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FUSION ENERGY ADVISORY COMMITTEE

Advice and Recommendations to the Department of Energy

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

June 1992

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

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

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

Members of FEAC

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FEAC/92-III June 22, 1992

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Preface

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

During its fourth meeting, held in May 1992, FEAC provided a detailed response to that part of the charge that requested review of the potential effectiveness, and hence the advisability, of implementing a more diverse U.S. fusion program. In particular, it responded to the paragraph:

> "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."

In order to respond to this charge in a timely manner, FEAC established a working group, designated "Panel #3", which reviewed the U.S. and international fusion programs in detail and prepared background material, included in this report as Appendix I, to help FEAC in its deliberations.

SEPTEMBER 24, 1991

CHARGE TO FUSION ENERGY ADVISORY COMMITTEE

Introduction

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

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

Charge

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

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

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

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

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

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

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

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

William Happer

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

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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
Fusion Energy Advisory
Committee

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June 8, 1992

Dr. David Baldwin, LLNL
Dr. Harold Weitzner, NYU

Dear David and Harold:

Thank you for agreeing to be chair and vice-chair of FEAC Panel 4 on "Priorities in the Intermediate Confinement Experiments." Your report will provide important input to the FEAC workshop in July on priorities in the overall fusion program. In addition, it will assist the FEAC in reaching its specific recommendation in September on the operation of ATF.

The facilities in the toroidal program that you are asked to evaluate and prioritize are the ATF stellarator and the PBX, and C-Mod tokamaks. This should be done against the background of the DIII-D and TFTR capabilities, assuming that full D-T operation in TFTR beginning in mid-1993 and a strong DIII-D program are supported as recommended in the April 1 FEAC letter to Dr. Happer. As described below, I ask you to focus more on a factual evaluation for our July meeting, leaving for September a more complete determination of a basis for FEAC recommendations on priorities.

For the July meeting, please provide the following information for each of the identified mid-scale toroidal facilities:

1. The physics issues that are addressable in this class of facility and the completeness with which each of the identified devices can address these issues: and

2. For each device, the goals and objectives, additional hardware, the strengths, uniqueness, limitations, present status, projected costs and time required to achieve its objectives.

In addition, for the July meeting, please provide preliminary priorities and their time scale that your Panel would assign to the operation of these facilities, along with an indication of the reasoning behind these priorities.

At the July meeting, the full FEAC will make use of your evaluations and your draft priorities in its examination of the broader program. Later, in time for the September meeting, I would like your panel to reexamine its preliminary priorities in light of the FEAC's July workshop and feedback provided there. Further, this will provide an opportunity for your Panel to hear responses from the programs reviewed. Your revised priorities will then serve as input to the September meeting of FEAC. This two-step process will provide ample opportunity for each program to have a fair opportunity to answer questions and concerns.

When this process has been completed, the FEAC must answer the following questions:

1. If the fusion budget is sufficient to do so, do all of the facilities warrant operation? If not, which ones do not warrant operation?

2. If the fusion budget is not sufficient to operate simultaneously all the facilities which warrant operation,

   a) Should their operation be phased, implying one or more machines would be mothballed, and if so how?
   b) Should all be operated at a reduced level? or
   c) Should one or more be closed down, and if so in what priority order?

The combination of your evaluations and priorities should be sufficient to permit FEAC to respond to Dr. Happer's request concerning the ATF and other priorities. I understand that this will not be an easy undertaking for your Panel, for FEAC, or for the programs involved since all are staffed by high quality groups. I will do all that I can to assist you in this endeavor.

Sincerely,

Bob Conn

Robert W. Conn

Appendix I

The report to FEAC of Panel #3, dated May 11, 1992.

CONCEPT IMPROVEMENT

A Report to the
Fusion Energy Advisory Committee

by

FEAC Panel 3

MAY 11, 1992

Stephen O. Dean (Chair)	Fusion Power Associates
Barrett H. Ripin, (Vice Chair)	Naval Research Laboratory
Don Batchelor	Oak Ridge National Laboratory
Klaus Berkner	Lawrence Berkeley Laboratory
William R. Ellis	Ebasco Services, Inc.
Kenneth W. Gentle	University of Texas
Stanley M. Kaye	Princeton Plasma Physics Laboratory
B. Grant Logan	Lawrence Livermore National Laboratory
Earl Marmar	Massachusetts Institute of Technology
Gerald Navratil	Columbia University
Norman F. Ness	University of Delaware
Tihiro Ohkawa	General Atomics
Richard E. Siemon	Los Alamos National Laboratory
Don Steiner	Rennselaer Polytechnic Institute
Harold Weitzner	New York University

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

INTRODUCTION

In a charge letter to FEAC dated September 24, 1991 and further amplified in a letter dated February 20, 1992, DOE Director of Energy Research Dr. William Happer asked the FEAC "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." He asked "What activities are appropriate on non-tokamak concepts and on small-scale exploration of tokamak improvements." He said "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 strategy."

Within the concept improvements area, Happer asked "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 international programs, be considered?"

Happer stated in summary, "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." The full text of Happer's February 20 letter is reproduced as Appendix A.

FEAC Panel 3 was appointed by FEAC to assist it in responding to Dr. Happer's charge. One of the Panel's first actions was a meeting with Dr. N. Anne Davies, Director of DOE's Office of Fusion Energy (OFE), for a discussion of the charge and her views on the possible role of concept improvement in a goal-oriented program like fusion. Dr. Davies indicated that OFE was re-evaluating its policies regarding concept improvement, particularly in areas of non-tokamak research, and was looking to FEAC and Panel 3 for guidance and advice.

The statements contained herein are the views of the Panel and do not necessarily represent the views of the full FEAC, which will respond formally to Dr. Happer following their review and consideration of this report.

ACTIONS TAKEN BY THE PANEL

The Panel met three times: on February 21, March 11-12, and April 29-30. In addition to meeting with OFE Director Dr. Davies, the Panel also met with OFE division directors, Dr. David Crandall and Dr. John Willis at the February meeting. During our March meeting, we met in executive session with two former directors of the U.S. fusion program, Dr. John F. Clarke and Dr. Robert L. Hirsch, and with Dr. Sidney Ossakow, Superintendent of the NRL Plasma Physics Division, which is the largest plasma physics group in the U.S. outside of the DOE system. The views of another former fusion director, Edwin E. Kintner were received by phone by the Panel chairman and relayed to the Panel.

The panel held a public session on March 11, at which it received oral presentations and had discussions with interested members of the fusion community; others submitted written statements to the Panel. The Panel also received presentations at that meeting from representatives of two large fusion-oriented organizations, the American Physical Society Division of Plasma Physics, and the University Fusion Association. A listing of these interactions is given in Appendix B.

BACKGROUND

Historically, the U.S. and World fusion energy development programs have consisted of a wide spectrum of approaches, each with their own dedicated, competent advocacy constituents. Over the years, the tokamak concept has emerged as the most scientifically successful concept. As real spending power declined in the U.S. during the 1980's, it became increasingly difficult to maintain the previous breadth and, periodically, approaches were dropped, e.g. the U.S. abandoned the magnetic mirror program in 1986 , shortly after completing, and before operating, the large MFTF-B facility.

In the Fall of 1990, faced with a Congressional cut of $50 million in the FY 1991 budget, and taking into account the pre-eminence of the tokamak concept, the DOE moved to close out support for all approaches except the tokamak. Although $25 million of the cut was restored, DOE stuck to its original decision and distributed the restoration among various tokamak efforts. DOE's intent in these drastic actions was to minimize the effect of the unplanned and very serious budget cuts on the program's mainline, the tokamak. Among the facilities slated for termination in the U.S. at that time were the operating ATF Stellarator, the under-construction ZT-H Reversed Field Pinch, the nearly-completed LSX Field Reversed Configuration device, and several other smaller, related programs. The Panel is not aware of any compelling reasons to stop research on the non-tokamak concepts in the absence of these budget pressures. With these actions, however, DOE eliminated research on essentially all the non-tokamak concepts in the U.S program.

Subsequent statements and communications by the Department led to the perception in the fusion community that proposals for research on non-tokamak concepts would not be supported by OFE, and should not be submitted. The only way that proposals on non-tokamak devices would be accepted for consideration was if the work was cast in direct support of tokamaks. The rationale given was that research on competing concepts could not be supported, since, even if the research were successful, no funds would be available to develop the concept to its next, more-expensive stage; thus it would be best not to begin.

CONCEPT IMPROVEMENT - GENERAL PRINCIPLES

The goal of the U.S. fusion program, as stated in the DOE's National Energy Strategy, is to "prove fusion energy to be a technically and economically credible energy source, with an operating demonstration plant by about 2025 and an operating commercial plant by about 2040." Other stated goals are to "ensure a cost-effective research and development program," and to "develop fusion as a safe, environmentally sound energy source."

The time scale of the fusion program dictates the importance of concept improvement. Science and technology thirty years hence will certainly be far different from what we might envisage today. Programs like fusion, that will continue over such periods of time, must retain breadth and flexibility to incorporate changes that will certainly accrue.

Although the tokamak has emerged as the most scientifically successful concept, uncertainties remain in the extrapolation of the tokamak to a competitive energy source. Reactor design studies indicate that improvements are required in both the physics and technology of tokamaks. This situation not only dictates that a high priority be placed on a tokamak concept improvement program, but also provides the rationale for having a more general concept improvement program not limited to the tokamak concept. Other concepts can benefit from the advances made in the tokamak program, especially in the technology areas. For example, tokamak performance has made advances as a result of new technologies becoming available (e.g., neutral beams, pellet injectors, divertors), and these technologies can in principle be applied to other, non-tokamak concepts. Thus, although the non-tokamak concepts are less advanced than the tokamak, they may move along their own development paths more rapidly.

The directions for fusion concept improvement are identified through fusion reactor design studies and other comparative analyses and systems studies. The results of these studies suggest that there are a variety of credible technical paths to commercial fusion power and that, consequently, there should be no arbitrary exclusion of non-tokamak fusion approaches. The fact that fusion must compete against other advanced energy sources in some vaguely-defined future marketplace, and the recent history of the consequences of too early narrowing in fission reactor development, also indicates the importance of maintaining several technical options. Continuing to pursue improvements in fusion reactor designs is an important element of the fusion program.

As a long-term endeavor, the development of fusion depends on a continual inflow of new, younger talent. It is not possible today to predict what line of approach will ultimately lead to the most acceptable commercial fusion power system. If fusion is to continue to attract and inspire a new generation of scientists and engineers, it must be clearly seen as an exciting field, open to achieving success by whatever path.

A serious negative consequence of the 1990 DOE decision to narrow the research program to tokamaks is the widespread impression, expressed to Panel 3 primarily by university researchers, that DOE has postured itself to be unreceptive to new ideas and innovation. It is important to reverse this impression.

After reviewing material and receiving input from many sources, Panel 3 concludes that a breakdown in communication occurred between DOE fusion program managers and the fusion research community in the Fall of 1990 when DOE decided to narrow the program to tokamaks. A more formal set of management procedures could help to ensure that such a breakdown does not occur in the future. When budget cuts come, either unexpectedly or with advance notice, DOE's research contractors should be thoroughly involved in the discussions with DOE that would lead to a re-optimization of the program.

The fusion program consists of programs to develop specific concepts and various, more generic, physics and technology support programs. In this study we have not considered overall program balance among all elements of the fusion program, but confine ourselves primarily to the question of concept improvement.

In formulating a policy on concept improvement, we found it useful to think of fusion concepts as dividing into three categories or groups, distinguished by their stage of development and the corresponding size of their budget requirements. We have called these:

* Highly-Developed Concepts

  Tokamaks and Stellarators fall into this group.

* Developing Concepts

  Reversed Field Pinch (RFP) and Field Reversed Configuration (FRC) fall into this category.

* Small-Scale Innovative Concepts

  A variety of such concepts currently exist, many of which were presented to the Panel by their advocates.

The Panel finds merit in maintaining some level of effort in each of these areas as a matter of policy, with the bulk of the effort on the Highly-Developed concepts; a variable but modest effort in Developing concepts, depending on their technical readiness, promise, and their relation to efforts in other countries; and a small, but reliable, funding commitment for research in the innovative concepts area. These are each discussed in later sections of this report.

International collaboration has long been recognized as an important component of the fusion program, both tokamak and non-tokamak. In the non-tokamak concept areas, because they are supported at a much lower level than the tokamak, international collaboration is vitally important. This is especially true as the concept matures and requires larger facilities. Maximum advantage should be taken of international efforts.

International collaboration in a concept development area requires some reasonable level of program activity domestically. Therefore, it makes sense to carefully consider international efforts in planning a cost-effective program.

In pursuing research on various fusion concepts, a "general principle" that may be useful in setting priorities is:

The larger the cost of doing frontline research and development on a given concept,

* the more stringent should be the requirement that it offer some advantage, such as lower cost, faster schedule for development, lower capital cost, ease of engineering and maintenance; and

* the more desirable it is that there be international coordination.

As a further statement of general principle, Panel 3 also endorses the following statement on program balance from the ESECOM study:

"Although research priority should reward the more successful fusion confinement and technology options, it is essential not to concentrate so heavily on a single line of development (no matter what the budget) that better concepts cannot continue to be developed for improved second-generation configurations. At appropriate levels, in the near and long term, we should always be seeking better plasma drivers, better blankets, better energy-conversion technologies and better fuel cycles. The resources for the better ideas may be very strained and limited and, thus, the second- and third-generation technologies may take longer, but their pursuit should always continue."

In order to properly balance the funding both between and within the various components of the fusion program, a vigorous, on-going effort on concept reactor design and systems analysis is required, covering all aspects of the fusion program.

HIGHLY-DEVELOPED CONCEPTS

Two concepts, Tokamaks and Stellarators, have the most extensive theoretical and experimental data bases of all the magnetic concepts. Furthermore, they are closely-related in both their physics and their reactor technologies (e.g., large superconducting magnets). They are part of a family of toroidal-magnetic-field-dominated concepts, that also includes Torsatrons and Heliotrons.

Tokamaks are clearly the front-running fusion concept in terms of the scientific data base, the performance achieved, the degree of international commitment to development, and the attention that has been given to the reactor embodiment. Tokamak performance has continually improved and there is both a recognition and a commitment to realize its potential for further optimization. This commitment is evidenced by the high priority that has been given to tokamak concept improvements.

Variations of the tokamak configuration that require further study include higher and lower aspect ratio compared to today's tokamaks. Tokamak concept improvement is being sought in the following inter-related areas:

* enhanced confinement modes
* high bootstrap fractions
* disruption control
* second stability physics
* current drive
* current and pressure profile control
* high beta
* steady-state power and particle handling
* engineering for simpler maintenance

All of these areas provide ample opportunity for exciting, innovative research. The ultimate goal of these concept improvements is to improve the attractiveness of a tokamak reactor by reducing unit size and cost, by increasing power density and efficiency, and by simplifying maintenance.

