A Restructured Fusion Energy Sciences Program

Advisory Report
Submitted to
Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy

By
The Fusion Energy Advisory Committee

January 27, 1996

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Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy
Washington, DC 20585

Dear Dr. Krebs:

The Fusion Energy Advisory Committee (FEAC) has proceeded to address the charge in your letter to us of December 5, 1995 with the conviction that the United States must field a program that seizes the opportunities of today, in a restructured format, to promote progress in fusion science and technology. This is a time of tremendous progress and opportunity in fusion. Yet, despite significant scientific and technical progress, constrained budget prospects place the United States fusion program at a dramatic crossroads.

In response to your charge letter, we are pleased to transmit the enclosed report, "A Restructured Fusion Energy Sciences Program," hereafter referred to as the Report. This Report was prepared to provide recommendations on how to restructure the fusion program in the light of congressional guidance and budgetary realities. Your letter to the FEAC referred to the Conference Report accompanying the FY 1996 appropriations bill, which indicated the necessity of restructuring the fusion program's strategy, content and near- to medium-term objectives, assuming a constant level of effort in the base program. You asked for advice on the strategy for the fusion program and plan for implementation of that strategy, including institutional considerations and the role of the International Thermonuclear Experimental Reactor (ITER) and other international collaborations in the program. You also asked the FEAC to consider the broader issue of plasma science that underpins fusion energy and that has important applications in science, technology, and industry.

To establish a mission, a set of goals, a strategy, and an implementation plan, the FEAC created in December two subcommittees: the Strategic Planning Subcommittee, chaired by Dr. Michael L. Knotek, to analyze the policy issues, and the Scientific Issues Subcommittee, chaired by Dr. James D. Callen, to provide scientific assessments to inform the deliberations. The two subcommittees worked closely together and prepared the Report we are transmitting to you. As part of the process, views were solicited from the entire fusion community, and key laboratories and facilities were visited by subcommittee members during the review. The FEAC wishes to thank officially the members of both subcommittees for their work, as well as those who presented material to the subcommittees, including our international partners and the many people who forwarded viewpoints to the subcommittee.

The FEAC voted unanimously to accept the Report "A Restructured Fusion Energy Sciences Program" and unanimously endorses the mission and policy goals given in the Report. By a vote of 10 to 2, the FEAC also endorses the findings and recommendations contained in the body of the Report.[1] The recommended restructured program is consistent with the new mission and policy goals and fits within the upper range of annual budget guidelines that you provided in your December 5th, 1995 charge to the FEAC.

In summary, the Report recommends that the mission of the U.S. Fusion Energy Sciences Program be modified to be consistent with both the most recent programmatic guidance and the level of resources provided by Congress. The new mission is to advance plasma science, fusion science and fusion technology--which constitute the knowledge base needed for an economically and environmentally attractive fusion energy source. The FEAC recommends, in no priority order, three policy goals: advance plasma science in pursuit of national science and technology goals; develop fusion science, technology and plasma containment innovations as the central theme of the domestic program; and pursue fusion energy science and technology as a partner in the international effort.

In 1995, the President's Committee of Advisors on Science and Technology (PCAST) reviewed the U.S. magnetic fusion program. In response to their charge, they recommended a $320M figure as a minimum annual funding level for a viable fusion energy program. This funding level would have allowed the United States to maintain a leadership role in the world effort to develop fusion power. The subsequent decision to fund the program below this minimum level and the guidance to expect flat out-year budgets have completely changed the position of the U.S. magnetic fusion effort relative to Europe and Japan. Efforts to build a next-generation world class experiment in the United States were abandoned, U.S. participation in the international burning plasma program on ITER was reduced, and many other important U.S. fusion science activities were curtailed.

The historically strong United States leadership role in the world magnetic fusion energy program came to an end with the decision on FY 1996 funding. However, we conclude that the United States can still play an important supporting role in magnetic fusion energy development, but only by recognizing the new dependence of U.S. efforts on the activities and decisions of Europe, Japan, and the Russian Federation. As such, progress will depend on maintaining a balance of domestic and international activities.

As requested by the Department in your charge letter to us of December 5, 1995, the FEAC and its subcommittees considered annual program funding levels in the range of $200 million to $275 million. The subcommittee examined as its base case a constant level-of-effort budget of $250 million. The restructured Fusion Energy Sciences Program in this case is described in great detail in the Report. This restructured program is consistent with the new mission statement and is built around the three policy goals.

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[1]It is inevitable that, given the tight deadlines imposed by the budget process, small inconsistencies may appear between the body of the Report and some of the appendices. The FEAC believes that these inconsistencies are not consequential and emphasizes that our strong endorsement is of the findings and recommendations in the body of the Report.

At this budget level, restructuring begins by providing incremental funding to pursue basic plasma science, to pursue plasma-containment research (plasma science and alternative concepts), and to achieve greater utilization of DIII-D and C-Mod. These priorities require, however, that Tokamak Fusion Test Reactor (TFTR) at the Princeton Plasma Physics Laboratory (PPPL) cease operation during FY 1997, foregoing the remaining unique scientific output possible from that facility.[2]

At the lower funding levels, (below $250 million per year), it is not possible to implement the goals of the restructured program, which include honoring our international commitments to the ITER engineering design activity (EDA) and obtaining further valuable scientific benefits from our existing experimental facilities. The FEAC does not recommend these lower levels of funding, At the highest budget level considered ($275 million per year), the restructuring would proceed with greater effectiveness (e.g., exploiting high priority scientific results before shutting down a major facility; strengthening our support for the international commitment of the United States to the ITER EDA; and allowing more vigorous pursuit of the new directions that are at the core of the restructuring), and we recommend this case to the Department.

With respect to international cooperation and the ITER effort, the broad physics and engineering challenges that ITER addresses are largely generic to any next step toward the goal of fusion energy. Therefore, the science and technology research within both the ITER EDA and the U.S. core program that addresses ITER's challenges is appropriate and valuable. Such work is also consistent with the recommended fusion energy science mission for the program. The FEAC finds that the most cost-effective way for the United States to maintain a strong research effort in burning-plasma physics is through continued participation in the ITER EDA and the ITER process. Further, the ITER EDA will provide a robust and thorough engineering design based on extensive R&D activities. This design has already highlighted certain important physics issues and has become a driver in current experimental and theoretical programs. It is important to recognize that the ITER EDA is the single most important mechanism for American industry to participate in fusion development.

The Report contains several recommendations regarding program governance. We strongly encourage efforts to enhance opportunities for "grass roots" participation in the scientific and programmatic leadership of the Fusion Energy Sciences Program. Such participation was mobilized effectively to develop the restructured plan contained in our Report. Continued and enhanced leadership from the "grass roots" will promote community consensus; it will sharpen the focus on the mission and goals; it will help foster a climate conducive to innovation; and it will strengthen outreach to the stakeholders, related science fields, and the public. One mechanism for this leadership participation is the continuation of the Science Subcommittee of the FEAC.

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[2]The FEAC did not assess or include closeout costs associated with the termination of facilities and programs. If these costs must be funded by the Fusion Energy Sciences Program, resources to do so must be added to the budget to accomplish the described restructured program.

Finally, as the nation's program-dedicated laboratory for fusion science, the PPPL must provide the leadership necessary for the restructured national Fusion Energy Sciences Program to succeed. The PPPL provided such national leadership for the Tokamak Physics Experiment (TPX) project, and we want to emphasize the importance of maintaining this critical resource and capability.

We are confident that the recommendations contained in the Report are responsive to the concerns raised by Congress and will allow the DOE Fusion Energy Sciences Program to advance the scientific knowledge-base needed for an economically and environmentally attractive fusion energy source for the nation and the world.

Sincerely,

Robert W. Conn, Chair
Fusion Energy Advisory Committee,
on behalf of the Fusion Energy
Advisory Committee and its two
Subcommittees

Enclosure

A Restructured Fusion Energy Sciences Program

Advisory Report
Submitted to
Dr. Martha A. Krebs
Director
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Executive Summary

In response to a request from the Director of the Office of Energy Research, this report provides recommendations from the Fusion Energy Advisory Committee (FEAC) on how to restructure the fusion program in light of congressional guidance and budget realities. The restructuring is based on: a survey of the field, including science and technology issues, capabilities, and programs; a new mission for the U.S. Fusion Energy Science program; and a set of policy and science goals. The report includes: the principles and outline of the restructured program; an analysis of the impact of annual budgets ranging from $200M to $275M; and recommended actions to implement the transition and establish a governance system for the restructured program. In this funding range, the United States must concede leadership of the world's fusion energy development effort to Europe and Japan.

The underlying theme of the restructuring is to redirect the program away from the expensive development path to a fusion power plant to focus on the less costly critical basic science and technology foundations. The proposed new mission and supporting policy goals are as follows:

MISSION: Advance plasma science, fusion science, and fusion technology – the knowledge base needed for an economically and environmentally attractive fusion energy source.

POLICY GOALS:

- ***Advance plasma science in pursuit of national science and technology goals.***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program.***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

As a first step, we recommend the adoption of the mission and goals and renaming the program the **Fusion Energy Sciences Program**, to reflect accurately the new focus. By incorporating the new mission and goals, the restructured program can fit within a constant annual budget and does not require increased outlays for construction of new facilities.

During the subcommittee process, we identified and assessed eight scientific and programmatic issues involved in the transition to the restructured U.S. fusion program: (1) Fusion Program

Scientific Goals, (2) Development of Basic Plasma Science[1], (3) Theory and Computation, (4) Major Tokamak Facilities, (5) Plasma Confinement Research (Alternative Concepts) (6) Inertial Fusion Energy, (7) International Thermonuclear Experimental Reactor (ITER), and (8) Fusion Materials and Technology. Summary assessments are included in the report.

Budget Impacts

The FY96 budget of $244M (a 32% reduction compared to FY95) forced hard choices and has had serious consequences. Looking toward the future, all the funding scenarios require us to close scientifically productive domestic facilities for budgetary, not scientific, reasons, in order to achieve cost-effective utilization of the remaining facilities and to begin the pursuit of new opportunities and directions essential to the restructuring.

The funding level in FY97 is critical, and it is possible only with $275M to move forward briskly on restructuring while accomplishing the full programmatic scope directed in the FY96 Appropriations Report from Congress. Below $250M, it would be necessary to consult again with our international partners on an affordable U.S. share in the ITER Engineering Design Activities (EDA). The restructuring transition would be prolonged and complicated and result in a program that is marginalized in the international context.

In FY98 and beyond, stable funding at or above the FY96 level of effort would allow the United States to remain abreast of international development across fusion science and technology and to continue world leadership in selected specialties. Such niche leadership is essential for us to be sought by international partners as a valued participant, though perhaps minor monetary contributor, for internationally launched major facilities, defining the path to fusion energy production. At all budget levels, an increase in international cooperation (creation of flexible mechanisms to exploit the capabilities of international facilities jointly) is of paramount importance.

Governance

Critical to the success of the restructured program is immediately starting a governance transition, as a mechanism for guiding and implementing the major programmatic changes in a smooth and effective manner.

The **Fusion Energy Sciences Advisory Committee** to DOE's Office of Energy Research, assisted by a continuing **Science Subcommittee**, should advise ER-1 and the program office on policy, goals, priorities, budget, direction, program balance, and governance.

Fusion Energy Sciences Program Management must be reorganized and downsized to match the science-dominated mission, and rely significantly on peer review as the primary input for funding allocations.

Specific programmatic reviews should be conducted and integrated during the remainder of FY96 to help set the technical priorities of the restructured program, given a funding level not to exceed the FY97 President's Budget Request.

ÿ **A Major Facilities Review** in association with a **User Access Working Group**

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[1] Leadership of the Nation's plasma science research effort is recommended in partial response to the recent Report of the Panel on Opportunities in Plasma Science and Technologies, *Plasma Science from Fundamental Research to Technological Applications*, National Academy Press, Washington, D.C., 1995.

ÿ **An Alternative Concepts Review**

ÿ Planning for **review of the ITER EDA and its results** and to establish **criteria for a decision** on future U.S. participation.

The current federal budget realities and the lack of a perceived domestic energy shortage demand program restructuring in accordance with the recommendations in this report, so that the U.S. program will focus on the science and technology foundations for a future or internationally led push toward fusion energy. United States involvement in fusion research and development will continue to be "a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research - - plasma physics - - that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and industry."[2] Enhanced public outreach is needed to keep the public, stakeholder groups, and the broader scientific community fully informed.

[2] *The U.S. Program of Fusion Energy Research and Development*, Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995.

A Restructured Fusion Energy Sciences Program

1 Introduction

1.1 Background In the Conference Report for the FY 1996 Energy and Water Development Bill, which included a significant reduction in the funding to pursue fusion energy, the Conferees directed the fusion energy program "to restructure its strategy, content, and near- to mid-term objectives." The Conferees further stated that "the restructured program should emphasize continued development of fusion science, increased attention to concept improvement and alternative approaches to fusion, and development and testing of low-activation structural materials so important to fusion's attractiveness as an energy source." While there exists an acknowledged long-term need for new energy sources for this nation and the world, the lack of a near-term need and the current national goal of balancing the budget necessitates that we redirect the fusion energy program to more closely match national needs over the near to mid-term, while positioning ourselves properly for the future.

The Conferees instructed the Department of Energy, with the participation of the fusion community and the Fusion Energy Advisory Committee (FEAC), to prepare a strategic plan to implement a restructured fusion program, assuming a constant level of effort in the base program over the next several years. On December 5, 1995, the Director of the Office of Energy Research, Department of Energy, requested FEAC to provide recommendations on how to restructure the fusion program in light of congressional guidance and budget realities (see Attachment 1). This report responds to that request.

1.2 Process Followed: To establish goals, a strategy, and an implementation plan, FEAC established two subcommittees: The Strategic Planning Subcommittee (SPS), specifically charged with developing the deliverables, and the Scientific Issues Subcommittee (SciCom), charged with providing scientific assessments to inform the deliberations. The two subcommittees worked closely throughout the study on the analysis of the key issues. In addition, extensive efforts were made to solicit (including through the Internet) opinions, proposals, facts and positions from the fusion community and interested stakeholders. Over 200 communications were received, and they generated considerable debate. Actions included the following:

- Three open meetings were held, at the Princeton Plasma Physics Laboratory (PPPL), General Atomics in San Diego, and the San Diego International Thermonuclear Experimental Reactor (ITER) Cocenter.

- The chairman of the SPS visited the facilities and held discussions at the Massachusetts Institute of Technology (MIT) Plasma Fusion Center.

- The subcommittees held discussions with the heads of the Japanese, European Union, and Russian fusion efforts, and the head of the ITER design team.

- Several discussions were held with DOE officials and staff, officials from the Office of Science and Technology Policy (OSTP) and the Office of Management and Budget (OMB), and the staff of relevant congressional committees.

The subcommittees: specifically carried out a survey of the field, including science and technology issues, capabilities, and programs; defined a new mission for the U.S. Fusion Energy Science program; established a set of policy and science goals; developed the principles and outline of a restructured program; studied the impact of budgets on goals; defined a program that assumes a constant level of effort of $250M (FY97 dollars) (as called for in the Conference Report Language) and describes the impacts of budgets up to $275M and down to $200M; and developed principles of governance to guide the transition of the program to the new structure.

1.3 Assessment of Fusion Energy Research: Our assessment of fusion energy research focused on identifying key science and technology issues affecting fusion policy and on evaluating the capabilities and strategies of fusion programs throughout the world. The recently completed report from the Fusion Review Panel of the President's Committee of Advisors on Science and Technology (PCAST),[3] the report from the Panel on Opportunities in Plasma Science and Technology published by the National Research Council (NRC),[4] and the Draft Strategy for a Restructured U.S. Fusion Energy Research Program prepared three months ago by DOE's Office of Fusion Energy (OFE) using input from leaders of the U.S. fusion program were carefully evaluated as part of our assessment. Overall, our subcommittee strongly endorsed the conclusion from the PCAST report describing U.S. funding for fusion research and development (R&D) as "a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research — plasma physics — that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and in industry."

During our subcommittee process, we also identified scientific and programmatic issues within the present U.S. fusion program. For each of these areas, we prepared an assessment that helped to motivate and define our Restructured Fusion Energy Sciences Program. These issue summaries also illustrate how the implementation of our new policy goals can strengthen key research areas and set necessary budgetary priorities. These issue summaries are attached to the end of this report, and they serve to expand and clarify program needs and solutions concerning (1) Fusion Program Scientific Goals, (2) Development of Basic Plasma Science, (3) Theory and Computation, (4) Major Tokamak Facilities, (5) Plasma Confinement Research (Alternative Concepts) (6) Inertial Fusion Energy, (7) International Thermonuclear Experimental Reactor, and (8) Fusion Materials and Technology.

2 A New Mission

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[3] *The U.S. Program of Fusion Energy Research and Development*, Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995.

[4] *Plasma Science from Fundamental Research to Technological Applications*, Report of the Panel on Opportunities in Plasma Science and Technology, National Academy Press, Washington, DC, 1995.

We recommend that the mission of the U.S. Fusion Energy Sciences Program be modified to be in accord with both the most recent guidance and resources made available by Congress. Specifically, the fusion energy program outlined in the Energy Policy Act of 1992 (e.g., a technology demonstration by 2010 that would verify the practicability of commercial electric power production) cannot be realized at the budget levels now foreseen. Consequently, we recommend that the mission for the U.S. program be restated to put it more in a world context, reflecting the reality that the leadership of the world's fusion energy development effort now lies outside the United States, as well as emphasizing both its science and technology goals. Based on discussions with our international partners, and guidance we have received, this mission can best be stated as:

> ***MISSION: Advance plasma science, fusion science, and fusion technology – the knowledge base needed for an economically and environmentally attractive fusion energy source.***

This mission and intent can best be met with the following set of policy goals:

- ***Advance plasma science in pursuit of national science and technology goals;***
- ***Develop fusion science, technology, and plasma confinement innovations as the central theme of the domestic program; and***
- ***Pursue fusion energy science and technology as a partner in the international effort.***

These goals have the same major elements as the PCAST key priorities, but emphasize more the science foundation of the program. They are elaborated below.

By incorporating the new mission and goals, the restructured program can fit within a constant annual budget and does not require increased outlays for construction of new facilities.

2.1 Advance Plasma Science: Plasma science is a cornerstone of the scientific infrastructure of the country, and is a prerequisite competency to pursue many national science and technology goals, from national security to industrial processing and astrophysics, requiring major advances at the frontiers of science and numerous enabling technologies. Fusion Energy is the Grand Challenge of Plasma Science, and is "...the largest driver for the intellectual development of Plasma Science." The people tackling the scientific and technological issues involved have created a wellspring of knowledge and capability which is a national asset of enduring value. As the centerpiece of the

nation's plasma science infrastructure, the restructured Fusion Energy Sciences Program must explicitly move to broaden its intellectual and institutional base in fundamental plasma science and attendant enabling technologies, preferably in partnership with other agencies.

2.2 Develop Fusion Science and Concept Innovation Fusion science has seen major advancements over the past 30 years, with attendant benefits that strengthen the national science and technology base. Fusion Science is the combination of plasma science, related disciplines, and enabling technologies required to develop fusion as an energy source. The restructured program will focus on continued development of fusion science with increased attention to concept innovation and alternative approaches to fusion. Key enabling technologies, including radiation resistant, low-activation material and blanket technology, central to fusion's environmental and economic attractiveness, must be pursued. Increased international collaboration must be a mechanism for maximum benefit from the world capital investment in advanced facilities. U.S. strengths in theory and modelling, diagnostics, and other areas where we can provide unique resources should be increasingly brought to bear in partnership on all domestic and international facilities to achieve critical scientific and technological goals with maximum dispatch and minimum cost. The restructured program will also explicitly take the lead in reaching out to other disciplines and areas of national need for mutual benefit.

2.3 Pursue Fusion Energy as an International Collaboration Fusion energy holds the potential to provide a vital, environmentally attractive energy option for a growing world population in the next century and beyond. The pursuit of fusion energy is of such cost and complexity that it can only be achieved through international collaboration.

As discussed in the PCAST report, much of the world will find energy availability a critical roadblock to progress in the next century and beyond, and it could become a major determinant of global political stability, as it has in the past. Among the major supporters of fusion energy development, the United States has larger domestic fossil energy resources and potential for renewable energy than the European Union and Japan. As a result, fusion energy research has a relatively lower priority in the United States. Europe and Japan are supporting a much larger fusion research effort than the United States to meet their own future needs, to provide for international energy options and stability, and to support trade with the developing world.

While the development of a new energy source is not a critical near-term need in this country, it is in our national interest to be a credible partner in this international pursuit and to pursue long-term energy options that alleviate the environmental problems of fossil fuels. National benefits include providing energy security for a growing world population, preventing our own scientific and technological isolation, positioning ourselves as a world provider of energy technology, and meeting our commitments as a reliable partner. To be a strong partner in this long-range quest, we need a vigorous domestic program in fusion science and technology. The domestic program provides the basis for leadership internationally and positions our industries to field and exploit the technology when it is mature.

The international collaboration is now focussed on the scientific base, technology development, and engineering design necessary to construct a long-pulse burning plasma experiment — the

ITER. The restructured program should make every effort to remain a strong partner in the worldwide fusion program and provide a structure for a coordinated international effort. In particular, the restructured program must strive to meet our commitment to the successful completion of the ITER Engineering Design Activities (EDA), to leave open the possibility of U.S. participation in ITER construction and/or other international collaborations to advance fusion science and technology toward electricity generation, at a moderate-cost, but high-leverage investment for the United States.

3 Implementing Principles

In executing the mission of the new U.S. Fusion Energy Sciences Program, the committee recommends that ten principles be applied:

1. ***Science Focus.*** Fusion science represents a combination of interrelated disciplines that advance through large- and small-scale experimentation, theoretical and computational modelling, and materials and technological innovation. We envision the restructured fusion program to be integrated around a set of national and international experimental and theoretical resources and interaction with numerous scientific communities.

2. ***Energy Goal.*** The new science program serves the U.S. DOE's energy mission. That is, the program supports science with the long term purpose of enabling the development of an abundant, safe, environmentally attractive, and cost-competitive energy source.

3. ***Reliability as an International Partner.*** Consultation with our international partners should be a major ingredient of the evolution of our commitment to the ITER EDA.

4. ***Complementarity to the International Effort.*** The program should be designed in such a way that it complements the international effort to field a fusion energy source in the first half of the next century. This principle positions the United States to reenter an international effort quickly, whenever it becomes nationally advantageous to do so.

5. ***Leadership in Selected Areas.*** Areas of U.S. expertise having high leverage in the international effort to develop fusion energy should be identified and pursued vigorously with healthy funding. A few examples of these areas are plasma theory and computation, high-performance operating modes for tokamaks, low-activation materials, diagnostics, and plasma confinement innovations.

