials with different functions and required properties. (Examples include structural materials, tritium-breeding materials, ceramics, and specialized materials for diagnostic and control systems.) These materials must retain their required mechanical, physical and/or electrical properties at elevated temperatures and while being exposed to high fluxes and lifetime fluences of 14-MeV neutrons.

Good progress has been made in the initial development of advanced materials using lower energy neutrons of fission reactors. However, in nearly every instance, we know that the neutron spectra in fission reactors provides an incomplete simulation or approximation of the fusion neutron environment. Using fission reactors, one can eliminate some materials from consideration and can carry out the initial stages of development for other materials; however, one cannot complete the development nor can one demonstrate the acceptable performance of any advanced material. This task must be carried out in a neutron spectrum that produces atomic displacements and transmutations (particularly helium and hydrogen) in amounts typical of a fusion neutron spectrum.

Judging from the experience in the fission area, the time scale to develop a new material to the point of production and qualification for design and service is measured in decades. Therefore, it is essential that a high-flux fusion neutron source for materials testing and development be built soon. Various approaches will be discussed by FEAC Panel 6.

The development of plasma facing components to handle the harsh plasma environment is underway now in experiments and specialized test facilities. Larger scale, steady-state testing of such components is needed in preparation for ITER.

4. The DIII-D Tokamak

During the past several years, the DIII-D tokamak at General Atomics Company in San Diego has been an extremely productive fusion experiment. Some of the high-impact results obtained during this period include: achievement of volume-averaged beta (the ratio of plasma pressure to magnetic field energy density) of 11%, and a peak value of 44%, well in excess of the corresponding values required for a reactor; characterization of H-mode plasma energy confinement and associated plasma edge-phenomena; discovery of the VH-mode, an operating regime with plasma energy confinement properties further improved over the standard H-mode; and development of methods for reducing the plasma heat loads to component divertor plates and enhancing divertor pumping capabilities. In carrying out its program, DIII-D has already made substantial contributions to the physics R&D required to support the ITER design. The advanced plasma-shaping capability, including the possibility to form a modern magnetic divertor configuration, will enable DIII-D to continue to be relevant to ITER physics R&D for some years. A strong DIII-D program has been endorsed previously by FEAC in it's letter to DOE dated April 1, 1992.

The DIII-D program supports both ITER and the development of an optimized tokamak. A program goal for DIII-D is to achieve simultaneously high confinement and high beta relative to H-mode and MHD (Troyon) beta limits, respectively. Theoretically, such tokamak operation is possible, provided one can control the plasma pressure profile and the combined profile of the plasma-driven bootstrap and externally driven current by non-inductive means. An additional key program element is an improved divertor in which the heat flowing from the main plasma is spread over a large fraction of the divertor surface area, while impurities generated by interaction of plasma with the divertor plates are effectively restrained from flowing back to the core plasma. Also, the helium ash must be efficiently pumped.

A number of subsystem upgrades for DIII-D are needed. These include: an advanced divertor; additional power supply and cooling capability; neutral-beam enhancements to extend the pulse length to 10 s; increases in RF power capability to 8 MW in the ion-cyclotron frequency range; and implementation of 10 MW of heating at electron cyclotron frequencies.

In the near term (FY93-FY95), the DIII-D program will be focused on developing individual elements important for an optimized tokamak. The emphasis will be on clarifying the properties of a biased divertor and on developing fast-wave current drive. Work will also continue on theory and model development needed to understand the physics of divertors. A new optimized divertor will be designed and constructed.

In the longer term (FY96-FY98), the emphasis of the DIII-D program will shift to integrated demonstration of optimized tokamak operation. One specific goal is to produce high performance, non-inductively sustained plasmas which are TPX- and ITER-relevant for pulse lengths of at least 10 s, with a beta value of 5% and a plasma current of 2 MA. Under these conditions, the goal of the advanced divertor is to demonstrate a reduction of peak heat loads by a factor 10 relative to conventional design and also to show efficient pumping of helium. A second goal is to demonstrate high-performance, advanced tokamak plasma operating modes which have efficient current drive. Such operation is appropriate for a DEMO or an advanced tokamak physics experiment (TPX) with extended pulse lengths of 1000 s or more.

5. The Advanced Toroidal Facility

The Advanced Toroidal Facility (ATF) at the Oak Ridge National Laboratory is a unique facility in the world stellarator program as a result of its configurational flexibility. Its near term program will be aimed at demonstrating a volume average beta of 4% or more, with confinement enhanced above the present levels, for pulses of 20 seconds duration. ATF will be able to make significant and timely contributions to developing scientific understanding and demonstrating stellarator capabilities in the period before the next generation of larger stellarators comes into operation in the late 1990's: the LHD stellarator in Japan and the proposed W VII-X stellarator in Germany.

ATF was mothballed in FY 1992, after a productive three-year period of operation, following sharp budget cuts which led to the DOE decision to focus narrowly on tokamaks. With minor refurbishing, ATF could be restarted in FY 1994, with full operation in FY 1995.

In a stellarator the magnetic field needed to confine the plasma is produced by currents in external coils. Consequently, the configuration simultaneously overcomes two tokamak shortcomings that might limit the tokamak as a reactor confinement concept: lack of an efficient steady-state scenario and the occurrence of disruptions, the tendency of current-induced instabilities to terminate the discharge suddenly. Thus the stellarator is viewed by many as the primary backup to the tokamak in the event that these issues prevent the tokamak from developing into an attractive and/or economic reactor. (See the FEAC letter to DOE dated June 12, 1992.)

The confining magnetic fields in a stellarator break the toroidal symmetry possessed by a tokamak. As such, questions arise concerning the effectiveness of plasma confinement in a stellarator. In present stellarators, the plasma energy confinement is comparable with similar-scale tokamaks operating in the so-called L-mode of operation. Evidence is emerging that confinement enhanced over stellarator

L-mode operation is possible. Operation at higher temperatures and lower collisionalities is required to establish the reactor relevance of the stellarator. A related requirement is for stellarators to operate at more than 4% volume-averaged beta. The ATF device, with a modest heating upgrade, should be capable of tackling both of these goals during a three-year operational phase following the restart phase.