The present fusion program contains a significant effort directed towards improving the tokamak concept in the above areas. The history of important contributions to tokamak concept improvement shows that ideas are frequently discovered or first tested on smaller experiments and then incorporated or verified in larger experiments. Examples of such contributions include lower hybrid current drive, role of electric fields on inducing transport enhancement transitions, second stability operation, and the identification of neoclassical bootstrap current. Contributions have also come from non-tokamak experiments, such as the ATF Stellarator. A continual flow of concept improvement data could be obtained from appropriate utilization and upgrades of existing tokamaks, and possibly from selected, new, specialized smaller experiments.

Most of the important concept improvement ideas would receive more definitive and integrated tests, beginning around the turn of the Century, in the proposed SSAT project recommended by FEAC in its April 1, 1992 letter to Dr. William Happer.

Though less advanced than the Tokamak, Stellarators offer an alternative approach to solving some of the needed improvements in the tokamak. Stellarators are inherently steady-state and thus do not require current drive. They do not suffer from disruptions, since they have no net plasma current. They may also offer alternative approaches to some engineering problems, such as maintenance relating to magnetic coil design.
Stellarators have many issues requiring further study (e.g., their performance at higher levels of auxiliary power). They need substantial funding in order to make further progress, a fact

consistent with their status as a "highly-developed" concept and their readiness to be taken seriously as a potential fusion reactor candidate.

There is a significant international effort underway on Stellarators. A new, large stellarator (LHD) is under construction in Japan, and another (Wendelstein 7-X) is under consideration in Europe. Both are multi-hundred million dollar class devices that would operate around the turn of the Century. The largest and most flexible Stellarator in the world today is the ATF in the U.S. This facility is now in standby status and is not presently funded for continued operation. The issue for the U.S. program is to formulate a stellarator policy in the context of the World effort to develop an optimized toroidal-field-dominated fusion reactor of the tokamak/stellarator type, while continuing to provide technical data and insights to the tokamak program. Panel 3 notes and endorses previous FEAC recommendations that a special panel be convened to evaluate Stellarator policy, including the possible restart of ATF.

DEVELOPING CONCEPTS

The term "developing concepts" is used to describe concepts for which sufficient data, understanding and promise exist to justify an experimental device of "intermediate" size.

Prior to the 1990 decision to narrow the fusion program to tokamaks, DOE had two programs in this category: the Reversed Field Pinch (RFP) and the Field Reversed Configuration (FRC). Of these two, the RFP was the most advanced. Both concepts are examples of a family of "compact" magnetic concepts in which the poloidal magnetic field dominates both the confinement physics and the reactor design (which is characterized by simpler magnet coils, higher power density, less-demanding heating systems and smaller unit size, compared to toroidal-field-dominated concepts). Small size and high power density give rise, however to a new set of engineering difficulties, e.g., relating to component lifetimes. Such concepts do, however, provide for consideration an alternate technical path to fusion power.

The most advanced RFP in the world would have been the $75 million ZT-H in the U.S., which was 75% complete when the 1990 decision to terminate non-tokamak work was implemented. The facility has since been scavenged and there appears to be no possibility for restoring the program. Another RFP experiment (called RFX), comparable in size to ZT-H, has been built in Italy and began operation in late 1991. Small experiments are also underway in Japan. In the U.S., a small, but vigorous, university-based experimental effort continues on the RFP. If in addition, the U.S. community of RFP-knowledgeable scientists works in collaborations with the Italian RFX group and with Japanese RFP researchers, the prospects for a successful world effort on RFP's would be increased.

The most advanced FRC in the world is the recently-completed $15 million LSX in the U.S., currently in stand-by mode. This device could be operated for a few years to obtain the fundamental scaling information for which it was designed. Opportunities for international collaboration also exist in the FRC area.

New candidates for the Developing Concepts category would come through two means: first, from small-scale innovative concepts that have demonstrated sufficient promise; and second, from experimental results abroad which appear promising enough for the initiation of a U.S. program.

If the U.S. chooses to maintain a presence in the RFP, FRC or some other developing concept, a combined theoretical, experimental and reactor analysis effort is required. The DOE would then be prepared to initiate new starts in this category when the technical readiness and promise of the concept so-warrants.

SMALL-SCALE INNOVATIVE CONCEPTS

As noted earlier, Panel 3 believes that the U.S. fusion program must reposture itself to encourage and be open to innovative ideas. These might include proposals to improve aspects of the tokamak, stellarator, RFP and FRC concepts, as well as proposals from competent individuals and institutions that do not necessarily fit well with current "mainline" thinking. This category is meant to encompass ideas that can be tested at relatively modest cost (on the order of a million dollars per year) and have a reasonable likelihood of providing critical data within a short time period (about 3-5 years).

A specific sum of money could be identified (a few million dollars the first year) and set aside for innovation, to be allocated after a solicitation for proposals followed by a batch peer-review. The review board would consist of individuals with no perceived conflicts of interest; membership on the review board would have a partial rotation each year. This would help to assure uniformity of evaluations. The process could be modeled on the procedures for SBIR programs or for a typical large institution IR&D program. Sunset clauses incorporated into the contracts would ensure that the programs would be brought to conclusion and money freed up in future years. If high-quality proposals are received, the program might eventually reach a steady-state value of $10-15 million per year, with a 20-30% turnover per year. Successful programs requiring enhanced funding in future years would have to compete for funding from the developing concepts program.

In addition to the obvious criteria of technical excellence, the selection criteria should include:

* the potential impact of the promised result, and
* the likelihood of addressing key issues in the time-frame and for the cost proposed.

Many industrial laboratories believe that successful product innovation requires that the innovation work not be carried out within the existing "product divisions." This is because innovation which could lead to displacement technology or upset technology is better placed in organizational components concerned with the creation of new products, without the pressure for delivery of the mainline product. Consideration should therefore be given to managing the innovative concepts program in a separate organizational entity within the Office of Fusion Energy. In addition to providing some protection from the schedule and budget pressures of the more advanced concept development programs, this step would give the concept innovations program some organizational stature, assist in institutionalizing the program, and help in maintaining a stable level of effort in the face of fluctuating or declining budgets. Priority in this program would be given to testing deserving new concepts on a small scale, as opposed to, for example, developing new diagnostic instrumentation or new tokamak components. Though the latter may be worthy goals, in most (but not necessarily all) cases these should be funded by the larger subprogram elements in OFE.

The ultimate goal of concept innovation has to be to make a significant difference in the attractiveness of fusion reactors. The focus of the program should be on programs which, if successful, can have a major impact on the fusion program and its product, the fusion reactor of the future. Collaborations with international efforts should not be a requirement for these small-scale programs.

INERTIAL FUSION ENERGY

Inertial Fusion Energy was excluded from the charge of FEAC Panel 3 on the grounds that a separate panel would be charged in this area. However, in the absence of such a charge to date, Panel 3 feels compelled to make the following observation.

The DOE National Energy Strategy indicated by a chart that a demonstration inertial fusion power plant could be operational by 2025, the same date given for a magnetic fusion power plant. The Strategy states "Programs that pursue both magnetic confinement fusion energy systems and inertial confinement fusion energy systems will continue under the National Energy Strategy. These two approaches . . . compete technically. This helps to ensure that the necessary - and the best - technology will become available. However, resource limitations eventually will require a decision to determine which course to pursue." Inertial Fusion Energy is thus characterized as an alternative concept to magnetic concepts. FEAC needs to examine the IFE program in this context. The Panel endorses DOE's intention to send a separate charge to FEAC to address the Inertial Fusion Energy issue soon.

SCIENCE AND TECHNOLOGY BASE

A program as large and as long-range as fusion requires, in addition to science in direct support of a confinement concept, some level of basic plasma science which is largely left to the discretion of individual investigators. The primary evaluation criteria for this work is the quality of the science. It is the impression of Panel 3 that very little, if any, such activity now is funded by the fusion office. Such "basic" plasma science provides the underpinning of "fusion" science and falls within the scope of at least three funding organizations: the Office of Fusion Energy, the DOE Office of Basic Energy Sciences, and the National Science Foundation. The deliberate influence of DOE top management would be required to assure a reasonable level of effort from all three sources.

In addition to the technology required for specific concepts, new developments which could impact the course or practicality of fusion may equally emerge from relatively undirected efforts on relevant technologies, such as superconductivity or materials. Modest exploratory efforts of this sort could also result in unexpected program dividends.

SUMMARY OF FINDINGS

The FEAC Panel 3, on fusion concept improvement policy, arrived at the following findings:

1. A breakdown in communications occurred between DOE fusion program managers and the fusion research community in the Fall of 1990 when DOE decided to narrow the program to tokamaks. A negative consequence of that decision is the widespread impression that DOE has postured itself to be unreceptive to new ideas. It is important to reverse this impression.

2. The time scale of the fusion program dictates the importance of concept improvement. Science and technology thirty years hence will certainly be far different from what we might envisage today. Programs, like fusion, that will continue over such periods, must retain breadth and flexibility to incorporate changes that will certainly accrue.

3. Among the many magnetic fusion confinement concepts, the tokamak has emerged as the most scientifically successful concept. However, uncertainties remain in the extrapolation of the tokamak to a competitive commercial energy source. Fusion reactor design studies indicate that improvements are required in both the physics and technology of tokamaks.

4. As a general principle, we find (as stated eloquently by the ESECOM panel), "Although research priority should reward the more successful fusion confinement and technology options, it is essential not to concentrate so heavily on a single line of development (no matter what the budget) that better concepts cannot continue to be developed for improved second-generation configurations."

5. Though less advanced than the Tokamak, Stellarators offer an alternative approach to solving some of the needed improvements in the tokamak. The issue for the U.S.

fusion program is to formulate a Stellarator policy in the context of the World effort to develop an optimized toroidal-field-dominated fusion reactor of the tokamak/stellarator type, while continuing to provide technical data and insights to the tokamak program.

6. The DOE decision to eliminate support for non-tokamak concepts was based, in part, on the philosophy that, even if the research were successful, no funds would be available to develop the concept to its next, more-expensive stage; thus it would be best not to begin. A change in the current policy would require that DOE retain the flexibility to test some non-tokamak concepts at intermediate scale, when their technical readiness and promise so-warrants.

7. A program as large and as long-range as fusion must find mechanisms for encouraging innovation. A small, but formal and highly-visible annual competition to foster new ideas, modelled after the IR&D programs of large institutions, is a mechanism that could serve this purpose.

8. In addition to the science and technology in direct support of a confinement concept, the fusion program should maintain some level of support for "basic" plasma science and forefront technology that provide the underpinnings of "fusion" plasma science and fusion technology.

APPENDIX A

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 B

INTERACTIONS WITH PANEL

FEAC Panel 3 received presentations and/or written input from the following individuals:

Daniel C. Barnes, LANL
Ronald Blanken, U.S. DOE
Allen H. Boozer, College of William and Mary (and University Fusion Association)
Robert Bussard, EMC2
Vincent Chan, GA
John F. Clarke, Battelle Pacific Northwest Laboratories
Ronald H. Cohen, LLNL
David Crandall, U.S. DOE
N. Anne Davies, U.S. DOE
Robert Dory, ORNL
William Dove, U.S. DOE
Dan Dreyfus, Gas Research Institute
George C. Goldenbaum, University of Maryland
Hans R. Griem, University of Maryland
Richard D. Hazeltine, University of Texas
Noah Hershkowitz, University of Wisconsin (and APS-DPP)
Robert L. Hirsch, EPRI
Alan Hoffman, STI Optronics
Paul Koloc, Phaser Corp.
Robert Krakowski, LANL
Rulon Linford, LANL
S. C. Luckhardt, MIT
Jim Lyon, ORNL
Michael Mauel, Columbia University
Bogdan Maglich, Advanced Physics Corp.
George Miley, University of Illinois
George J. Morales, UCLA
Edward C. Morse, University of California/Berkeley
Erol Oktay, U.S. DOE
Sidney Ossakow, Naval Research Laboratory
Martin Peng, ORNL
Francis Perkins, PPPL
Richard F. Post, LLNL
Stewart Prager, University of Wisconsin
A. E. Robson, Naval Research Laboratory
John Sethian, Naval Research Laboratory
John Sheffield, ORNL
Leon Shohet, University of Wisconsin
Dieter J. Sigmar, MIT
Loren C. Steinhauer, STI Optronics
Ravi Sudan, Cornell University
Roscoe B. White, PPPL
John Willis, U.S. DOE

RECOMMENDATIONS FOR FEAC RESPONSE LETTER

1. Among the many magnetic fusion confinement concepts, the tokamak has emerged as the most scientifically successful concept. However, uncertainties remain in the extrapolation of the tokamak to a competitive commercial energy source. Consequently, tokamak concept improvement programs are essential and should receive a high priority. In addition, as long as these uncertainties remain and the projected date of commercial fusion power remains so far in the future, a non-tokamak fusion concept development program, at some appropriate level, must also be maintained.

2. Tokamaks and Stellarators are the most highly-developed magnetic fusion concepts and should receive the bulk of the funding for concept development and concept improvement. A special panel should be established to evaluate Stellarator policy, including the possible restart of ATF, in the context of the World effort to develop an optimized toroidal-field-dominated fusion reactor of the tokamak/stellarator type.

3. The decision by DOE in late 1990 to eliminate essentially all non-tokamak-related work from the fusion program has had a chilling effect on many scientists in the fusion community, resulting in the widespread impression that DOE has postured itself to be unreceptive to new ideas. It is important to reverse this impression. If fusion is to continue to attract and inspire a new generation of scientists and engineers, it must clearly be seen as an exciting field, open to achieving success by whatever path. Therefore, although tokamak concept improvement must receive a high priority, we believe that there should be no arbitrary exclusion of non-tokamak fusion approaches.

4. DOE should retain the flexibility to test some non-tokamak concepts at intermediate scale, when their technical readiness and promise so-warrants. In deciding when and what to fund in this area, DOE should attempt to coordinate their decisions with those of other countries active in the same concept area. Prior to the 1990 decision to narrow the fusion program, DOE had two programs (other than the Stellarator) we would place in this category: the Reversed Field Pinch (RFP) and the Field Reversed Configuration (FRC). We recommend that a small theoretical and experimental RFP effort be maintained in the U.S., and that RFP experts should engage in collaborations with European and Japanese RFP efforts. We also recommend that DOE undertake a peer review of the benefits that might accrue from operating the LSX Field Reversed Configuration (FRC) facility for a few years, to obtain the fundamental data for which it was designed.