6. ***Scientific Excellence.*** All elements of the fusion program should be peer reviewed and held to the highest standards of scientific excellence. This principle is particularly important to guide program restructuring.

7. ***Facility Balance.*** An appropriate balance should exist between a few well-integrated, large national facilities investigating a spectrum of fusion science issues, and smaller facilities more narrowly focussed on well-posed scientific investigations. Our larger facilities produce plasmas

with conditions resembling those found in future fusion energy sources. They provide scientists with unprecedented opportunities to explore fusion and plasma science, and they should be used as centers of collaboration both nationally and internationally. Smaller facilities can be constructed at significantly reduced investment; thus, they provide an effective route to high-risk, high-benefit experimentation.

8. ***Importance of a National Laboratory for Plasma Physics.*** The Princeton Plasma Physics Laboratory (PPPL), the nation's program-dedicated laboratory for fusion science, is a critical national resource for the fusion program. As an internationally recognized national center of excellence, it must maintain a critical mass of core competencies for national leadership and international collaboration for fusion science. Its technical infrastructure represents decades of investment and must be effectively utilized.

9. ***Education and Human Resources.*** A strong educational component is essential to ensure the lasting benefits resulting from the fusion program, to attract the brightest people to address the challenges facing fusion, and to maximize the application of our expertise and knowledge base to related fields such as photonics, surface science, semiconductor fabrication, and coherent radiation sources.

10. ***Diversity of Participation.*** Project participants should represent a geographically, scientifically, and institutionally diverse set of intellectual resources.

4 Budget Impacts

In its budget deliberations, FEAC and its subcommittees focussed on program funding levels in the range of $200M to $275M. In all the cases, most especially below $250M, facilities are terminated, and closeout and decommissioning costs will be required. FEAC is unable to assess these costs, and recommends that the DOE provide for them outside the scope of the program, since their inclusion in the program budget would significantly erode the productivity of the program and seriously compromise the restructuring.

The FY96 budget of $244M (a 32% reduction compared to FY95) has had a number of consequences: a major loss of scientific and technical manpower; the foregoing of further significant capital facilities; termination of some critical enabling technologies; a curtailment of university research programs in experimental plasma physics; a subcritical and inefficient utilization of our major tokamak and other facilities; reductions in numerous programs in industry and the national laboratories; and a renegotiated, minimal contribution to the ITER EDA. These were hard choices among meeting our international commitments to the ITER EDA, our utilization of world-class facilities located within the United States (facilities which are in a period of unprecedented scientific productivity), and terminating valuable elements of the core U.S. scientific program.

Restructuring in the absence of a thoughtful transition could further lose significant useful human and capital assets that are at the heart of our current strong position in this field. We recognize the need to close existing domestic facilities to allow pursuit of new opportunities and directions that keep the field vital, exciting, and productive in this depressed budget climate. As we restructure the fusion science program with a new set of policy goals and priorities, and constant budgets, a vital and viable long-term program must be developed that creates scientific progress for the nation and a real contribution to our goals.

The Conference Report Language states:

"The high cost of fusion development points to the increasing importance of international cooperation as a means of designing, building, and financing major magnetic fusion facilities in the future. Because the United States has committed to such an approach, it is crucial that a restructuring of the fusion program maintain a strong domestic base and not undermine our credibility as a reliable international partner."

An important factor in our deliberations is that at a relatively constant level of effort, a strong domestic program and international collaborations are constructively complementary goals, while at the lower budget levels we were asked to consider they become conflicting and divisive, especially during the transition phase over the next two years. The great challenge is to find the proper balance among the program elements, in both the near and longer terms.

4.1 The Constant Level of Effort ($250M) Case: In response to the congressional budget guidance of a constant level of effort, we will speak to a $250M FY97 budget. To move resolutely to a restructured program, the following must occur in FY97:

- ***TFTR operations must cease during FY97, running at high utilization for part of the year, and at a significant reduction from a full utilization budget.***

With this action, we have made some painful choices. The TFTR is a $1B facility that is now in a period of extraordinary scientific productivity, exploring newly discovered regimes with new diagnostics in a deuterium-tritium environment. For lack of ~$25M, we are forced to terminate this program prematurely, foregoing unique scientific opportunities to study plasma self-heating and reacting-plasma phenomena. It is unclear when these lost opportunities would return.

- ***DIII-D and C-Mod and the leading smaller facilities must move toward full, maximally productive utilization.***

- ***The ITER EDA commitment is constant in as-spent dollars at the renegotiated lower level, with scope determined in consultation with our international partners.***

- ***There must be increases in plasma science and alternates, with PPPL taking the lead in some of the effort, and including greater international collaboration.***

- ***There must be modest increases in materials and technology budgets.***

- ***There must be a reduction in the total DOE program staffing, including field offices.***

This plan begins the recommended redirection on a flat budget. It better utilizes the surviving facilities, which are currently subcritical. It continues to strain our ability to deliver our ITER commitment for the remainder of the ITER EDA. It abandons the unique scientific opportunities lost by the premature termination of TFTR.

In the outyears in the constant level of effort case, we envision the following:

- ***Continued full utilization of DIII-D and C-Mod at least through 2001, including some upgrades, as user facilities to pursue the rich science to be gained.***

- ***A growing portfolio of new experiments including one or two smaller but scientifically aggressive new facilities, at least one taking advantage of the PPPL infrastructure.***

- ***A robust theory and modelling program.***

ÿ A fundamental plasma sciences budget in the range of 5% of the funding for the base program.

ÿ A healthy materials and technology effort, including capturing that part of our technology program now dedicated to the ITER EDA, and redirecting it to new technologies.

ÿ A potential commitment to ITER construction determined by a rigorous review of the ITER design and in consultation with our international partners, but with any increase over the current ITER EDA level requiring overall budget growth.

ÿ A growing set of international collaborations which focus our niche strengths on major science and technology goals.

4.2 At Lower Funding Levels: There is a very painful conflict among implementing the goals of the restructured program, honoring our international commitment to ITER, and obtaining any further valuable scientific benefits from TFTR. The TFTR's premature termination would lose imminent discoveries central to the advancement of fusion science and put them out of reach for many years.

Significantly below the $250M level, while we envision a continued level of support for the ITER EDA, it would be necessary to consult again with our international partners on the level and nature of our participation in the final two years of the fully integrated ITER design and technology-development phase. Further withdrawal from the ITER EDA would severely strain the relationship with our partners, complicating any attempts to strengthen science and technology collaborations with them in other areas.

The productivity of all major U.S. facilities would be adversely impacted, possibly requiring a reduction to only one major operating facility. Opportunities for new scientific initiatives would be severely constrained, defeating the key objectives of the program restructuring. The nation's technical credibility as an international collaborator would be further damaged by the shrinking of the domestic base. At the $200M level both the niche leadership and the resources available for the United States to put into an international collaboration as a junior partner in ITER would be only marginally attractive to the major partners. The United States would not be adequately prepared for the further development of fusion energy by the international community and would be at a significant competitive disadvantage.

4.3 The $275M Case: A $275M budget in FY97 would allow the restructuring to proceed with much less destructive consequences than the $250M case. Specifically,

- It would allow the highest-priority scientific opportunities on TFTR to be exploited before terminating its operation during FY98.

- It would enable us to strengthen our support of the ITER EDA and restore some of our original commitments.

- It would allow more vigorous pursuit of new directions that are at the core of the restructuring.

- It would allow more productive near-term utilization of DIII-D and Alcator C-Mod.

For these reasons, we conclude that the goals of the restructured fusion program can be accomplished most effectively at a funding level of $275M.

In FY98 and beyond, stable funding at $275M would allow the United States to pursue some aggressive small-scale fusion science initiatives after the TFTR is closed, to remain abreast of international developments in fusion science and technology, and to continue world leadership in selected specialties. Such niche leadership is essential for us to be sought by international partners as a valued participant, though perhaps minor monetary contributor, for internationally launched major facilities, starting with ITER, defining the path to fusion energy production.

5 Governance

5.1 Purpose and Principles: The governance system for the restructured Fusion Energy Sciences Program needs to: ensure focus on the policy and scientific goals; provide oversight; establish an open process for obtaining scientific input for major decisions, such as planning, funding, and terminating facilities, projects, and research efforts; build community consensus; orchestrate international collaboration fully integrated with the domestic program; and promote effective outreach to and communication with related scientific and technical communities domestically and internationally, industrial and government stakeholders, and the public.

General governance principles to accomplish these goals include open communication within the fusion community and with stakeholders, managing transitions to be constructive, not destructive, "due process" for major decisions, no entitlements, and community consensus on priorities and balance consistent with the government's agenda. Critical to the success of the restructured program is immediately starting the governance transition, as a mechanism for guiding and implementing the major programmatic changes inherent in the restructuring in a smooth and effective manner. Some elements of the recommendations below are already in place and can serve as a foundation for restructured governance.

5.2 Fusion Energy Sciences Advisory to the Office of Energy Research: This committee, populated by stakeholders, practitioners, and scientific 'outsiders,' should advise ER-1 and the program office on policy, goals, priorities, budget, direction, and program balance. It can be used by the DOE to obtain community and stakeholder input on a broad spectrum of scientific and policy issues as they arise (or in anticipation thereof). An immediate priority is to oversee and provide policy integration for the specific immediate actions recommended here. It should recommend an

appropriate governance system for the program for the longer term. The membership must be reconfigured to oversee and institutionalize the changes involved in restructuring.

A continuing Science Subcommittee, including experts representing the diverse fields of science and engineering underpinning fusion science as well as selected other fields should be established to provide an important channel of communication from the full breadth of the fusion community to the Fusion Energy Sciences Advisory Committee, and to provide the best possible scientific input for priority setting. Beyond providing input to the Fusion Energy Sciences Advisory Committee, the Science Subcommittee would be a new locus of scientific leadership and offer a mechanism for the fusion community to build consensus.

5.3 DOE Fusion Energy Sciences Program: The primary role of program management is the funding, management, administration, and oversight of the program. To accomplish this role effectively, it is essential to preserve the core of highly qualified, scientifically knowledgeable staff, but to reorganize and downsize (headquarters and field elements) to match the science-dominated mission, now replacing the milestone-driven energy development mission. The program structure and budget categories should become aligned with the goals of the restructured program. A peer review process should be used as the primary mechanism for evaluating proposals, for assessing progress and quality of work, and for initiating and terminating facilities, projects, research programs, and groups. This approach will allow program management costs to be brought into parity with the other program offices in the Office of Energy Research.

5.4 Specific Immediate Actions: The remainder of FY 1996 presents a limited window for devising and implementing suitable processes and for starting to align the program for a smooth transition into FY97. Without prejudging the optimum complement of responsive, flexible, and minimally bureaucratic processes needed for governance of the restructured program in the long term, FEAC recommends the following specific immediate actions:

- A major facilities review should be held as soon as practicable, to examine and evaluate the progress, priorities, and potential near-term contributions of TFTR, DIII-D, and C-Mod (other facilities should be included if appropriate) in order to develop an optimum plan for gaining maximum scientific benefit from their operation, at a funding level not exceeding the FY 1997 President's Budget Request for fusion.

- A User Access Working Group, composed of facility managers and user representatives, should be convened to work with the Program Office and facilities to develop and publicize a mechanism for encouraging, enabling, and funding the highest quality proposals from the broad fusion community to run experiments, taking advantage of the unique capabilities of the major U.S. facilities to address forefront issues in fusion science and technology.

- An Alternative Concepts Review should be held, including inertial confinement fusion, to prioritize approaches and determine a reasonable, healthy, and productive funding range for each in the context of the goals of the restructured fusion program and the FY97 President's Budget Request. An additional product of this review should be a recommendation for an ongoing

mechanism for evolving the priorities and balance of confinement concept development (inclusive of all concepts, including tokamaks) and for recommending action on specific proposals from specific groups, consistent with the principle of "due process."

ÿ If the FY97 budget request is less than $250 million, a process to consult again with our international partners to establish a mutually agreeable and potentially revisedrole in the ITER EDA will be urgent.

ÿ The U.S. ITER Home Team should move aggressively to strengthen its outreach to the entire domestic fusion community. In addition, it is timely to plan for broad U.S. participation in the review of the ITER EDA and its results and to establish criteria for a decision on the level and nature of U.S. participation in ITER construction, if the international partners decide to go forward. Regardless of the decision on ITER, international collaboration represents the best path for the United States to obtain energy generation from fusion. Appropriate U.S. participation in international initiatives and alignment of the domestic program, including technology development, to complement the foreign and international effort are areas requiring fusion community input and consensus building in both the immediate and longer term.

ÿ An evaluation should be initiated to determine the projected cost and benefits of the international 14 MeV neutron source, which is being considered for construction to test and develop low-activation fusion materials. During this evaluation, the budgetary restrictions now limiting the U.S. fusion program must guide any consideration of cost-sharing. Members of the materials research community should continue to use lower cost options for low-activation research, such as sample radiation using fission sources.

ÿ The Subcommittees did not assess the small, accelerator-based effort addressing Inertial Fusion Energy funded by the Office of Fusion Energy, but acknowledge its potential as a fusion energy source and recognize that the major scientific and plasma physics issues are being addressed through DOE Defense Programs, as a component of stockpile stewardship. A review should be conducted, involving all cognizant DOE program offices and appropriate scientific and technical experts, to recommend the priority and management of IFE, in the context of the mission, policy, and scientific goals of the restructured program.

ÿ A vigorous outreach effort should be initiated, with the goal of broadly communicating the goals and progress of this important effort to the public, to the broader scientific community, and to stakeholders, such as the energy industries and environmental groups.

The results of the above actions and the recommendations of the reviews must be integrated into a coherent and balanced program plan for FY1997 matched to the budget level. In addition, more permanent and streamlined governance mechanisms, encouraging continued community and stakeholder input, must be put in place to guide program priorities and evolution into the future.

6 Assessment Summaries Prepared by the Scientific Issues Subcommittee (SciCom)

The Scientific Issues Subcommittee (SciCom) was asked to provide scientific assessments of key areas strongly influencing the direction of the restructured Fusion Energy Sciences Program. These assessments together with joint meetings between SciCom and SPS helped to motivate our conclusions and recommendations. The full text of these assessments are attached as appendices. In some cases, these appendices also illustrate how the implementation of our new policy goals can strengthen key research areas and help set budget priorities. In this chapter, executive summaries, prepared by SciCom, are presented to serve as a guide to the appendices.

6.1 Fusion Program Scientific Goals: Two general goals follow from the mission statement of the U.S. Fusion Energy Sciences Program (see p. 3):

1. Science — develop the core science of plasma physics; produce, understand and optimize fusion plasmas.

2. Energy — develop an attractive electric power producing system from fusion plasmas.

The suggested science objectives for the restructured U.S. fusion program follow (key elements, not in priority order):

ÿ Promote plasma science:

Reinvigorate OFE leadership of U.S. plasma science.

Enhance understanding of fusion plasmas.

ÿ Push fusion innovation

Focus tokamak effort on improvements and demonstrate their potential.

Advance state of alternative concept research.

Develop and implement some enabling technologies.

Develop low activation materials.

ÿ Study burning plasmas:

Develop a viable U.S. role in an internationally developed, constructed burning plasma experiment.

Contribute to a successful completion of the ITER EDA.

These objectives are in accord with the priorities set forth in the PCAST report and are amplified in subsequent appendices.

6.2 Development of Basic Plasma Science: The underlying core science of fusion energy is plasma science: the study of the ionized states of matter. The containment of high temperature plasmas required for the production of fusion energy was (and remains) the primary motivation for the DOE/OFE to strongly support the development of the field of plasma science. As stated in the PCAST Report, "This [fusion] funding also sustains an important field of research - plasma science - in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable to other fields of science and in industry."

Further progress in the development of fusion energy will require continuing developments in the field of plasma science. Fusion program support for plasma science provides the primary interface between work in fusion energy and the scientific community. It is an important vehicle for extending the impact of advances in fusion energy to the wide array of interdisciplinary fields supported by plasma science (as documented in detail in the recent NRC Plasma Science Report).

However, Congressional policy direction to the magnetic fusion program (Energy Policy Act of 1992) called for a schedule-driven energy development program in an era of reduced budgets. In response, the program reduced the breadth of support for plasma science.

For these reasons, a key goal of the restructured fusion program is to expand support for fusion science with basic plasma science as a key element. To achieve this goal, the fusion program should explicitly assume the responsibility to advocate and act as a steward for basic plasma science. While other agencies of the federal government provide limited support in this area, the fusion program is the primary beneficiary of advances made in the field of plasma science. The fusion program should take the lead by establishing a program to support basic plasma science, while continuing to work with other federal agencies to provide additional support for more fundamental plasma science research.

The expected benefits to the fusion program from taking on this new responsibility include aiding the development of fusion energy through advances in fundamental understanding of the behavior of high temperature magnetized plasmas and an improved interaction with related disciplines in the scientific community.

An effective program to broaden the plasma science activity supported by the fusion program would require building up to a support level of about 5% of present fusion funding. Some of the mechanisms the fusion energy sciences program could use to implement this program in plasma science are as follows:

1. Take the lead and work with other funding agencies to establish a "critical mass" plasma science research effort in a larger fraction of the nation's research universities;

2. Incorporate plasma science in the structure of the program office in a manner that provides visibility and supports the unique character required to carry out the plasma science program mission;

3. Support cutting-edge theory and experiments that might contribute to the long term rather than only the short-term development of fusion energy;

4. Seek proposals for fundamental plasma science experiments which can be performed on major tokamak facilities in analogy to outreach programs on the University of Rochester Omega laser fusion facility;

5. Establish a Plasma Science Young Investigator Award program to stimulate appointment of and provide support for new university faculty researchers in plasma science modeled after the successful programs in the NSF;

6. Broaden the academic base in the field through outreach to institutions not currently supporting plasma science;

7. Support undergraduate programs in plasma science research and education.

While the need for establishing this program is immediate, success will require a sustained effort by the fusion program to establish the necessary research infrastructure in plasma science. Adoption of the mechanisms suggested above would also be a significant step by the federal government in implementing the recommendations of 1995 National Research Council Report on Plasma Science.

6.3 Theory and Computation: Theory and computation, in conjunction with experiment, provide the predictive capability at the core of the scientific research endeavor. Great progress in the understanding of plasma and fusion physics has been made in recent years (e.g., turbulence, stochasticity and chaos, magnetic reconnection, wave-particle interactions).
Theory is cost effective and has high leverage. Quantitative predictions of plasma behavior are becoming increasingly accurate and contribute to the effectiveness of the experimental program. Progress in the experimental program has produced measurements of more theory-relevant quantities (such as current density profiles and core turbulence, for instance), which in turn uniquely guide the theory effort towards a more realistic description of the plasma. This impetus should be maintained.

The United States remains a world leader in theory and computational modeling. We are well positioned to contribute to and benefit from the worldwide fusion effort.

Even though the U.S. theory program has been effective and successful in the past, with its expanded scope and with an expected increase in computational activity, the theory program will be hard pressed to meet its objectives even if budgets remain steady. In consequence, we point to some recommendations that would improve the present status.

The restructured program will support fundamental research in both plasma and fusion science. This enhances cross-fertilization and scientific visibility, stimulating greater topical diversity in fusion research.

Increased coordination and flexibility in applied theory and computation, based on scientific goals, should be established at both the OFE level and within and among institutions. Evaluation and implementation structures may need re-alignment.

The adequacy of computational resources needs to be assessed and provided as needed. Expected hardware and algorithm optimization developments should be exploited whenever possible.

On the educational level, the new program encourages a broad treatment of plasma science in undergraduate and graduate courses, including non-fusion basic plasma physics subjects such as low temperature and non-neutral plasmas. This will help attract young talent into the field and provide better postgraduate opportunities for young scientists.

Targeted support for valuable young scientists needs to be provided to ensure renewal.

Fostering communication paths within the fusion program and with the broader scientific community will make the whole fusion program more effective. This calls for a diversity of approaches, including expanding the scope of existing meetings, promoting goal-oriented working groups and task forces, and nurturing diversity.

6.4 Major Tokamak Facilities: The U.S. fusion program has three major operating tokamak facilities, the Tokamak Fusion Test Reactor (TFTR), the DIII-D tokamak, and the Alcator C-Mod tokamak. These facilities all have strong scientific teams and provide a strong collection of hardware capabilities for tokamak and basic plasma physics research. Their scientific programs contribute in a major way to the primary U.S. goal of tokamak concept improvement and contribute stimulating ideas and results to the world fusion program. They are centers of collaboration for both U.S. and foreign scientists, providing opportunities to test new ideas and to advance fusion science. They provide a basis for international collaborations, such as scientific personnel exchanges, joint experiments, and contributions to the physics basis for ITER. These are world-class facilities with outstanding records of scientific accomplishment.

A primary goal of the major tokamaks is to understand the plasma science of high-temperature, magnetically-confined plasmas, with concept improvement as the focus of “a strong domestic core program in plasma science,” as identified by PCAST. The concept improvement goals for tokamaks are to establish the scientific foundations for steady-state operation, low frequency of disruption, and high power density operation. In the long term, these improvements could significantly enhance the attractiveness of tokamaks, by making them less expensive and more reliable. The scientific issues in this area include the understanding of plasma stability at high pressure, plasma transport, plasma profile control, non-inductive current drive, power and particle exhaust, and alpha particle effects. The highly nonlinear coupling among these elements makes their integration a scientific challenge.

Concept improvement is an exciting and dynamic research area, in which scientific progress worldwide has been rapid in the last few years. The U.S. tokamaks have led this area of research and contributed greatly to its progress.

The three facilities together comprise the largest element of the U.S. magnetic fusion program (about 40% of the total magnetic fusion budget). As a matter of good practice, we consider it essential that any operating experiment be supported with healthy funding to operate cost-effectively. It must have the resources (operating time, hardware upgrades, and scientific staff in appropriate balance) to be fully productive. All three major tokamaks contribute to and advance the goals of the restructured U.S. fusion science program. Each is well positioned to make further scientific advances. However, restructuring the fusion program within budget levels that are greatly reduced from previous years may make it necessary, in the near term, to retire one of the major tokamak facilities. The program of deuterium-tritium plasma studies currently under way in the Tokamak Fusion Test Reactor (TFTR) can be completed in the relatively near term, whereas the programs on DIII-D and Alcator C-Mod extend for a longer term. It is appropriate, then, that TFTR should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from this facility.

The combination of constrained budgets with rapid shifts in program directions may necessitate a premature termination (i.e., within a period significantly less than two years) of the TFTR program. If this is required, a number of important research objectives requiring deuterium-tritium plasmas would not be completed. It is unclear when these lost scientific opportunities would return. For this reason, we believe that sufficient resources should be provided to operate TFTR at high productivity throughout FY97, while also operating DIII-D and Alcator C-Mod at high productivity. In any event, the DIII-D and Alcator C-Mod programs should be supported for full productivity after TFTR is retired during FY98.