6. The PBX-M Tokamak

As part of the Advanced Toroidal Program, the PBX-M tokamak at the Princeton Plasma Physics Laboratory (PPPL) is aimed at exploring two physics elements important to developing an attractive tokamak reactor: a plasma operating regime characterized by high beta, high confinement, and high self-driven currents; and avoidance of disruptions. PBX-M has already operated in advanced tokamak regimes with high confinement (about three times L-mode scaling) and high beta.

PBX-M is uniquely dedicated to the mission of advanced tokamak physics studies relating to second MHD stability and high beta achieved through current profile control and through strong inboard indenting (up to 30%) of the plasma ("bean-shaping"). The PBX-M aspect ratio (the ratio of major to minor radii of the plasma torus) of 5.5 is the highest of presently operating tokamaks. High aspect ratio is expected to make easier the attainment of second stability, the generation of bootstrap current, and of improved plasma heat and particle confinement. A flexible lower hybrid current drive system on PBX-M provides localized power deposition to be used as a tool for plasma profile control. Thus PBX-M will be able to verify theoretical predictions of the boundary for second-stable operation as a function of plasma triangularity and safety factor. A close-fitting conducting wall provides for stabilization of MHD fluid-like instabilities such as the external kink mode. In recent studies (namely, the ARIES reactor study), the regimes which PBX-M attempts to attain and investigate have been identified as highly desirable and even necessary for an attractive tokamak reactor. However, the specific configuration and geometry in which these regimes are being addressed in PBX-M may not be directly transferable to a DEMO reactor.

Limitations of present PBX-M capabilities include a modest plasma current by standards of larger tokamaks, and the restriction of its pulse length to 1-3 seconds.

The completion of the PBX-M program will require a number of modifications and improvements in order to test long-pulse, disruption-free operation. The improvements include increasing the toroidal field from 1.5 T to 2 T, extending auxiliary heating in power and pulse duration, an upgrade of the divertor, and improved diagnostics.

7. The Alcator C-Mod Tokamak

The purpose of Alcator C-MOD at the Massachusetts Institute of Technology (MIT) is to address a range of critical tokamak issues, including power and particle handling, control, enhanced transport and RF heating and current drive. A main focus of the program during the first 3-5 years of operation will be the study of high-heat flux divertor concepts, with special emphasis on the problems of ITER. The 4 MW of ICRF heating corresponds to a surface-averaged power density of 0.5 MW/m^2, which equals or exceeds the ITER requirements. The initial approach employs reactor-relevant metallic plasma-facing components, and addresses the high-recycling radiative and gaseous target modes of divertor operation using an inclined plate, closed, re-entrant divertor geometry.

The high magnetic field (9T) and strong shaping (K = 1.8) of Alcator C-MOD result in plasma currents up to 3 MA, projecting to significant plasma performance in terms of $n\tau T$. At B = 5T, the 7 second pulse length corresponds to about 10 skin times. Such long-pulse operation will be extensively employed in advanced tokamak and non-inductively sustained experiments which will dominate the later years of the program. RF current drive will be used for current profile control as well as for bulk current generation. High bootstrap current regimes will be studied both in the first-stability regime plasma at lower beta, and at higher beta, in the second-stability regime. The ultimate goal of the C-MOD program is the demonstration of the combination of essential features of an attractive tokamak reactor, namely high confinement, non-inductively sustained current, low impurity content, and reactor-relevant divertor power density.

Alcator C-MOD is expected to resume operation early in FY 1993. Full-field capability and 4 MW of ICRF power will be available by FY 1994. An additional 2 - 4 MW of ICRF power and 4 MW of lower hybrid current drive power are planned for succeeding years. In addition to divertor and disruption studies of importance to ITER, the first 3-5 years of the program include studies of enhanced confinement and tests of dimensionless transport scaling.

8. Advanced Tokamak Physics Experiment (TPX)

Following the SEAB Task Force recommendation, a year-long study by a National Fusion Task Force concluded that a key facility with adequate capabilities for essential progress towards the DEMO could indeed be built within the budget guidelines suggested. A new, advanced tokamak physics device referred to as the TPX has been designed and proposed as a national facility to be located at the Princeton Plasma Physics Laboratory. (See FEAC letter to DOE dated April 1, 1992.) The mission of the Tokamak Physics Experiment/Steady-State Advanced Tokamak (TPX) is to extend the tokamak concept to the steady-state regime, achieving pulse lengths of at least 1000 seconds. The study of steady-state fueling and confinement properties of hydrogen, helium, and impurities is essential. Another key purpose of TPX is to pursue advances in tokamak performance that are predicted when there is full control of the plasma current profile (which require time-scales much longer than the global resistive skin time), and to explore tokamak configurations and operating regimes that are predicted to lead to an improved tokamak reactor. Recent tokamak experimental results suggest that confinement and beta-limits can be increased via current profile control. Advances in these areas would permit a tokamak reactor to be more economic and smaller in unit size, key attractive features to industry for a power source. Measurements of the "bootstrap current" in existing tokamaks confirm theoretical predictions and point to regimes with large self-driven toroidal current. This would in turn allow a steady-state reactor with a low level of recirculating power as embodied in recent reactor studies such as ARIES. Advanced divertor concepts are also needed to permit steady-state non-inductive current drive at acceptable values of plasma temperature and density. The very long pulses and high duty factor of TPX will be useful in qualifying new divertor designs for reactor application. Finally, reliable techniques of disruption control are required to achieve the availability goals of an economic fusion reactor. The TPX is designed to address all of these key reactor issues in one device, employing reactor-relevant remote maintenance and superconducting magnet technologies. A steady-state tokamak experiment would also make important contributions to the later phase of ITER, in which steady-state and high-duty-factor operation will play an increased role. (See the FEAC letter to DOE dated April 1, 1992.)