5. We recommend that a small, but formal and highly-visible, annual competition should be established to foster new ideas, from whatever source. Programs funded under this program should be limited to 3-5 years in duration so that eventually the program has a 20-30% turnover per year. A few million dollars should be set aside for this competition the first year. If high quality proposals are received, this

program should grow modestly in future years, eventually reaching a steady-state value of perhaps $10-15 million per year (about 3-5% of the total budget). In addition to the obvious criteria of technical excellence, the selection criteria for this work should be the potential impact of the promised result, and the likelihood of addressing key issues in the time-frame and for the cost proposed. Collaborations with international efforts should not be a requirement for these small-scale programs.

6. In addition to the concept improvement programs discussed above, a program as large and as long-range as fusion requires some level of fusion-related basic science, which is largely left to the discretion of individual investigators. Very little, if any, such activity now is funded by the DOE. We recommend that DOE top management use its influence to assure that a reasonable level of effort on basic plasma science is supported from at least three sources: the Office of Fusion Energy, the Office of Basic Energy Sciences, and the National Science Foundation. Modest exploratory efforts in selected forefront technologies important to fusion should also be supported by the Office of Fusion Energy.

7. The policy we have recommended above is one we believe provides an appropriate balance between a strong mainline program and a receptiveness to new ideas. Consequently, we recommend that this balance be implemented and maintained even if the fusion budget should decline.

Appendix II

Minutes of FEAC Meeting of
May 19-21, 1992.

MINUTES

Meeting of Fusion Energy Advisory Committee
Institute of Plasma and Fusion Research
University of California, Los Angeles
405 Hilgard Avenue
Los Angeles, CA 90024

May 19-21, 1992

Present:

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

Tuesday, May 19, 1992

Welcome and Opening Remarks

Dr. Conn called the meeting to order and welcomed the committee members to UCLA. He indicated that two meeting rooms, in different buildings, would be used, and outlined the parking arrangements that had been made for committee members. He stated that the main purpose of the meeting was to receive the report of Panel #3 on concept improvements. He informed the meeting that several members of the public had already made known their intentions to speak during the time set aside for public comment, and suggested that others who wished to speak should contact the committee secretary.

Up-Date from DOE

Dr. N. Anne Davies announced that Dr. Jim Decker had been unable to come to the meeting and that he sent his apologies to the committee. She stated that she would therefore make the entire presentation on behalf of DOE.

She began by summarizing the activities of FEAC to date; she outlined the status of the various charges to the committee, the responses of FEAC to those charges, and the subsequent actions taken by DOE.

The charges that had been investigated by Panel I concerned:

- The appropriate scope and mission of ITER and suggestions to lower its cost or accelerate its schedule.
- The relative importance to the U.S. of the various technology tasks, the role of U.S. industrial involvement in the ITER EDA, and the balance between ITER project-specific R&D and the base program.

In response to these charges, FEAC had suggested that:

- Complementary activities dedicated to acquiring nuclear testing data would permit shortening the ITER test program, and such activities should be initiated.
- The U.S. should begin the necessary preparations leading to the earliest possible site selection and commitment to the construction of ITER.
- DOE should develop a plan for more integral industrial participation in the fusion program.

- The Development and Technology base program should be enhanced beginning with FY93.

In turn, the DOE had responded by agreeing to:

- Develop information on the cost and technical feasibility of a complementary nuclear testing program.
- Increase the OFE's Development and Technology base program in FY93, including some increase for design efforts associated with a 14 MeV neutron source.
- Begin develoment of a U.S. strategy for industrial participation in fusion.

The charge that had been investigated by Panel II concerned:

- 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, including consideration of international collaboration.

The ground rules relating to this charge were that the program must be experimental, that it must pertain to a clearly identified and needed niche in the program, and that the cost of the device be kept at $400 million.

In response to this charge, FEAC had suggested that:

- The U.S. design and construct a Steady-State Advanced Tokamak (SSAT) capable of addressing advanced tokamak physics and steady-state issues, with a target date for first operation in 1999.
- OFE work with national laboratories, universities and industries to develop a plan for the management of the design, construction and operation of SSAT as a national facility.
- OFE support two priority activities of the tokamak confinement program through about 1995:
  - Full D-T operation in TFTR beginning in mid-1993.
  - A strong DIII-D program to support ITER and tokamak physics improvements.

The DOE had responded to these suggestions with the following actions:

- ER had requested a DOE ESAAB (Approval of Mission Need) for the TPX.
- OFE had formed a TPX Business Strategy Group to examine options and recommend an optimum approach to structuring the TPX project organization.
- OFE accepted that the D-T experiments on TFTR should be given the highest priority within the domestic program.
- OFE had proposed increased funding for both TFTR and DIII-D in the FY92 reprogramming activity.
- OFE had agreed that once the ITER technical objectives had been defined, it would proceed with the development of a fusion nuclear technology plan.

The charge that had been under investigation by Panel III, and that would be reviewed in detail during the current meeting, concerned:

- A U.S. concept improvement program, including relative priorities and taking into account on-going and planned work abroad.

Dr. Weitzner asked if the TFTR D-T program was on time. Dr. Davies responded that the program had slipped a little due to safety reviews but that she was attempting to keep this delay to a minimum. She anticipated that the slippage would be limited to three months. She added that she was concerned over the level of funding that might be available for TFTR in FY93.

Dr. Davies reviewed, briefly, the status of ILSE (Induction LINAC Systems Experiments). The physics design review had been completed in November 1991, the Mission Need Statement (KD-O) had been approved in March 1992, and the project review had been completed in April 1992. The DOE would now consider when ILSE should become a line item in the budget.

Dr. Davies reviewed the FY92 Reprogramming Proposal. OFE had asked that $30 million appropriated for BPX R&D and design be redirected. The principal areas for redirection were:

BPX/TPX - $16 million

- To cover BPX close-out costs.
- To conduct preliminary scoping studies followed by conceptual design of a new

lower-cost device that can support ITER and improve the tokamak concept.

TFTR - $3.4 million
- To support timely completion of the TFTR D-T program.
- To provide for increased overhead burden due to BPX close-out.
- To cover increased costs due to ES&H improvements.

Tokamak Improvement Experiments - $5 million
- To provide increased operating time on DIII-D.
- To support additional heating systems on PBX-M.
- To support repair of ATF

ITER Physics Support - $2 million
- To increase physics R&D and coordination in support of ITER EDA.

Dr. Davies indicated that the Department of Energy had received no approval of its reprogramming request to date. The total amount contained in the request for reprogramming was $250 million since programs other than fusion were also involved. The suggestion had been made that Congress take back the entire $250 million and use it to improve the FY93 budget. However, the subcommittees and Congress were now becoming concerned over the loss of jobs that would occur due to any failure to reprogram and, as a result, loss of the funding appeared to be less likely. She pointed out that DOE was still continuing to spend the money while it awaited the outcome of the reprogramming request and that consequently the amount that was at risk was steadily diminishing. She explained that DOE must spend the money that has been appropriated unless the department were to offer Congress a rescission.

Dr. Davies stated that the summer study that was being planned for the last week in July would be a "closed" meeting since, in essence, it would be a panel meeting similar to those held for MFAC some years ago. She indicated that the outcome would be made public at the next meeting of FEAC which was scheduled for the fall. Dr. Weitzner asked that OFE attempt to keep the size of the summer meeting much smaller than the equivalent meetings of MFAC.

Dr. Baldwin expressed concern over the uncertain level of the FY93 budget and its impact upon TPX. He indicated that if the financial clock were to start at KD-O, then significant funding could be consumed before sufficient funding was available to start on the conceptual design, and this would add to the total amount spent on the project. Dr. Davies stated that the financial clock would start when the conceptual design started. She pointed out that the signs for FY93 were not good and added that she would not advocate starting TPX on a flat budget. However, she stressed that the project should "go through all the necessary hoops" so that it would be ready to start when funding became available.

Dr. Baldwin pointed out that it was the roll-off from TFTR that was expected to provide the funding for TPX. If the shut-down of TFTR was delayed, then this would impact the budget for TPX. He asked Dr. Davies if OFE intended holding to the finish date for TFTR even if the start of the D-T program was delayed. Dr. Davies answered affirmatively and stated that this was the reason why OFE was striving to prevent the start date from slipping by more than three months. Dr. Conn added that Dr. Happer understood the need to keep up the pace on the fusion program on a project-by-project basis. Mr. Culler said that in his opinion the FY93 budget would not be dealt with before March 1993 because of the up-coming elections. He suggested that one third of the House would not be going back and reminded everyone that this was a highly political year. He indicated that FEAC should factor this into its thinking. Dr. Davies agreed, adding that the budget was very uncertain at the moment. Dr. Davidson pointed out that KD-O was scheduled for June: The conceptual design phase was scheduled to start in October.

Referring to the use of tritium in TFTR, Dr. Weitzner suggested that in view of the paucity of available tokamaks in the USA, both the total amount of tritium used, and when it was used, should be subject to very careful review. Dr. Conn suggested that DOE may care to ask FEAC to re-affirm its recommendations for TFTR and, if FEAC were to recommend changes to its original recommendations, FEAC should explain why such changes should be made. He pointed out that it would involve a very fundamental change in direction if FEAC were to do that. Dr. Davies stressed that the U.S. program was committed to D-T experiments in TFTR.

Dr. McCrory stated that the impact of a delay in the TFTR project would be difficult to assess in the absence of a national plan. Dr. Davies agreed. She added that OFE had worked out a plan that took into account the uncertainties associated with "new" facilities but that the U.S. did need a strategic plan that everyone could agree to. Dr. Overskei pointed out that the FEAC recommendations had all been aimed at enhancing the fusion program while the budget scenario leaned towards a reduction in the program. This was inconsistent. Dr. Davies agreed that, to achieve consistency, a modest increase in the budget for FY93 was required. She emphasized that OFE would not start on TPX if the FY93 budget was "flat", and cautioned that the budget may even decline.

Dr. Parker asked if OFE had a plan for dealing with budget changes. Dr. Davies responded by recounting recent budget history. At the time that the abrupt $50 million cut was made in the fusion budget, OFE had two choices as far as funding major programs was concerned: (a) to fund BPX and ITER, or (b) to fund the alternative concepts and ITER. OFE chose option (a) because the alternative concepts were being inadequately funded anyway and any cut would only make matters worse. The subsequent restoration of $25 million of the cut made no real difference to the grounds upon which the decision had been made. Dr. Davies explained that at the time the cut occurred, MFAC and FPAC no longer existed and were unavailable to provide advice. The fusion community was therefore not consulted on the matter. However, the Secretary of Energy wanted advisory committees to help in such matters in the future, and this was one reason for the establishment of FEAC.

FEAC Reports

Dr. Conn announced that reports of the activities to date of FEAC and its sub-panels were now available to anyone interested in receiving them. Additional reports would be prepared covering current and future FEAC activities, and these also would be available generally.

SSAT Up-Date

Dr. Davidson presented the status of the design activities relating to TPX/SSAT. He started by recounting the conclusions and recommendations of the New Initiative Task Force concerning the steady-state advanced tokamak, which had been used as the starting point for the design activities. For clarity, the mission that had been determined for the tokamak had been broken into two main parts; the advanced tokamak portion of the mission, and the steady state portion.

The advanced tokamak mission aimed at:

- Achievement of high bootstrap fraction in the first stability regime.
- Investigation of confinement and stability properties at high aspect ratio.
- Second-stability operation with nearly 100% bootstrap current.
- Investigation of high beta, enhanced-confinement tokamak operating modes.

The steady-state mission aimed at:

- High-power, long-pulse/steady state operation.
- High-duty-factor operation.
- Divertor power and particle control.
- Efficient non-inductive current drive.
- Effective disruption control near operational limits.

While the primary mission of the device is very-long-pulse advanced tokamak operation, the impact of including up-grades for enhanced performance and limited D-T operation was assessed. Both copper and superconducting options were evaluated. The machine parameters that emerged were summarized as:

- For the SSAT-R, which would use copper magnets:

  R = 3.04 m   B = 5 Tesla   I = 3.5 MA

- For the SSAT-S, Option 1, which would use superconducting magnets:

  R = 2.25 m   B = 3.35 Tesla   I = 1.7 MA

- For the SSAT-S, Option 2, a higher field version of Option 1, which would also use superconducting magnets:

  R = 2.25 m   B = 5 Tesla   I = 2.5 MA

Details of the comparison between the copper (resistive) and superconducting toroidal field magnets are given in Table 1, the comparison between the resistive and superconducting poloidal field magnets is given in Table 2, and a comparison of power supply requirements is provided in Table 3.

Dr. Davidson reported that the costs of providing the upgrades in service had been estimated by the utility company (PSE&G) at $20 million for the resistive tokamak and at $1 million for the superconducting device. The utility had indicated that it was prepared to provide the capital needed for either of the service upgrades and to recover the investment over time through an increase in rates. The anticipated costs of electricity for both types of machine, based upon 1,000 pulses per year of 1,000 seconds duration each, are given in Table 4.

Dr. Conn remarked that the estimated electricity costs for the copper machine were approximately $20 million per year. He asked how much of the current $79 million operating budget at Princeton Plasma Physics

Table 1: Comparison of Toroidal Field Magnets

Design Feature	Resistive Version	Superconducting Version
Conductor	Copper	Nb_3Sn
Conductor configuration	Wound	Wound
Size	3 in x 13 in	1 in x 1.5 in
Conductor current	136 kA	45 kA
Cooling medium	Water	Supercritical He @ 4.5^{o} K
Cooling configuration	Two channels per turn	Internally cooled cabled SC
Flow	20,000 GPM	Forced flow
TF weight	1,600 Tonnes	200 Tonnes

Table 2: Comparison of Poloidal Field Magnets

Design Feature	Resistive Version	Superconducting Version
Conductor	Copper	Nb_3Sn and NbTi
Solenoid configuration	18 double pancakes	8 wound coils
Ring coil configuration	Half turn sections similar to those of JET	Large ring coils wound on site
Cooling medium	Water	Supercritical He @ 4.5^{o} K
Cooling configuration	One turn per circuit	One double pancake per circuit
Flow	15,000 GPM	Forced flow
TF weight	430 Tonnes	53 Tonnes

Table 3: Comparison of Power Supply Requirements

Requirement	Resistive Version	Superconducting Version
Peak PF	150 MW	40 MW
Peak TF	135 MW	4 MW
Heating	140 MW	140 MW
Refrigeration	N/A	5 MW
Peak power	425 MW	189 MW
Utility configuration	Substantial service upgrade to 425 MW @ 230 kV	Modest service upgrade to 215 MW @ 138 kV

Table 4: Anticipated Costs for Electricity

Nature of Usage	SSAT-R	SSAT-S
First year	$24.5 million	$13.0 million
Subsequent years with interruptible service credit, 30 minutes notice	$18.2 million	$11.0 million
"No demand charge" operation: 10:00 pm to 8:00 am and Sundays	$12.4 million	$ 9.3 million

Laboratory was spent on electricity to run TFTR. Dr. Davidson replied that electricity for TFTR cost $4 million per year. Dr. Conn asked if this meant that PPPL would require approximately $20 million more than at present when a copper machine started operation. Dr. Davidson answered affirmatively.