6.5 Plasma Confinement Research (Alternative Concepts) Different approaches to magnetic configurations of plasmas can provide a variety of research vehicles to investigate basic plasma physics as well as physics issues common to the magnetic confinement approach to fusion. The term "alternative concepts" refers to magnetic confinement configurations other than the standard or advanced tokamak that is the focus of the worldwide tokamak program. The division of fusion research into mainline tokamaks and alternatives is historical and problematical. It understates the strong physics connections between most magnetic confinement approaches, and the research techniques which they share. It also does not convey the greatly differing stage of development of tokamaks and non-tokamak plasma confinement approaches to fusion. Ultimately, a fusion reactor will likely draw on the broad-based physics foundation that comes from experimental and theoretical studies in a variety of plasma confinement approaches including "alternative concepts."

A prime reason for broadening the scope of the program to include studies in alternative concepts is that the study of more than one plasma confinement system configuration advances plasma science and fusion technology in ways not possible in one system only. Examples of past discoveries and innovations of significance to tokamaks and physics in general are numerous (including discovery

of the bootstrap current, invention of helicity injection current drive, development of neutral beam heating, discovery of the dynamo effect in the laboratory, to name a few). For each alternative concept there are challenging scientific issues to understand and resolve. Understanding these issues may lead to improved concepts for reactor applications. Indeed, with decades to go to fusion power, it would be premature to narrow to one concept. Finally, broadening the scope of the magnetic confinement fusion program to include alternative concepts opens the plasma research community to new ideas, an important feature in attracting new talent to the field.

Of the large number of known alternative magnetic confinement concepts, several are arguably ready (in terms of present scientific understanding) for theoretical evaluation and modest-scale experimental studies that might yield important scientific insights into plasma confinement. These include the field reversed configuration, the reversed field pinch, the spherical tokamak, the spheromak, and the stellarator. There have been substantial advances, both worldwide and in the residual activity in the United States, in the theoretical and experimental understanding of various alternative configurations since 1990 (the time at which the alternative concepts research program was nearly eliminated in response to budget pressure).

Reinitiation of an alternative concepts research program will increase the breadth of plasma research and the emphasis on science and innovation, consistent with an increased emphasis on basic plasma research. The resulting diversity will increase the visibility and impact on the larger scientific community. Under the constrained budgets anticipated in coming years, alternative concepts is an area in which the United States can maintain excellence in the world context, with modest expenditure. The science program carried out on alternative confinement concepts should be closely integrated with the tokamak program, recognizing the universality of the physics issues and increasing the attention to underlying science issues.

Existing alternative concept research is already strongly coupled to the international effort. Japan and Europe have large programs in alternatives. They are mainly concentrated on the stellarator, with other concepts pursued mostly in experiments of modest to medium scale. The United States can be at the forefront of research in selected areas of alternative concepts research. If experiments in an alternative concept should prove very successful, such that a large experiment is needed for further progress, then such an endeavor could be pursued internationally, in analogy with research in tokamaks.

We recommend that the concept improvement program be expanded to include a spectrum of alternative concepts plasma confinement systems, including experimental and theoretical research. This is consistent with the report of the PCAST and the Office of Fusion Energy draft strategy, both of which strongly endorse alternative concepts research and include it as part of their highest priority goal. Several concepts may be ready for experiments to elucidate key physics issues. The precise funding level for alternatives cannot be prescribed here. It must be driven by peer-reviewed proposals (from national labs, universities, and industry), as for any scientific research program. As for the program's major facilities, any experiment which is operated should be supported with healthy funding to operate cost-effectively.

6.6 Inertial Fusion Energy In Inertial Confinement Fusion (ICF) the fusion energy is released by imploding a spherical small pellet of deuterium and tritium using energetic lasers or particle beams as drivers. Supported primarily by Defense Programs in DOE ICF is now the largest fusion program in the U.S. The principal purpose of the ICF program is stockpile stewardship to provide the scientific base for nuclear security applications. The National Ignition Facility (NIF) is an approved billion-dollar ICF facility designed to demonstrate ignition in ICF pellets by about 2005. Study of the hot spot and burn in NIF will also settle the main scientific issues of high-gain targets and establish the driver requirements.

Four different drivers have been identified and studied for ICF purposes: glass-based lasers, KrF lasers, light-ion accelerators, and heavy-ion accelerators. The development of the first three drivers was funded by Defense Programs, and glass-based lasers were chosen for NIF. Heavy-ion accelerators have been investigated for Inertial Fusion Energy (IFE) power applications, because for these applications the drivers must be reliable and efficient with a high pulse-repetition rate (several Hertz) and long life.

Many recent reviews of the IFE program have indicated that accelerator development is ready to proceed to the next step - - the Induction Linac Systems Experiment (ILSE) project, which would provide an integrated demonstration of induction-linac technology and the beam physics required to provide the data base for scaling to a heavy-ion driver for an inertial fusion power plant. In 1993 the Fusion Energy Advisory Committee Panel 7 on IFE reviewed the status of IFE efforts in the Office of Fusion Energy and recommended a "balanced program that includes an experimental and analytical program for supporting IFE technologies as well as accelerator development and beam physics." This panel recommended a "reference" annual budget of $17M, with $14M for ILSE and accelerator research, and $3M for supporting technology and systems studies. The panel also found that for $10M per year it is not possible to complete the ILSE project, although a significant set of accelerator experiments could be conducted to increase understanding of key technical issues.

At an annual budget of $8M, IFE cannot proceed to its logical next step (ILSE). This budget reflects the limited scientific synergy between accelerator development and magnetic fusion (MFE). However, IFE and MFE share a large number of scientific issues: MFE plasma science and IFE driver-independent plasma science issues (funded in Defense Programs), and the fusion-support technologies of both IFE and MFE.

The Subcommittees did not assess the IFE effort in detail, but acknowledge its potential as a fusion energy source and the major role of DOE Defense Programs in addressing key scientific and plasma physics issues. A programmatic review should be conducted involving all cognizant DOE program offices and appropriate scientific and technical experts to recommend the priority and management of IFE, in the context of the mission, policy, and scientific goals of the restructured program.

6.7 International Thermonuclear Experimental Reactor (ITER): The logical follow-on to the experimental program being carried out in the present generation of tokamaks is a burning plasma

experiment, that is, the demonstration of controlled ignition and extended burn of a DT plasma. Goals for a burning plasma physics experiment include both the demonstration of long-pulse ignition, and the demonstration of a driven, high-beta, high bootstrap-current-fraction steady-state burn. This latter goal represents the continuation of the concept innovation program being pursued at our large tokamak facilities. The new physics issues that must be addressed on such a facility mainly relate to fusion alpha physics, dynamic control of the operating point, control of plasma profiles (through non-inductive current drive and the formation and control of transport barriers), particle and heat removal, and disruption avoidance and/or mitigation.

Burning Plasma Physics will be pursued through international collaboration. At present, the principal international collaboration aimed at the construction of a burning plasma experiment is the ITER EDA — a collaboration involving the United States, the European Community, Japan, and the Russian Federation that seeks to demonstrate the scientific and technological feasibility of fusion power. The ITER mission addresses both the physics and technology issues essential to an engineering test reactor. ITER is both a great physics challenge (if ITER is built, it will test burning plasmas at the reactor scale and provide a test bed for most of tokamak physics, and much generic physics) and a great technological challenge (it requires the development of high-field superconducting magnets, high-heat-flux components, plasma heating and fueling systems, and other reactor-relevant fusion technologies). U.S. industry has been given a major role in designing and building prototype components of the ITER during the EDA, while having access to all design and development activities of the other parties. This role helps to assure that American industry will be able to compete for construction elements, if ITER is built and the United States participates. In addition to the potential benefits of sharing the ITER construction cost, this important international collaboration focuses the world fusion program on a concrete objective.

The U.S. program extracts substantial benefits from the ITER EDA. At present, ITER is the primary vehicle for plasma technology development in the U.S. program (as discussed in the technology section below). ITER Physics R&D also provides one of the focuses for our large tokamak experiments. Important physics issues must still be resolved in the areas of divertors, disruptions, density limits, transport scaling, L-to-H and H-to-L mode power thresholds, current and pressure profile control, and beta-limits. The ITER EDA has added urgency to the international effort to address these issues. The results of this physics R&D, together with the ITER physics design requirements, will form the physics basis for ITER. This ITER Physics Basis must be assessed by all parties before there is a decision to construct ITER. In particular, the U.S. program will review the ITER EDA output (including both physics basis and engineering design) prior to a decision to seek participation in ITER construction.

Given the pivotal nature of a decision on ITER construction to the U.S. program, we recommend that a mechanism be established immediately to expand involvement of the U.S. fusion community in the assessment of the evolving ITER physics and technology basis to ensure that the ITER design reflects our current best understanding of tokamak physics and to ensure that the U.S. community appreciates the issues that have driven ITER design decisions.

The recent budget changes in the U.S. fusion program indicate that the United States is very unlikely to participate as a full partner in ITER. Nevertheless, we recommend that the U.S.

program continue participation (as allowed by the budget) in the EDA phase of ITER to which the United States is committed through FY98, thereby fulfilling our existing commitments to our international partners and leaving open the possibility of some U.S. participation in ITER construction or other major international collaborations which provide a cost-effective means of advancing fusion science.

In the remainder of the EDA, it is important for the U.S. program to increase its emphasis on advanced tokamak scenarios to ensure that the ITER facility provides a suitable vehicle to pursue the study of tokamak concept innovation in a burning D-T plasma. This effort would include increased attention to operational flexibility, the definition of the ITER heating and current drive systems, diagnostics, and control systems. These are areas in which the U.S. program might have maximum impact at minimum cost during ITER construction and operation.

Regardless of the outcome of the post-EDA phase, the United States will have benefited from our involvement in the ITER EDA, because ITER has acted as a driver for technology development, for engineering design innovation, and for involving the world in an enhanced collaborative attack on the primary physics and operational issues of tokamaks. It has also forced the fusion community to face squarely the engineering challenges of designing a steady state, high power, D-T burning plasma device.

In 1998 the EDA agreement will be concluded, and the parties will be faced with a decision about ITER construction. Since ITER construction is expected to cost in excess of $6B, clearly the U.S. program cannot participate as an equal partner at present budget levels. However, the European, Japanese, and Russian programs have indicated that they may welcome U.S. participation as a limited financial partner. Hence, if our ITER partners agree to pick up the bulk of the cost of ITER construction, and the U.S. program agrees that the ITER design complements our goals, then we should seek to participate in ITER construction as a limited financial partner to provide a continuing focus for our tokamak physics and technology program, so that U.S. scientists will have access to the ITER facility after it is completed, and so that the United States can benefit (to an extent commensurate with our contributions) from lessons learned in ITER construction and operation. Possible U.S. contributions to ITER construction include engineering and component production, as well as physics design. As an example, a focus on plasma diagnostics and control systems would lead naturally to the provision of a remote ITER access site in the United States for our participation in ITER operation.

A low-level U.S. domestic effort to search for less expensive means of studying burning plasmas would be useful insurance against the possibility that ITER is not constructed. Note that any such effort could not be part of the U.S. contribution to the ITER EDA. Should that search prove successful and in the event the international partners decide to modify their objectives for a next-step device, then the United States should explore with its former ITER partners (and other nations if this becomes appropriate) the possibility of international collaboration on a less expensive means of fulfilling the goal of DT ignition and burn.

6.8 Fusion Materials and Technology: Fusion science encompasses fundamental materials and enabling technology development, including (1) low activation materials and fusion technologies essential to achieve safety and environmental potential of fusion and (2) critical plasma technologies required to support advances in plasma physics. The science associated with fusion materials and technology development acts as a driver for a wide range of scientific applications far beyond fusion (e.g., superconducting magnets, high temperature and radiation resistant materials, cryogenic materials, electromagnetic power systems, heat removal systems, and plasma diagnostics).

The development of low activation materials is a long-term endeavor and a critical issue on the path to fusion energy. The performance requirements for materials are unprecedented. Materials must withstand 14 MeV neutron irradiation damage, high operating temperatures, thermal and mechanical loads, and chemical compatibility requirements. The materials science associated with these requirements must be understood to determine performance and lifetime limitations of candidate materials with low activation characteristics. The structural materials effort is currently limited to only three candidate materials: vanadium alloys, SiC composites, and ferritic steels. In addition, development of low activation materials for non-structural applications (e.g., plasma facing materials, designs and developments, electrical insulators, tritium breeding) is critical for fusion.

Fission reactors are currently used for irradiation studies. However, a 14 MeV neutron source will ultimately be needed to fully qualify materials for fusion reactor applications. Conceptual development of a materials test facility is being pursued as part of an international collaboration. A cost estimate for this facility is expected to be available in May 1996. Since preliminary information suggests that the cost will approach $1B, it is difficult to see how the U.S. share of this facility cost could be accommodated within the present budget.

The U.S. low activation materials program must address this problem over the next year. Possible resolutions include substantial down-sizing of the facility, participation as a limited financial partner, or seeking funding outside of the fusion program. In any event, the low activation materials program should be continued with extensive international collaborations.

The safety, environmental attractiveness, and economic competitiveness of fusion will depend to a large extent on the blanket system, since this is the largest component exposed to a high neutron fluence. Development of a blanket system that meets performance requirements of tritium self-sufficiency and efficient energy recovery, while meeting the safety and environmental goals of fusion, remains a critical issue. This technology also includes tritium and safety-related technologies. A fundamental understanding of key materials and technology issues is required to develop a compatible combination of materials (breeder, coolant, and structure) for the blanket system. Key issues include chemical compatibility, neutronics, thermohydraulics and stress analysis in addition to the irradiation effects and tritium interactions.
The U.S. program should focus on those issues which are: critical to the safety/environmental goals of fusion; lead in areas in which the United States has demonstrated particular expertise; and make extensive use of international collaboration. To be an effective participant in international collaborations the United States must maintain a significant base program in fusion technology. In

addition, it is important that the low activation structural materials program be closely associated with the blanket technology effort.

Plasma technologies provide the enabling capabilities for near term and future plasma science and fusion energy applications. Plasma and enabling technologies for fusion include the following:

- Superconducting magnets
- Steady state heat and particle control components
- Plasma heating and fueling systems
- Plasma diagnostics

The application of new plasma technologies has been essential to the advancements in fusion. In the 1960's hot plasmas were of short duration with many impurities, and the state of the vacuum vessel walls was not characterized. During the 1970's auxiliary heating systems provided controlled multi-kilovolt plasmas. More recently, high field copper and superconducting magnets, improvements to fueling, choice of plasma facing materials and wall conditioning techniques have led to significantly higher temperatures and longer pulse lengths in tokamaks and other configurations.

ITER is now the primary vehicle for plasma technology development in the U.S. program. The United States has the lead in two of the seven major ITER deliverables — the central solenoid model coil and the divertor cassette prototype — and plays an important role in the shielding blanket module, vacuum vessel sector, and remote handling demonstrations. The ITER R&D is highly leveraged, since all parties are contributing to these deliverables in a coordinated program. We obtain all of the R&D results for less than one third of the cost.

Any current or next generation device will benefit from plasma and enabling technology R&D. The superconductor and fabrication concepts adopted by ITER originated in the U.S. program. This basic concept of the "cabled" niobium-tin superconductor is embodied in the design of superconducting fusion devices. Plasma-material interactions are intrinsic to all magnetic fusion concepts. In addition, compact advanced tokamaks and alternative concepts will require enhanced heat removal technology due to their higher heat fluxes. The United States is a leader in developing actively cooled plasma facing components. Heating and fueling systems are required for any magnetic confinement concept. In electron cyclotron heating, the United States holds the world record for pulsed gyrotron energy output and CW operation. U.S. ion cyclotron heating antennas are used on fusion devices around the world. The United States is preeminent in pellet fueling technology. Finally, innovative U.S. plasma diagnostics also provide a convenient means for obtaining international collaborations in foreign facilities.

Regardless of the longer term role of the United States in ITER construction, further investments in plasma technology must be made to insure optimal advancement of plasma science in the future. These activities may be undertaken both nationally and internationally. Industry will continue to play a valuable role, especially when a particular technology comes from applications outside of fusion, so that fusion can leverage off of a broader support.

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Appendices

Appendices

The Scientific Issues Subcommittee (SciCom) of the Fusion Energy Advisory Committee assisted the review of the fusion program by providing scientific assessments of major issues covering:

A. Fusion Program Scientific Goals

B. Development Of Basic Plasma Science

C. Theory and Computation

D. Major Tokamak Facilities

E. Plasma Confinement Reasearch (Alternative Concepts)

F. Inertial Fusion Energy

G. International Thermonuclear Experimental Reactor (ITER)

H. Fusion Materials and Technology

These scientific assessments are treated in white papers that comprise the following appendices. The white papers were prepared to inform the Strategic Planning Subcommittee (SPS) in its deliberations. Short summaries are included in Section 6 of the body of the report. In cases where there are inconsistencies, the recommendations and conclusions in the main body of the report, especially in Sections 1 through 5 which take into account other factors, take precedence.

Appendix A

Fusion Program Scientific Goals

The **Mission of the Fusion Program** is to:

Advance plasma science, fusion science and fusion technology -- the knowledge base for an economically and environmentally attractive fusion energy source.

The two general goals that follow from this mission statement are:

1) Science -- develop the core science of plasma physics; produce, understand and optimize fusion plasmas.

2) Energy -- develop an attractive electric power producing system from fusion plasmas.

The suggested science objectives for the restructured U.S. fusion program are (key elements, not in priority order):

*** Promote Plasma Science**:

Reinvigorate OFE leadership of U.S. plasma science [1].
Enhance understanding of fusion plasmas.

*** Push Fusion Innovation** (natural U.S. niche in world program):

Focus tokamak effort on improvements and demonstrate their potential.
Advance state of alternate concept research.
Develop and implement some enabling technologies.
Develop low activation materials.

*** Study Burning Plasmas**:

Develop a viable U.S. role in an internationally developed, constructed burning plasma experiment.
Contribute to a successful completion of the ITER EDA.

These objectives are in accord with the priorities set forth in the PCAST report [2], and are amplified in the following text and subsequent appendices.

A.1 Plasma Physics And Fusion

The fusion program has always been both a science and an energy program -- a use-inspired science program. The core, unique science of fusion is plasma physics or plasma science -- the study of ionized gases in which the effects of collective interactions of charged particles dominate over their binary interactions. Plasma physics embodies most elements of classical physics (mechanics, electrodynamics, kinetic theory, and fluid mechanics). It has practical applications in many areas (plasma processing, particle accelerators, microwave generation, etc.) and it has contributed significantly to diverse fields of science (e.g., nonlinear dynamics, space physics, and astrophysics). Finally, it is a modern science that has been developed primarily through the fusion program over the past few decades. While many fundamental aspects of high temperature plasma physics have been developed, it is still an intellectually challenging, vibrant, growing science -- now particularly in comparisons of theory with experiment.

A.2 Plasma Science

Because plasma physics is the core science for fusion and provides so many spin-offs, it is incumbent on the fusion program, which has historically been the principal driver for the development of modern plasma physics, to assume a leadership role in promoting and supporting plasma physics research. The Office of Fusion Energy (OFE) has historically been the dominant steward of plasma science. However, during the recent past, as fusion funding has decreased and the emphasis was placed on fusion energy development, the OFE support for plasma science has decreased and narrowed. A recent National Research Council report [1] calls for "reinvigoration of basic plasma science," particularly small-scale, university-based basic experimental plasma science. In the restructured fusion program it is recommended that OFE be the primary supporter of high temperature magnetized plasma physics, and related basic plasma studies. In addition, it is recommended that OFE take the lead and work with other federal agencies to increase support for basic plasma science research -- low temperature plasmas (BES, NSF, NIST), high energy density plasmas (DP), space physics (NASA, AFOSR), fundamental plasma physics (NSF), astrophysics (NSF), high density beams and x-ray sources (ONR, DNA), etc.

A.3 Fusion Plasma Physics Issues

A set of key fusion plasma physics issues (adapted in part from the NRC report[1]) are:

1) Magnetohydrodynamic Equilibrium, Stability, and Dynamics (Plasma Control);
2) Transport Processes (Plasma Confinement);
3) Plasma-Wall Interactions (Limiters, Divertors);
4) Wave- and Particle-Plasma Interactions (Plasma Heating, Fueling, and Current Drive);
5) Burning Plasma Physics (Alpha Physics, Burn Control);
6) Composite Issues (Systems Integration).

Many elements of most of these issues can be studied in small to medium scale experiments; the burning plasma physics issues and most composite issues are studied only in the largest experiments in deuterium-tritium plasmas.

A.4 Enabling Technology Issues

Fusion science encompasses not only plasma physics but also many frontier technologies and their related sciences. Various types of technologies are critical for fusion energy:

1) Plasma Technologies -- high power beams of energetic atoms, many precision diagnostics, wall cleaning methods, high frequency microwave generators, hydrogenic ice pellets, compact high power density wave antennas, divertors at the plasma edge, etc.

2) Power Plant Technologies -- superconducting magnets, tritium-breeding and energy-conversion blankets around the plasma (which must simultaneously handle fluxes of high energy neutrons and radiation, and time-varying electromagnetic fields), and remote handling.

3) Low Activation Materials -- to achieve the full safety and environmental potential of fusion it is essential to develop low activation materials to minimize the radioactivity induced in surrounding structures by fusion-produced neutrons and radiation-resistant materials capable of functioning in the fusion power plant environment.

4) Experiment, Power Plant Designs -- design projects seek definitive engineering solutions for given physics requirements. They and power plant systems studies are used to identify critical issues and innovation opportunities in both the plasma and technology areas.

A.5 Recent History

The U.S. fusion program led the development of most fundamental plasma physics concepts and fusion technologies during the 1970s and 1980s. However, because U.S. fusion funding has been reduced by a factor of more than three (in inflation-adjusted dollars) since 1977 as those in Europe and Japan have increased, the U.S. fusion program is now less than 1/6 of the worldwide effort on developing fusion energy. Nonetheless, the U.S. fusion program continues to play a strong, prominent role in many fusion plasma physics and technology areas -- theory, computation, diagnostics, modeling, comparisons of theory with experiment, new modes of experimental operations, non-inductive current drive, plasma-facing components, modern divertors, low activation materials, tritium breeding blankets, etc. The present niche of the U.S. fusion program is science-based innovation in fusion plasma physics, enabling technologies and low activation materials.