9. A Program for Innovative Fusion Concepts

In the FEAC letter to DOE dated June 12, 1992, the FEAC recommended that even as the US fusion program implements a goal-oriented program strategy, the program should encourage innovative ideas. In addition to the innovation encouraged by the existing APP program, FEAC recommended that a small but structured and highly visible periodic competition be established to foster new concepts and ideas that, if verified, would make a significant improvement in the attractiveness of fusion reactors. Predefined sunset clauses would help ensure that funds for new ideas were available on a periodic basis. The ideas to be funded might relate to improving aspects of the tokamak or other established confinement concepts, or to proposals from individuals and institutions that are not now part of the primary program activities. Priority should be given to testing scientifically well-founded concepts at the small-scale, proof-of-principle level.

10. Technology Development for Fusion

The Development and Technology (D&T) part of the fusion program fills three important functions: (1) supporting ITER; (2) providing enabling technology for fusion machines; and (3) contributing directly to the development of a DEMO.

Technology research and development in direct support of the ITER project now constitutes a large fraction of the total effort in the U.S. on fusion technology R&D. However, the tasks undertaken in the U.S. in response to specific ITER requirements need to be augmented by a domestic base program effort to result in a balanced program for a DEMO. (Again, see the FEAC letter dated February 14, 1992.) In some areas, task sharing among the international partners in ITER is likely to result in a low level of U.S. participation; augmentation via U.S. base program research will be necessary to maintain sufficient expertise to work on specific problems of special interest to the U.S. program (such as low-activation materials development), and to maintain an adequate level of participation by U.S. industry.

Technology R&D will continue to play a vital role in providing the enabling technologies needed for plasma experiments. Emphasis currently is on meeting the requirements posed by plasma devices with longer pulse length and higher input heating power. These devices require highly efficient heating and fueling methods, special first-wall and high-heat-flux component materials, and high reliability of all components. Technology topics covered by the program are far-ranging and reflect the breadth of technology requirements for fusion energy development. Examples of key fusion technologies include normal and superconducting magnets, plasma-facing high heat flux components, auxiliary plasma heating using neutral beam and radiofrequency power, plasma fueling exhaust and fuel cycle, vacuum systems, containment structures, and remote handling. Fusion nuclear technology includes blanket components to recover the heat of fusion and to produce new tritium fuel, neutron shielding, and materials development. Depending upon the timing and scope of the ITER blanket test program, development of these components may require a plasma-based, large-volume, fusion neutron source. Safe and environmentally attractive designs are central to fusion energy development and this area is tied closely to the programs on tritium technology and low activation materials development. Increased emphasis will need to be given to the development of methods and codes for the prediction of fusion reactor accident consequences and to accident avoidance or mitigation.

Directly related to the definition of a DEMO are reactor systems studies which develop design approaches for attractive power reactors based on varying levels of extrapolation from current physics and technology. Such research helps to clarify the role of intermediate steps and facilities such as ITER, and identifies specific long-range research priorities. Industry involvement in this area is particularly important, both to provide a realistic perspective from industry on the process of developing fusion power and to give industry the best insight into fusion technology at an early stage of development. The D&T program also addresses key long-term technologies required for an attractive fusion reactor including the development of tritium-breeder, heat-recovery blanket systems and the development of low activation materials. (See the FEAC letter to DOE dated February 14, 1992.)

11. Applied Plasma Physics for Fusion

The program elements grouped into the category of Applied Plasma Physics encompass various activities in experimental plasma physics, in plasma theory, and in operation of a national computer center. Applied Plasma Physics activities are aimed at supporting the strategic goal of the fusion program in several ways: (1) promoting fundamental understanding; (2) encouraging innovations; and (3) providing for continuing development of skilled personnel.

An "understanding" of fundamental fusion plasma physics would permit the prediction of the performance of plasma systems for given sets of specifications. Advances in fundamental fusion plasma physics are brought about by developing theory to support all magnetic fusion concepts; by operating specialized, modest-scale experiments having a flexibility not available on the largest machines; and by developing large-scale computing capabilities for prediction, design, and data analysis. With improved predictive capability comes also the ability to control plasma behavior—this capability will ultimately be applied to a functioning reactor core, such as in a DEMO.

Examples of activities supporting the objective of improved fundamental understanding are studies of transport physics through experimentation on devices such as the CCT tokamak at the University of California, Los Angeles and the TEXT tokamak at the University of Texas-Austin, through the fielding of specialized diagnostics on the large tokamaks, TFTR and DIII-D, and through efforts on theory and modeling. A new, national initiative referred to as the "Numerical Tokamak" will advance basic plasma transport studies, code development, and visualization. Other activities include Alfvén wave heating and current drive studies conducted on the Phaedrus tokamak at the University of Wisconsin, Madison, on the TEXT tokamak at the University of Texas, Austin, and on the CDX-U machine at PPPL. All of these activities have supporting theory and modeling research. Other theoretical efforts include development of non-linear theories, plasma edge and divertor modeling and work on stability theory relating to burning plasmas and alpha particles.

Innovation is sought through the development of new reactor concepts, new methods to control plasma properties, and new diagnostic techniques. Experiments provide physics tests of new fusion concepts and of ideas for tokamak improvement such as current drive. Examples are small-scale experiments on reversed field pinch configurations such as the MST device at Wisconsin (a collaboration of Wisconsin, SAIC, and LANL), stellarator experiments at Wisconsin (involving Wisconsin, Auburn, and William and Mary), and a field-reversed configuration experiment (involving the University of Washington and Spectra Technologies Inc.).

Tokamak improvement research includes current drive studies, current profile control, disruption control, improvement in MHD beta limits, divertor experiments, configuration modifications, and fueling studies. Examples include: Alfven wave heating and current drive studies conducted on the Phaedrus tokamak at the University of Wisconsin, Madison, on the TEXT tokamak, and on the CDX-U machine at PPPL; MHD mode control and beta limit studies on the HBT-EP tokamak at Columbia University; and plasma gun fueling studies on Phaedrus-T in collaboration with CalTech.

An important part of the support of innovations is the development and implementation of new diagnostics, such as those to measure alpha distributions, current profiles, radiation resistance, neutron and gamma fluxes, and plasma fluctuations.

Underlying all activities is the objective of training of personnel. The Applied Plasma Physics program is well suited to this task because there is extensive university participation, there are grants of fellowships, and there is support of many international exchanges.