Dr. Davidson presented estimates of the capital costs of constructing the machines. These were:

For the SSAT-S at 3.35 Tesla, but upgradable to 5 Tesla:	$447 million
For the SSAT-S, upgraded to 5 Tesla:	$502 million
For the SSAT-R, at 5 Tesla:	$483 million

Dr. Davidson emphasized that the operating costs would be higher for SSAT-R than for SSAT-S, and that the duty-cycle would be longer because of the greater line power and cooling requirements.

Dr. Parker asked why this particular resistive design had been selected since the estimated cost of the device was too high. Dr. Sheffield responded that it had been chosen in order to keep the machine as close to TFTR as possible.

Dr. Davidson reported that a majority of the New Initiative Task Force had voted in favor of the superconducting option, based upon the facts that the cost difference between the two options was small at the same performance level and that the superconducting machine would provide greater technological relevance, would yield lower operating cost and would provide a higher duty factor. The minority opinion, which was very strongly held, was that the resistive option should, on the basis of the costs presented, provide greater performance (available parameter space) at a given capital cost.

Dr. Parker asked if any attempt had been made to review the relative technological risks involved. Dr. Davidson responded that such a review had been undertaken and it was concluded that it would be possible to construct either machine.

Dr. Davidson summarized the recommendations of the task force. The TPX/SSAT effort should focus on the superconducting option, through conceptual design. Because of current cost uncertainties, the scope of the project should be adjusted to make it consistent with a cost target of $400 million in FY92 dollars. Cost reductions in auxiliary systems such as power supplies, cryogenic equipment and external remote maintenance should be considered. The task of refining the design of a lower-cost copper-coil device should be undertaken to provide a possible back-up option should it prove impossible to meet the cost target with a superconducting design. The present baseline SSAT-S parameters, viz. $B_t = 3.4$ Tesla and $I_p = 1.7$ MA, are the minimum necessary to satisfy the SSAT mission. Higher-field capability would appear to be achievable and should be implemented even if proportionately higher plasma currents are precluded by cost constraints. Opportunities for this enhanced performance should be explored within the range R = 2.0 - 2.25 m, with an appropriately optimized aspect ratio.

Dr. Baldwin questioned whether it should be inferred from the last of these recommendations that the task force had encouraged the design team to reduce the aspect ratio. Dr. Sheffield responded that this would be the consequence if the minor radius were held constant.

Dr. Davidson reviewed the progress that had been made with the project since the last meeing of FEAC and outlined what was planned for the next few months. Dr. Weitzner commented that while the mission generalities were satisfactory, the specifics of performance were worrisome. Dr. Rosenbluth asked if the project was relying on the results of any of the work currently planned for ITER. Dr. Sheffield responded that the SSAT/SC would rely upon relevant work being undertaken for ITER. Dr. Rosenbluth asked how much the project cost would be increased if work on ITER were to stop. Dr. Bruce Montogomery, in the audience, responded that if ITER ceased to exist, work would be needed on the magnets for the new device, at a cost of approximately $10 million per year. He continued that the design team would rely on knowledge that was already available. Dr. Rosenbluth commented that the risk seemed to be quite small. This led to a discussion between Dr. Overskei and Dr. Montgomery on the status of niobium-tin magnets, and to the conclusion that a great deal of knowledge did exist and that most of the anticipated difficulties could be readily overcome.

Dr. McCrory asked how impervious was the proposed design of machine to magnet failure. Dr. Montgomery responded that both magnet systems, resistive and superconducting, carried risk. He explained that taking a machine apart and correcting a magnet defect was not the problem: The activation of the machine is what would give rise to difficulties.

Dr. Sheffield explained that in determining the performance criteria for the machine, when faced with the option of asking for an increase in the field or an increase in the current, the task force had unhesitatingly chosen an increase in the field. Dr. Parker asked what thought had been given to performance trade-offs versus cost trade-offs. Dr. Montgomery responded that a lot of "headroom" had been left in the design.

How much should be taken out to reduce cost, and how much should be left in to allow for the subsequent up-grade to 5 Tesla, was still open to debate. The only criterion that had been viewed as absolutely fixed was the total cost of the device, which should not exceed $400 million.

Management Structure for SSAT

Dr. Davidson reviewed the management structure that was under consideration for the SSAT. He indicated that a task force, comprising members from national laboratories, universities and industry, had been established to provide advice on the management structure that should be established for the TPX/SSAT construction project. The charge to the task force had requested a review of organizational approaches used in the design and construction of other major projects of a similar nature, and the development of preferred options for organization of the post-TFTR initiative project.

Initially, the task force recommended a "teaming" model (later designated as Option I) that would take maximum advantage of existing expertise and infrastructure in the national fusion program. Later this was modified to incorporate the early involvement of an industrial construction management contractor that would provide management support and expertise during the design phase, and would have responsibility for fabrication, installation and commissioning during the construction phase. (This arrangement was subsequently referred to as Option II).

The task force had determined that the TPX/SSAT would be a national activity. It was expected that more than 50% of the design activity would be undertaken by other than PPPL personnel. A National Advisory Council would be established to provide strong oversight of the project. A Program Advisory Committee would be established early in the project to provide program guidance to the physics design group, and subsequently to guide the experimental planning and execution.

Dr. Davidson indicated that the National Advisory Council was in process of being established. It would advise the Director of PPPL on important issues relating to the mission, project management structure, technical scope, cost and schedule of TPX/SSAT, and would provide advice on the appointment of the project manager.

Dr. Davidson asked that the written record of this meeting contain a statement that he wished to solicit recommendations for the post of TPX/SSAT Project Manager.

It was anticipated that the Project Manager would request Council response on:

- The project mission, major device parameters, and configuration changes as defined in the TPX/SSAT General Requirements Document.
- Project cost estimates and key schedule milestones.
- The organizational structure for the TPX/SSAT project.
- Major changes in scope/responsibility for project participants.
- Major safety and environmental submittals.

The membership of the council would comprise:

- One person from each of the institutions with subsystems design assignments.
- Four "user" representatives selected from universities likely to be involved in TPX/SSAT experimental operations.
- Up to three persons selected from industry.

The Council chairperson, who would serve for a two-year term, would be selected from "user" members.

Dr. Davidson reviewed, briefly, the function of the Program Advisory Committee, and described the management approach that would be employed for the project together with details of its organizational characteristics. Finally, he provided the following list of TPX/SSAT milestones:

• Submit management plan to DOE	May 1992
• National Advisory Council established	June 1992
• Mission approval received (KD-0)	June 1992
• Organization established	June 1992
• Program Advisory Committee established	July 1992
• Begin conceptual design	October 1992
• Issue RFP for subsystem assignments	December 1992
• Issue RFP for construction management	February 1993
• Conceptual design review	February 1993
• Issue RFP for magnet design	March 1993
• Approval for new start (KD-1)	May 1993
• Award construction management contract	August 1993

- Design-only funding available — October 1993
- Construction funding available — October 1994
- Start of Phase II (construction management) — October 1994
- Start assembly in TFTR test cell — September 1997
- Start of plasma operation — October 1999

Dr. Weitzner asked, assuming that the project held to the proposed schedule, when the final engineering decisions would be made. Dr. Davidson responded that the engineering decisions would be made over the next several months. Mr. Culler commented that there were so many sub-units that needed to be set up, that establishing these would be the schedule-controlling factor rather than what they had to accomplish. He felt a more rigid structure might be better during the conception, design and construction stages, with everyone located at one site, rather than having volunteer help scattered throughout the nation. Dr. Davidson responded that the project would use a lot of expertise that related to BPX that already existed at many institutions. He felt that the time-frame was realistic but conceded that the urgent hiring of a project manager was key to timely performance.

Referring to the two organizational options that the task force had presented, Dr. Baldwin suggested that a blend of the two may be more desirable. He was unhappy that the conceptual design should be the prerogative of one group while construction of the machine became the responsibility of another. He emphasized the need to identify the collaborating industry early on and pointed out that this may be difficult. He stressed his concern over the potential loss of continuity in the project.

Dr. Dean recalled that Dr. Davidson had discussed the original management plan privately with committee members during a recess in the last FEAC meeting. He reminded the committee that he had objected strongly to the plan and that this had resulted in the preparation of the alternative structure that had been presented today. He continued that this new plan still did not go far enough and presented a viewgraph summarizing comments that he had received from an interested division of General Dynamics. The comments were:

- That Management Option I was completely unsatisfactory since it reflected the current lack of private-sector participation in the fusion program.
- That Management Option II moved in the right direction, but not far enough, and required more detailed exploration of specific roles and responsibilities.
- That Management Option II would be improved by rapidly integrating industry into total-system responsibility.
- That Phase I of the project should focus on the communication of requirements to industry, and on the review and approval of industry's plan to satisfy those requirements.
- That Phase II should be viewed as the execution of industry's approved plan.

Similar comments had been received from other industries. The basic complaint was that industry did not have responsibility for the total system in either plan. Industry has construction responsibility but not design responsibility. The design will already be fixed when industry is called in. Dr. Davidson responded that many sub-systems would be incorporated into the design. In some areas, the best expertise lay in industry, but in many it lay elsewhere.

Dr. Dean presented viewgraphs that paraphrased comments that had been recevied from Grumman Space and Electronics Division and from Ebasco. Grumman had stated:

> "Both Options offer a business-as-usual approach to fusion program management; . . . The Options avoid the call in the National Energy Strategy and echoed by Congressional staff that a constituency must be developed to include an industrial base (comprising more than manufacturers of hardware components). Without intellectual involvement, industry fails to develop the required competitive position in the world. . . . The draft TPX program reeks of inefficiency, with committee involvement, packaged out throughout the country."

Dr. Dean stated that the Ebasco comments had contained a suggestion for improving the management structure:

> "Option II is significantly better than Option I. . . . Option II as presently structured, would not satisfactorily accomplish the goal of furthering the creation of a fusion industry. Such industry must be involved in the design of fusion systems and not simply in the construction after final design. A better option is to revise Phase I to include conceptual design only . . . the industry team, reporting to the project

manager, would be responsible for design, constructability and interface with vendor engineering. Experience has shown that the latter approach, with single lines of responsibility, is far superior. This is why Phase II should start at the preliminary design phase and not after completion of design.... In the Phase II (beyond conceptual), the industrial contractor should report to the project office, and be responsible for design and construction. The laboratories can report to the project office in an advisory capacity, or as 'seconded personnel'.... The laboratories, as needed, can also be placed under contract by the industrial contractor . . . cannot overstress the importance that industry attaches to being given responsibility for the design and not simply construction after final design... see here the opportunity for PPPL to truly lead the technology transfer from laboratory to industry... "

Dr. Dean made the following points in summary:

- The proposed TPX organization plan does not go far enough in giving sufficient responsibility to the industrial contractor(s) at a sufficiently early time in the project.
- It is essential that the industrial contractor(s) be given responsibility for engineering design, in addition to that for construction.
- Phase I should cover conceptual design only; Phase II is then preliminary and engineering design, and construction.
- There should also be a Phase III showing a continuing role for industry in machine maintenance.

Mr. Culler asked Dr. Dean if the TPX project office would still have control over the design in Phase II if the arrangement suggested by industry was implemented. Dr. Dean replied in the affirmative. Dr. Conn asked which industries had failed to respond to notification of the proposed structure, and whether their failure to respond was indicative of their satisfaction with the plan. Dr. Dean answered that McDonnell-Douglas was the only industry that did not respond in writing. A verbal response had been received that was similar to the written responses.

Dr. Parker asked who would appoint the project manager. Dr. Davidson responded that the National Advisory Council would provide advice on the appointment of the project manager. Dr. Parker then asked what if anything would be different between what was being proposed here and the previous BPX situation. Dr. Davidson replied that everyone would be more responsible and would strive to avoid cost escalation. Dr. Conn added that one major difference lay in the fact that assembly of the management team would occur much earlier in the process. Dr. Parker stated that the BPX/CIT project had a strong advisory committee, but not much notice had been taken of it. He expressed concern over the extra cost to the project that the proposed approach would entail and suggested that it might be better for PPPL to play a stronger role. He emphasized that he did not view this as a national project but rather as a PPPL project with national participation.

Dr. Overskei asked how much money would be spent on the design in the first two-and-a-half years. Dr. John Schmidt, in the audience, provided a figure of $7 million. Dr. Overskei asked if it was intended that most of this design work would be done by the existing fusion community. Dr. Schmidt replied that it was. Dr. Overskei pointed out that if the first RFP, for subsystem assignments, was issued on time in December 1992, there simply would not be enough time for industrial input before the proposed detailed KD-1 presentation scheduled for May 1993. Dr. Schmidt confirmed that this was correct and added that the project would only be able to issue small contracts to industry during that period.

Dr. Overskei asked if DOE had actually considered bringing in an industrial team at the start of the design, rather than completing the design before involving industry. The difference in the scenarios that were under discussion was that in one instance a national facility would operate in the year 2000, whereas in the other there would be national participation until the year 2000. This comment invoked considerable discussion but led to no clear conclusion. Dr. Overskei stated that when the conceptual design activity was finished, the cost and construction schedule would become of paramount importance. He asked if PPPL/DOE had considered issuing a firm fixed-price contract, with penalties. Dr. Davies responded negatively. Dr. Overskei asked if the lack of control by one entity over the integration of various components into the facility had caused DOE to ignore this option. He once again raised the question of whether the fusion community should be actively involved now and control the design of the machine, or whether it would be wiser to permit industry to take the lead now and have a machine with which the community could become involved in the year 2000. Dr. Conn asked what it was that industry really needed in order to construct the machine. Dr. Overskei responded that industry needed performance specifications, not complete designs. Dr. Conn commented that the performance specifications would have to be much better developed than they were at present.

Dr. Baldwin suggested that if industry were to be involved in the project from the start, in the manner that several industries had already suggested to Dr. Dean, then asking for a firm fixed-price bid would appear reasonable. Dr. Dean interjected that he thought DOE would only get such bids from Japanese companies. Dr. Davies explained that it had not occurred to her to ask for fixed-price bids from industry since she has been told over and over again by industry that fusion is too young a technology to risk a fixed-price bid. She had therefore considered it unreasonable to ask for such bids. She conceded that it was possible that Japanese companies may be prepared to underwrite the cost but had no real confidence that such would happen.