A.6 Confinement Concepts

Of the various possible confinement concepts, tokamaks achieved the best plasma parameters early (about 25 years ago) and have continued to lead in this area. Consequently, most of the world's fusion resources have been expended in furthering their development. There remains, however, a strong impetus to develop a better understanding of confined plasmas to reduce risks and increase margins for success in future power plants, and to broaden the scientific underpinnings of the fusion science program. Thus, efforts should be focused on producing "tokamak improvements" with significantly improved performance, and "alternative concepts" (both magnetic and inertial) that offer the prospect of providing more attractive fusion concepts in the long run. Inertial fusion energy (IFE) research is leveraged on a large inertial confinement fusion effort supported by DOE Defense Programs and is the primary alternate to tokamaks in the United States. The Office of Fusion Energy component of inertial fusion (IFE) is presently focused on developing heavy-ion accelerators.

A.7 Innovation Focus

While modest improvements in some elements of a fusion energy system would improve its attractiveness, qualitatively new concepts that offer the prospect of significant improvements would be much more helpful for the ultimate development of fusion energy. Thus, it is appropriate in the current highly constrained budget environment for the U.S. fusion effort to ensure a strong, vibrant and continually developing plasma and fusion science base program that is focused on concept innovation in both tokamaks and alternates (IFE and magnetic). Also, it should foster the continuing development of selected critical enabling technologies and low activation materials in which the United States can play a leadership role.

An emphasis on innovation will reduce program risk in the long run. The U.S. fusion program should also leverage its investments into the much larger world fusion program to the maximum extent possible through international collaborations in areas where the United States can play a significant and innovative role. The key criterion for support should be the largest anticipated fusion science impact per unit expenditure -- irrespective of from which institution(s) or type of institution(s) the proposed research originates, the taxonomy of the confinement concept, or the size of the group or device.

Finally, since the development of fusion is intellectually very challenging, of great interest to students ("combined opportunity to address a fundamental societal need for new energy resources and to engage in research at the frontier of science" -- input to FEAC from current graduate students in the fusion program), and a long term effort (50 years or more into the future before commercialization, at least in the United States), there should be provision for recruiting,

educating, developing and retaining outstanding, innovative scientists and engineers for the fusion program in a sustainable manner.

A.8 Burning Plasmas

The study of the key fusion issue of burning plasma physics requires the successful integration of all elements of fusion science and technology into a large, integrated fusion burning experiment. Since the cost of such an integrated facility is in the range of billions of dollars, it has been decided that such a device should be built through international collaboration -- so as to reduce the cost and programmatic risk by any one country. The United States should continue to participate with the world fusion program in developing an integrated burning plasma experiment.

The present focus of this international collaboration is the ITER project, whose six year Engineering Design Activity (EDA) phase is due to be completed at the end of 1998. The United States should continue its participation in the ITER EDA and thereby help to bring about a successful completion of the EDA. This allows the United States to obtain maximum benefit (in the worldwide establishment of the physics and technological basis for a burning plasma experiment) from the ITER EDA, and to facilitate continued U.S. influence on the development of the ITER device. In this continued participation in the ITER EDA process, the United States should focus at least some effort on areas where the United States can expect to be able to participate in an ITER construction project at moderate cost as a limited financial partner and where the United States could play an innovative, leading role -- for example, in diagnostics, control and data acquisition, some key magnet areas (e.g., magnet instrumentation and quench protection), divertor cassette fabrication and assembly. Within the ITER EDA, U.S. participation in any new areas would have to be negotiated with our ITER partners.

A low level U.S. domestic effort to search for less expensive means of studying burning plasmas would be useful insurance against the possibility that our international partners (Europe, Japan and Russia) decide not to construct ITER. Should that search prove successful and in the event that our international partners decide not to construct ITER, then the United States should explore (with its former ITER partners and others) the possibility of international collaboration on a less expensive means of fulfilling the goal of studying burning plasmas.

REFERENCES:

[1] “Plasma Science -- From Fundamental Research to Technological Applications,” NRC Report (Nat. Academy Press, Wash., DC, 1995).

[2] "The U.S. Program of Fusion Energy Research and Development," The President's Committee of Advisors on Science and Technology (PCAST) Report, July 1995.

Appendix B

Development Of Basic Plasma Science

B.1 Introduction

Plasma science is the study of ionized states of matter. It includes the core discipline of plasma physics, but as defined in the NRC Report on Plasma Science,[5] it is now a much broader collection of phenomena in ionized matter in which atomic, molecular, radiation-transport, excitation, and ionization processes as well as chemical reactions can play significant roles. The common research themes in plasma science are:

- Wave-Particle Interaction and Plasma Heating
- Chaos, Turbulence, and Transport
- Plasma Sheaths and Boundary Layers
- Magnetic Reconnection and Dynamos

All four of these intellectual problem areas are central to the work being carried out to develop magnetic confinement systems for the high temperature plasmas required for the production of fusion energy. It is through these four research problem areas that the plasma science developed for fusion energy makes connection with the other important fields of application of plasma science including low-temperature plasmas (e.g., semiconductor fabrication), non-neutral plasmas (e.g., atomic clocks), inertial confinement fusion plasmas (e.g., energy production and weapons simulation), particle accelerators and coherent radiation sources, space plasmas, and astrophysical plasmas.

B.2 Role of Plasma Science in the U.S. Fusion Energy Program

Plasma science is central to the effort to develop magnetically confined plasmas as a fusion energy source and the DOE led effort to develop fusion energy has been (and continues to be) the largest driver for the intellectual advancement of plasma science. As stated in the PCAST Report, "This [fusion] funding also sustains an important field of research -- plasma science -- in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable to other fields of science and in industry." Advances in each of the four intellectual problem areas listed above are essential to make further progress on fusion energy development as illustrated with a few examples listed below:

Wave-Particle Interaction and Plasma Heating: Present fusion research in this area is centered on the development of efficient methods to drive locally controllable plasma currents non-inductively with RF excited waves in the plasma which can be used to optimize the magnetic geometry in toroidal systems. If successful, significant improvements in the stable plasma pressure limits and confinement in a steady-state plasma equilibria may be possible.

Chaos, Turbulence, and Transport: One of the challenges in controlling an ignited fusion plasma is the dominance of the fusion heating source over any external power source in determining the plasma pressure profiles. Recent discoveries in the suppression of plasma turbulence in the core of a collisionless tokamak plasma open up the possibility of a significant improvement in confinement as well as a possible method of modifying the pressure profile in an ignited fusion plasma for improved stability in a steady-state equilibrium. (See Attachment 1 of Appendix D on Major Tokamak Facilities for additional detail.)

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[5] NRC Report on Plasma Science, National Academy Press, Washington, DC 1995.

Plasma Sheaths and Boundary Layers: A critical unresolved problem in fusion energy is the development of techniques for the removal of heat and particles at the edge of the magnetically confined ignited fusion plasma. The leading candidate for this in a tokamak device is a divertor system which makes the transition from a very hot plasma at the plasma edge to a much denser low temperature plasma in contact with a material wall. (See Attachment 2 of Appendix D on Major Tokamak Facilities for additional detail.)

Magnetic Reconnection and Dynamos: A fundamental property of any magnetically confined plasma with finite conductivity is the possibility of magnetic reconnection which is a very important phenomenon in astrophysical, solar, and magnetospheric plasmas. Reconnection phenomena, usually described with a resistive-MHD model, are centrally involved in the dynamo activity seen in reversed field pinch configurations and in the sawtooth phenomenon seen in the core of tokamak plasmas.

B.3 Narrowing of the Support for Plasma Science

In the early 1980's, the fusion energy program in the United States was a relatively broad program which included a wide spectrum of experimental facilities and an array of alternative approaches to the tokamak. As the program positioned itself to move toward energy development with a short-pulse fusion ignition device in an environment of declining budgets, most of the program in alternatives was abandoned for budgetary reasons in the late 1980's. In response to Congressional policy direction in the early 1990's, the move to a schedule-driven energy development program has led to a further narrowing of the breadth of support for plasma science. This situation has led to considerable concern within the fusion and plasma science community and has been noted by both the PCAST review of magnetic fusion and the NRC Report on Plasma Science. Extracts from each of these reports are summarized below:

PCAST Report (page 36):

- although a strong core program has existed and continues to exist within the US effort, it is not as broad or as strong as is desirable, it is focusing increasingly on support of the ITER and TPX projects, and it is in danger of being squeezed down to inadequacy by the drain on the budget generated by these projects as they enter their construction phases;
- the effort on alternative concepts, which was practically eliminated in the series of project cancellations brought on by program cuts through the 1980's and into the early 1990's, is now wholly inadequate;

NRC Plasma Science Report (pages 23 and 89):

- Unfortunately, in the past, many opportunities for fundamental scientific exploration were missed, in some instances because of funding constraints and in others because of changing priorities within the fusion program.
- ...painful choices have often had to be made between upgrading larger facilities to operate in high-performance regimes and increasing the scope of scientific investigations in intermediate-scale devices.
- If the present trend toward large experiments continues without adequate attention paid to a broader base of experimental facilities, a dangerous gap will develop in our ability to address the wide range of questions important to fusion-relevant plasma physics.

The damage caused by this narrowing of support for plasma science can be viewed from two distinct perspectives. From an internal perspective, there is a weakening of the fusion program's capability to innovate and to produce the needed advances in fusion-relevant plasma science. From an external perspective, the fusion program has diminished its capability to make significant and

enduring contributions to the advancement of plasma science of broad benefit to all the other important fields of application of plasma science.

B.4 Conclusions and Recommendations

Conclusion: *The narrowing of the breadth of support for plasma science by the fusion program is seriously weakening the capability of the fusion program to make significant and enduring contributions to the advancement of plasma science.*

Recommendation: *A key goal of the restructured fusion program is to expand support for fusion science with basic plasma science as a key element. To achieve this goal, the fusion program should explicitly assume the responsibility to advocate and act as a steward for basic plasma science.*

While other agencies of the federal government provide limited support in this area, the fusion program is the primary beneficiary of advances made in the field of plasma science. In turn, the many related fields and applications benefit from developments in plasma science from the fusion and other programs. The fusion program should take the lead by establishing a program to support basic plasma science, while continuing to work with other federal agencies to provide additional support for more fundamental plasma science research.

The expected benefits to the fusion program from taking on this new responsibility include aiding the development of fusion energy development through advances in fundamental understanding of the behavior of high temperature magnetized plasmas and an improved interaction with related disciplines in the scientific community.

Recommendation: *An effective program to broaden the basic plasma science activity supported by the fusion program would require building up to a support level of about 5% of present fusion funding.*

Some mechanisms the fusion program could use to implement this program in plasma science are:

- take the lead and work with other funding agencies to establish a "critical mass" plasma science research effort in a larger fraction of the nation’s research universities;
- Incorporate plasma science in the structure of the program office in a manner that provides visibility and supports the unique character required to carry out the plasma science program mission;
- support cutting-edge theory and experiments that might contribute to the long term rather than only the short term development of fusion energy;
- seek proposals for fundamental plasma science experiments which can be performed on major tokamak facilities in analogy to outreach programs on the University of Rochester Omega laser fusion facility;
- establish a Plasma Science Young Investigator Award program to stimulate appointment and provide support for new university faculty researchers in plasma science modeled after the successful programs in the NSF;
- broaden the academic base in the field through outreach to institutions not currently supporting plasma science;
- support undergraduate programs in plasma science research and education.

While the need for establishing this program is immediate, success will require a sustained effort by the fusion program to establish the necessary research infrastructure in plasma science.

Adoption of these suggested mechanisms would also be a significant step by the federal government in implementing the recommendations of 1995 NRC Report on Plasma Science.

Appendix C

Theory and Computation

Theory and computation, in conjunction with experiment, provide the predictive capability at the core of the scientific research endeavor. Great progress in the understanding of plasma and fusion physics has been made in recent years (e.g., turbulence, stochasticity and chaos, magnetic reconnection, stability and wave-particle interactions), thanks to the interplay between theory and the other elements of the fusion research program.

Theory is cost effective and has high leverage. Quantitative predictions of plasma behavior are becoming increasingly accurate and contribute to the effectiveness of the experimental program. On the other hand, progress in the experimental program has produced measurements of more theory-relevant quantities (such as current density profiles and core turbulence, for instance), which in turn uniquely guide the theory effort towards a more realistic description of the plasma. This impetus should be maintained.

The United States remains a world leader in theory and computational modeling. For example, theory input would be valuable in foreign collaborations such as on ITER and stellarators, and in assessment of proposed innovative experiments. We are well positioned to contribute to and benefit from the worldwide fusion effort.

The theory program combines many areas of expertise -- from fundamental physics issues, to development of numerical algorithms to solve specific problems, to application of such tools in the context of a particular experimental need. All parts of the program benefit from interaction amongst all those areas, and with experiment. For instance, it seems clear that new parallel computers and gyrokinetic algorithms, combined with experimental benchmarking, will yield dramatic progress in our understanding of electrostatic turbulence within a few years. MHD tools are just now being usefully extended to cover resistive modes.

Even though the U.S. theory program has been effective and successful in the past, with its expanded scope and with an expected increase in computational activity the theory program will be hard pressed to meet its objectives even if budgets remain steady. In consequence, we point to some **recommendations** that would improve the present status:

1) Basic plasma physics: The restructured fusion program will support fundamental research in both plasma and fusion science, both experimentally and theoretically. This enhances cross-fertilization opportunities and scientific visibility, stimulating greater topical diversity in fusion research. It provides a better context for plasma physics as a legitimate science and an important

participant in the quest for knowledge. From the theory point of view, this renewed emphasis on scientific research poses a welcome challenge, which will strengthen the whole program internally and externally.

2) Evaluation: In a constrained budget environment, the efficient utilization of available resources is of paramount importance. A transparent, merit-based peer-review process should be in place to coordinate the theory and computation effort, choose and support the best performers, and terminate funding when undeserved. At the same time, a purely goal-oriented, project-driven management structure would be detrimental to the most innovative elements in the program. A balance needs to be maintained between short-term needs and the long-term pursuit of excellence.

3) Coordination: Applied theory and computation are usually team efforts. Increased coordination and flexibility in applied theory and computation, based on scientific goals, should be established at both the OFE level and amongst institutions. Where possible and appropriate an effort should be made to restructure the review process into consideration of units of topical working teams, rather than by institutions. There is clear benefit in pooling the available resources and reducing unnecessary multiplication of efforts towards the development of numerical tools. Utilization of numerical tools for experimental purposes may need to be funded in conjunction with experimental proposals.

4) Equipment: The rapidly evolving computational hardware and technology environment calls for flexibility in the assessment and purchasing of equipment. The fusion program needs to position itself to take advantage of new developments such as DOE's Advanced Scientific Computing Initiative (ASCI) project, which is expected to deliver 0.5 Teraflop performance this year. Increased computational speed, together with algorithm development, will contribute to essential predictive capabilities in plasma physics. Adequacy of computational resources needs to be assessed and provided as needed.

5) Education: On the educational level, the restructured program encourages a broad treatment of plasma science in undergraduate and graduate courses, including basic physics subjects such as low temperature and non-neutral plasmas, nonequilibrium statistical mechanics, nonlinear dynamics, aspects of astrophysics, etc. This intellectual diversity will help attract young talent into the field. At the same time, it will provide better postgraduate opportunities for young scientists. At the moment there is great demand in the semiconductor industry for knowledge in basic plasma physics, since the manufacturing process requires research on plasma-material interactions. The fusion program is best positioned to meet this educational demand, and theory efforts should consider these short-term needs as well as the longer-term pursuits of scientific excellence and fusion energy.

6) Renewal: Targeted support for valuable young scientists would help renew the human resources of the program. This is needed to help talented young scientists remain in the field after graduation, or enter it from other disciplines (in all types of institutions). A Plasma Science Young Investigator Award program should be considered. As in other areas of research, a long series of postdoctoral appointments is detrimental to the healthy renewal of the field. Provisions should be made for the availability of less evanescent employment.

7) Communication: The Sherwood Theory Meeting could contribute to the broadening of the program by explicitly welcoming research in all areas of plasma physics, and seeking participants from relevant related fields of physics. Joint theory-experiment ventures (such as the Transport Task Force, the Divertor Theory Task Force, or the MHD working group) facilitate the internal interaction of physicists in the program, by being specifically goal-oriented. They should be encouraged. At the same time, other possible avenues for communication (besides meetings) should continue to be explored: exchange of researchers, visiting positions, electronic-mail bulletin boards, dissemination of drafts and preprints, publications, etc. These other forms of communication are less constraining and allow for greater diversity than purely topical efforts.

Appendix D

Major Tokamak Facilities

This appendix concerns the program's three major tokamak research facilities DIII-D, Alcator C-Mod, and the Tokamak Fusion Test Reactor (TFTR). Together they comprise the largest element of the U.S. magnetic fusion program (about 40% of the budget). These are world-class facilities with outstanding records of scientific accomplishment. Their scientific programs contribute in a major way to U.S. goals and contribute stimulating ideas and results to the world fusion program. They are centers of collaboration for both U.S. and foreign scientists, providing opportunities to test new ideas and to advance fusion science. They provide a basis for international collaborations such as scientific personnel exchanges, joint experiments, and contributions to the physics basis for the International Thermonuclear Experimental Reactor (ITER). In this appendix we provide an assessment of the major facilities' scientific capabilities, expected contributions to program goals, and their future in the restructured fusion program.

D.1 Context: U.S. Program Strategy

In considering the tokamak facilities, the priorities for the restructured fusion science program are: 1) basic science of high temperature magnetically confined plasma; 2) confinement concept improvement; 3) burning plasma physics; and 4) low-activation materials. This set of priorities is consistent with those recommended by the President's Committee of Advisors on Science and Technology (PCAST) [6] and those reflected in the Office of Fusion Energy's draft strategy document [7]. The major tokamak facilities must clearly be considered in light of this strategy:

Basic Plasma Science. Underlying progress towards fusion is the development of a basic understanding of the behavior of magnetically confined plasmas. Fusion program support for plasma science provides the primary two-way interface between the field of fusion energy and the scientific community. The highly collisionless plasmas created in the major tokamak facilities are a unique plasma environment and have served to drive important advances in areas such as magneto-hydrodynamic (MHD) stability, plasma turbulence and transport, wave-particle interactions, and atomic physics. In the restructured fusion program seeking to reinvigorate its support for basic plasma science, the major tokamak facilities will play an important role as national centers hosting experiments directed at advancing our basic scientific understanding.

Confinement Concept Optimization. This is the focus of "a strong domestic core program in plasma science and fusion technology, with funds to explore both advanced tokamak research and research on concepts alternative to the tokamak, leveraged where possible on related activities worldwide." (See PCAST report.) The OFE strategy calls for "research to enhance the performance and attractiveness of tokamaks, conducted primarily through reliance on existing facilities." [8] Thus, concept improvement, with a primary reliance on U.S. domestic resources in the near term, is a key goal for the major tokamak research.

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[6] "The U.S. Program of Fusion Energy Research and Development," Report of the Fusion Review Panel, President's Committee of Advisors on Science and Technology (PCAST), July, 1995, p. 2.

[7] "Strategy for a Restructured U.S. Fusion Energy Research Program," U.S. Department of Energy, Office of Fusion Energy, draft of 11/29/95

[8] The other "prong" of the OFE strategy, "expansion of the exploration of promising alternate plasma confinement approaches," is the subject of another appendix and is not addressed in this appendix except insofar as it relates to possible replacements for the three major tokamaks.

Burning-Plasma Physics. Both the PCAST and the OFE strategy recognize that an understanding of the physics of burning plasmas is a critical goal for fusion. The major U.S. tokamak facilities have an important role in contributing to a sound physics basis for burning plasma experiments. Essential contributions to the physics basis for ITER are needed from the U.S. tokamaks, and they represent commitments to critical ITER Engineering Design Activity (EDA) deliverables. For the exploration of long-pulse, strongly self-heated plasmas, the U.S. strategy is to use international collaboration.

D.2 Scientific Issues for Tokamak Concept Improvement

There has been enormous improvement in plasma confinement and stability over the last 20 years. This has been the direct result of our increased understanding of plasmas from our experimental and theoretical investigations. The developed theory and modeling indicates that further substantial improvements are possible, and are supported by recent encouraging experimental results. The "improvements" sought will reduce the construction and operating cost of tokamak-based fusion power plants:

- Make tokamaks steady state. To eliminate cost of energy storage systems needed to smooth output, and to reduce cost of extra structure needed to handle cyclic loads.
- Reduce the frequency and severity of disruptions. To reduce cost of service interruption and recovery.
- Make tokamak systems more compact. To reduce cost of the reactor core and associated hardware.

Table 2.1 summarizes the scientific issues and key capabilities associated with realizing these improvements. Some of these are discussed in greater detail in attachments on Transport Barrier Issues, on Power and Particle Exhaust, and on Profile Control and Auxiliary Heating Physics and Technology.

Table 2.1. Scientific Issues for Tokamak Improvement

Scientific Issues	Tokamak Improvement Benefit	Key Capabilities
High beta stability	High self-generated current (steady state) High fusion power density (compactness)	Plasma heating and current-drive systems, plasma shape control.
Transport control	Reduce plasma current and recirculating power requirements; compactness; control of self-generated currents	Plasma heating and current-drive systems: neutral beams and RF; shear flow and rotation control; particle reflux & boundary control.
Non-inductive current drive, including self-generated currents	Plasma sustainment (steady-state) and profile modification to allow operation near stability limits (reduced disruptivity)	Plasma heating current-drive systems.
Power and particle exhaust, boundary physics	Compatibility between the first wall/pump and a high-performance plasma (steady state, compactness, reduced disruptivity); increased fuel density	Fueling systems; wall-conditioning techniques; helium pumping techniques; high heat-flux targets.
Integration of improvements	Optimum performance and high availability	Control systems.

The keys to tokamak improvement are plasma control, and power and particle exhaust. The exciting advances in plasma performance that have been achieved in recent years have come about through control of the plasma shape, plasma profiles, plasma transport, and boundary conditions. Recent progress in the development of methods for exhausting the heat and particles out of tokamaks that are compatible both with clean, high-performance plasmas upstream and practical target structures downstream is extremely encouraging. In both of these areas, outstanding diagnostics, analytical tools, and theory have allowed us to understand the cause-and-effect relationships well enough to design improved techniques that can be implemented and tested on the available facilities at low cost.

To be successful, the improvements discussed must ultimately be achievable simultaneously in a self-heated (burning) plasma. The enabling control and diagnostic technologies must be developed to be efficient and lead to economical reactor solutions. Well-integrated tokamak design projects are needed to determine realistic engineering constraints and guide the scientific research toward practical solutions. Simply stated, the aim of tokamak concept improvement research is to develop plasma control and exhaust strategies to improve the performance and reliability of the tokamak, and to test them in a burning-plasma experiment.