Appendix A

Letter of Charge

Department of Energy
Washington, DC 20585

June 22, 1992

Dr. Robert W. Conn
Chairman, Fusion Energy
Advisory Committee
University of California, Los Angeles
Los Angeles, CA 90024-1597

Dear Bob:

The Fusion Energy Advisory Committee (FEAC) has now reviewed and reported on the primary elements of the magnetic fusion program. Given that background, it would be quite helpful if FEAC would provide recommendations on strategic program planning. Please provide your views for three different out-year funding assumptions: starting with the FY 1993 House Appropriation Mark of $331M for magnetic fusion, (A) 5 percent real growth; (B) level funding, i.e., with only inflation; (C) flat, without inflation. Of course, the FY 1993 budget process is still incomplete, and I will revise this guidance if we have better figures before you meet.

Within these assumed cases, which program elements should be enhanced, protected, reduced, or eliminated and on what schedule? In all cases the primary goal should be maximum progress toward a Demonstration Power Plant. I am asking for your best technical judgment on how to structure the magnetic fusion program within these different funding assumptions, but without change in the basic goal of demonstrating fusion power and within the basic assumption of strong international collaboration.

Please provide your recommendations by the end of September 1992. I know that all FEAC members have worked intensely to develop your recommendations on the individual program elements in my first set of charges. Therefore, I believe it is most useful to take this overview now while the contextual information is fresh. I realize that this will require additional dedication on top of your already extensive labors. I do appreciate your efforts.

Sincerely,

William Happer

William Happer
Director
Office of Energy Research

Appendix B

Membership of FEAC

Membership of FEAC

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Dr. Peter Staudhammer
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Dr. Harold Weitzner
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New York, NY 10012

Appendix C

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Members of FEAC Panel 5

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* Member of FEAC

Appendix D

Expanded Terms of Reference

MEMORANDUM

To: Members, FEAC Panel #5 on Magnetic Fusion Energy Strategy
From: R. W. Conn RW Conn
Re: Panel #5 Summer Retreat - Charge; Approach; Schedule
Date: July 20, 1992

The letter from Will Happer dated June 22, 1992 is the Charge for our FEAC panel workshop. Since then, I have worked with John Sheffield to develop an approach for our week together and had feedback from you on a first draft of this memo. I urge you to review the earlier FEAC letters and Panel reports.

I. Charge

Our main objective for the week is to establish priorities within the fusion program among the major program elements and their components. In other words, our objective is to provide for consideration by FEAC "recommendations on strategic program planning." Will Happer asks what to enhance, protect, reduce, or eliminate, and on what schedule. He asks us to retain "the basic goal of demonstrating fusion power" and the utilization of strong international collaboration. In each case, "the primary goal should be maximum progress toward a Demonstration Power Plant."

The three budget levels are:

a. 5% Real Growth (Upper Budget Case)
b. Inflation Only (Middle Budget Case)
c. Flat, No Inflation (Lower Budget Case)

If we want to consider any other scenario, we can discuss this on the first day. Also, I suggest we consider these budget cases for the years FY93 through FY97.

We can consider the program as being composed of Elements and Components:

Elements: Major program areas and/or major facilities.
Components: Sub-elements of a given element.

As specific examples of existing Elements, one can take ITER, TFTR, and DIII-D; confinement experiments in the $10-30 million/year range; technology and diagnostics; plasma experiments; theory and computing. Each team, and then Panel 5 as a whole, should agree on the major Elements and their Components. Of course, we should also define elements that may not yet exist, such as SSAT or a fusion nuclear facility or a 14 MeV neutron source, etc. Recall that Elements may have several key Components - e.g. within the Element "Confinement Experiments" are the Components PBX, Alc.-C/Mod and ATF.

We can also ask questions to sharpen our thinking for each budget case. Some example questions might be:

> What priority is given to the ITER design process?
> What priority is given to the construction of ITER?
> What is the impact of ITER construction going ahead, or not, in 1997?
> What priority is given to construction of TPX?
> What priority is given to full D-T in TFTR?
> What priority is given to technology and materials development for fusion?
> What "breadth" should there be in the confinement program?

and so on, for all the Elements of the program. Please construct and put forward any other questions of this type.

Finally, we should keep in mind the key question - **WHERE DO WE WANT TO BE IN THE YEAR 2000?** WHAT MAJOR ELEMENTS OR PROGRAMS OR FACILITIES DO WE WANT TO HAVE? WHAT ELEMENTS OR PROGRAMS OR FACILITIES WILL BE GONE - AND BY WHEN? WHAT ARE THE ANSWERS TO THESE QUESTIONS FOR EACH BUDGET CASE?

II. Approach

The following approach is proposed for our week's work:

i) Hear from DOE a repeat of the charge and any additional guidance and constraints.
ii) Ask the OFE Program Directors for APP, Confinement, ITER, and D&T to present the elements and budgets for each area - and call out international activities - to provide us with a benchmark. They will also inform us of recurring costs.
iii) Hear a presentation on the actual out-year budgets for the 3 budget cases, again as a reference. (To be presented by John Sheffield).
iv) Hear an interim report from Panel 4.
v) Hear a report on the EPRI meeting of July 6-8.
vi) Break up into 3 separate teams. Each team is to address the Charge for the 3 budget cases and develop their recommendations for a strategic program plan, with clear priorities - what to enhance, protect, reduce, or eliminate - and on what schedule.
vii) Reconvene to hear reports from the three teams.
viii) Accept the points that are common and work to get agreement by the panel in areas where the teams differ.
ix) Draft recommendations of the special panel.

III. <u>Schedule and Timing</u>

The schedule and agenda we will follow is attached. There will be morning (8:30 - 12:30) and early evening (4:00 - 7:00) sessions. This should leave time for discussion and "reflection." It will also provide time if some work is needed between sessions.

As you can see, we will spend Day 1 on items i through v. Day 2 and Day 3 will be devoted to Team deliberations (item vi) and then to reports from the Teams (item vii). Days 4 and 5 will be devoted to reaching common agreements and draft recommendations (points viii and ix).