Dr. Davies and Dr. Parker entered into a discussion concerning whether this program should be viewed as constituting a truly national program as opposed to one with national participation. Dr. Parker commented that while the SSAT would be the largest project, the SSAT would not be the only machine in the fusion program. He stated that he had only agreed to a ceiling being placed on the cost of SSAT in order to permit others to continue operations. He said that at present the U.S. fusion program had five or six major projects in progress. In the year 2000 there would only be the SSAT project; the others would have disappeared by then. Hence, a balanced program for SSAT would have each current major player enjoying equal weight. Dr. Davidson countered by explaining in more detail the role of the Program Advisory Committee in relation to the overall management plan. This invoked further discussion on the entire organizational structure. Dr. Sheffield commented that the plan as presently proposed could lead to conflicts of interest. Dr. Davies asked if she was correct in inferring from Dr. Sheffield's statement that those entities involved in the design activities would be excluded from subsequent construction activities. Dr. Schmidt responded that PPPL's lawyers thought that this would indeed be the case: This reply caused Dr. Davies to state that DOE's lawyers and PPPL's lawyers needed to discuss the matter. Dr. Overskei interjected that DOE has the means to do as it wishes as far as the issuance of contracts is concerned.

Dr. Dean pointed out that there was absolutely no reason to select just one industry or consortium with the view that the chosen entity should undertake every task concerned with the new initiative. The really important factors that should be undertaken by industry were the integration of the design, the construction of the machine, and maintenance of the facility. Dr. Davies agreed that different laboratory-industry teams were needed for different subsystems. Dr. Dean stressed that it was important that the systems integration contractor have control over all of the subsystem contractors. He suggested that each industry be selected and introduced into the program at the beginning of the design process rather than when the design was complete. Mr. Culler agreed with Dr. Dean that the primary control should be exercised by industry but cautioned that just one industry should control the process: Utilizing different industries as leaders at the "conceiving", "design" and "construction" stages of the project would not work. He considered that industry should play a secondary role during design and should rely heavily upon those with the necessary detailed knowledge of what was needed. Dr. Conn commented that some degree of commonality appeared to be emerging. Dr. Davidson agreed and added that what seemed to be needed was what PPPL had been proposing with the exception that industry would be brought into the program at the very beginning. Dr. Rosenbluth cautioned that the proposed management structure would introduce many newcomers from industry to the fusion program during difficult financial times. The national laboratories would need to pare down their engineering staffs to compensate for this industrial influx. Dr. Rosenbluth reminded the meeting that ITER would also be making progress during the start of the SSAT project: There should be some synergy between ITER component design and SSAT component design.

Returning to the topic of national involvement, Dr. Baldwin pointed out that PPPL had put the management plan together, in consultation with others. The organization that had resulted was similar to that developed for BPX/CIT. He felt that there were programmatic issues and legal issues that needed to be resolved. He suggested that the Council was needed early in the program and that the Council, not the Director of PPPL, should control the program. He agreed with Dr. Parker that, as currently conceived, the plan provided no feeling of "group" ownership. Dr. Davies asked Dr. Baldwin if he would have preferred it if the Council had been in place and the task force had reported to it rather than to Dr. Davidson. Dr. Baldwin gave an affirmative reply.

Dr. Dean pointed out that the viewgraph of the organization that he had presented earlier showed how he felt the project should be managed, whether it was a PPPL project or a national project. The management and national project issues were different.

Dr. Conn stated that three main points had emerged. He summarized that the advice of the committee was that PPPL/DOE should attempt to implement a management structure that would permit the legal requirements to be met, while permitting industrial involvement at the design stage and allowing the scientific program to operate as a national project. He felt that different management structures would be needed for the different phases of the project. Mr. Culler suggested that the Council should act as a board of directors. Dr. Conn stated that the management structure was a critical issue and that the committee needed to reach agreement on it.

Dr. Sheffield indicated that he was strongly in support of industry taking a major role in the project, but pointed out that when the cost of the machine had been calculated, it had been assumed that the project would rely heavily upon the availability of existing capabilities. If the fusion community were to hand the project over to industry, an additional cost would be incurred by the program while industry learned what the community already knew. He emphasized that industry would not accept a fixed price contract.

Dr. Parker stated that a strong project manager was required whose performance would be reviewed by the Council. The infrastructure needed to support him should be provided by PPPL. The project manager would then report on project issues to the Council and on administrative and legal issues to the Director of PPPL. For this reason his responsibilities must be very clearly defined. Dr. Baldwin stated that such an arrangement would make the project manager's role advisory on legal and administrative issues but much stronger on program issues.

Referring to the suggestion that the Council act as a board of directors, Dr. Conn stated that the council members' roles would have to be defined very carefully. This was not a company that was being proposed. There were laws that defined a board of directors' responsibilities but these would not apply to the Council. Dr. Weitzner asked who would have the ultimate responsibility for controlling cost, and what would happen if the board of directors disagreed with the project manager. Dr. Davies stated that having the project manager report to a board of directors would not be acceptable to DOE. She would want the project manager to report to the laboratory director who, in turn, would report to DOE. She also was unhappy with the suggestion that the project manager report to one person on legal and administrative matters and to another on technical matters, since responsibilities would become unclear.

Dr. Overskei returned to the subject of whether the SSAT should be a community construction project or a community facility. He advised against using a committee to oversee the construction and operation of the facility. He suggested that the project management should only refer back to the board of directors when there was a need to change the boundary conditions of the project. A cost overrun should be the responsibility of the project management. He emphasized that the project could not be run by committee. Dr. Parker pointed out that throughout its existence the project could expect a constant stream of technical surprises that would require expert attention. Hence the project could not simply be tossed to industry.

Public Comment

Dr. Alex Glass, ITER Home Team Leader, indicated that the primary issue that should be of concern in determining the organizational structure is how much R&D is likely to be involved in the project. More often than not, the basis for such R&D resides in the national laboratories. The R&D should therefore be carried out in the laboratories and then transferred to industry. He stressed that it was absolutely critical to bring industry into the program at the earliest possible moment.

Mr. Anthony Chargin, Lawrence Livermore National Laboratory, pointed out that the SSAT project was yet to be approved. He suggested that the involvement of industry early on in the project would help in the approval process. He made the point that if people were determined and willing to make a management structure work, then any structure could be made to work.

Dr. John Schmidt, Princeton Plasma Physics Laboratory, stated that each unit within the fusion program should look after its own specialty in the design of the SSAT. *Dr. R. J. Goldston, Princeton Plasma Physics Laboratory,* concurred, stressing the need for national involvement in defining the details of the program.

Dr. Robert W. Bass, representing the *Cold Fusion Advocacy Group,* provided a description of recent developments in the field of cold fusion.

Wednesday, May 20, 1992

Change of Agenda

Dr. Conn opened the meeting by informing the committee that the agenda for the balance of the meeting had been modified because slower progress than anticipated had been made with the discussion of items on the previous afternoon.

Fusion Materials Development: Status and Requirements

- Overview of U.S. Materials Program

Mr. R. J. Dowling, Director of the Division of Development and Technology, OFE, presented an overview of the U.S. fusion materials program. He indicated that the goal was to provide qualified materials for magnetic fusion devices that would contribute to the realization of fusion as an economically viable, safe, and environmentally acceptable energy source. The technical approach had focussed, for the long term, on materials for the power-producing reactor and, for the short term, on the needs of devices that would serve as intermediate steps to the reactor. Recent accomplishments included the generation of data that would help in the selection of materials for use in ITER, and early progress in the development of reduced activation alloys.

Mr. Dowling recounted the history of materials development in the fusion program and provided a viewgraph that illustrated how the materials development budget had varied from FY80 through FY92. He stated that the fusion materials programs in Japan and the EC were larger than that in the U.S., and that the Russian program was comparable with that of the U.S. He stressed that the Japanese materials program was significantly larger than that of the U.S. The total number of materials scientists working in the U.S. fusion program was twenty, whereas over one hundred Japanese fusion materials scientists were working in universities alone. Mr. Dowling provided breakdowns of the FY92 materials budget by mission, by budget category, and by material class and application.

Finally, Mr. Dowling summarized U.S. Program achievements. The U.S. program had provided most of the data that led to the selection of austenitic stainless steel for the ITER structure, and that defined critical work that remained to be done. Recent activity concerned with reduced activation materials had identified promising base compositions of ferritic steels and vanadium alloys for further development. Evaluation of silicon carbide composite materials had begun, and active investigations of alternative structural materials, of divertor structural materials, and of ceramics for RF, diagnostic, optical and insulator applications were in progress.

Dr. Overskei asked what fraction of the materials budget went to universities, to national laboratories and to industry. Mr. Dowling replied that 5% went to universities and the balance to the national laboratories. No funding went directly to industry although the laboratories did sub-contract out some work. Dr. Overskei asked if persons could purchase materials from the national laboratories when those materials had been shown to work. Dr. Everett Bloom responded that there was a stockpile of material available for research purposes. Dr. Overskei said that what he really wanted to know was whether one could buy fully developed material for use as structural components. Dr. Bloom responded that it was not possible to purchase fully developed materials for use as structural components from the national laboratories. [*Comment added by Dr. Bloom in review of the minutes: The objective of the Fusion Materials Program is to develop materials with properties and characteristics that will allow fusion to be realized as an economic, safe, and environmentally attractive energy source. The development process progresses through three steps which are characterized as (1) feasibility studies, in which properties and characteristics that limit performance of the materials are identified and approaches to materials development are explored, (2) the materials development step in which limiting properties and characteristics are improved through chemical and microstructural modifications of the materials and materials engineering issues are initially addressed, and (3) materials engineering in which the technology for using the material in engineering applications is developed. In the latter stages of materials development and in materials engineering, industry becomes increasingly involved. Indeed it is industry that will produce materials for use as structural components.*]

Dr. Conn asked if anyone knew the nature of the breakdown of materials development funding in Japan. No one had any specific information. Dr. Ripin said he thought that DOE funded a very large program in materials development. Mr. Dowling said that this was correct: The DOE presently funds a $300 million materials program in Basic Energy Sciences but little of it contributes to fusion. Dr. Bloom pointed out that although it is not the mission or objective of the BES Materials Program to develop materials for fusion applications, the BES program was nevertheless very important to the efforts to develop materials for fusion. As examlpes, the HFIR, one of two major irradiation facilities, is a BES facility; BES supports a major program in radiation effects research which provides much of the foundation of understanding upon which the development of materials for fusion is based; and the BES program supports major efforts to develop the tools and technologies to characterize the microstructure and microchemistry of materials, tools which are essential to the Fusion Materials Program. Dr. Conn pointed out that, because of the extremely high cost of materials development, it was vitally important that the fusion program determine how to leverage off this large program.

- Development of Structural Materials for Fusion

Dr. Everett E. Bloom, of Oak Ridge National Laboratory, presented a summary of the current situation in the fusion materials development program. He explained that, due to time constraints, he would omit from his presentation divertor materials and ceramics needed for electrical applications. He indicated that he would deal with the performance of materials in a fusion environment, and also explain the approach and time-scale needed for the development of structural materials.

Dr. Bloom emphasized that it was important to understand the working environment and to use the correct properties of the materials during component design. He illustrated the response exhibited by materials to specific operating environments and outlined the issues involved in the development of materials for use in those environments. The important features that would enable a material to function in a fusion environment included:

- ability to withstand radiation damage, where swelling, irradiation creep and degradation of physical and mechanical properties were important factors;
- chemical compatibility, where corrosion, hydrogen embrittlement and degradation of mechanical properties occurred;
- ability to withstand elevated temperatures, where creep was important;
- ability to withstand mechanical loads, both steady state, cyclic and high rate (such as during disruptions) requires adequacy of several properties including tensile strength, fatigue and fatigue crack growth, and fracture toughness;
- suitability for use in complex structures, where ease of fabrication, welding/joining, and ease of maintenance are important factors.

Dr. Conn asked what characteristics were distinctly different, in terms of materials response, between composite materials and metals. Dr. Bloom responded that both types of materials were affected similarly to some extent, but that the magnitude of the effects were dependent upon the actual conditions. He felt reasonably certain that his list of environments and responses was comprehensive and applicable to all types of materials.

Dr. Bloom described, in detail, the metallurgy of radiation damage effects upon the engineering properties of materials. Dr. Weitzner asked if it was possible to use such knowledge to develop superior materials. Dr. Bloom responded that it was not possible to design a material on purely theoretical grounds. Rather, materials scientists try to understand, after the fact, what has happened and then feed back that knowledge into the development process; this technique has been found to work successfully. Dr. Bloom stated that there were three main steps in the development of a new material. The first step involved a design study from which would emerge suggestions for a material with good potential. Ways of synthesizing the material would be studied and its basic properties and responses to the operating environment determined. Consideration would be given to any unique characteristics, and the effects of structure and chemistry determined. Dr. Conn commented that very little was known concerning the effects of radiation upon composite materials, and that the fabrication methods for such materials were in their infancy. Dr. Bloom agreed.

The second step in a material's development related to its refinement. Here composition and microstructure variations are investigated to obtain acceptable performance for limiting properties; estimates are made for end-of-life; fabrication and joining techniques are developed; and chemical issues related to design are explored. The third step involves materials engineering, and is the stage at which the technology and data base required to utilize a new material are developed.

The length of time required to develop a new material for fusion applications depends upon the size of the program and upon the available budget. But, the rate-controlling step in the fusion materials development process is the investigation of radiation effects. On the average, it takes 20 - 30 years to develop a material for fusion; 7 years for the feasibility study, 10 years for refinement, and 12 years to develop the engineering data base. Most candidate materials are shown to be unsuitable early on in the process and are not pursued further. Only a very few will proceed to maturity.

Dr. Bloom provided a review of the materials requirements for ITER. He identified the issues that were likely to impact the feasibility and design of ITER: They are shown in Table 5.

Dr. Overskei asked Dr. Bloom what was meant by the term 0% uniform strain. Dr. Bloom explained that it meant that deformation was confined to local areas and was not distributed evenly throughout a material.

Dr. Bloom explained that radioactive decay and basic material characteristics had reduced to four the number of candidate systems for use as low-activation materials. These systems included: Vanadium alloys; ferritic stainless steels; austenitic stainless steels; and silicon carbide. He reviewed, in detail, the strengths, weaknesses and uncertainties associated with each of

Table 5: Materials Requirements for ITER

Design Issue	Impact	Materials Issue
Design of workable divertor	Divertor design and life	Corrosion, radiation effects on mechanical properties
First wall/blanket/shield coolant tube failures	Availability	Stress corrosion cracking (SCC) and irradiation assisted SCC (IASCC)
Structural failure during disruption loading	Major failure	Effects of irradiation on fracture toughness
Performance of ceramics in heating and diagnostic systems	Unacceptable design, limited life	Radiation induced conductivity and radiation induced electrical degradation
Deformation of first wall/blanket structure	Must be maintained within limits	Radiation creep of structural materials
Welding of irradiated materials	Ability to repair and maintain	Helium at appm level seriously degrades weldability

the four potential groups. Dr. Weitzner asked if it was necessary to evaluate each material in a group separately, or whether it was possible to look at each group of materials generically. Dr. Bloom responded that it was possible to start with any particular group and to quickly narrow it down to a few materials. In general, it was only necessary to measure a certain few properties in order to do this, which conserved both time and money. Dr. Parker asked why it had been indicated that vanadium was not compatible with a helium coolant. Dr. Bloom replied that compatibility would require either strict maintenance of the purity of the helium, or the discovery of a way in which to protect the vanadium from any impurities, especially oxygen, in the helium coolant. Dr. Parker asked if that was the reason that a lithium system was indicated as being preferred for vanadium alloys. Dr. Bloom replied that it was.