Conclusion: *The aim of tokamak concept improvement research is to gain the predictive capability needed to develop plasma control and exhaust strategies that will improve the performance and reliability of the tokamak and, ultimately, to test them in a burning-plasma experiment*

D.3 Scientific Issues for Burning Plasma Physics

The motivation for studying the physics of burning plasmas is compelling: an economical fusion power reactor must be primarily self-heated. The power injected from external systems to control the plasma and to offset its energy losses cannot be more than a few per cent of the fusion power output. Self-heating involves the deposition of the energy from alpha particles produced in fusion reactions into the plasma. As PCAST noted (p. 21-22), “the alpha particles can influence the plasma behavior in ways that are difficult to predict,” and therefore the issues must be studied experimentally in order to gain the predictive capability needed to design fusion power plants. While some progress on these issues can be made in existing experiments, it is clear that a burning-plasma experiment will be needed to resolve them completely. Table 3.1 summarizes the scientific issues associated with attaining this predictive capability.

Conclusion: *The aim of burning-plasma physics research is to gain the predictive capability on the behavior of burning plasmas needed to design fusion power plants. While some progress on these issues can be made in existing experiments, a burning-plasma experiment will be needed to resolve them completely.*

Table 3.1. Scientific Issues for Burning Plasma Physics

Key Feature	Scientific Issues
Fusion Alpha-particles	• Identify and minimize the impact of alpha-driven instabilities on plasma performance. • Understand alpha-particle thermalization and heating of the core plasma. • Interaction of alpha-particles with RF waves (for heating and current-drive) • Effect on beta limits and particle transport.
DT Isotope Effects	• Effect on energy and particle transport and transition boundaries between different plasma confinement modes. • Compatibility of external heating and current drive techniques with DT fuel.
Helium transport and exhaust	• Scaling of helium ash transport out of the core and in the scrapeoff / divertor plasma.
Test of advanced plasma control and exhaust strategies in burning plasmas.	• Assess prospects for realizing tokamak concept improvement benefits in reactors. • Determine beta-limits consistent with long pulse lengths and self-heating. • Develop models for the scrape-off and divertor plasma performance to ensure compatibility between the first wall/pump and a burning plasma. • Develop models for plasma fueling.

The U.S. burning plasma physics strategy is to collaborate in an international burning-plasma experiment, for which ITER is the current vision. In order to design a burning-plasma experiment (whether ITER or another embodiment) and to support a decision on its construction, it is necessary to first establish a sufficient physics basis to permit an assessment of the facility's potential capability to accomplish its mission. That physics basis is currently incomplete. For that reason, the ITER EDA includes a physics research and development (R&D) program, involving all the parties, whose purpose is to complete the necessary physics basis for ITER.[9] The current scientific issues for this program are summarized in Table 3.2. This table also indicates the U.S. tokamaks expected to contribute toward each issue.

As part of the U.S. burning-plasma physics program, the major tokamaks contribute to the resolution of ITER Physics R&D issues. The U.S. has maintained a reputation for innovation and excellence in its contributions to the scientific basis for ITER since the ITER program's inception in 1988. This is because developing good scientific models, comparing to well diagnosed experiments, and then extrapolating to burning plasma conditions is a major strength of the US fusion program. Without these critical contributions, the ITER physics basis will be weakened, and it will likely become necessary for our international partners to re-plan their programs to provide the needed data. This is an important consideration in the event that one or more of the facilities must be closed.

Conclusion: *The major U.S. tokamaks make critical contributions to the physics basis for the International Thermonuclear Experimental Reactor (ITER), currently planned as the next-step burning-plasma physics experiment.*

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[9] For planning purposes, it should be noted that ITER physics R&D is "voluntary," meaning that it is funded out of the fusion programs but not the ITER budgets. All three U.S. tokamaks devote part of their program to addressing ITER R&D needs.

Table 3.2. Scientific Issues for Constructing a Burning Plasma Facility: Current ITER Physics Research and Development Needs

Scientific Issues	Information required from tokamak facilities and primary U.S. contributors	Benefit to Burning Plasma Facility Assessment Capability
Power and particle handling	• Demonstration of critical performance attributes, e.g., peak heat load reduction via radiation, He pumping. (DIII-D, C-Mod) • Database on scrape-off and divertor plasma conditions over a range of parameters and machines. (DIII-D, C-Mod)	• Reduce uncertainty in predicting divertor performance. • Reduce uncertainty in predicting first-wall erosion rates.
Disruptions	• Data on the characteristics of disruptions under conditions prototypical of ITER over a range of parameters and machines. (C-Mod, DIII-D, TFTR)	• Determine electromagnetic loads for design of in-vessel structures. • Reduce uncertainty in predicting first-wall erosion rates.
Confinement	• Database from demonstration discharges prototypical of ITER over a range of parameters and machines. (DIII-D, C-Mod) • Data on confinement in DT plasmas. (TFTR)	• Reduce uncertainty in confinement projections and confinement-mode transition boundaries. • Assess prospects for exceeding standard density limits.
Beta limits	• Data on the role of non-ideal magnetohydrodynamic effects. (DIII-D, TFTR, C-Mod)	• Reduce uncertainty in predictions of beta limit and fusion power output.
Alpha-particle effects	• Data on instabilities and losses from DT experiments or beam simulations. (TFTR, DIII-D)	• Reduce uncertainty in predicting the impact of alpha effects on performance.
Advanced plasma modes (with Improved-Tokamak characteristics)	• Data on operating modes with enhanced performance compared to ITER physics model. (DIII-D, C-Mod, TFTR)	• Assess benefits to mission of expanded operational flexibility. • Determine hardware upgrade requirements to provide plasma controls for advanced modes.

D.4 Machine Capabilities for Tokamak Improvement Research

Our tokamak-improvement research to date has given us a good understanding of the machine capabilities that are important for optimum performance and for making further progress toward the goal. These are described in Table 4.1. Further research is needed to fully define the requirements for control of the plasma shape and of the current, pressure, density, and rotation profiles. We know these will involve the use of plasma heating and current-drive techniques (neutral beams and radiofrequency waves) and fueling techniques, but we do not yet know the best combination and configuration of such systems. In addition, power and particle control techniques and flow-shear control techniques consistent with these advanced operating regimes must be evaluated. These are important areas for innovative research and development both in the plasma physics and in the enabling technologies.

Table 4.1. Tokamak Machine Capabilities Important for Tokamak Improvement Research.

Capability	Importance
Cross section shaping flexibility	–Maximizes b. –Minimizes major radius at fixed ignition margin. –Controls transport and stability.
Poloidal divertor	–Tests advanced-mode compatibility with reactor-relevant power and particle exhaust system. –Control of edge profiles. –Control access to high-performance regimes. –Control of particle sources (hydrogen isotopes, helium, impurities).
Reactor-like core or divertor plasma parameters	–Reactor relevant beta and collisionality (transport). –Reactor relevant non-ideal effects on magnetohydro-dynamics. –Reactor relevant atomic physics (power handling).
Deuterium-tritium plasma	–Reactor-relevant fuel (affects confinement and wave-heating physics). –Tests advanced-mode compatibility with a-particles, He exhaust, and a heating (if reactivity is high enough). –Tests of alpha-heating control (e.g., a-channeling).
Current profile control.	–Increases b-limit beyond standard scaling. –Compensate for misalignments in bootstrap current profile. –Control access to high-performance regimes.
Heat and fuel deposition control	–Control transport and pressure, density profiles –Optimize pressure profile for stability, bootstrap alignment, reactivity. –Simulate a-heating profile in experiments.
Rotational shear control	–Controls transport and stability via radial electric field.
Close-fitting conducting wall and rotation control	–Controls stability limits of high-b, high-bootstrap modes.
Diagnostics (spatially and temporally resolved measurements)	–Profile and fluctuation diagnostics for understanding of plasma-control effects. –Boundary and divertor diagnostics for understanding of exhaust mechanisms. –Alpha diagnostics for understanding a dynamics in advanced regimes. –Real-time signals for active feedback control. –Essential for developing predictive capability through theory and modeling.
Long pulse length	–Tests advanced-mode sustainment beyond current-relaxation time (if greater than t_{skin}). –Tests advanced-mode sustainment beyond plasma-wall equilibration time (if greater than 100's of seconds). –Tests plasma reliability against disruptions (if many hours).

The tokamak-improvement capabilities of the three major U.S. tokamak facilities are summarized in Table 4.2.

Table 4.2. Capabilities of Major U.S. Tokamaks for Tokamak Improvement Research (future capabilities in parentheses)

Capability	DIII-D	Alcator C-Mod	TFTR
Plasma shape*	Strong D-shape: k=0.9-2.6, d=-0.1–1.0	D-shape: k=0.9-1.85, d=0-0.8	Circular: k=0.8-1.2
Boundary	Single or double null poloidal divertor, inner-wall limiter	Single or double null poloidal divertor, inner-wall limiter	Inner wall limiter, inboard X-point.
Deuterium-tritium fuel			yes
Particle control	neutral beam injection, pellets, pumped divertor, pumping carbon wall	pellets, metal wall, (pumped divertor)	neutral beam injection, pellets, pumping carbon wall
Current profile control.	fast-wave, electron cyclotron, neutral beam, (mode-conversion)	fast-wave, mode-conversion, (lower hybrid)	fast-wave, mode-conversion, neutral beam, (lower hybrid)
Heating profile control	neutral beam, ion-cyclotron, electron-cyclotron, fast-wave, (mode-conversion)	ion-cyclotron, mode-conversion, fast-wave	neutral beam, ion-cyclotron, mode-conversion, fast-wave, (ion Bernstein waves)
Rotation-shear control	controlled L-H transition, co- neutral beam/non-axisym-metric field perturbation, RF-NBI interaction.	controlled L-H transition	co-, ctr-, or balanced neutral beam, RF-NBI interaction, (ion Bernstein waves)
Conducting wall stabilization	yes		yes
Diagnostic strengths	profile, fluctuations, and divertor	divertor and disruptions	profile, fluctuations, and alphas
Long pulse length	10 s @ B=2.1 T, 20 s @ B=1.7 T	1 s @ B=9 T 10 s @ B=4 T	2 s @ B=6 T 10 s @ B=4.4 T

*Shape parameters: elongation, k = (plasma height)/(plasma width); triangularity d : 0=elliptical, 1=dee.

While none of the three U.S. major tokamak facilities is fully prototypical of an advanced fusion reactor, together they provide a strong collection of capabilities for tokamak improvement studies as well as strong scientific teams. DIII-D's strong shaping and shape flexibility, pumped divertor, and high beta values give it the important advantage of a plasma configuration that most closely resembles that expected in an attractive tokamak reactor. TFTR's important advantage is its capability for deuterium-tritium operation and potential capability for significant alpha heating. Its high field and larger size result in its having a normalized gyroradius closer to that expected for tokamak fusion reactors. Alcator C-Mod is presently configured to study divertor physics, confinement, and disruptions. Its high magnetic field strength and compact size put it in a unique parameter regime and provide useful "scaling leverage" through its contributions to world databases. Profile control systems in preparation would enhance its tokamak-improvement capabilities in the future.

The capabilities of the major foreign tokamak facilities are presented in Table 4.3. The capabilities and program plans for these machines are considered in the assessment of the U.S. plans in Section 5. Clearly significant potential for tokamak improvement research exists outside the United States. Tokamak data analysis increasingly relies on multi-machine databases which are used to test predictive models and determine scalings. Observations of performance advances in one facility require confirmation in others to build confidence in their reliability, as is usual in science. The existence of multiple tokamaks worldwide with overlapping but distinct capabilities and programmatic foci is therefore important for making steady progress toward scientific goals.

Table 4.3. Capabilities of Major Foreign Tokamaks for Tokamak Improvement Research. (future capabilities in parentheses)

Capability	JT-60U	JET	Asdex/U	Tore Supra
Plasma shape	D-shaped	Elongated	Elongated	Circular
Boundary	Single null poloidal divertor	Single null poloidal divertor	Single null poloidal divertor; metal coating	Inner wall and local limiters; ergodic magnetic limiter
Pumping and walls	pumping carbon wall, (pumped divertor)	pumped divertor, pumping carbon and beryllium wall	pumped divertor, pumping carbon wall	pumped limiter, pumping carbon wall
Deuterium-tritium fuel		yes		
Current profile control.	lower hybrid, fast-wave, neutral-beam	fast-wave, lower hybrid, neutral beam, mode-conversion	neutral beam, fast-wave, mode-conversion, (electron-cyclotron)	fast-wave, lower hybrid, mode-conversion, neutral beam, (electron-cyclotron)
Rotation and rotation-shear control	co- and perp-NBI	co- NBI	co-NBI	
Long pulse length	20 s	20 s	5 s	60-(1,000) s

Conclusion: *The major U.S. tokamaks provide a strong collection of capabilities for tokamak improvement studies as well as strong scientific teams. The U.S. tokamaks have led this area of research and contributed greatly to its progress.*

D.5 Program Plans

The vision of tokamak improvement presented here is the same one that set the goals for the formerly planned Tokamak Physics Experiment (TPX). Stimulated by this vision, the plans for the major U.S. tokamaks have been formulated in recent years to include a strong emphasis on tokamak improvement (often denoted “advanced-tokamak, ” or “AT”) issues.

DIII-D. The DIII-D program focuses on advanced-tokamak operating modes, divertor physics, and core transport physics in a shaped, poloidally-diverted configuration. An upgrade program including on- and off-axis current-drive systems and a flexible divertor structure is in the process of being implemented to extend their capabilities for tokamak-improvement research. The strong coupling among plasma stability, transport, and power and particle exhaust is highly nonlinear in a strongly shaped plasma, and DIII-D is unique in its ability to evaluate this complex relationship. Integration of an advanced high-beta core plasma with a pumped radiative divertor for 5-20 s pulses is a key goal. Also, DIII-D has been a strong contributor to the physics basis for ITER, especially in areas of divertor physics, disruption physics, confinement scaling and data bases, beta limits, and helium transport. A number of foreign tokamaks have capabilities similar to DIII-D’s: shaping (JET, JT-60U), current-drive (JET, JT-60U, Tore Supra), and divertors (JET, Asdex-U, JT-60U). However, DIII-D is unique because of its shaping flexibility and pumped double-null divertor with slot-length variability, in combination with advanced-tokamak profile controls and diagnostics.

Alcator C-Mod. Designed with high magnetic field capability, Alcator C-Mod provides physics data and understanding on numerous critical issues, including enhanced core confinement, dissipative divertor studies, and detailed disruption investigations. The high field, compact approach results in a flexible research facility which accesses unique regions of dimensional parameter space. Because of its closed, all metal vertical-plate divertor geometry, combined with unique reactor level parallel power flows and excellent divertor diagnostics, Alcator C-Mod explores the dissipative divertor physics which will be important for any reactor, including the present ITER design. Long pulse capability (at somewhat reduced field), combined with planned current drive upgrades, will allow investigation of advanced tokamak plasma regimes for up to 10 skin times. The Alcator program is integrated into the academic environment at MIT, with more graduate students than Ph.D. scientists on the team, and as such, it is cost effective and well suited for training members of the next generation of fusion plasma scientists.

Tokamak Fusion Test Reactor (TFTR). In FY-1995, a proposal for a three-year extension of TFTR was put forward. Its aim is to utilize improved performance regimes and profile control to increase fusion power output and extend studies of alpha-particle dynamics and their influence on the main plasma. The program extension includes upgrades to the radiofrequency heating and current drive systems to develop and test profile-control tools in a deuterium-tritium plasma. Significant alpha heating is predicted, including the possibility of thermal runaway in the center of the discharge. The TFTR program is investigating techniques for controlling the core plasma transport in reacting plasmas, and for demonstrating “alpha channeling,” a novel concept for profile control and reactivity enhancement. Important areas where TFTR contributes to the ITER physics data base include isotope effects on transport and radiofrequency heating techniques, helium transport, MHD stability limits, and operation in a deuterium-tritium, high-neutron-flux environment. In the world program, only the Joint European Tokamak (JET) also has the capability to perform deuterium-tritium experiments. However, TFTR is conducting a more extensive deuterium-tritium physics campaign through the end of the ITER EDA, and has unique diagnostics for measuring the confined alpha population and alpha particle losses to the boundary. It is uniquely pursuing the study of alpha heating control techniques. In addition, TFTR has more extensive diagnostics and pressure-profile control capabilities for studying improved-performance regimes.

In Table 5.1, we summarize the key contributions expected from the three machines in the next few years.

Conclusion: *All three major tokamaks have program plans which are consistent with the goals of the restructured U.S. fusion science program. Each is well positioned to make further scientific advances.*

Table 5.1. Expected Near-Term Contributions to U.S. Program Goals from the Three Major Tokamaks

Strategic element:	**Tokamak Improvement**	**Burning-Plasma Facility (ITER) Physics Basis Support**
DIII-D Contributions:	• Develop self-consistent high-performance advanced-tokamak (AT) scenarios suitable for steady state - high beta - high confinement - high bootstrap fraction - current drive - power and particle control • Demonstrate long pulse AT (10 -20 sec) scenarios. • Non-inductive current drive development/applications (ECCD, FWCD, MCCD, bootstrap). • Evaluate dependence of stability on shape, current profile, pressure profile, rotation profile, stabilizing effect of a resistive wall. • Develop understanding of enhanced confinement in AT scenarios, effects of sheared rotation, current profile, and shape. • Demonstrate density control with divertor pumping and pellets. Demonstrate real-time feedback control of the profiles.	• Confinement scaling with effective size (a/r) in reactor relevant plasma. • Develop physics understanding and scaling of L-H and H-L transitions, with detailed edge diagnostics. • Validate disruptions models with detailed diagnostic measurements; demonstrate avoidance and mitigation. • Understand beta limit in shaped discharges with fully-penetrated profiles. • Develop physics understanding and scaling of ELMs. • Develop and validate divertor models with detailed edge diagnostics. • Understanding of different divertor geometries, single and double null, variable slot. • Demonstrate density beyond Greenwald limit in high-performance ELMing H-mode. • Helium transport and exhaust with core source. • Demonstration of advanced tokamak modes compatible with ITER design • r* scaling in strongly shaped plasma to demonstrate compact ignition scenario at high beta. • NBI driven Alfvén eigenmodes in shaped discharges, dimensionally similar to ITER.

continued...

Table 5.1. Expected Near-Term Contributions to U.S. Program Goals from the Three Major Tokamaks, continued

Strategic element:	**Tokamak Improvement**	**Burning-Plasma Facility (ITER) Physics Basis Support**
Alcator C-Mod Contributions:	• Demonstrate/understand radiative and detached divertor operation consistent with enhanced confinement modes (H-Mode, PEP) - closed vertical plate pumped divertor - impurity screening, impurity transport • High-Z first wall with low-Z coatings - impurity radiation, transport - wall conditioning physics for metal walls • ELM physics - type III, grassy • Enhanced confinement physics - high density, field, current density - effects of shape • ICRF physics - mode conversion - fast wave at high density - fast wave current drive • Understand disruption dynamics (halo currents) • Evolution of AT profiles (FY 2000) - ~10 skin time capability	• Investigation of dissipative pumped divertor - ITER/reactor dimensional parameters (density, temperature, power flow) - reactor-like dimensionless parameters (collisionality) • Disruption dynamics - halo currents (detailed diagnostics) - shaping, high current density - amelioration (killer pellets) • Metal first wall operation - divertor dynamics, atomic physics - plasma wall interactions (erosion, sputtering, redeposition) • Study of techniques for low Z coating of metal walls (ECDC boronization, low Z pellets) • Investigation of B-field compatible wall conditioning (ECDC) • Density limit physics (H and L Mode) - scaling with current density - compare gas with steady state pellet fueling • Core and edge confinement - size, field and density effects - barrier thresholds - ELM physics • r* scaling (with DIII-D and JET)

continued...

Table 5.1. Expected Near-Term Contributions to U.S. Program Goals from the Three Major Tokamaks, continued

Strategic element:	**Tokamak Improvement**	**Burning-Plasma Facility (ITER) Physics Basis Support**
TFTR Contributions:	• Develop/evaluate reactor relevant pressure profile controls: - sheared rotation control (IBW) - alpha heating profile modification - alpha channeling • Documentation of transport in advanced-tokamak (AT) configurations - role of q-profile - role of electric fields/rotation - particle transport • Evaluate alpha-Alfvénic and MHD stability with AT profiles - low central shear with q(0) > 1 - strongly reversed central shear • Initial effect of self-heating on pressure profile control • Evaluate current drive techniques for AT scenarios - MCCD, LHCD - bootstrap with strong flow shear • Active fueling of AT configurations (pellets and NBI) • Low-Z first wall coatings	• Beta limit dependence on non-ideal effects • Disruption studies: - runaway electron production - killer pellets - integrated modeling • Tritium isotopic effects on confinement and ELMs • Systematic validation of predictive transport models • Helium transport with core sources. • Effect of sawteeth and MHD activity on alpha profiles and loss • Effect of alphas on MHD and Alfvénic stability • Alpha heating • ICRF heating and current-drive in DT plasmas - fundamental deuterium / second-harmonic tritium - minority tritium current drive - off-axis MCCD in DT plasmas - high-density effects - antenna optimization - advanced high power-density launchers • Density limits with enhanced confinement.
Goal:	Provide the physics basis for a attractive-tokamak control and exhaust strategy for a burning-plasma experiment. (long-term)	In collaboration with the partners, provide a sound physics basis for ITER construction.

In FY-1996, all three tokamaks are operating on severely limited schedules with reduced staff and little upgrade activity because of severe budget reductions. This is highly undesirable as a long-term solution. As a matter of good practice, any operating experiment should be supported with healthy funding to operate cost-effectively. It must have the resources (operating time, hardware upgrades, and scientific staff in appropriate balance) to be fully productive.

Conclusion: *All three major tokamak facilities are currently operating in a low-productivity manner because they are under-funded.*

Recommendation: *As a matter of good practice, any operating experiment should be supported with healthy funding to operate cost-effectively. It should have the resources (operating time, hardware upgrades, and scientific staff in appropriate balance) to be fully productive.*

D.6 Near-Term Program

Restructuring the fusion program within budget levels that are greatly reduced from previous years may make it necessary, in the near term, to retire one of the major tokamak facilities. The program of deuterium-tritium plasma studies currently under way in the Tokamak Fusion Test Reactor (TFTR) can be completed in the relatively near-term, whereas the programs on DIII-D and Alcator C-Mod extend for a longer term. It is appropriate, then, that TFTR should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from this facility.