IV. Teams

Smaller teams working in parallel will, in my view, make it more efficient to address all the issues rather than for us to operate as a whole throughout the week. I suggest that the team leaders be drawn from the chairs and co-chairs of the panels which analysed specific topics for FEAC. There is also the thought to add a very few (2-4) other persons to the members of this special FEAC panel in order to balance the technical knowledge base of the panel. A suggestion for the teams is:

Team 1	Team 2	Team 3
Baldwin, Chair	Sheffield, Chair	Weitzner, Chair
Ripin, V. Chair	Siemon, V. Chair	Dean, V. Chair
Holdren	McCrory	Berkner
Parker	Rosenbluth	Davidson
Navratil	Staudhammer	Overskei
DeFreece	Gauster	Baker

V. Attendees at the FEAC Panel 5 Meeting

A list of attendees and panel members is attached.

Attendees at the FEAC Panel 5 Meeting

Panel Members

Charles Baker	ORNL
David Baldwin	LLNL
Klaus Berkner	Lawrence Berkeley Laboratory
Robert W. Conn (Chair)	IPFR/UCLA
Ronald C. Davidson	PPPL
Stephen O. Dean	Fusion Power Associates
Dale DeFreece	McDonnell Douglas
Wil Gauster	Sandia NL, Albuquerque
Alex Glass	ITER/LLNL
John P. Holdren	UC Berkeley
Robert L. McCrory	University of Rochester
Gerald Navratil	Columbia University
David O. Overskei	General Atomics
Ronald R. Parker	PFC/MIT
Barrett H. Ripin	Naval Research Laboratory
Marshall N. Rosenbluth	UCSD
John Sheffield	ORNL
Richard Siemon	LANL
Peter Staudhammer	TRW
Harold Weitzner	New York University

DOE Personnel

David Crandall
Anne Davies
James Decker
Tom James
Warren Marton
John Willis

Panel Support Staff

Terry Davies,	IPFR/UCLA
Claudette Duncan	LANL
Debbie Lonsdale	DOE, Germantown

FEAC Panel #5 on Magnetic Fusion Energy Strategy
Summer Workshop
Crested Butte, CO.
July 27-31, 1992

AGENDA

Monday, July 27	**Introductory Day**
9:00-10:15 am	Orientation Session - Conn, Davies, Decker (Happer) Analysis of Will Happer's charge letter Discussion of objectives and basic strategy
10:15-10:30 am	Break
10:30-12:20 pm	DOE up-date - Davies, Decker (30 mins) APP program - Crandall (40 mins) ITER program- James (40 mins)
12:20- 3:50 pm	At leisure
3:50- 5:30 pm	Confinement program - Willis (40 mins) Technology program - Marton (40 mins) Review of July 1992 EPRI Meeting - Dean (20 mins)
5:30- 5:45 pm	Break
5:45- 7:00 pm	Interim Report - Panel #4 - Baldwin (75 Mins)
Tuesday, July 28	**Team Strategy Day**
8:30-10:30 am	Individual team sessions
10:30-10:45 am	Break
10:45-12:30 pm	Individual team sessions
12:30- 4:00 pm	At leisure
4:00- 5:30 pm	Individual team sessions
5:30- 5:45 pm	Break
5:45- 7:00 pm	Individual team sessions
Wednesday, July 29	**Team Strategy Morning/ Team Reports Evening**
8:30-10:30 am	Individual team sessions
10:30-10:45 am	Break
10:45-12:30 pm	Individual team sessions
12:30- 4:00 pm	At leisure
4:00- 4:45 pm	Report of Team #1
4:45- 5:30 pm	Report of Team #2
5:30- 5:45 pm	Break
5:45- 6:30 pm	Report of Team #3
6:30- 7:00 pm	Discussion

Thursday, July 30	**Development of Panel Strategy**
8:30-10:30 am	Budget scenario A: - Comparison of Team Approaches
10:30-10:45 am	Break
10:45-12:30 pm	Budget scenario B: - Comparison of Team Approaches
12:30- 4:00 pm	At leisure
4:00- 5:30 pm	Budget scenario C: - Comparison of Team Approaches
5:30- 5:45 pm	Break
5:45- 7:00 pm	Continuation, if needed, of discussion on open points or development of recommendations
Friday, July 31	**Development of Panel Recommendations**
8:30-10:30 am	Development of recommendations
10:30-10:45 am	Break
10:45-12:00 noon	Continued development of recommendations
12:00- noon	Finish

Appendix E

Earlier Charges to FEAC

1. Letter of September 24, 1991
2. Letter of February 20, 1992

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

William Happer
Director
Office of Energy Research

Department of Energy
Washington, DC 20585

February 20, 1992

Dr. Robert W. Conn
Chairman, Fusion Energy
Advisory Committee
University of California, Los Angeles
6291 Boelter Hall
Mechanical, Aerospace, and Nuclear
Engineering Department
Los Angeles, CA 90024-1597

Dear Dr. Conn:

I am writing to expand on the portion of the charge you received September 24, 1991, regarding concept improvement. Specifically, that charge asked "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." I understand that you discussed this charge element at your meeting on February 6 in California, forming a panel (#3) to develop information and requesting some points of clarification from DOE. I further understand that possible major program elements which address tokamak improvement, such as TPX and the ATF/PBX-M facilities, are already well along in your review process through Panel 2.

Given that tokamak reactor development will be the primary focus of the U.S. magnetic fusion program, it is reasonable to ask what activities are appropriate on non-tokamak concepts and on small-scale exploration of tokamak improvements. There are a number of ideas on alternate concepts and tokamak improvements, and the exploration of these ideas has historically added richness and innovation to magnetic-fusion development. It would be useful if you could recommend a policy and selection criteria to help guide our program choices on concept improvements within our goal-oriented program strategy. The overall policy question is whether, given the demands of the mainline tokamak program and current budget constraints, we should encourage and fund proposals on concepts other than tokamaks.