Dr. Bloom discussed SiC/SiC composites. He explained that one difficulty with these materials was porosity. Existing materials were not very encouraging; they needed tailoring to improve performance. Dr. Bloom said that he felt that the U.S. could develop a suitable material by the year 2025 but that the U.S. did not currently have the right environment for testing: A 14 MeV source was needed. He indicated that the tools existed to disqualify materials, but not to qualify them. Dr. Bloom presented a projection of the budgetary support, exclusive of the requirements of the neutron source, that needed to be available through the year 2000 in order to meet the low-activation materials program objectives. The fusion neutron source itself must be operational by the year 2000 in order to support the goal of a fusion demonstration reactor by 2025.

Referring to the projection of $20 million per annum shown for the end of the decade, Dr. Rosenbluth asked if this figure covered the world-wide program in low-activation materials. Dr. Bloom answered that the estimate was for the total program. It was assumed that the main materials development collaborations would be with the Japanese, who have on-going collaborations with ORNL (ORNL - JAERI) and PNL (PNL - MOMBUSHO). If the Japanese were to join the low-activation materials program also, then their contribution would permit a reduction in the U.S. support. Dr. Rosenbluth stated that the USSR had operated a large materials development program for years and asked if any attempt had been made to look into their progress. Dr. Bloom answered affirmatively. Dr. Rosenbluth asked if all the Russian materials and fabrication techniques related solely to military applications. Dr. Bloom replied that they did not and that some of them may have uses in fusion technology.

Dr. Berkner asked the size of the largest pieces of SiC that had been used to date. Dr. Bloom replied that SiC composites had been used in some aerospace applications, including nose cones. Dr. Shahram Sharafat, from the audience, responded that pieces as large as 1.5 m x 1.5 m x 1/2 - 1 cm thick had been fabricated. Dr. Berkner pointed out that after a suitable material had been developed, it would be necessary to find out how to use it. Dr. Bloom agreed, stating that it would be necessary to develop a whole industry in order to provide these materials. He continued that while materials scientists might be able to determine the recipe for a low-activation material, the difficulty would be to develop the experience base needed to manufacture and fabricate the material, in the same manner as was presently possible for steels.

Dr. Berkner asked if it would really be possible to develop the material and put everything that was needed in place by 2025. Dr. Bloom responded that it would take a sustained budget of a few tens of millions of dollars per year in order to complete the task in time. Dr. Conn commented that this requirement was small in terms of the overall fusion program. Mr. Dowling pointed out that the fusion program would not drive the development of composite materials: Rather, automotive and aerospace requirements would. Dr. Davies stated that there would be decades of experience here by 2025. Dr. Sheffield cautioned that if the automotive and aerospace industries did not pursue the right composite materials, then the fusion program would have to find all the money for their development, if these materials provided the real solution to fusion's problems.

Dr. Holdren presented the radiation hazard indices for the four types of material that were the subject of the low-activation materials program. He expressed concern over the potential use of austenitic stainless steel and asked why tens of billions of dollars should be spent developing fusion energy if austenitic steels represented the best environmental result. He pointed out that SiC would be by far the best choice. Dr. Holdren stated, however, that he did not believe that the waste issue will be the critical one for fusion. Viewed from the perspective of long-term waste, fusion would be much better than fission irrespective of the materials used. He stressed, however, that the materials development budget must be increased significantly to emphasize SiC: He pointed out that the bulk of the available funding was being spent on the development of steels. Dr. Bloom cautioned that considerable debate was taking place over the true characteristics of SiC with respect to low-activation properties. There were still a lot of questions to be answered. Dr. Parker asked if carbon had been looked at. Dr. Bloom replied that graphite had been shown to be one of the poorest materials from the point of view of radiation damage.

Dr. McCrory commented that the inertial fusion energy community had yet to look at materials issues. Dr. Conn asked if other industries were working in materials areas that would be of value to fusion. Dr. Bloom responded that there were very significant, government-funded programs in the U.S. looking into materials that could be of potential use. Dr. Conn commented that there were also some industry programs and that the fusion program must gain leverage off all of these. He continued that he could not understand the use of niobium as the underpinning structural material for use in divertors, in the Plasma Facing Components program. The issue was one of after-heat after shut-down; SiC would present far fewer problems than metals in this respect and would enhance safety. Dr. Conn asked if any problems had been experienced for ferritic materials under severe magnetic loads. Dr. Bloom replied that this matter had been considered and that originally it was felt that the loads would be manageable. Now, some scientists were having second thoughts and the matter would again be reviewed.

Dr. Conn asked Dr. Bloom when the 14 MeV source would be needed, pointing out that it might be needed at different times for different materials. Dr. Bloom indicated that it needed to be in place and operating by the year 2000 if materials were to be available for use in DEMO by 2025, regardless of which material was being pursued. Dr. Baldwin stated that a DEMO by 2025 was simply not feasible with the current budget scenario. He suggested that FEAC should stop permitting a 2025 DEMO to drive their thinking.

Dr. Ripin asked how the resources provided by the fusion program for development of these materials compared with resources being deployed in other programs that might result in the development of fusion-relevant materials. Dr. Bloom responded that a very large effort was being funded outside of the fusion program in the field of ferritic steels, but that this effort did not extend to low-activation aspects. The work in vanadium alloys was being funded mainly by the fusion program. A vast program was being funded in the field of SiC; the fusion effort here was very small.

Public Comment

Dr. Bogdan C. Maglich, Advanced Physics Corporation, presented information on a self-sustained aneutronic fusion reaction in Prigogine-plasmas confined in strong-focusing field-reversed self-colliders. He provided a detailed description of a simulated reactor. He indicated that he had signed an agreement with the Russian Federation to design and build these self-colliders, which would use a magnetic field of 10 Tesla.

Review of Charge to FEAC

Dr. Conn drew the committee's attention to the Letter of Charge of September 24, 1991 and reviewed the matter for which a response was required from FEAC immediately following the current meeting. The relevant paragraph stated:

> "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."

This charge had been supplemented by Dr. Happer's letter to FEAC of February 20, 1992, which highlighted four major points:

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

Panel III Report

Dr. Dean presented the principal findings of Panel III. He stated that the panel did not review ICF matters since it was felt that a separate charge to FEAC relating to this field might soon emerge. After deliberation, the panel had developed a number of general principles related to the pursuit of alternative concepts.

> **Five of these principles became the topics of considerable subsequent discussion by the committee. The principles concerned have been clearly identified in the following paragraphs to assist with comprehension of what was said.**

Principle #1:

> *Programs such as fusion, that will continue over prolonged periods of time, must retain breadth and flexibility in order to embrace technological changes that would most certainly occur.*

Since it is a long-term endeavor, the development of fusion will depend upon a continual inflow of new, younger talent. If fusion is to continue to attract and inspire a new generation of scientists and engineers, it must be seen clearly to be an exciting field, open to achieving success by whatever path. A serious negative consequence of the 1990 DOE decision to narrow the fusion research program to tokamaks is the current widespread impression, held primarily by university research workers, that DOE is unreceptive to new ideas and innovation. The panel considered it essential that this impression be reversed.

Principle #2:

> *In setting priorities for research on various fusion concepts, the larger the cost of undertaking research and development on a given concept, the more stringent should be the requirement that it offer some tangible advantage, and the more desirable that there be international collaboration.*

Principle #3:

> As a further statement of general principle, Panel III had endorsed a finding on program balance contained in a recent ESECOM study:
>
> > *"Although research priority should reward the more successful fusion confinement and technology options, it is essential not to concentrate so heavily on a single line of development (no matter what the budget) that better concepts cannot continue to be developed for improved second-generation configurations. . . . . The resources for the better ideas may be very strained and limited and, thus, the second- and third-generation technologies may take longer, but their pursuit should always continue."*

Principle #4:

> *In order to properly balance the funding both between and within the various components of the fusion program, a vigorous, on-going effort on concept reactor designs and systems analysis is required, covering all aspects of the fusion program.*

In formulating a potential policy in concept improvement, the panel divided fusion concepts into three categories, viz. highly-developed concepts, developing concepts, and small-scale innovative concepts. Tokamaks and stellarators were viewed as highly-developed concepts, while the reversed field pinch (RFP) and field reversed configuration (FRC) were viewed as developing concepts. The panel found merit in maintaining some level of effort in each of these areas as a matter of policy and recommended distribution of effort as outlined below:

Principle #5:

> *The bulk of the effort should be concentrated on the highly-developed concepts, a variable but modest effort in developing concepts, and a small, but reliable, funding commitment for research in the innovative concepts area.*

Placed in the category of highly developed concepts were tokamaks and stellarators. The category of developing concepts included reversed field pinch (RFP) and reversed field configuration (RFC) devices. The small-scale innovative concepts category was meant to encompass ideas that could be tested at relatively modest cost and have a reasonable likelihood of providing critical data within a short time period. A specific sum of money could be identified and set aside for innovation, to be allocated after a solicitation for proposals followed by a batch peer-review undertaken by individuals with no perceived conflicts of interest. If high-quality proposals were received, the program might eventually reach a steady-state value of $10 - 15 million per year, with a 20 - 30% turnover per year. In addition to the obvious criterion of technical excellence, the selection criteria should include the potential impact of the promised result, and the likelihood of addressing key issues in the time frame and for the cost proposed.

Industry believes that successful innovation requires that the innovative work not be carried out within existing manufacturing divisions. Rather, innovation is better placed in organizational components concerned with the creation of new products, where pressure due to delivery schedules is absent. The panel therefore felt that consideration should be given to managing the innovative concepts program from a separate organizational entity within OFE, thus to provide some protection from the schedule and budget pressures of the more advanced concept development programs. Priority in this program would be given to testing deserving new concepts on a small scale, as opposed to, for example, developing new diagnostic instrumentation or new tokamak components. Though the latter may be worthy programs, these should, in general, be funded by the larger subprogram elements in OFE. Finally, the panel thought that collaborations with international efforts should not be a requirement for these small-scale programs.

Dr. Overskei commented that the approach of establishing a separate, insulated organizational component within OFE to manage small innovative concepts would give rise to the generation of solutions to problems that did not exist. Mr. Culler suggested that FEAC should be careful not to give the impression that they had considered every conceivable option and had determined that only projects aimed at magnetic confinement fusion should be funded. Dr. Conn said that the competition that this program would generate between it and the balance of the fusion program needed to be discussed. Dr. Dean pointed out that the kinds of conceptual programs that the panel had been considering here, in total, would not ever consume more than 10% of the total fusion budget.

Dr. Dean provided a summary of Panel III's findings.

Discussion of Panel III Findings

Dr. Conn suggested that before reviewing the recommendations that the panel had made to FEAC, they should review the report that they had received and discuss any issues that needed clarification. Dr. Overskei asked if the panel viewed the testing of a new idea using an existing facility as truly a new concept. Dr. Dean answered that the panel would view it that way, but that the innovator would have the option of asking the innovative concepts program to fund the project, or of asking the program related to the existing facility to fund the project. He explained that, basically, the panel was trying to get the fusion program to reposture itself to encourage innovation. Dr. Overskei pointed out that when an innovative concept was developed or tested on an existing unit, that unit in effect became a "user" facility. In such circumstances it would be necessary for DOE to not only fund the innovation but also to ask the "user" facility to provide "time" for the innovative project. Dr. Ripin indicated that the panel had considered that aspect in reaching its conclusions.

Dr. Conn asked Dr. Dean to clarify whether or not the panel had felt that the innovative concepts should really be basically different from existing main stream projects. Dr. Ripin replied that the panel had felt that no restriction should be placed upon the type of concept that should be funded. Such concepts could even be directly associated with an existing tokamak. He emphasized, however, that this category of funding was intended for high risk concepts that one would normally expect to fail; it was not intended that the funding be reserved for projects with a high certainty of success. Dr. Overskei asked if theoretical projects would be regarded as fitting into this category. Dr.

Dean answered affirmatively. Dr. Conn commented that the criteria should be applied to materials and components also.

Dr. Overskei stated that what the panel was suggesting was laudable but would be very difficult to implement. Previously, so many good ideas have failed, that the outcome of any peer review process will be support for concepts of a conventional nature only, and the virtual exclusion of truly innovative concepts. Dr. Dean countered that the program should look for independent, open-minded reviewers. This comment led to an open discussion of the peer review process during which the integrity of reviewers in general was challenged.

Dr. Baldwin commented that new innovations were likely to lie outside of present parameter space, and thus outside of current experience. He asked if DOE would be able to fund a category of work that was unspecified. Dr. Davies replied that it might be possible to provide as much as $2 million per year for that type of category. This led to a wide discussion concerning budgets for the smaller projects in the fusion program. Dr. David Crandall pointed out that if he was currently unable to justify funding a project as promising as the LSX, he would find it very difficult indeed to justify funding any other novel concept. Dr. Dean responded that the panel had reviewed such matters but had still come to the conclusion that the fusion program should provide money for use outside of the tokamak area.

Dr. Linford asked for an indication of the range of operating budget required for an intermediate facility. Dr. Dean replied that the budget required to operate LSX was approximately $5 million: The maximum operating budget for an intermediate facility should not exceed $20 million. Dr. Linford pointed out that if one were to add together one small concept and one intermediate concept, the combined operating budget was greater than the AFC (alternative fusion concepts) budget before the project was axed. He asked if that was the level of budget that the panel was advocating for the innovative concepts program. Dr. Dean replied that it was: The figure could be as great as 15% of the total fusion budget, although it would not reach that level during the first few years following establishment of the innovative concepts program. Dr. Conn pointed out that the panel had developed a set of criteria for plasma physics that were generic and that could be applied equally well to materials and components.