Recommendation: *The Tokamak Fusion Test Reactor (TFTR) should be the first of the three tokamaks to be retired, after a period of operation to extract the remaining scientific benefit from it, since its program of deuterium-tritium plasma studies can be completed in the near-term relative to the other major facilities.*

The combination of constrained budgets with rapid shifts in program directions may necessitate a premature termination (i.e., within a period significantly less than two years) of the TFTR program. If this is required, some or all (depending on the time remaining) of the following research objectives would not be completed:

- Plasma self-heating with core alpha-particle heating locally comparable to external heating.
- Control of plasma transport and stability using ion-Bernstein-wave generation of sheared flow.
- Control of alpha-particle thermalization and heating profiles using externally driven waves.
- Radiofrequency heating and current drive in deuterium-tritium plasmas (e.g., for ITER).

It is unclear when these lost scientific opportunities would return. For this reason, we believe that sufficient resources should be provided to operate TFTR at high productivity throughout FY-1997, while also operating DIII-D and Alcator C-Mod at high productivity. In any event, the DIII-D and Alcator C-Mod programs should be supported for full productivity after TFTR is retired during FY-1998. If this is done, the progress that can be expected, based on current planning, is indicated by the contributions listed in Table 5.1. Moreover, it is anticipated that the scientific output of these facilities will broaden in a restructured program that is less tightly focused on energy development. Research results will lead in new directions and we can expect many scientific contributions in addition to those listed in the table. To plan effectively, we believe that there must be a national review (with international participation) of the program for the three facilities. This review should be used to identify the highest-priority objectives to be accomplished on TFTR prior to its termination, and to prioritize and coordinate the efforts on all three facilities in the context of the restructured national program.

Recommendation: *A national review (with international participation) of the program for the major facilities should be conducted to identify the highest-priority objectives to be accomplished on TFTR prior to its termination, and to effectively prioritize and coordinate the efforts on all three facilities in the context of the restructured national program.*

D.7 Implications for Future Facilities

For the next several years, the U.S. fusion program will be limited to two major tokamak facilities (after the completion of the TFTR program). After about 2001, it is likely that DIII-D will have completed its useful life and a new facility will be needed to maintain a strong program. Based on present knowledge, one could envision possible goals for successor facilities to extend tokamak-improvement research, for example:

- Integrating attractive core and divertor operating scenarios for long pulse lengths.
- Further developing the physics basis for extreme compactness.
- Integrating advanced-tokamak scenarios in a deuterium-tritium environment.

Indeed, a number of interesting configurations have been suggested. We expect, however, that our visions will evolve over the next few years based on new developments, not only from the major

U.S. and foreign tokamaks, but also from smaller experiments and new ideas stimulated by the re-orientation of the U.S. program. For this reason, we believe that the range of options for future major facilities should be expanded greatly. Innovative physics and engineering concepts, including non-tokamak designs, should be encouraged and considered. Assessments of the key physics and engineering characteristics of a wide range of options for future major facilities should be undertaken.

Recommendation: *Assessments of the key physics and engineering characteristics of a wide range of options for future major facilities should be undertaken.*

A burning-plasma experiment is a logical follow-on to the experimental program being carried out in the present generation of tokamaks. This can best be accomplished through international collaboration. The goals for such an experiment include not only the demonstration of controlled ignition and extended burn of a DT plasma, but also driven, high-beta, high bootstrap-current-fraction steady-state burn. This latter goal represents a continuation of the tokamak improvement program, and would be a logical niche for U.S. participation, even as a minor partner. At present, the principal international collaboration aimed at a burning plasma experiment is ITER. We believe that the goals of ITER are compatible with the aims of the U.S. fusion science program. To the extent possible, we should structure our program in such a way as to maintain the opportunity to be a credible participant in the scientific program of ITER.

Recommendation: *To the extent possible, the United States fusion science program should be structured in such a way as to maintain the opportunity to be a credible participant in the scientific program of an international burning plasma experiment.*

D.8 Attachment 1 to Major Tokamak Facilities Appendix
Transport Barrier Issues

Basic Goals

The goals of a research program aimed at developing an attractive steady-state tokamak clearly include pressure profile control and current profile control. These are non-orthogonal in the sense that pressure profile control may be achieved through control of the current profile, control of radial electric field shear via toroidal and poloidal rotation control, and control of particle sources and sinks. The tokamak performance benefits that are expected include: good energy confinement, high beta (for high power density), minimal current-drive power requirements, low (vanishing) disruptivity, and, most likely, high density (relative to present empirical limits), and compact size. It is understood that ultimately all of these benefits must be achieved self-consistently and simultaneously. This is the real challenge.

As pressure profile control requires understanding the space-time response function of the pressure profile, transport barrier dynamics is the key scientific concern at present. A "transport barrier" exists when the cross-field transport is reduced to near-neoclassical levels over some portion of the plasma. The dynamics of interest include:

a) threshold conditions for barrier formation, determined by local gradients in profiles, electric field, magnetic field structure, and shaping.
b) barrier propagation speed and profile steepening rate.
c) barrier location, determined by heat and fuel deposition profiles, momentum deposition profile, magnetic field structure, and shaping.
d) barrier limits, and mechanisms for their relaxation and termination [of which edge-localized modes (ELMs), x-events, and disruptions are examples].

Most improved-performance operating modes that have been observed in tokamaks exhibit transport barriers. These include:

a) "ELM-y H-modes," which have an edge barrier, obtained especially with peaked current profiles. Also external regulation techniques (e.g., shaping control, flow-shear control, etc.) are evolving.
b) "Enhanced reversed-shear (ERS)" or "negative central shear (NCS)," which have internal barriers.
c) "CH-mode" (and others) which have internal barriers facilitated by momentum deposition control with ion Bernstein waves (in PBX-M) or neutral beam injection (in JT-60U).
d) "VH-mode," with an edge barrier extending inward.
e) "Pellet-enhanced performance (PEP) mode," with an internal barrier driven by fuel deposition.

Our interest in barrier dynamics is driven by the recognition that a "steady state" tokamak will not in fact be static but will necessarily undergo control fluctuations, i.e., barriers will have to be lowered from time to time to facilitate particle removal or avoid stability limits. It is therefore critical to understand how to escape from barrier regimes to maintain high-performance operation. Specific scientific issues include:

1) H-mode to L-mode power threshold requirements.
2) Reversed-shear (either ERS or NCS) mode to L-mode transition hysteresis. Here, the nature of the back-transition is important for particle control (does the reversed-shear profile peel back like an onion, allowing artificial ELMs, or just collapse?)

One can expect that the various high-performance operating modes, each exploiting an optimized type of barrier mode, will evolve with time.

Research Leverage on Transport Barriers

A high-performance steady-state tokamak will need control of both pressure and current profiles to optimize transport within stability limits. As we have discussed, transport barriers will need to be controlled for regulation purposes. Means of barrier control include:

1) Magnetic field structure [q(r)] control via current-profile control.
2) Magnetic geometry control via active plasma shape control.
3) Electron and ion heat deposition profile control via heating-system configuration.
4) Particle deposition and removal control via pellets, neutral beams, wall conditioning, and divertor recycling control.
5) Momentum shear control via radiofrequency (e.g., ion Bernstein waves) torque shear and deposition control.
6) Boundary control via divertor, electrostatic biasing, pumping, and controlled L-to-H mode transition.

The flexibility to vary plasma parameter regimes also represents a means of control. Specifically, in addition to the well known critical dimensionless parameters such as beta, collisionality, and reduced ion gyro-radius, several others need to be identified, including:

1) The local parameters (density gradient?) underpinning the empirical (Greenwald) limit on density. The well-known result that shallow pellet injection triggers a slow relaxation suggests that disruption is not a fundamental problem. A key question is whether or not the Greenwald limit constrains the performance of reversed-shear modes.
2) The local parameters that determine barrier transition hysteresis, and relaxation below transition thresholds.
3) The local parameters that define regimes of tolerable relaxation compatible with heat and particle exhaust capacity. In H-mode, we prefer “grassy” ELMs to “giant” ELMs. In particular, the threshold of access to a tolerable ELMy H-mode regime is a central issue for edge barriers. Disruption avoidance and particle control demand that an analogous regime be identified for core barriers. This once again illustrates the importance of understanding barrier dynamics.
4) The parameters (local and global) determining barrier width and depth. This is quite fertile territory.

It is clear from this discussion that the capacity for flexibility is essential to a science program. The ability to vary and compare different shapes, current profiles, profiles, etc. is needed to understand the cause-and-effect relationships.

The ability to compare different transport barrier regimes under various conditions is needed to assess their relative performance, e.g.,

1.) ELM-y H-mode edge barriers versus reversed-shear core barriers. Specific points of comparison include thresholds, hysteresis factors, stability and disruptivity, and particle control.
2.) VH-mode internal barriers (controlled via edge fueling) versus reversed-shear mode.
3.) The optimal transport barrier mode for high density, compact regimes.

D.9 Attachment 2 to Major Tokamak Facilities Appendix
Power and Particle Exhaust

In a commercial fusion reactor based on the burning of D-T fuel in a magnetically confined plasma, the plasma facing structures will need to handle substantial heat and particle loads, and exhaust thermalized helium ash. In a tokamak reactor, the configuration most likely to satisfy all of these requirements will be some form of poloidal divertor.

In a tokamak, the scrape off layer, consisting of the plasma on open flux surfaces outside the separatrix, diverts the power and particle exhaust away from the reacting central plasma to remote material surfaces, the divertor plates. This plasma also serves as a buffer zone between the core and first wall, to isolate the cold surfaces from the hot plasma and to screen impurities from re-entering the core plasma. The key engineering challenges which must be met for successful operation of the divertor, and therefore the reactor, are:

1) Power dissipation. Channel and dissipate the core power exhaust over surrounding surfaces in such a way that those surfaces are long-lasting, which requires minimization of surface heat loading and erosion.
2) Impurity control. Impurity levels in the reacting plasma must be kept very low; deleterious effects of impurities include volumetric power loss due to radiation (line and continuum) and dilution of the reacting fuel. Estimates show that if high-Z contamination (tungsten, for example) were to exceed 10^{-5} of the electron density, line radiation would prevent ignition. In ITER, with an effective charge (Z_{eff}) of ~1.5 due to low-Z impurities and helium, calculations show that 30% of the input power to the plasma (including alpha power) would be radiated by free-free bremsstrahlung. Aside from the helium ash produced by the fusion reactions, the main natural sources of impurities are the plasma facing surfaces, primarily in the divertor itself. Moreover, it may also be necessary to introduce impurities artificially into the scrape-off plasma, in order to satisfy the power dissipation requirement. In all cases, the ability of the divertor plasma to prevent these impurities from reaching the core reacting plasma will be crucial.
3) Helium exhaust. Excessive helium buildup in the core plasma would destroy its reactivity because of fuel dilution. Once the alpha particles have thermalized, and assuming they are transported to the edge of the plasma across the separatrix, it is the goal of the scrape-off and divertor plasmas to keep the helium from returning to the core (goal 2) and to maximize the helium concentration in the divertor, thus maximizing the capability of pumps connected to the divertor to exhaust helium from the system.

The areas of physics research associated with the above goals are:

a) perpendicular and parallel transport (hydrogenic, He and impurity species, energy and momentum);
b) atomic and molecular physics (charge-exchange, ionization, radiation, momentum balance, shielding);
c) magnetic boundary control (x-point, separatrix-wall intersection point and gaps); and
d) plasma-material interactions (sputtering, recycling, particle ballistics).

The end result of successful divertor physics and engineering studies will be the ability to make credible predictions of ITER/reactor performance by:

a) Extrapolation/interpolation to reactor conditions through a combination of experiments, numerical modeling (benchmarked to experiments), and scaling arguments. This requires both achievement of parameters as close as possible to those of ITER, as well as data over a wide range of parameters (magnetic field, physical dimensions, density, wall material, divertor geometry) to test theory and modeling.

b) Demonstration of the mutual compatibility of the goals (1-3), simultaneously with good core plasma performance. The conditions required for good divertor operation are somewhat in conflict with those required for enhanced core confinement. The current strategy to dissipate power in the divertor requires a cool dense divertor plasma, perhaps with sufficient impurity content to increase radiation. Efficient He pumping also requires a high divertor density. On the other hand, the transition to H-mode seems to require a hot plasma edge and relatively low density plasmas. A reactor would require very low impurity levels to minimize fuel dilution and radiation. Wall conditioning is another area where further development is needed. Up until now, most, if not all enhanced confinement modes are in some way dependent on, or improved by changes in edge conditions. Examples include H-mode (diverted), H-factors and VH-mode (boronization), and super-shots/high T_i (lithium wall conditioning). Enhanced core confinement and self-consistent alignment of pressure and current profiles for large bootstrap fraction will require control of density profiles, and thus recycling and particle pumping can be expected to play important roles.

What is the status of these studies to date? There are many encouraging results for goals 1-3. However there has been no demonstration of full compatibility even at relatively low power flow levels, much less at reactor-like conditions.

A number of foreign tokamaks (JET, JT-60U, ASDEX-U) are actively studying divertors. In addition, both Tore-Supra and TEXTOR are studying pumped limiters and ergodic magnetic limiters as alternate plasma boundary concepts. Most divertor designs use carbon plasma facing components, though JET has used beryllium and ASDEX-U is installing tungsten-coated carbon. JET, JT-60U, and ASDEX-U are modifying their divertors to designs that approximate the ITER design, to address ITER R&D issues.

In the U.S. program, high power dissipation has been demonstrated on DIII-D and Alcator C-Mod. In C-Mod, this has been done in the ITER divertor geometry at the expected divertor densities and within a factor of two of the expected parallel power flow levels. C-Mod is the only divertor experiment in the world using all-metal plasma facing components, which will most likely be required in a reactor to bring erosion within acceptable limits. DIII-D has demonstrated successful pumping of dissipative divertor operation and is the only tokamak in the world that will operate a pumped double null divertor with variable slot length.

D.10 Attachment 3 to Major Tokamak Facilities Appendix
Profile Control and Auxiliary Heating Physics and Technology

The need for auxiliary heating of magnetically confined plasmas in order to achieve thermonuclear temperatures has been recognized and studied experimentally for a long time. In addition to providing the means to reach ignition, auxiliary heating has also been found to enable operation in enhanced confinement regimes, such as H-modes or Supershots, and the formation of internal transport barriers. Methods devised for delivering power to plasmas include neutral beam injection (NBI) as well as various schemes based on interaction of externally launched electromagnetic waves with the plasma. Neutral beam injection differs significantly from wave heating in that it is also a fueling source for the plasma. Its utility as a central heating source in tokamak reactors is limited because beam penetration to the plasma core decreases as the density and temperature of the plasma increase. Research and development of neutral beams based on negative-ion accelerators, which feature higher injection energy and therefore greater penetrating power, is underway in Japan and Europe and may lead to a beam injection system suitable for use in reactors. Because of the difficulties with existing NBI technology, most reactor designs have featured some combination of electromagnetic wave heating. Outstanding issues regarding wave heating that remain to be addressed include the performance of these methods in deuterium-tritium plasmas and compatibility of hardware design with reactor environments.

Noninductive current drive methods have also been studied for a long time because a combination of self-generated bootstrap current coupled with external sources of noninductive current is required for steady state tokamak operation. In the high beta, high fusion reactivity plasmas envisioned in advanced tokamak configurations, the total plasma current will be dominated by the bootstrap current while the pressure profile will be dominated by the alpha heating profile. However, since the bootstrap current profile is determined by both the pressure gradients and the poloidal field profiles, and since the overall MHD stability of the plasma is determined by the net current and pressure profiles, while the fusion reactivity is mainly determined by the pressure profiles, operation in this regime is inherently nonlinear. One must create and maintain a plasma with self-consistent pressure and bootstrap current profiles which simultaneously provide maximum fusion reactivity, MHD stability, and steady-state operation. The auxiliary input power used for control must be minimized in the interest of overall plant economics. Because the bulk of the plasma current is provided by the bootstrap current in these regimes, external current drive will be used in initiating operation and to fine-tune the current profile locally.

Methods for core pressure profile control are critical, since the pressure profile influences the fusion reactivity, the plasma stability and the bootstrap current. Methods that have been demonstrated include boundary control via plasma shaping, divertors, or wall conditioning; particle fueling; current profile control; and flow shear generation using ion Bernstein waves. Proposed methods for core pressure control are limited at this time to schemes for controlling the core transport, such as internal transport barrier formation with ion Bernstein wave heating or controlling the alpha heating profile via alpha channeling. Considerable further research is necessary to establish the utility of these proposed schemes for future reactors. In the case of alpha channeling, operation in a deuterium-tritium plasma is required to either confirm or disprove the utility of the scheme.

Appendix E

Plasma Confinement Research (Alternative Concepts)

The study of plasma science, and the many scientific issues which underlie fusion research, is greatly aided by experiments in which a range of confinement configurations is employed. A key feature of a laboratory experiment is that the experimenter has some control over the plasma conditions, permitting a controlled study of plasma phenomena. However, the range of variation of key parameters in any given plasma configuration is limited. Study of fusion science issues in different confinement configurations greatly expands the range of physical conditions which are accessible; such an approach leads to enhanced understanding and innovation which would not be attainable through the investigation of one confinement concept only. For example, from one concept to another variations occur in magnetic curvature, magnetic shear, fluctuation spectra and amplitudes, electric fields, plasma current, plasma flow, plasma pressure, and many other properties. Moreover, the quest to optimize the properties of a magnetic configuration, with regard to fusion reactor attractiveness, leads scientists to investigate a range of configurations. For example, alternative concepts research aims for smaller size, higher plasma pressure, less plasma current, less plasma disruptions, and lower magnetic field.

Since fusion research worldwide is focused on the tokamak, confinement concepts sufficiently different from the tokamak (or advanced tokamak) have come to be called "alternative concepts." In addition, within the alternative concepts, different magnetic configurations, often differing by only small changes in magnetic field structure, are known by different names. This categorization understates the strong scientific connections among the different concepts, and their cross-fertilization. Inertial confinement fusion is a major, separately funded program (within Defense Programs in DOE), with comparably strong international activity. It is discussed in the next appendix. The decision to concentrate resources on tokamaks, beginning around 1970, was made because confinement times were superior to those achieved in other magnetic configurations at that time. Of course the gap widened in time. In 1990, in response to budget pressure, the alternative concepts program was essentially terminated in favor of schedule-driven development of the tokamak reactor concept. A significant element of the restructured program will be a reinitiation of an alternative concepts research program.

The status of most alternative concepts is such that pressing scientific issues can be addressed in experiments of modest scale. Thus, this is one area in which the United States can participate at the forefront worldwide. The very strong alternative concepts programs in Japan and Europe are focused on the stellarator. In other concept areas the international effort, although much stronger than that in the United States at present, is distributed over medium scale and small experiments. There is a spectrum of alternative concepts which are arguably ready for modest-scale experimentation and theoretical study, exploiting advances in theory/modeling and experimental

techniques which has occurred in recent years. FEAC has not attempted to evaluate the scientific value or reactor significance of different alternates. However, for illustration we describe below the status and opportunities of five magnetic confinement concepts.

Conclusions: *Alternative concepts offers opportunities to advance fusion science in ways not possible with one concept only, to pursue configurations with possibly attractive reactor features, to excel in selected areas with modest expenditure (even in the context of a constrained overall fusion budget), and to encourage new ideas -- all goals aligned with the restructured fusion program.*

Recommendation: *The concept improvement program should be expanded to include a spectrum of alternative concepts, including experimental and theoretical research. Several concepts may be ready for experiments to elucidate key physics issues. The precise funding level cannot be prescribed here. It must be driven by peer-reviewed proposals (from national labs, universities, and industry), as for any scientific program. As for the program's major facilities, any experiments which are operated should be supported with healthy funding to operate cost-effectively.*

E.1 Field Reversed Configurations

Description and Strengths. A Field Reversed Configurations (FRC) is an elongated compact toroid (torus with unity aspect ratio, i.e., no central hole) with negligible toroidal field. It is thus extremely high b (minimum possible value of 50%) and is thought to rely upon a combination of kinetic effects and velocity shear for its observed stability and robustness. FRCs may have extremely desirable attributes, being nearly linear in external geometry while providing internal toroidal confinement. Only a simple, low field, solenoidal external magnet (essentially the vertical field coil of a tokamak) is required. A natural divertor with linear exhaust out of the ends allows for simple heat removal and possible direct conversion. The extremely high b provides for a compact design and the possibility of burning advanced, aneutronic fuels.

Status. The confinement and stability physics of FRCs are necessarily quite different from that of tokamaks since there are large, nearly field-free regions and no magnetic shear. The observed stability of small scale experiments had been ascribed to kinetic effects represented by the parameter s (the effective number of ion gyro-orbits between the field null and the separatrix), but recent results in the Large s Experiment (LSX), demonstrating robust stability at s values as high 4, have led to a rethinking about the stabilization mechanism. Minimum energy states (akin to those in Spheromaks and RFPs, but involving the total canonical angular momentum rather than magnetic helicity alone) have been calculated. These involve velocity shear as a stabilizing mechanism, which is also being studied in tokamak devices. Since compact toroids have no ohmic

transfer, even quasi-steady-state operation must be provided by RF or neutral beam current drive. Current drive involving bulk electron acceleration by Rotating Magnetic Fields (RMF) has been demonstrated in small scale, cold FRC experiments (rotomaks). The physics involved in such a method is extremely interesting, involving basic questions of electron orbits and collisionality parameters (affecting slippage off the rotating flux), and would illuminate fundamental processes in plasma turbulence. RMF current drive is very efficient, and could be beneficial to tokamaks as well as FRCs. It can be seen that study of the above features not only provides breakthrough possibilities in reactor engineering, but their study will also lead to new understandings of fundamental fusion plasma physics and technology.

LSX was only operated for one year due to the decision to terminate all alternative concept research. However, in that time impressive plasma parameters of over 1 keV ion and 0.5 keV electron temperatures were reached, and stable operation at s values over 4 were demonstrated. Confinement parameters of nt (product of plasma density and energy confinement time) over 10^{12} $s\text{-}m^{-3}$ also have been achieved. Relatively low voltage formation techniques were developed, to the point where flux build-up to large sizes through neutral beam or RMF application can be conceived as a reactor relevant technique.

Scientific Challenges/Opportunities. A principal outstanding question in FRC development is that of maintaining stability as the size increases to that required for reactor level confinement, s ~ 20-30. Recent theoretical calculations indicate that velocity shear (which will not diminish with increasing size) may be a more important contributor to stability than kinetic orbits, but this has not yet been verified experimentally. Energy transport remains an additional key area for future study. The old LSX facility was transferred to the University of Washington and modified to conduct translation experiments related to tokamak fueling. This modified facility is ideal for continuing FRC investigation at modest cost, particularly to studying the effects of external profile controls and steady state current drive in a simple chamber separated from the high voltage formation section. If the RMF current drive technique can be successfully applied to pre-existing hot FRCs, then high s values can also be achieved in the same device. This would provide an extremely cheap path toward reactor development, which is probably a necessity in today's limited budget environment.