Within the concept improvements area, what priorities should be given to exploratory tokamak improvement proposals, like the compact toroid fueling and helicity current drive that are now under small scale investigation? Should the priority be higher for U.S. alternate concept activities that connect to major significant international programs or for unique U.S. activities? Under what conditions and within what criteria should concepts that have little connection to tokamaks, or to other major international programs, be considered?

I know that these issues are of intense interest to some members of the U.S. fusion community. It is important to have your best judgment on these questions within the context of overall magnetic fusion program goals, strategies, and funding constraints.

Sincerely,

William Happer

William Happer
Director
Office of Energy Research

Appendix F

Earlier FEAC Letters of Advice to DOE

1. Letter of October 7, 1991
2. Letter of February 14, 1992
3. Letter of April 1, 1992
4. Letter of June 12, 1992

UNIVERSITY OF CALIFORNIA, LOS ANGELES UCLA

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO SANTA BARBARA • SANTA CRUZ

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October 7, 1991

Dr. William Happer, Director
Office of Energy Research (ER-1)
U.S. Department of Energy
Washington D.C. 20545

Dear Will:

This letter is the response of the Fusion Energy Advisory Committee to the charge you provided to us on Sept. 25th. As you know, the Committee heard presentations and held extensive discussions, for two days, before arriving at this initial response.

First, we wish to reaffirm that the preferred fusion program strategy and time table are those outlined in the National Energy Strategy and given in the report of the Fusion Policy Advisory Committee. This strategy would permit a vigorous base program, a revitalized national effort focussed on the physics of burning plasmas in BPX, and strong participation by the U.S. in the ITER EDA phase. The design developed by the BPX team is ready now to proceed to construction. Implementing such a program would require, in the time period FY '93 - FY '97, real funding growth in the range of 10 - 15% per year. The FEAC encourages the Department to continue to consider this option. It is the option that best permits the U.S. to achieve the goals outlined in the National Energy Strategy. Options involving lower levels of funding for several years will lengthen the schedule in the NES for fusion energy development and probably increase the total cost. In particular, the target date of 2025 for a demonstration reactor is jeopardized by lower budgets, as is the near-term involvement of industry.

Given the stringent budget circumstances outlined in your charge to the committee, the FEAC considered options for BPX other than not going forward with the current baseline program. These included options for BPX that were somewhat reduced in cost and extended in schedule, which will reduce funding outlays in the near term. We analyzed in particular the two funding scenarios discussed in your charge, namely, a constant level of effort case and a 5% real annual budget growth case. In either case, it does not appear possible to proceed with the construction of the BPX without either diminishing its mission or timeliness, or severely affecting the important core programs which remain.

For the case of constant level of effort budget, the FEAC is concerned that the available resources, even without the BPX, are too low. This case would not allow room for an alternative new experiment to BPX unless funds are taken from those now planned to support ITER, TFTR, and the physics and technology base activities. FEAC does not recommend that the DOE extract further funds from existing programs, given the reductions and cancellations that have already occurred.

Therefore, of your two possible budget cases, the FEAC strongly urges you to adopt, at a minimum, the 5% real annual growth case. This appears also to be the recommendation of the SEAB Task Force.

Even in the case of 5% real annual growth, FEAC concludes that funds are insufficient for even a scaled down BPX program. However, this case would include funds to undertake initiatives that are less expensive than BPX. Such initiatives are critical in order to strengthen and revitalize the U.S. magnetic fusion program, and to permit the U.S. to be a truly effective participant in ITER. Moreover, exciting options exist which, if successfully pursued, would greatly increase the attractiveness of a fusion reactor.

Removing BPX from the fusion program, produces a significant gap in the current DOE program plan. We know this is recognized by you and the Secretary. Immediate attention must be focussed on reformulating this plan. A new strategy without BPX and based on modest real growth requires a strengthening of existing programs. There are critical needs for additional funding to optimally utilize present facilities. Further, immediate action should be taken to initiate planning for modern, affordable, and productive new initiatives. A fusion program whose major facilities are based upon the understanding and perceived needs from 1980 or earlier may not remain viable much longer. While ITER will address burning plasma issues and carry out some significant technology testing and development, the U.S. must plan for and implement a program that will address the remaining key issues of concept improvement, steady-state operation, D-T facility issues , and low activation materials development. Options exist that can be firmly formulated in the next six months and which have clear mission statements. We believe the Department can receive pre-proposals from the fusion program by as early as March, 1992 and decide, at least, on the appropriate mission of one or more new, reduced cost initiatives. This process merits the widest possible input from the fusion community.

Specifically then, if BPX does not go forward, funds currently earmarked for BPX should be used to strengthen already weakened programs and to plan the initiatives to fill the gap between the end of TFTR and the start of operation of an ITER. During the next six to nine months, as the program adjusts to your budget guidance for FY '93 and beyond, it is crucial that the program receive the strong support of the Department. This is needed to insure an orderly adjustment and to maintain program momentum and direction, and to to obtain maximum productivity from the $337M funding level appropriated for fusion for FY '92.

Finally, we address preliminarily another of your specific questions, namely, the impact on the mission and timing of ITER if BPX does not go forward. In the FPAC report, BPX was seen as filling an important niche between TFTR/JET and ITER in developing an early understanding of the physics of burning plasmas. Without BPX, high-Q regimes will be encountered for the first time in ITER. Additional pressure in ITER's burning-plasma phase will come from the need to produce a more complete data-base than would have been required with a BPX. As a result, an extension of the physics phase of ITER by at least 2 years can easily be envisioned, and more time could be required if unforeseen problems develop.

Another impact on ITER of cancelling BPX will be the elimination of pre-ITER experience in large-scale remote handling. Although difficult to quantify, the down time for routine remote maintenance will probably be longer than anticipated, thereby delaying the completion of both the physics and technology phases of ITER.

The loss of BPX may also impact the technology mission of ITER; in particular, it may reduce that part of the mission. Any lessening of the technology role of ITER will affect the mission and scope of the follow-on device, and may preclude a demonstration reactor as the step after ITER. However, this is difficult to determine without further study.