Dr. Linford stated that it would be foolish for DOE to build up an innovative concepts program as suggested, only to destroy it again when budgets so dictated. He cautioned FEAC to consider very carefully the advisability of establishing such a program without a thorough review of what happened last time. He pointed out that to start small-scale innovative projects when no intermediate size facilities existed to provide such programs with future upward mobility would not work successfully and would be wasteful of resources. He also expressed reservations about the proposed review process, particularly with respect to how one determined what was "good" and what was not. He suggested that a mentoring arrangement between national laboratory personnel and innovators might have some merit. He pointed out that the demise of the reversed field pinch program came about because its value was judged using "fringe" knowledge. He emphasized the need to foster a process that would lead to a greater understanding of the "basics".

Dr. Parker stated that the panel's suggestion that a fixed percentage or fraction of the fusion budget be set aside for innovative concepts must be reviewed in the light of each new budget. He suggested that the fraction should vary with the level of the budget. Dr. Dean commented that the amount set aside for the innovative concepts program does not have to be a fixed fraction of the overall budget. The panel's concern was that it should not be zero. Dr. Parker commented that the panel's recommendations had left a void in the tokamak area. He pointed out that there were very few facilities where one could use a tokamak in an exploratory mode: Tokamaks were too big to modify readily. Dr. Parker suggested that tokamaks should be included in the innovative concepts program and added that one barrier to tokamak innovation was the existence of a "wall" between APP and Confinement Systems at OFE. Dr. Dean pointed out that tokamaks were included in the panel's innovative concepts program. The panel did not see the need to specify a special set-aside for tokamak innovation since it was thought that the tokamak program would look after its own.

Dr. Sheffield stated that he had a problem with the notion of setting aside a fixed percentage of the fusion budget for innovative concepts. He also disagreed with 100% of the funding being channeled into tokamaks. He pointed out that the majority of the outstanding issues were technical. He questioned the need for the U.S. to operate five tokamaks, and asked if they were all really needed.

Public Comment

Dr. Dale Smith, Argonne National Laboratory, referred to the fusion materials program and indicated that there was general agreement on the critical issues that needed to be addressed. He suggested that the development of structural materials should be separated from the development of blanket materials.

Dr. Smith indicated that during the selection of niobium as the divertor material for ITER, waste management was not the primary criterion. Rather, safety had been given the top priority and in this respect niobium was superior to copper and molybdenum.

Dr. Smith pointed out that in any materials development strategy it was essential to realize what properties of a material it was possible to change, and what properties could not be changed. While certain properties could be modified, others could not. This latter category included thermal properties such as thermal conductivity and melting point.

Dr. Smith raised the issue of low activation materials. He indicated that it was necessary to make trade-offs between waste management issues and other issues associated with the provision of desirable properties. The issue of safety was very complex and involved many interacting parameters.

Dr. Shahram Sharafat, UCLA, pointed out that the comments that had been made earlier during the meeting concerning SiC composites, had been based upon first-generation material which contained oxygen as a significant impurity. Oxygen was believed to be the major contributor to degradation of the material's properties. A newer version of the material that would not contain oxygen was under development in Japan. The original material had been stable up to 1200°C, while the new generation of SiC was expected to be stable up to 1800°C. The properties of SiC at elevated temperatures would then be much superior to those of metals. In addition, the new method of fabrication for the composite material would permit much thicker sections to be produced. Originally it had only been possible to produce material with an maximum thickness of between 4 and 5 mm, at a density of 85 - 95% of theoretical maximum. With the new technique, it would be possible to produce composite material 20 cm thick at close to 100% density.

FEAC Deliberations

Dr. Conn suggested that the committee review the five principles that had emerged from Panel III's report, starting at the beginning. Dr. Ripin explained that what was now being referred to as Principle #1 had come about since it was possible that tokamaks may not result in a working reactor. The intent of the panel had been to acknowledge this and to allow for the possibility that an improved reactor could be developed. He stressed that FEAC should, during its review of the panel's report, bear in mind the best interests of the fusion program as a whole. Committee members should not permit a vested interest in an on-going program to result in failure to support a program relating to deserving new concepts. Dr. Rosenbluth asked if such concepts could include tokamak improvements. Dr. Dean replied that concepts of every nature could be included: The aim was to develop a good reactor.

Dr. Rosenbluth pointed out that programs leading to improvements in the community's understanding of fusion had been omitted from the panel's report. While such programs provided no change in concept, they were innovative. He suggested that another general principle could be added that was directed towards an improvement in fundamental knowledge. Dr. Dean responded that the panel had not been asked to review the entire fusion program, only concept improvements and innovation. Dr. Rosenbluth commented that the fusion program was more concerned with pushing parameters than with gaining an understanding of the process.

Dr. Holdren stated that if FEAC was not too confident that the main line of approach, which relied upon tokamak technology, would lead to a viable reactor, then it would be prudent for the fusion program to seek some insurance. Thus, some funding would be required to support alternative concepts. Dr. Conn suggested that FEAC should take account of what was going on internationally, since this could provide some measure of insurance, and in making its recommendations should balance what the U.S. program was pursuing with what was being pursued overseas. Dr. Holdren agreed that the U.S. should take account of what was going on elsewhere, and should participate whenever the opportunity arose. Dr. Conn asked if the panel had taken this into account. Dr. Dean responded that in the area of small-scale innovative concepts, they had not. With regard to the larger projects, they had. Dr. Dean stated that international cooperation was a two-way street and cited the example of the reversed field pinch program in which persons from abroad had come to the U.S. to participate in the program.

Dr. Holdren said that there were two classes of innovation that needed to be considered. The first class was related to tokamaks, and the second was not. He pointed out that the report had not made the mistake of trying to equate the two. He stated that FEAC's letter should make the same distinction.

Dr. Overskei asked if the panel had considered the process that would be pursued in making the awards. Several panel members answered positively and added that they had gone as far as to develop a list of criteria.

Dr. Dean said that a major need would be the establishment of a credible, even-handed peer review process. Dr. Conn commented that it was clear that the panel had given considerable thought to the matter.

Dr. Linford stated that at the time that the alternative concepts were eliminated from the fusion program, the U.S. was ahead in stellarator, RFP and FRC technologies, yet DOE chose to eliminate areas where the U.S. was in the lead and to continue to support the tokamak where it was not. He suggested that DOE should apply the principles that it had used in supporting the alternative concepts to the tokamak program and concentrate its support in those areas where the U.S. was ahead. Dr. Conn pointed out that FEAC had already made that recommendation twice in its letters of response.

Mr. Culler said that he endorsed the notion that there was a reasonable chance that the tokamak might not be successful, for example, due to unfavorable economics or to lack of suitable materials, and that this uncertainty provided a good reason for the fusion program to continue to pursue alternative technologies. Dr. Dean emphasized that the panel had taken the approach that even if one knew with absolute certainty that the tokamak would make a viable reactor, one should still explore alternatives in an attempt to develop an even better reactor.

Dr. Conn asked if the committee was agreeable to accepting Principle #1, that the U.S. fusion program needed innovation in order to maintain its intellectual integrity. Dr. Parker asked if the panel had made any assessment of the current program. Dr. Weitzner answered affirmatively. Dr. Parker then asked if the panel had agreed that the right amount of money was being spent. Dr. Dean responded that the question was not relevant; the panel had been well aware that insufficient money was being spent everywhere. Dr. Parker stated that tokamak concept improvements should not be left to the main-line tokamak program. Dr. Dean responded that the panel had not been asked to review and make recommendations concerning tokamak improvements per se. Consequently, the panel had not addressed the physics of the tokamak in any depth.

Dr. Baldwin commented that it was imperative for the fusion community to establish and retain an appropriate posture with respect to alternative concepts. The greatest recent criticism of the fusion program had been the fall-back to a tokamak-only mode and the acceptance by the fusion community of the loss of all else. Dr. Sheffield pointed out that the alternative concepts programs were being used to investigate fundamental physics. He stressed that FEAC must find some suitable phraseology for the letter that would ensure that proposals and programs were judged on their scientific merit. There followed a discussion of the restrictions that the tokamak-only syndrome had placed upon promising proposals since the budget cut-back, leading to the comment by Dr. Sheffield that the restrictions were so severe that if the work proposed was not actually going to be undertaken in a tokamak, it would not get funded irrespective of its relevance to the program.

Dr. Davies explained that there was absolutely no scientific basis for the manner in which the program was reduced two years ago. She emphasized that it was simply a budgetary decision. All of the alternative programs were good scientific projects, but they were all under-funded and all were in need of facility upgrades. They all needed larger operating budgets and DOE simply did not have the money. At that time, DOE was also making a major commitment to ITER, another tokamak program, and was trying to fund BPX. It was a question of cutting either the entire BPX program or a handful of under-funded alternatives. The decision went the way it did because the tokamaks were all interlinked, those in the U.S. and ITER, and OFE needed to retain the integrity of the tokamak program and to fund BPX. Now that BPX has been cancelled, OFE is able to look into alternative concepts again. If, in turn, TPX is not funded, then an alternative concept of significant size might move ahead.

Dr. Conn and Dr. Weitzner entered into a discussion concerning the desirability of providing "breadth" to the fusion program and whether a tokamak-only program could be viewed as providing such breadth. Dr. Overskei commented that anything leading to an improvement in confinement or reliability should be pursued, but cautioned that FEAC's letter should not give the impression that the current path is incorrect and that the pursuit of alternatives will solve all the problems. He stressed that FEAC would do the fusion community a great disservice if it was overly critical of the tokamak program. Dr. Holdren pointed out that spending a small amount of money on alternative concepts, even in difficult financial times, should not be viewed as a criticism of tokamaks.

Dr. Parker suggested that it might be better if FEAC were to review the recommendations of the panel rather than to review the general principles, since the recommendations and principles followed similar lines. Referring to Recommendation #1, Dr. Parker said that he could not understand why the committee was so concerned over saying that there are some uncertainties with the tokamak. Dr. Conn pointed out that if FEAC agreed with Recommendation #1, then members would in effect be saying that alternative concepts may be pursued. On a show of hands, 11 members indicated that they agreed with the recommendation while 2 members did not.

Public Comment

Dr. Leo Mascheroni, of Los Alamos, New Mexico, indicated that various members of Congress had suggested that he make a presentation to FEAC of his ideas relating to inertial confinement fusion. The program that he advocated was based upon chemical energy as opposed to more-expensive electrical energy, and involved the use of hydrogen fluoride technology in the generation of fusion energy.

Dr. Rosenbluth commented that the situation regarding the development of glass lasers or hydrogen fluoride lasers was the same as that for the tokamak and alternative concepts in the magnetic fusion program. Under conditions of very limited funding, it was not possible to pursue all of the alternatives. Each program could only afford to pursue the most advanced concept.

Dr. George Morales, Department of Physics, UCLA, congratulated the panel on a job well done. He expressed concern that the committee, during its discussions, appeared to be dividing itself into pro-tokamak and anti-tokamak factions. He suggested that, because of budget uncertainties, the committee should divide issues of principle from issues of strategy. He stated that it was never good to be forced to change a principle due to financial pressures. FEAC should, therefore, separate its principles from the implementation of the program.

Dr. Conn commented that this was good advice. The principles should be such that they can stand apart. Their implementation could be more or less aggressive depending upon available resources.

FEAC Deliberations

Dr. Conn brought the meeting back to the subject of the panel's report. He said that FEAC would accept the report, and thanked the panel for it. He stated that he and the committee appreciated the work that had gone into it. He added that what he had earlier described as Principle #5 could become the basis of FEAC's letter to Dr. Happer.

Dr. Parker asked why, in Principle #5, stellarators had been placed in the same category as tokamaks. Dr. Dean replied that the stellarator had been raised to a high level of priority because of international interest in the device and because of the significant worldwide program in stellarators that was now developing. Dr. Parker commented that it would be more logical to place tokamaks on their own as the first choice of a device to yield a working reactor, with stellarators as the major back-up device. These would then be followed by RFP and FRC devices. Dr. Dean pointed out that if the panel were to provide gradation within any one category of devices, they would have to do so for the others and this could lead to further contention. Dr. Ripin commented that it might be dangerous to name examples of each category in the letter. He suggested allowing the principle to stand on its own. Dr. Conn explained that if FEAC were to do that, then the letter would need to include words to describe what the committee meant by highly-developed, developing, and small-scale innovative concepts. It was simpler to let the examples themselves do that.

A further discussion took place over the degree to which distinction should be made between the competing devices. No agreement was reached and Dr. Conn suggested that if FEAC could not agree on general guidance then they should not provide any. Dr. Overskei commented that FEAC had been asked to respond to some very clearly worded questions in the letter of charge. Dr. Conn responded that he had been trying to develop the principles by which to answer the questions.

At this juncture, it was determined to review, instead, the recommendations that Panel III had prepared for possible inclusion in FEAC's letter of response.

> **The seven recommendations appear, in order, below. Each is followed by a synopsis of the committee's discussion, including whether or not, and in what form, to include the recommendation in the letter of response.**

Recommendation #1:

> *Among the many magnetic fusion confinement concepts, the tokamak has emerged as the most scientifically successful concept. However, uncertainties remain in the extrapolation of the tokamak to a competitive commercial energy source. Consequently, tokamak concept improvement programs are essential and should receive a high priority. In addition, as long as these uncertainties remain and the projected date of commercial fusion power remains so far in the future, a non-tokamak fusion concept development program, at some appropriate level, must also be maintained.*

A lengthy discussion concerning the exact wording that should be used in the letter for this recommendation took place. Dr. Sheffield pointed out that the sentiment seemed to be aimed at the development of new capabilities. He indicated that the development of new understanding was equally important and suggested adding this thought to the letter.

Recommendation #2:

> *Tokamaks and Stellarators are the most highly-developed magnetic fusion concepts and should receive the bulk of the funding for concept development and concept improvement. A special panel should be established to evaluate Stellarator policy, including the possible restart of ATF, in the context of the World effort to develop an optimized toroidal-field-dominated fusion reactor of the tokamak/stellarator type.*

Dr. Linford asked why Panel III did not review the ATF in detail. Dr. Dean replied that the panel had not undertaken a review of stellarators since it was thought that a special panel would be appointed to do just that. Dr. Linford commented that there were two types of panel; the technical type, which reviewed the value of the technology; and the programmatic type, which dealt with policy issues and reviewed the advisability of restarting the program. He asked which type of panel would undertake the review. Dr. Davies responded that the real issue was whether the panel should be an ad hoc committee or a panel generated by FEAC. Dr. Conn commented that the ATF should be viewed not just with respect to the worldwide effort on stellarators, but as a back-up to the tokamak. Hence he felt it would be more appropriate to establish a special panel of FEAC, as opposed to an ad hoc committee, to review the matter. He indicated that the letter of response should state that FEAC has not yet completed its review of alternative concepts and that it will form a panel to review stellarator policy.