E.2 Reversed Field Pinch

Description and Strengths. The reversed field pinch (RFP) is similar to the tokamak, except that the toroidal magnetic field is about ten times smaller than that of a tokamak (for similar plasma current). The reduced field yields a reactor concept possibly characterized by high power density, high plasma beta, low forces on magnet coils, non-superconducting coils, absence of disruptions, and free choice of aspect ratio (chosen by engineering, not physics, constraints). The benefit of these features has been demonstrated in several reactor studies.

The RFP is particularly useful for the study of fundamental fusion physics issues such as the dynamo effect (related to the astrophysical dynamo problem), magnetic fluctuation induced transport, nonlinear mode coupling, resistive wall effects on MHD modes, helicity injection, current profile effects on fluctuations, electrostatic fluctuations in bad curvature and high shear systems, and mode locking to field errors. The RFP, FRC, spheromak, ST (and the advanced tokamak) are all aimed at more compact reactors with weaker magnetic field. The RFP and spheromak attain the low field by operating at safety factor $q < 1$. If the confinement can be made favorable in this regime, as discussed below, then a new domain of parameter space is made available for toroidal reactors.

Status. Significant RFP experiments are in operation in four laboratories worldwide: MST at the University of Wisconsin, RFX in Italy, two devices in Japan, and T2 in Sweden. MST and RFX are the largest devices, and of similar size (minor radius $a \approx 0.5$ m). RFX has higher current capability (2 MA versus about 0.5 MA for MST). The four RFP laboratories are well-coordinated, with complementary work underway in each lab.

RFP experiments operate at high values of beta (~ 10%); however, significant advances are necessary in energy confinement. Recently, substantial progress has been made in confinement understanding and improvement. In MST, transport specifically driven by magnetic fluctuations has been measured. Toward the plasma interior magnetic fluctuations drive transport, as anticipated theoretically. Detailed comparison of magnetic fluctuation properties with nonlinear MHD computation indicates good agreement. Hence, reasonable understanding of transport in the RFP may be emerging.

This understanding suggests that fluctuations and transport can be reduced through control of the current density profile. This has opened up a new route to confinement improvement. Initial attempts with inductive current profile control in MST have succeeded in tripling the energy confinement time, from about 1.3 ms to about 4 ms (and modestly reducing fluctuations). This result encourages the more sophisticated methods described below. The fluctuations are part of the well-known dynamo effect, or self-generation of magnetic field. It has been confirmed in experiment that the MHD dynamo (similar to that of astrophysics) accounts for the edge current of collisionless RFP plasmas. Interestingly, in the edge of collisional RFP plasmas, a new dynamo mechanism driven by pressure fluctuations has been discovered.

Particle confinement in the extreme edge has been measured to arise from electrostatic fluctuations, as in a tokamak. However, the cause of energy confinement in the extreme edge remains unknown.

Theory has played an important role in RFP understanding. Nonlinear MHD theory and computation yields a comprehensive understanding of many features of the magnetic fluctuations, the dynamo, and the mean fields. In addition, MHD has been used to examine the effect of a resistive wall on stability, helicity injection current drive, the effect of current drive on fluctuations, the effect of finite pressure on fluctuations, and the scaling of fluctuations with Lundquist number. Description of the RFP and plasma relaxation by the minimum energy state approach has been very useful for RFPs and other plasma venues.

Transport by magnetic fluctuations has been studied by analytic turbulence theory (MHD codes cannot treat energy transport in the collisionless regime), which has provided insight into the effects of ambipolar constraints on the transport of current and energy. Studies of RF current drive (lower hybrid and fast wave) for the bulk plasma and for current profile control, has been initiated with early results indicating, for example, that lower hybrid waves are suitable for profile control for transport suppression.

Scientific Challenges/Opportunities. Four key areas are confinement improvement, bulk current drive, resistive wall stability, and power exhaust. Confinement improvement can proceed along two paths. First, empirical scaling trends might imply that confinement will improve with current. The goal of RFX is to test this conjecture to 2 MA. The MST experiment will study confinement improvement by current profile control. Three techniques are planned: inductive current profile control, electrostatic current injection (helicity injection), and lower hybrid current drive. These experiments require a strong theoretical complement in MHD computation, fundamental turbulent transport analysis, and RF current drive calculations.

For MHD stability the RFP must be surrounded by a conducting shell. The shell requirements have not been determined experimentally, nor have means for alleviation such as plasma rotation. The T2 experiment in Sweden plans to address this issue.

Steady state current drive has not yet been studied extensively for the RFP. The RF problem is akin to that of the ST, in that the toroidal magnetic field is low. However, the RFP does not benefit from bootstrap current. This is an important area for further theoretical research and new ideas. Two techniques which have been suggested from theory, fast wave current drive and oscillating field current drive (in which oscillating loop voltages produces a net current), require experimental study.

Confinement, resistive wall stability, and current drive can be advanced aggressively in experiments of modest scale. A program in RFP theory should include work in these areas.

E.3 Spherical Tokamak

Description and Strengths. Spherical tokamaks have low aspect ratio, in the range A ~ 1.1 - 1.5. By definition they are compact magnetic confinement devices. The steep 1/R variation of the toroidal field allows high plasma current operation at low-medium safety factor with small toroidal field. Other characteristics include natural elongation, good vertical stability to position displacements, good MHD stability at high beta, and natural plasma edge exhaust. All the usual advanced tokamak operation modes are also available in STs.

The above-mentioned features of STs makes them an attractive candidate for either a fusion reactor or a volume neutron source.

From a scientific point of view, important physics issues can be resolved in STs because in them the cylindrical q value is very different from the flux surface q (for instance, one can distinguish current scaling from q scaling for confinement or Troyon coefficients). Neoclassical theory and much of MHD may need reassessment because the large A approximation is no longer valid.

Status. Ideal MHD properties of STs have been extensively explored by theory. Neoclassical effects in the extreme low A limit have shown that a bootstrap current may exist even in the collisional limit. Preliminary studies of RF current drive need some refinement for the low A regime. Gyrofluid simulations of transport arising from ion-temperature-gradient-driven turbulence are underway. Many numerical tools developed for standard tokamaks are applicable to STs (equilibrium, time dependent equilibrium and transport, MHD, kinetic solvers, etc.).

There are presently three very small experiments in the United States (CDX-U at Princeton, HIT at the University of Washington, and Medusa at the University of Wisconsin) investigating spherical tokamaks, and one somewhat large ST in England (START, which has delivered particularly interesting results). Experimental results from these low current STs indicate further benefits of low aspect ratio: absence of disruptions, expanded scrape-off layer widths, large shaping, good confinement -- all of this observed in a hot plasma with a reasonable pulse length. Internal reconnection events (described as sawteeth or minor disruptions by some) reduce the discharge performance, but do not destroy the plasma. They are not understood at present. Helicity injection can produce low current plasmas. Reactor studies based on extrapolations of the present performance of START indicate that an ST reactor may have the following properties: low plasma thermal and particle diffusivities, order unity beta, order unity fraction of well-aligned self-driven current, absence of plasma disruptions, a highly dispersed plasma exhaust channel, small currents in toroidal and poloidal coils.

Scientific Challenges/Opportunities. How do the characteristics of STs scale with aspect ratio? In particular, how does confinement (core and edge) scale? Is low disruptivity a characteristic of low

current plasmas or of STs? Can we produce a non-inductively driven tokamak plasma? Will current drive requirements force excessively high betas and disruptions?

A new device with higher plasma current and auxiliary heating would allow exploration of the potential advantages of STs in more fusion relevant regimes. A reasonable next step would be a device with about 1 MA of plasma current. Smaller devices can contribute to specific issues, as well as innovative ST research.

Non-inductive current drive is essential to STs, since there is little space for a solenoid. Innovative current drive techniques may need to be developed (helicity injection, fast wave, for instance). The small surface-to-volume ratio of the ST, characteristic of small, compact reactors, may create very high heat loads on plasma facing components. These difficulties are shared with advanced tokamaks.

Specific to STs are the design of the TF and PF systems. To maintain low aspect ratio, the TF central leg has to be small, hence resistive (it would have to be replaced often in the presence of neutron damage). Detailed shape control may be difficult, since poloidal field coils are not allowed inboard of the plasma, and internal current redistribution in the plasma can greatly affect the plasma boundary.

E.4 Spheromak

Description and Strengths. Spheromaks are compact toroidal configurations including both toroidal and poloidal magnetic fields, with the fields maintained by plasma currents rather than external coils linking the torus. The spheromak potentially has many attractive reactor features. It is compact, with high energy density. It is the simplest device which has a toroidal field, while the lack of a hard-core conductor potentially makes a reactor maintainable and capable of a low cost-of-electricity. Electrostatic helicity injection can allow steady-state operation, and other current drive options may be possible. Low edge toroidal field can facilitate divertor design as well as reduce coil costs. Such compact toroids allow the plasma to be transported, which has both reactor and plasma fueling applications. The spheromak can be viewed as an RFP in the limit of unity aspect ratio. There may be similarities in the transport physics since both concepts have safety factor less than one. It differs from the FRC only in that it contains a substantial toroidal magnetic field.

Status. Spheromak experiments achieved core electron temperatures of 400 eV, comparable to the results of the T-3 tokamak in 1969 which led to the rapid increase in the world tokamak program. The ion temperature in spheromaks was higher, the density 10 times higher, but the global confinement time much less. These results came as the result of understanding the importance of several physics issues, including the role of a conducting wall, of field errors and edge physics effects, and of MHD stability.

Spheromak research has made many other physics contributions to fusion research. It was recognized for many years that the magnetic geometry is predicted by the Taylor-Wells theory of energy minimization at constant helicity. Now recognized are the many similarities in "relaxation" physics between spheromaks and reversed field pinches and the importance of helicity generation, conservation, and dynamo effects. And accelerated spheromaks or compact toroids have been used successfully to inject particle density in tokamak experiments.

Scientific Challenges/Opportunities. The spheromak confronts many of the issues faced by the RFP, particularly confinement and current drive, as well as beta limits (optimization of magnetic shear to maximize the Mercier limit). There are a number of key physics topics which can be studied in a spheromak experimental program at relatively low cost. One path would start with a short-pulsed, hot spheromak to study energy confinement in a sustained plasma. This experiment would also be used to study shaping of the plasma to maximize beta and to provide initial data on the transition from a plasma equilibrium supported by a conducting shell to one supported by external coils. Success in this experiment would lead to a long-pulse experiment to demonstrate multi-keV equilibria supported by external coils, control of low-n instabilities, long pulse helicity injection, and other steady-state issues. Another avenue of potential gain is to develop current drive techniques for profile control to suppress fluctuations and transport, similar to the research plan for the RFP.

E.5 Stellarators

Description and Strengths. The stellarator confining magnetic field is produced entirely by currents flowing in external conductors. It does not require a plasma current. Consequently, stellarators can be intrinsically steady-state reactors and avoid the problems associated with current-driven disruptions, current drive, high bootstrap fraction and positional stability. The lack of current leads to low circulating power. Other reactor advantages include a natural helical divertor and a lower power density on the divertor plates than for a tokamak. To date, no density limits have been observed. Finally, control of the magnetic geometry and rotational transform by means of external coils allows for the possibility of enhanced confinement that is more robust than in a tokamak. Magnetic properties, such as shear, well depth, and localization of particles to the good curvature region can be "hard-wired" into the magnetics.

Status. Stellarator design has advanced such that experiments are now designed from the inside out: the boundary shape is calculated based on a set of prescribed physics properties and a set of helical or modular coils is then determined which produces the desired boundary. This pioneering computational methodology, known as the HELIAS (Helical Advanced Stellarator) approach, was developed in Garching, Germany.

The worldwide stellarator program contains many stellarator configurations. Two large steady-state stellarators with superconducting coils are under construction at a cost in the range of $0.5 - 1.0 B each: LHD, a torsatron with helical coils in Japan; and Wendelstein 7X, an advanced stellarator with modular coils in Germany. An advanced stellarator, W7-AS, operating in Germany, was designed to have a lower Pfirsch-Schluter current than a tokamak or conventional stellarator. A heliac is a class of stellarators with a large magnetic well, with circular coils arranged toroidally about a central conductor so that the center of each coil lies along a helix. A three-field period heliac is operating in Australia (H-1) and a four field period heliac (TJ-II) will begin operation in Spain at the end of 1996. Two low aspect ratio torsatrons are in operation: CHS in Japan and CAT at Auburn University in the United States. HSX, under construction at the University of Wisconsin, is a quasi-helically symmetric stellarator with magnetic properties similar to that of a tokamak (but achieved without plasma current). Conventional stellarators with helical windings are in operation in Japan, Russia, and the Ukraine.

A major liability of the stellarator had been the poor confinement of trapped particles at low collisionality. Experimentally, it has been observed that large neoclassical transport in this regime can exceed anomalous losses. The HELIAS approach to optimizing magnetic configurations was developed in part to overcome these limitations. Neoclassical transport in W7X, for example, is expected to be greatly reduced. The mission of the HSX device is to verify the improved neoclassical transport properties. A difference in the magnetic properties between W7X and HSX is the relative size of the bootstrap current. W7X is designed to minimize the bootstrap current so that finite beta effects will have minimal impact on the magnetics. Confinement scaling in stellarators is similar to that in tokamaks, yet to date significant progress has yet to be made in finding improved confinement regimes. H-modes have been observed, however the energy confinement improvement is small.

Scientific challenges/Opportunities. The optimal trade-off between neoclassical transport, simplicity of coil design and bootstrap current remains to be determined. In addition, study of anomalous transport (including trapped particle effects) is needed to discover the optimal combination of shear and curvature. The role of electric fields, and concomitant flow, is particularly important to confinement in the stellarator.

Another issue is whether there exist magnetic configurations which offer a possible route to higher beta than presently envisioned for a stellarator reactor (about 5%). Theoretical work is needed to understand the ideal MHD stability of stellarators and the breakup of magnetic surfaces from finite pressure effects. Experimental beta values of 2% have been achieved. Exploration of higher beta regimes await TJ-II and LHD.

The U.S. role in the world stellarator program should encompass three research elements (as follows, but not listed in priority order). First, a stellarator theory program, formerly an area of excellence in the United States, could play an important role in the world program. Second, innovative experiments of modest scale can influence the world program. The flexibility of the magnetic configuration of the stellarator offers opportunities to explore variations in coil structure, magnetic field spectrum, aspect ratio, beta stability limits, and improved confinement properties. For example, the geometry of HSX is unique, such that the planned experiments cannot be performed in the large stellarators in Europe and Japan. Third, the United States will benefit significantly from participation in the substantial physics experiments abroad.

Appendix F

Inertial Fusion Energy

In Inertial Confinement Fusion (ICF), the fusion energy is released by imploding a small pellet of deuterium and tritium using energetic lasers or particle beams as drivers. The physics of pellet implosion up to the point of ignition and burn is complex due to the interplay of hydrodynamic motion, electron and radiation transport, equations of state under extreme conditions, and the appearance of instabilities. Basically, the energy from the driver explodes the outer layer of the target, causing an implosion which pushes the fuel towards the center of the pellet and compresses the fuel to densities of several hundreds of g/cm^3. Fusion is initiated in the central region of the pellet (called the hot spot) which then drives a burn front through the remaining target fuel. The hot spot is crucial for obtaining high gain (high ratio of fusion energy released to driver energy), because only a small fraction of the total fuel has to be driven to fusion conditions in the hot spot to initiate fusion burn of the remaining fuel in the pellet.

During the target implosion, high-density material is accelerated by a layer of lower density material. Their interface is subject to the Rayleigh-Taylor instability. Ignition can only be achieved if the distortion of this interface layer is small. Therefore, high-gain targets require a high degree of spherical symmetry in the implosion (< 1%-2%). The direct-drive approach in which the pellet is heated directly by the driver requires this degree of symmetry to be achieved in the target illumination. Significant progress has been made in this area and is expected in the next few years from the Omega facility. Another solution is the indirect-drive approach which converts the incident beam energy into soft x-rays in a hohlraum which then symmetrically drive the implosion of the pellet. The drawback of this scheme is the inefficiency of the conversion process. The pellets also cost more, which may be a factor if the pellet fabrication cost becomes a major cost-driver item for IFE.

Inertial confinement fusion is now the largest fusion science program in the United States and is primarily supported by Defense Programs in DOE. The principal near-term purpose of the ICF program is stockpile stewardship, to provide the scientific base for nuclear security applications. The DOE, through Defense Programs, has approved the National Ignition Facility (NIF), a billion-dollar ICF facility that is designed to demonstrate ignition in ICF pellets by about 2005. Study of the high spot and burn in NIF will also settle the main scientific issues of high-gain targets and establish the driver requirements.

Four different drivers have been identified and studied for ICF purposes: glass-based lasers, KrF lasers, light-ion accelerators, and heavy-ion accelerators. The development of the first three drivers was funded by DOE Defense Programs, and the glass-based lasers were chosen for the NIF. Heavy-ion accelerators have been investigated for Inertial Fusion Energy (IFE) power applications, because for these energy applications the drivers must be reliable and efficient with a high pulse repetition rate (several Hertz) and long life. In this regard, heavy-ion accelerators have been deemed as the most promising driver option for IFE. It should be noted that significant progress has been made during the past few years both in KrF lasers (impressive smoothing and high bandwidth) and in glass lasers with diode pumping.

It should be noted that the primary approach to heavy-ion ICF and glass-laser-based NIF is the indirect-drive approach. At the same radiation temperature, x-ray hohlraum wall losses, radiation-driven hohlraum wall motion, and radiation transport for laser-driven hohlraums are directly applicable to heavy-ion ICF. These are the primary issues that affect coupling efficiency and hohlraum symmetry for the heavy-ion ICF hohlraums. Because of these similarities, Defense

Programs ICF work provides a solid basis for calculating the most critical elements of the heavy-ion target. Therefore, the national IFE program is highly leveraged on the large national ICF effort. Internationally, energy applications of ICF are pursued in Europe and Japan and international collaboration in this area has increased substantially after the recent declassification of most of the ICF program in the United States.

In its early years, heavy-ion-based IFE received funding from high-energy physics in the Office of Energy Research and from ICF in the Defense Programs. Beginning in FY84, heavy-ion IFE work was concentrated in the Heavy Ion Fusion Accelerator Research (HIFAR) Program in Basic Energy Sciences. In 1990, the Fusion Policy Advisory Committee (FPAC) to the Secretary of Energy stated "The promise of an inertial fusion energy program (IFE) seems to us to be sufficient to begin investment now in a small collateral program covering those areas not required for the Defense Programs, e.g., repetition-rated, efficient drivers and reactor studies." As a result of FPAC recommendations, the HIFAR Program was moved to the Office of Fusion Energy and became the primary element of the new IFE program. Noting the target physics will be mainly supplied by the Defense Programs ICF, the IFE program at OFE aims at addressing the following scientific issues: 1) Development of a suitable heavy-ion driver; and 2) Driver-independent issues for IFE such as chamber wall protection and design, affordable and mass-produced targets, power plant and integration studies to identify key critical issues, etc. The FPAC also recommended a large increase in the funding for the national fusion program that did not materialize. As a result, the funding for IFE research (at OFE) has remained relatively flat since 1990 and the work is mainly focused on the heavy-ion accelerator development.

Many reviews of the IFE program during the past few years have indicated that the accelerator development program is ready to proceed to the next step -- the ILSE project, which would provide an integrated demonstration of induction linac technology and the beam physics required to provide the data base for scaling to a heavy-ion driver for an inertial fusion power plant. In 1993, the Fusion Energy Advisory Committee Panel 7 on Inertial Fusion Energy reviewed the status of the IFE program in the Office of Fusion Energy and recommended a "balanced program that includes an experimental and analytical program for supporting IFE technologies as well as accelerator development and a beam physics program." This panel recommended a "reference" annual budget of $17M for IFE with $14M identified for ILSE and accelerator research, and $3M for supporting technology and system studies. The panel also found that for $10M per year ($8M for accelerator research and $2M for supporting technology) it is not possible to complete the integrated ILSE project, although a significant set of large accelerator experiments could be completed to increase understanding of key technical issues.

As a whole, the recommendation and conclusion of the FEAC Panel remains valid today. The IFE program annual budget of $8M is focused on accelerator development and related beam theory. At this level of funding, the heavy-ion IFE program cannot proceed to its logical net step (i.e., the ILSE project). This budget level also reflects the fact that there is limited scientific synergy between accelerator development and magnetic fusion. However, IFE and MFE share a large number of scientific issues: MFE plasma science and IFE driver-independent plasma science issues (funded in Defense Programs), and fusion support technologies of both IFE and MFE.

The SciCom did not assess the IFE effort in detail, but acknowledge its potential as a fusion energy source and the major role of DOE Defense Programs in addressing key scientific and plasma physics issues. A programmatic review should be conducted involving all cognizant DOE program offices, and appropriate scientific and technical experts to recommend the priority and management of IFE, in the context of the mission, policy, and scientific goals of the restructured fusion energy sciences program.

Appendix G

International Thermonuclear Experimental Reactor (ITER)

G.1 Need for a Burning Plasma Physics Experiment

It is not possible to demonstrate the full physics basis that will be required for fusion power reactors in the present generation of large tokamaks. The critical issues that cannot be resolved in these facilities relate to the formation and control of burning deuterium-tritium (DT) plasmas. In this context a key parameter is the energy multiplication factor,

$Q \int$ (total fusion power) / (net auxiliary heating power).

Four fifths of the fusion power from DT reactions escapes from the plasma as 14 MeV neutrons, while 1/5 of the power is returned to the plasma in the form of 3.5 MeV alpha particles. Hence, Q must be greater than 5 for the self-heating of a burning DT plasma to dominate the auxiliary heating. An economic fusion power reactor must operate in the strongly self-heated regime with $Q >> 5$. Issues relating to the control of the plasma operating point and plasma profiles may be qualitatively different in this strongly self-heated regime. It might be possible to reach Q of about 2 in the largest U.S. facility (TFTR); in an international context similar efforts in JET (the only other tokamak in the world planning substantial DT experiments) might lead to qualitatively similar results. Since neither of the present generation of DT experiments is likely to reach the self-heated regime, a necessary follow-on is a burning plasma experiment -- that is, the demonstration of controlled ignition and extended burn of a DT plasma.