Finally, if there is no BPX, FEAC is concerned that there will be a diminution of the momentum built up by the successful operation of presently operating devices. In the near term, every effort should be made to extract the maximum burning plasma data from TFTR and JET. Further, we recommend that the Department work diligently to shorten the engineering design phase of ITER, and attempt to reach an early agreement on the construction of ITER.

Despite the length of this letter, we are in fact providing only the initial feedback you requested. We have organized ourselves to address by January, 1992, the questions you asked about ITER. By March, 1992, we shall address in more detail the three enumerated questions in your charge, as well as the questions on filling the gap between TFTR and ITER, and on the U.S. concept improvement program. Like you, the FEAC hopes that the overall budget outlook for Energy Research can be improved through the hard work of all of us.

Sincerely,

Bob Conn

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

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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

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April 1, 1992

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

Dear Will:

In your September 24, 1991 Charge to the Fusion Energy Advisory Committee, you asked for advice by March 1992 on the issue: "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". You added, "In addressing this issue, please include consideration of international collaboration, both here and abroad". As background to this request, you stated in your Charge letter that the Task Force of the Secretary of Energy Advisory Board chaired by Professor Townes had recommended that "The DOE not proceed with the Burning Plasma Experiment (BPX)" but "Recommended that the U.S. fusion program continue to grow modestly (even in an ER budget that is declining in constant dollars)". The Charge letter also stated that the Task Force "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".

This letter is our response to your request for advice by March. To prepare our response, we established FEAC Panel II, co-chaired by Drs. David Baldwin and John Sheffield, to provide the full FEAC with information to help us formulate our advice. FEAC received and discussed the Panel II report and used it in formulating its recommendations. Both FEAC and our Panel II were greatly aided by a National Fusion Task Force (NTF), which was chartered by the Princeton Plasma Physics Laboratory (PPPL) to coordinate the activities within the magnetic fusion program, including the work of advocacy groups, to develop options for a new tokamak initiative. The FEAC Panel II and the NTF did extensive work and we greatly appreciate their efforts.

The plan for magnetic fusion energy (MFE) development recommended to DOE by the Fusion Policy Advisory Committee (FPAC) discussed two classes of important tokamak issues that could potentially be addressed in a new facility: "advanced-tokamak physics" and "steady-state". Advanced tokamak physics issues fall into three areas, all of which require confirmation in long-pulse operation:

1. Stable plasma operation at high beta (e.g., in the "second-stability" regime) with enhanced confinement, which will permit a smaller, more attractive power station;

2. Stable operation with a high fraction of self-sustained plasma current ("bootstrap" current), which will permit low recirculating power in a power station; and

3. Successful disruption control, which would improve the availability of a reactor.

Successful resolution of these advanced tokamak issues have been shown in reactor studies both here and abroad to lead to an attractive tokamak power reactor.

The common thread to all these issues is control of the current profile, which must be demonstrated for a time longer than the greatest natural relaxation time scale. Consequently, research on these advanced physics issues fits naturally with studies of "steady-state" issues such as:

1. Plasma power and particle handling, and helium transport and exhaust at reactor conditions;

2. Efficient techniques and technologies to drive the plasma current and to control the plasma current profile.

FEAC and our Panel II agree with the National Fusion Task Force that the investigation of power and particle handling requires pulse lengths at least as long as 1000 seconds, and extending ultimately to steady state. Therefore, the design of a new tokamak experiment should not preclude steady-state operation.

An important conclusion of FEAC is that a long pulse advanced tokamak machine with ultimate steady-state capability can be built for about $500 million in as-spent dollars by making use of the TFTR test cell and existing equipment at the PPPL site. We refer to this machine as the SSAT. The SSAT will offer the world fusion program a unique combination of advanced-tokamak physics capability and at least 1000 second pulse lengths in reactor-relevant plasma configurations. This conclusion is reached on the basis of preconceptual design work and is also the conclusion of the National Fusion Task Force.

Given this basic conclusion, FEAC strongly recommends that the design and construction of an SSAT tokamak, capable of addressing advanced tokamak physics and steady-state issues, be initiated now and have a target date for first operation of 1999. In our own deliberations, in our guidance to FEAC Panel II, and in the guidance to the National Task Force, the budget scenario given in your letter of 5 percent real growth per year through at least FY 1996 has been assumed. Considering other program needs and consistent with the SEAB Task Force recommendations, FEAC recommends a constraint on Total Project Cost (TPC) for the SSAT of about $500M in as-spent dollars (or about $400M in constant FY 1992 dollars.)

A U.S. SSAT machine will complement the international program in an important way. There is today no facility in either the U.S. or the world fusion program that is capable of developing, in an integrated way, advanced tokamak physics in steady-state. Yet this is one key to developing a more attractive tokamak reactor.

Supplementary to this recommendation, FEAC recommends that the DOE and PPPL, working with the national MFE community, (which includes national laboratories, universities, and industries), develop a plan for the management of the design, construction, and operation of the SSAT as a national facility. This plan should include the early establishment of a National Steering Committee to provide the SSAT project with guidance on issues related to mission, machine concept, cost and schedule. We request that the recommended management structure and, if possible, the selection of the final design option for the SSAT, be presented to us at the next FEAC meeting scheduled for May 20-21, 1992 at UCLA.

Turning now to another issue in your charge, FEAC identified two priority activities of the tokamak confinement program for the period up to about 1995. These are full D-T operation in TFTR beginning in mid-1993 and a strong DIII-D program both in support of ITER and tokamak physics improvements. Our committee has not yet dealt with the relative priorities among other elements in the magnetic confinement experimental program.

In reflecting on the sum of our advice to you at this point, the Committee has come to recognize that our responses to your Sept. 24, 1991 Charge letter will not constitute a complete assessment of the long-term strategy of the U.S. fusion program. As such, the FEAC recommends that further work be undertaken to develop the MFE and IFE program and strategy in greater detail. Examples of important issues are: the priority and phasing among all the elements of the program; the time and procedures to obtain a U.S. fusion power development site; the budget implications relating to these issues; and the effects of the conclusions on the goals in the National Energy Strategy. Following this, the Department should estimate the number of scientists, engineers, technical and non-technical staff that are required each year to carry out the fusion program between 1992 and 2005.