Recommendation #3:

> *The decision by DOE in late 1990 to eliminate essentially all non-tokamak-related work from the fusion program has had a chilling effect on many scientists in the fusion community, resulting in the widespread impression that DOE has postured itself to be unreceptive to new ideas. It is important to reverse this impression. If fusion is to continue to attract and inspire a new generation of scientists and engineers, it must clearly be seen as an exciting field, open to achieving success by whatever path. Therefore, although tokamak concept improvement must receive a high priority, we believe that there should be no arbitrary exclusion of non-tokamak fusion approaches.*

The committee agreed to omit this recommendation.

Thursday, May 21, 1992

FEAC Deliberations

Recommendation #4:

> *DOE should retain the flexibility to test some non-tokamak concepts at intermediate scale, when their technical readiness and promise so-warrants. In deciding when and what to fund in this area, DOE should attempt to coordinate their decisions with those of other countries active in the same concept area. Prior to the 1990 decision to narrow the fusion program, DOE had two programs (other than the Stellarator) we would place in this category: the Reversed Field Pinch (RFP) and the Field Reversed Configuration (FRC). We recommend that a small theoretical and experimental RFP effort be maintained in the U.S., and that RFP experts should engage in collaborations with European and Japanese RFP efforts. We also recommend that DOE undertake a peer review of the benefits that might accrue from operating the LSX Field Reversed Configuration (FRC) facility for a few years, to obtain the fundamental data for which it was designed.*

A discussion took place over the advisability of recommending a peer review concerning the LSX facility. It was generally agreed to omit the statement, which was likely to lead to confusion, since a peer review had already been undertaken and completed. The topic of whether or not the LSX facility should be placed back in operation in the near future was discussed. Dr. Rosenbluth indicated that this represented a good test case for operating something other than a tokamak, since the facility already existed. He pointed out that the fusion program would be wasting time and money if it were to construct yet another facility while a good one existed and stood idle. Dr. Overskei stated the the LSX facility could be "moth-balled" for years without coming to harm. He saw no point in operating it simply for the sake of generating fundamental data, and suggested that it should only be recommissioned to fulfill a well-defined mission that was really needed by the fusion program.

The extent of any future LSX program, and whether the MST program at the University of Wisconsin should be included in this general area of research, were discussed, especially in view of the fact that the research group at Wisconsin is now the core of the RFP activity that remains in the U.S. Dr. Parker pointed out that the U.S fusion program had made a habit of building facilities and then not using them. The FRC facility existed and should be used. The RFP project was intended to permit the U.S. to keep in touch with the world program. The rationales behind the two projects were different and should not be confused. Dr. Ripin argued against the implicit principle that because something was built, more money should automatically be put into using it. While it may well prove more cost effective to use what already existed, the mission must be correct and needed. Dr. Linford pointed out that the fusion program had learned nothing technical between conception and construction of these facilities to suggest that they were not valuable. Rather, their demise had been due purely to budgetary constraints. He pointed out that the original reasons for their construction were still valid, and the fundamental data they were designed to generate was still needed.

Dr. Overskei pointed out that merely suggesting in the letter that DOE reconsider operating the existing alternative-concept facilities, would be an abdication of FEAC's responsibilities. Rather, FEAC should make decisions concerning which facilities to operate and should provide solid advice to DOE. Dr. Parker suggested that in recommending to DOE that it plan to restart one or another facility, FEAC should point out that the budget may not permit a start now, or next year, but that when the TFTR roll-off occurred there should be room for maneuver. Dr. Overskei stated that FEAC should ask DOE to provide a plan relating to the start-up of the facility concerned.

Dr. Dean clarified that the panel had not been asked to undertake a technical review of any particular facility: They were not in a position to make recommendations. He emphasized that the panel had only been asked to provide guidance on policy and procedures to DOE. Concerns over the potential costs of starting up quiescent facilities were not investigated. Dr. Overskei asked if DOE had considered which tokamak it intended to shut down in order to fund the alternative concepts program. Dr. Davidson pointed out that FEAC was advisory only, and should be very careful in recommending that a particular facility be re-started or that another be shut down, without undertaking a thorough technical review of the matter.

Dr. Baldwin made the point that the policy that was currently under review by FEAC had been developed by OFE at a time of budget crisis. That policy concentrated the research effort upon the tokamak. The panel had said that the DOE's decision was too inflexible, and had cited examples of alternative concepts that were potentially deserving of support. He stressed that while the specific examples may be appealing, it was not FEAC's current task to evaluate them. Rather, FEAC had to establish the policy and criteria by which to evaluate them. Dr. Davies concurred. She added that she was concerned over the cost that would be involved, for example, in operating LSX. Ambiguous results had been obtained prior to shut-down that pointed to the need for expensive up-grades, including the provision of neutral beams. She anticipated that a significant increase in operating budget would also be required.

The advisability of retaining the verbiage relating to peer-review was raised again. Dr. Weitzner reminded the committee that the LSX program had not been stopped for any technical reason: He did not see the need to undertake a peer review before a re-start was authorized. However, he emphasized that the project must be held accountable. If the facility was unable to fulfill its mission without expensive up-grades, then that matter should indeed be reviewed. Dr. Conn emphasized that projects needed to yield positive results in order to encourage continuity of funding in sparce budgetary times. Dr. Davies cautioned that the fusion program should not set projects up for failure through an inability to provide sufficient funding to ensure their success. She emphasized that this was her major concern with regard to the alternative concepts at the time that OFE shut them down.

Dr. Berkner agreed that FEAC needed to make strong recommendations but stated that they should not be made based upon too little information. He suggested that the committee keep its recommendations on a philosophical level; "reconsider" would be good enough at present. Dr. Overskei commented that to suggest that the DOE reconsider or review a particular matter was not establishing a policy; and a policy was what DOE had requested of FEAC. Dr. Ness suggested that the letter should not become too precise but should leave room for DOE to take its responsibilities seriously.

Dr. Weitzner suggested that the committee vote on whether or not to name specific devices in its letter of recommendation. Dr. Dean pointed out that the RFP facility had been scavenged and could not be restarted. He suggested that the U.S. maintain expertise in this area, but argued against a new program. He remarked that the LSX facility was capable of being restarted. Dr. Conn commented that the committee was going beyond principle, and at a time when the budget was uncertain. He stated that FEAC would end up being accused of recommending that the U.S. pursue everything again. He suggested that FEAC had three options open to it: The letter could say nothing about LSX; it could ask DOE to reconsider restarting the facility; it could ask DOE to prepare a plan to restart the facility. Mr. Culler asked if FEAC had been asked to make specific concept recommendations. Dr. Davies replied that FEAC had not. She added, however, that she would like some to emerge from the forthcoming summer workshop, but was not looking for any from the current meeting.

Dr. Holdren commented that while the statement of principle in this recommendation was satisfactory, he was concerned over placing too much emphasis on specific devices in its latter sentences. He suggested that a suitable transition might be: "Consistent with these principles, we think that . . . " which would be weaker than saying that FEAC recommends that this and that be done. Dr. Parker asked if the panel had agreed, with a strong majority, that the RFP effort should continue. Dr. Dean replied that the panel had. He said that the panel realized that the RFP facility at LANL could not be restarted and therefore saw a need to keep abreast of developments in the world program.

On a show of hands, the committee voted 10-to-2 in favor of including specific devices in its recommendations. Dr. Parker asked if this meant that the RFP program would be supported forever. Dr. Davies responded that she did not view the recommendation that had been proposed as saying that DOE must support the RFP program forever.

Public Comment

Dr. Conn explained that *Dr. Francis W. (Rip) Perkins, PPPL, Chairman of the Plasma Science Committee,* had telephoned and requested time to comment to FEAC on the need for support for basic plasma research. Since Dr. Perkins was unable to attend the meeting in person, *Dr. Charles F. Kennel, UCLA,* had agreed to substitute for him.

Dr. Kennel indicated that Rip Perkins would like to be invited to talk to FEAC on the topic of basic research in plasma science and fusion energy. Dr. Kennel briefly outlined the benefits that could be expected from such research, described the current state of decay of basic plasma research, and explored the reasons for such decay. He explained that it was an inevitable outcome of the concentration of resources on fewer, larger experiments. He said that the Plasma Science Committee agreed with, and endorsed, Panel III's call for a program that would support individual-investigator-initiated innovative plasma science proposals within the Office of Energy Research. The Plasma Science Committee would also be appreciative of anything that the Office of Energy Research could do to influence other agencies to similarly support individual investigator initiatives.

Dr. Rosenbluth commented that in order to support its position, the Plasma Science Committee needed to generate a document that outlined existing examples of the benefits that such initiatives had provided.

FEAC Deliberations

Recommendation #5:

> *We recommend that a small, but formal and highly-visible, annual competition should be established to foster new ideas, from whatever source. Programs funded under this program should be limited to 3-5 years in duration so that eventually the program has a 20-30% turnover per year. A few million dollars should be set aside for this competition the first year. If high quality proposals are received, this program should grow modestly in future years, eventually reaching a steady-state value of perhaps $10-15 million per year (about 3-5% of the total budget). In addition to the obvious criteria of technical excellence, the selection criteria for this work should be the potential impact of the promised result, and the likelihood of addressing key issues in the time-frame and for the cost proposed. Collaborations with international efforts should not be a requirement for these small-scale programs.*

Dr. Sheffield asked if it was intended that this recommendation should apply to all matters concerning fusion. Dr. Dean answered affirmatively, stating that the projects could relate to technology, materials or theoretical advancements; it was not intended to confine the competition to experimental physics, and the panel did not want to exclude anything that could have a major impact upon the fusion program. Dr. Davies pointed out that, with such breadth and diversity, it would be very difficult to appoint one peer review panel that would be capable of reviewing all of the proposals competently. Dr. Conn commented that the recommendation was highly definitive. He continued that while he agreed with the principle, he found the recommendation proscriptive, and pointed out that it was telling DOE exactly what to do.

Several members expressed support for the principle but several raised questions over the size of the funding that had been suggested. Dr. Parker asked if there was any intention to restrict the size of the support provided to a project on the basis of the size of the institution putting forward the proposal. Dr. Dean answered that no restriction had been foreseen or anticipated. Dr. Ness explained that the specificity of the recommendation was intended to "fence off" some funding as a deliberate set-aside. Dr. Crandall explained that all proposals received by OFE underwent programmatic reviews. If a particular proposal fitted within the program limits and objectives, it was sent out for peer review. He was concerned that this recommendation could result in innovative programs being funded at the expense of better conventional projects of similar size. A further lengthy discussion ensued concerning how large a budget should be allocated to this activity.

Dr. Conn made the point that a great deal of the panel's report represented a reaction to the aftermath of the budget cut of two years ago. He felt that what the panel was proposing was overly ambitious in the light of the current uncertain budget situation. He pointed out that, on the other hand, if the fusion budget were to decrease to a level at which some of the larger existing programs could not be supported, this might well result in the freeing up of additional funds that could be applied to enhance this program; he was therefore opposed to placing any sort of limit upon the budget. Dr. Ripin indicated that the panel had felt that some money should be set aside for this program every year, whether the budget increased or decreased. The program should be viewed as providing a reliable, regular source of funding. He emphasized that, in a particular year, no worthy proposals may be received, but the money should be available just in case.

Dr. Parker stated that he would be unhappy if a good idea, for example one relating to novel diagnostics, were to be omitted from consideration simply because it was tokamak-related. He felt that the recommendations, in general, gave the impression that the innovative concepts funding should be used for non-tokamak concepts only. Dr. Ness responded that the panel had thought that the tokamak program itself would support all the better proposals that related to tokamaks. Dr. Parker countered that the tokamak program was already unable to support many good new ideas. Dr. Dean referred the committee to the second paragraph on Page 9 of the panel's report:

> "The ultimate goal of concept innovation has to be to make a significant difference in the attractiveness of fusion reactors. The focus of the program should be on programs which, if successful, can have a major impact on the fusion program and its product, the fusion reactor of the future."

In defining their recommendations, the panel had not intended that all innovative-concept proposals received by OFE should be referred to this program. The panel had hoped that the tokamak program would look after its own small proposals, and that technology-related proposals would fall under either the base program or the ITER program. The panel had biased the innovative concepts program against tokamaks and other existing programs for this reason. The new program was intended primarily, but not exclusively, to support non-tokamak activities.

The committee entered into a lengthy discussion concerning how exactly to word this recommendation, and how much money should be allocated to the program.

Establishment of Panel IV

Dr. Conn pointed out the need to establish a panel to review the stellarator issue and to provide a comparative assessment of the several medium-sized facilities in the fusion program. He asked Dr. Baldwin to chair the panel. He indicated that it was necessary that several members of FEAC join the panel and stressed the need to appoint members who would not be subject to conflicts of interest. Dr. Conn suggested that Dr. Weitzner, Dr. Dean, Dr. Rosenbluth and Dr. Ripin might be prepared to join the panel.

Other Matters

Dr. Conn requested that information relating to the Summer Workshop planned for July be sent out to members as soon as possible.

FEAC Deliberations

Recommendation #6:

> *In addition to the concept improvement programs discussed above, a program as large and as long-range as fusion requires some level of fusion-related basic science, which is largely left to the discretion of individual investigators. Very little, if any, such activity now is funded by the DOE. We recommend that DOE top management use its influence to assure that a reasonable level of effort on basic plasma science is supported from at least three sources: The Office of Fusion Energy, the Office of Basic Energy Sciences, and the National Science Foundation. Modest exploratory efforts in selected forefront technologies important to fusion should also be supported by the Office of Fusion Energy.*

Dr. Davidson expressed concern with this recommendation since he felt that no funding agency would agree to support unspecified research. Dr. Berkner commented that the real difficulty was that the Office of Basic Energy Science was perceived as not supporting basic plasma science. Dr. Weitzner stated that a "disconnect" existed between the different government agencies: It would appear that each agency wished to transfer responsibility for plasma science to the others. This gave rise to the difficulty that Dr. Kennel had explained to them.

Dr. Conn stated that he was unhappy with this recommendation and would prefer that DOE provide FEAC with a separate charge to review the plasma science funding situation. Dr. Sheffield pointed out that while basic plasma science is very important to fusion, it has other applications also. He commented that the various programs were not well coordinated.

A general discussion took place concerning how other funding agencies might be included in a program involving basic plasma science, and which agencies might have an interest in the matter.

In answer to an objection to the inclusion of the final sentence of the recommendation, Dr. Dean explained that the forefront technologies referred, in particular, to future materials development that would be related to requirements beyond those of the next device.

Recommendation #7:

> *The policy we have recommended above is one we believe provides an appropriate balance between a strong mainline program and a receptiveness to new ideas. Consequently, we recommend that this balance be implemented and maintained even if the fusion budget should decline.*

Dr. Ripin commented that the sentiment that the panel wished to express here was that the budget for innovative concepts not fall to zero under any budget scenario, although the panel accepted that if no worthy proposals were received the funding would not be used.

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

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