Finally, either in preparation for this more complete long term strategy assessment or as part of it, FEAC recommends that the U.S. program develop a plan for fusion nuclear technology development. A key element here is the need for a fusion-power-capable U.S. site which will serve as a candidate site for ITER and for other fusion nuclear technology facilities. This recommendation is consistent with our earlier recommendations in February, 1992, namely:

1. that the U.S. begin the necessary preparations leading to the earliest possible site selection and commitment to construction of ITER;

2. that the materials development program be enhanced to develop materials for testing in ITER and for DEMO construction with special emphasis on long-life, low-activation materials.

3. that a study be undertaken to investigate what additional complementary activities might be needed to acquire part of the fusion nuclear technology data so as to make more realistic the 2025 goal for operation of a fusion power demonstration reactor.

We trust that you will find our advice here and earlier to be helpful on questions so crucial to the development of fusion power. The FEAC is unanimous and strong in our recommendations to you. And we can report that we are on track to provide you with the advice you requested by May.

Sincerely,

Bob Conn

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

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

June 12, 1992

Dear Will:

In your charge of September 24, 1991, you requested by May, 1992, that FEAC provide "recommendations on a U.S. concept improvement program, including relative priorities and taking into account on-going and planned work abroad." You clarified this request further in a letter to me dated February 20, 1992. A key premise in your February 20th letter is that "tokamak reactor development will be the primary focus of the U.S. magnetic fusion program." Given this, you asked for our advice on several more specific questions.

FEAC formed a panel, Panel 3, to consider your requests and to provide us with a background report that served as a basis for our discussions. Dr. Stephen O. Dean served as chairman and Dr. Barrett H. Ripin served as vice-chairman. The Panel held several meetings and heard from many interested parties. On behalf of FEAC, I express our sincere thanks to the Panel.

Your broadest request was for FEAC to:

- "Recommend a policy and selection criteria to help guide our (DOE's) program choices on concept improvements within our goal-oriented program strategy." In particular, "should (DOE) encourage and fund proposals on concepts other than tokamaks, given the demands of the mainline tokamak program and current budget constraints?" In addition to this broad request, you asked several related and specific questions:

- "What priority should be given to tokamak improvement proposals?"

- "What activities are appropriate on non-tokamak concepts and on small-scale exploration of tokamak improvements?"

- "Should the priority be higher for U.S. alternative concept activities that connect to major international programs or for unique U.S. activities?"

and

- "Under what conditions and within what criteria should concepts be considered that have little connection to tokamaks or to other major international programs?"

We begin our response by re-emphasizing the point that, among the many magnetic fusion confinement concepts, the tokamak has emerged as the most scientifically successful. With this in mind, DOE's policy should be based on the recognition that tokamak concept improvement programs are essential and should receive the highest priority. A vital aspect of "concept improvement" is the continued improvement of our scientific understanding of plasma behavior, such as plasma transport.

It is also true that uncertainties remain in the extrapolation of the tokamak to a competitive commercial reactor. As long as such uncertainties remain, a non-tokamak fusion concept program, at some level, should be supported as a matter of policy. FEAC recommends that DOE retain the flexibility to test some non-tokamak concepts at intermediate scale when warranted by their technical readiness and promise as a reactor. In deciding when and what to fund in this area, DOE should coordinate its decisions with those of other countries active in the same concept area.

As for specific magnetic fusion concepts, the stellarator is a well-developed alternative magnetic fusion concept that is closely related to the tokamak. FEAC will address U.S. policy regarding the stellarator, including the possible restart of ATF, in the context of the world effort to develop an optimized fusion reactor of the tokamak/stellarator type. We have established a Panel 4 with David Baldwin as chair and Harold Weitzner as vice-chair to provide input to FEAC on priorities in the toroidal confinement program. FEAC will provide its advice to you by the end of September, 1992.

Two other promising alternative concepts are the field-reversed configuration (FRC) and the reversed-field-pinch (RFP). Both of these are less well-developed than the tokamak or stellarator concepts. The largest part of the relatively small FRC program has historically been carried out in the U.S. while the RFP has been actively pursued in other countries in addition to the U.S. FEAC recommends that DOE consider the benefits of operating the LSX field-reversed configuration (FRC) facility in order to determine the validity of its physics principles. We also believe that the U.S. should maintain a small theoretical and experimental RFP effort, including some level of collaboration with the European and Japanese RFP efforts.

Because fusion is a long-term program, FEAC suggests that a small but formal and highly visible periodic competition be established to foster new concepts and ideas that if verified would make a significant improvement in the attractiveness of fusion reactors. Priority should be given to testing concepts, which are well-founded scientifically, at the small scale, proof-of principle level. Projects funded under such a program should be limited in duration (e.g., 3-5 years) so that eventually the program has turnover. Resources for this program could eventually grow to a few percent of the annual program budget. Given that any individual new program will be relatively small in size and cost, collaborations with international efforts should not be a requirement.

The broader principles of policy and the specific suggestions we have made provide a balance between a strong mainline program and attention to other concepts. We believe this policy regarding concept improvement is appropriate even in the case of substantial budget changes. More generally, FEAC recognizes that, depending on budgets, we may have identified more needs than there are funds. FEAC plans a summer workshop to consider the overall program in light of recent program developments and FEAC recommendations made to you over the past eight months.

Finally, FEAC discussed the general situation of basic plasma science research in the U.S. A report on this topic was published by the Plasma Science Committee of the National Research Council in 1991. Fusion and other applied plasma areas require that there be some level of basic research in plasma science. To assure this, we recommend that you use your influence to achieve an increase in basic plasma science research supported by offices in Energy Research such as the Office of Fusion Energy and the Office of Basic Energy Sciences. This would support a recommendation made in 1990 by the BESAC Ad hoc Subcommittee on Physics in OBES. We also urge you to work for coordination and increased plasma science research from other agencies such as the National Science Foundation, the Office of Naval Research, and the National Aeronautics and Space Administration. Together, these offices and agencies can ensure that a national basic research effort in plasma science is maintained.

Sincerely,

Bob Conn

Robert W. Conn
Chairman, for the
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Committee