Goals for a burning plasma physics experiment include both the demonstration of long-pulse ignition -- that is, the maintenance of a burning DT plasma without the application of significant auxiliary heating power -- and the demonstration of driven operation at high plasma pressure (as measured by $b \int 2\mu_0 p/B^2$), allowing steady-state (as opposed to pulsed) tokamak operation in which the plasma current is supported through a combination of non-inductive current drive (which requires the application of auxiliary power) and self-generated currents (that is, the "bootstrap-current," which is a well-known consequence of neoclassical transport theory). The ignition goal reflects the view that tokamak optimization is best achieved through simplifying the reactor concept by removing all unnecessary elements (like the auxiliary heating and current drive systems). In contrast, the goal of achieving a high-b, steady-state driven burn builds on the concept innovation program being pursued at our large tokamak facilities, and reflects the view that the intelligent application of modest amounts of auxiliary power will allow greater control over the plasma, leading to greatly increased fusion performance relative to what might be achieved in self-ignited operation. The new physics issues that must be addressed to achieve these two goals mainly relate to fusion alpha physics, dynamic control of the operating point, control of plasma profiles (through non-inductive current drive and the formation and control of transport barriers), particle and heat removal, and disruption avoidance and/or mitigation.

G.2 The Role of ITER in the U.S. Program

Given the high projected cost of a burning plasma physics experiment, and the fact that the US presently funds only about 1/6 of the world effort in magnetic fusion, we concur with the existing program strategy:

Burning plasma physics will be pursued through international collaboration.
At present, the principal international collaboration aimed at the construction of a burning plasma experiment is the ITER EDA -- a collaboration involving the United States, the European Community, Japan, and the Russian Federation.

The ITER mission is "to demonstrate the scientific and technological feasibility of fusion power. The ITER will accomplish this by demonstrating controlled ignition and extended burn of a deuterium and tritium plasma with steady state as an ultimate objective, by demonstrating technologies essential to a reactor in an integrated system, and by performing integrated testing of the high-heat-flux and nuclear components required to utilize fusion power for practical purposes."[10] This mission addresses both the physics and technology issues essential to an engineering test reactor. An engineering test reactor, of which ITER is an embodiment, has been in all the Parties' fusion development plans. It is the penultimate step in the development of a particular fusion energy concept. If successful, ITER would be followed by a demonstration reactor. This final step would demonstrate the reliable production of electrical power and provide a basis for utilities to evaluate the financial viability of fusion power plants based on the tokamak concept. Accomplishing this commonly required step internationally reduces the costs to the U.S. and the other Parties, while drawing upon the competence of all the Parties enables the highest quality team possible to implement the project. If ITER is successful, it will reduce the time required and total development cost of fusion power at the price of increased risk and greater near-term cost.

ITER is both a great physics challenge -- if ITER is built, it will test burning plasmas at the reactor scale and provide a test bed for most of tokamak physics, and much generic physics -- and a great technological challenge, requiring the development of high-field superconducting magnets, high-heat-flux components, plasma heating and fueling systems, and other reactor-relevant fusion technologies. U.S. industry has been given a major role in designing and building prototype components of the ITER during the EDA, while having access to all design and development activities of the other parties. This role helps to assure that American industry will be able to compete for construction elements, if ITER is built and the United States participates. In addition to the potential benefits of sharing in the ITER construction cost, this important international collaboration focuses the world fusion program on a concrete objective.

G.3 U.S. Participation in the ITER EDA

The U.S. program extracts substantial benefits from the ITER EDA. At present, ITER is the primary vehicle for plasma technology development in the U.S. program (as discussed in the Materials and Technology Appendix). The Physics R&D plan developed by the ITER project (in consultation with the home teams of the ITER parties) provides one of the foci for our large tokamak experiments. Important physics issues must still be resolved in the areas of divertors, disruptions, density limits, transport scaling, L-to-H and H-to-L mode power thresholds, current and pressure profile control, and b-limits. These issues are generic to tokamak fusion reactors and, so, must be addressed ultimately with or without the ITER project if we are to develop fusion reactors based on the tokamak concept. However, The ITER EDA has added urgency to the international effort to address these issues, motivating a substantial increase in the international effort in some areas (e.g., divertor physics, and the characterization of disruptions), while providing a mechanism for very substantial increases in the international dissemination of relevant data from all ITER parties in other areas (e.g., transport scaling and H mode power thresholds).

G.4 Review of ITER EDA

The results of the ITER physics R&D, together with the ITER physics design requirements will form the physics basis for ITER. This *ITER Physics Basis* will be assessed by all parties before there is a decision to construct ITER. In particular, the U.S. program will review the ITER EDA

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[10] *Establishment of ITER: Relevant Documents,* (IAEA, Vienna, 1988).

output (including both physics basis and engineering design) prior to a decision to seek participation in ITER construction. Given the pivotal nature of a decision on ITER construction to the U.S. program, we recommend that a mechanism be established immediately to expand involvement of the U.S. fusion community in the assessment of the evolving ITER physics and technology basis to insure that the ITER design reflects our current best understanding of tokamak physics, and to insure that the U.S. community appreciates the issues that have driven ITER design decisions. Given the time pressure that FEAC is acting under in preparing this report, we suggest four avenues for achieving expanded community involvement in the assessment of the ITER physics basis and design, while recognizing that further refinement of these suggestions may be necessary:

1) The U.S. Home Team should make an effort to increase contact with members of the U.S. fusion community through presentations and informal conversations at meetings, making themselves available to present talks on ITER at universities and national laboratories, and making an effort to attend presentations by others that relate to ITER. Similarly, members of the U.S. fusion community should seek to open and maintain communication with members of the U.S. Home Team and colleagues on the ITER JCT.

2) The ITER Joint Central Team (JCT) has developed a framework for interacting with the worldwide tokamak science community via the overview ITER Physics Committee and seven ITER Physics Expert Groups in the areas of:

1) Confinement and Transport Physics,
2) Confinement Database and Modeling,
3) Disruptions, MHD, and Plasma Control,
4) Energetic Particles, Heating and Current Drive,
5) Diagnostics,
6) Divertor Physics, and
7) Divertor Database and Modeling.

These expert groups provide a formal channel through which the U.S. program can raise concerns which have developed regarding the ITER physics basis. We can make better use of them by insuring that there is adequate funding provided to U.S. members of these expert groups to allow them to spend substantial amounts of time preparing for meetings of the group and disseminating the information obtained at these meetings. In particular, U.S. members of these expert groups should take responsibility for seeking out areas of legitimate concern with the ITER physics basis within the U.S. fusion community, communicating this concern to the ITER project, and facilitating a dialogue between concerned members of the U.S. community, experts from other ITER parties, and members of the ITER JCT.

3) The U.S. ITER Home Team already has an oversight committee, the ITER Steering Committee U.S. (or ISCUS). The ISCUS should be encouraged to take an active role in defining key issues that must be resolved prior to a U.S. decision on participation in ITER construction, evaluating plans to address these issues in a timely manner, and working with their home institutions to see that appropriate efforts are made to resolve technical issues in a timely manner.

4) The ITER detail design will be available by the end of calendar year 1996. The U.S. program should consider launching an assessment of the ITER detail design modeled after the European Domestic Assessment of the ITER Interim design -- that is, it should be performed by a working group composed of about 50 experts drawn from the U.S. fusion program. While it should include members of the U.S. ITER Home Team (who, being familiar with the ITER design and physics basis, can assist other members of the working group in understanding the ITER design choices), the majority of the members should not be associated with the U.S. ITER Home

Team, and should represent all elements of the U.S. program (universities, national laboratories, and industry). Subgroups should be formed to address major issues, and the assessment should extend over a period of several months (involving multiple meetings and/or teleconferences between members of the subgroups) to allow time for a thorough evaluation of the technical issues before the full working group prepares a final report.

G.5 Strategy Regarding ITER Through the Construction Decision

The recent budget changes in the U.S. fusion program indicate that the United States is very unlikely to participate as a full partner in ITER. Nevertheless,

- **we recommend that the U.S. program continue participation (as allowed by the budget) in the EDA phase of ITER to which the United States is committed through FY98,**

thereby fulfilling our existing commitments to our international partners, and leaving open the possibility of some U.S. participation in ITER construction or other major international collaborations which provide a cost-effective means of advancing fusion science.

In the remainder of the EDA, it is important for the U.S. program to increase its emphasis on advanced tokamak scenarios to insure that the ITER facility provides a suitable vehicle to pursue the study of tokamak concept innovation -- the current focus of our tokamak experimental program -- in a burning D-T plasma. This effort would include increased attention to operational flexibility, the definition of the ITER heating and current drive systems, diagnostics, and control systems. These are areas in which the U.S. program has expertise, and ones in which the United States might have maximum impact at minimum cost during ITER construction and operation.

Regardless of the outcome of the post-EDA phase, the United States will have benefited from our involvement in the ITER EDA, because ITER has acted as a driver for technology development, engineering design innovation, and for involving the world in an enhanced collaborative attack on the primary physics and operational issues of tokamaks. It has also forced the fusion community to face squarely the engineering challenges of designing a steady state, high power, DT burning plasma device.

In 1998 the EDA agreement will be concluded and the parties will be faced with a decision on ITER construction. Since ITER construction is expected to cost in excess of $6 B, it is clear that the U.S. program cannot participate as an equal partner at present budget levels. However, the European, Japanese, and Russian programs have indicated that they may welcome U.S. participation as a limited financial partner. If ITER is built, then ITER will set the world standard for reactor-relevant fusion plasmas. If the United States is not able to participate in ITER operation, then our experimental fusion program will lag behind this world standard. Some may object that ITER will not demonstrate the "scientific and technological foundations for an economically and environmentally attractive fusion energy source." However, it is our present best understanding that:

1) The advanced steady-state operating modes that we presently envision as the route to tokamak concept improvement can be demonstrated in ITER.

2) Our ITER partners are prepared to modify the ITER design to accommodate such advanced operating modes as the relevant physics database becomes available.

3) If we are allowed to participate in ITER as a limited financial partner for about $50M per year, then the ITER device will be the most cost-effective means for the U.S. program to obtain the physics database on advanced operation of burning DT plasmas that would be required before the construction of a demonstration reactor based on such an operating mode. (If construction takes 10 years, our construction cost is only about $500 M -- far less than any credible alternative that the United States might build on its own.)

Hence, if our ITER partners agree to pick up the bulk of the cost of ITER construction, and the U.S. program agrees that the ITER design complements our goals, then we should

∑ **seek to participate in ITER construction as a limited financial partner**

to provide a continuing focus for our tokamak physics and technology program, so that U.S. scientists will have access to the ITER facility after it is completed, and so that the United States can benefit (to an extent commensurate with our contributions) from lessons learned in ITER construction and operation. Possible U.S. contributions to ITER construction include:

Diagnostics and control systems. At the end of the ITER EDA much work will remain to be done in the design of diagnostics and plasma controls, which are crucial to extracting the maximum amount of information regarding burning plasma physics and advanced operating modes during ITER operations. The U.S. Home Team presently has lead responsibility for design of the ITER diagnostics systems. We might seek to build on the existing capability of the U.S. fusion community in the area of plasma diagnostics by retaining leadership for diagnostic design and seeking a leading role in fabricating the ITER diagnostics and control systems. This will insure a strong role for the U.S. experimental physics community in ITER operations; it will build on the existing leadership of U.S. industry in the area of computers and control systems; and it will position the United States to champion the concept of remote experimental sites for ITER operation. The presence of one (or more) ITER control rooms within the United States would allow substantial involvement of the U.S. experimental community in ITER operations while minimizing the costs and personal hardships associated with relocation of U.S. physics personnel to the ITER site.

Final ITER Design work. Continued involvement of the U.S. physics community in the final ITER design work (e.g., developing disruption control systems, modeling divertor operation, etc.) and in the preparations for ITER operations (e.g., developing shot plans for ohmic, ignited, and advanced tokamak operational scenarios consistent with the design limits of ITER systems and expected plasma performance) would also help to insure a strong role for the United States in ITER operations.

Magnet instrumentation. An area in which the U.S. program has unique capabilities is the instrumentation, protection control, and data acquisition for the magnet systems. The United States has a niche leadership in this area, and this activity would give global access to fabrication details and components, like strand and jacket material.

Divertor Cassettes. The cost for building the prototype, the 60 production divertor cassettes, and the spare parts for nine additional cassettes is about $200M in current dollars. This would be an affordable key system for the United States to manufacture and supply to the project. It is an area where the United States has the lead in the EDA and has the most experience in actively-cooled in-vessel components.

Fueling Systems. The cost for manufacturing the gas and pellet injection fueling system for ITER is approximately $40M. This is another area where the United States has the lead in the EDA R&D program and, in fact, the United States is the only party developing the high velocity tritium

pellet fueling systems required for ITER. This would be a natural system for the United States to supply.

A possible outcome of negotiations over US participation in ITER construction is that the U.S. fusion community will have little control over how the U.S. contribution to ITER construction is spent. Even in the absence of a direct scientific and technical role for the U.S. fusion community in ITER construction, we should still consider participation in ITER construction in order to gain access to the ITER device after it is constructed: so that U.S. experimentalists can participate in the planning, execution, and analysis of experiments in a long-pulse, ignited plasma; so that U.S. industry can benefit (to some extent) from the experience in fusion-relevant technologies obtained in the construction and operation of ITER; and to further the development of the "scientific and technological foundations for an economically and environmentally attractive fusion energy source."

G.6 What if ITER Isn't Built?

We foresee three basic avenues for continued international collaboration on fusion if the ITER partners decide not to construct ITER but remain interested in further collaboration with the U.S. program. These are:

1) An international collaboration to design and construct a less ambitious (and costly) device aimed at achieving ignition. A design study of a long-pulse (~100 seconds) ignition tokamak based on liquid nitrogen cooled copper magnets was recently completed in response to suggestions made in the report of the PCAST committee. This device had a major radius of 5 meters, an aspect ratio R/a = 3.3, a plasma current of 15 MA, and a projected cost of between 44% and 60% of the ITER device. The BPX/CIT project developed the design of a short-pulse ignition device (about 7 seconds), and found a cost of about 1/4 to 1/5 that of the ITER device. The IGNITOR project, taking a more aggressive approach to the engineering limits, concludes that short-pulse ignition can be achieved in a high-field liquid-nitrogen cooled tokamak at a cost less than 1/10th that of ITER. While a short-pulse ignition experiment based on any of the designs mentioned above may prove technically feasible, it is important to note that our ITER partners have either shown no interest in such suggestions, or have been actively hostile to suggestions that the present ITER design be abandoned in favor of such a device. Hence, persuading one, or more, of our ITER partners to join us in this project would be a major undertaking.

2) An international collaboration to design a very-long pulse (1000 seconds to "steady-state") driven tokamak. This might be something like the TPX tokamak (total project cost was estimated at $700 M); or it might follow on the more ambitious Japanese JT60-super upgrade design which envisioned operation in both deuterium and DT. Project goals might include demonstration of steady-state plasma operation and control in both conventional and advanced tokamak operating modes (similar to the goals of the recently canceled TPX project).

3) A less ambitious approach would be to defer advances in plasma confinement and control, and focus international collaboration on the construction of an International Fusion Neutron Source. Such a facility is estimated to have a cost of about $1B in as-spent dollars.

The present sentiment within the fusion community strongly favors pursuing the most ambitious of these alternatives -- a burning plasma experiment. Hence, a low-level U.S. domestic effort to search for less expensive means of studying burning plasmas would be useful insurance against the possibility that ITER is not constructed. Note that any such effort could not be part of the U.S.

contribution to the ITER EDA. Should that search prove successful and in the event the international partners decide to modify their objectives for a next step device, then the United States should explore with its former ITER partners (and other nations if this becomes appropriate) the possibility of international collaboration on a less expensive means of fulfilling the goal of DT ignition and burn. However, we must recognize that we cannot expect to take international leadership in a project for which we are not willing to accept an equal financial commitment with prospective partners. We must pay careful attention to the views of possible partners, and be prepared to accept their leadership in the event that they choose to pursue a different avenue to advance fusion science and technology.

Appendix H

Fusion Materials and Technology

Fusion science encompasses fundamental materials and technology development including low activation materials and fusion technologies essential to achieve the safety and environmental potential of fusion, and critical, enabling plasma technologies required to support advances in plasma physics. Historically, advances in fusion materials and technology have been a driver for progress in plasma physics. Fusion materials and technology have also made major scientific contributions through multi-discipline exploration of new phenomena in the unique fusion environment. The three major research areas are: 1) low activation materials; 2) fusion technologies; and 3) plasma technologies.

H.1 Low Activation Materials

It is widely recognized that achievement of the safety and environmental potential of fusion energy requires the successful development of low activation materials for components immediately surrounding the plasma. To attain high performance, these materials must satisfy complex operating requirements including temperature, stress, chemical environment and radiation exposure. Materials applications include structural materials, tritium breeding materials, electrical insulator materials, and plasma facing materials. Because of the radiation damage issues and the complex operating requirements, materials development is recognized as being a long-term endeavor. In addition, there exist only a limited number of materials which exhibit low activation characteristics along with a potential for high performance and long lifetime. Although materials development is usually presented as a separate issue because of the unique expertise required, it is important that materials development for fusion be closely coordinated with the respective component, e.g., blanket, divertor, etc. A key to the success is development of a compatible combination of materials for each component.

The emphasis in this section is on the structural materials application. The issues related to tritium breeding materials and plasma facing materials are included in the following sections on fusion technologies and plasma technologies. A 14 MeV neutron source for materials testing will eventually be required to fully qualify materials for fusion applications. The issues associated with this materials test facility are also summarized below.

Structural Materials

Development of high performance, low activation structural materials is recognized as one of the key issues in the development of fusion energy. In addition to the safety and environmental considerations, development of low activation structural materials is a priority since the critical issues are well defined, the issues are common to all plasma confinement concepts, and materials

development is a long term endeavor. The performance requirements for the structural materials are unprecedented. The operating requirements and materials issues involved are shown in Table 1 on page H-3..

The materials science associated with these requirements must be understood to determine performance and lifetime limitations of candidate materials with low activation characteristics. The structural materials effort is currently limited to only three candidate materials: ferritic steels, SiC/SiC composites, and vanadium alloys.

Ferritic Steels. The ferritic steels under consideration are a modified composition of a conventional iron-9%chromium steel. Molybdenum in the conventional steel has been replaced by tungsten and certain trace elements have been reduced to improve the low activation characteristics. Favorable characteristics relate to proven manufacturing and joining technology for the conventional steels, resistance to oxidation, and resistance to void swelling. Major issues include potential irradiation embrittlement at the lower temperatures of interest, limited high temperature creep strength, and effects of ferromagnetic properties. Development of a suitable electrically insulating coating will be required for application in a liquid metal blanket option.

SiC/SiC Composites. SiC/SiC composites represent a highly advanced structural ceramic material with unique low activation characteristics and high temperature properties particularly applicable to helium-cooled ceramic breeder blanket options. Favorable characteristics of SiC/SiC composites relate to favorable short- to medium-term low activation and high temperature mechanical properties. Major issues relate to the limited data base. Further development is required to determine its: suitability as a structural material including effects of neutron irradiation and high helium transmutation rates on swelling, thermal conductivity and mechanical properties; fabrication and joining technology; gas permeability due to porosity and micro cracking; compatibility with breeder materials; and hydrogen retention characteristics.

Vanadium Alloys. Vanadium alloys with 4-5% chromium and titanium represent an advanced alloy option with a limited but encouraging data base, particularly applicable to liquid-metal-cooled blanket options. Favorable characteristics associated with vanadium alloys include very low long-term radioactivity, excellent thermal stress capacity and creep strength, resistance to void swelling, and resistance to irradiation embrittlement. Major issues relate to sensitivity to oxidation by gaseous impurities and atmospheric exposure, development of stable insulating coatings for liquid metal coolants, lifetime under irradiation, fabrication and welding development, and compatibility with helium coolant and ceramic breeder materials.

Table 1. Operating Requirements and Materials Issues

Operating Requirements	Materials/Issues
Radiation damage including displacement damage and transmutations	Swelling, irradiation creep, and degradation of physical and mechanical properties
Chemical compatibility with coolants, tritium breeding materials and plasma facing materials	Corrosion, mass transfer, degradation of mechanical properties & hydrogen interactions
Elevated temperatures	Thermal creep and reduced mechanical strength
Mechanical stresses including primary, thermal, cyclic, and high strain rate (disruption) stresses	Mechanical properties including tensile, fatigue, and fracture toughness
Fabrication of complex structures	Fabricability and welding/joining
Plasma interactions	Thermomechanical response including plasma compatibility, vaporization/melting during disruptions, sputtering, tritium retention and ferromagnetism

Development of a fundamental understanding of the performance limits of these materials for the fusion conditions will require a major effort. A key issue in all cases relates to the effects of simultaneous helium transmutations and displacement damage produced by high energy (14 MeV) neutron exposure. Irradiation data are currently obtained from fission reactor irradiations which produce lower energy neutrons, and hence, much lower helium transmutation rates. Since a high flux 14 MeV neutron source for materials testing is not available, the effects of helium must be determined by artificial simulations. These techniques provide valuable information on the influence of helium transmutations on the materials responses such that important advances in the development of materials can be made; however, as discussed later, a 14 MeV neutron source will be required to fully qualify materials for a fusion power plant.

The current materials development program makes extensive use of international collaboration, particularly with respect to the irradiation facilities and sharing of developmental materials. The materials efforts in Europe, Japan and Russia are complementary with extensive sharing of results. This type of internationally coordinated program should continue in order to make maximum use of available resources.

Non-Structural Ceramics

Requirements for non-structural ceramics, particularly electrical insulators and diagnostic materials, are less well defined; however, it is important to develop a fundamental understanding of selected phenomena that will be important to various fusion applications. The relatively limited data available regarding radiation effects on non-structural ceramics gives cause for serious concern for their use in the fusion environment. At relatively low fluences some candidate insulators appear to degrade markedly in resistivity, and fused quartz normally used in RF windows and fiber optics becomes opaque.

The current program also makes extensive use of international collaboration to conduct difficult irradiation experiments which require in-situ electrical measurements. This type of collaboration should continue and close coordination with the ITER project should be maintained to guide in the development of priorities and requirements.

Conclusion: *Development of high performance, low activation materials is essential to achieve the safety and environmental potential of fusion energy. Because of the demanding operating requirements and radiation damage issues, materials development is recognized as a long-term endeavor. The low activation materials program should be continued with extensive international collaboration.*

Neutron Source for Materials Testing

Fission reactors provide a valuable means for simulating the atomic displacement of a fusion radiation environment; however, the 14-MeV neutrons produced by the fusion reaction cause significant transmutation reactions that do not occur in a fission reactor spectrum. Although fission reactor irradiations do not fully simulate the effects of a fusion spectrum, the results are valuable in developing an understanding of the effects of irradiation on the properties of materials. In addition, for some materials, additional simulations have been devised to mimic the effects of certain transmutations, particularly helium transmutations, which occur at a much higher rate in a fusion spectrum and are known in some cases to have a major effect on the properties of materials.

Previous reviews of the materials programs (Fusion Policy Advisory Committee Report of 1991 and FEAC Report of 1993) concluded that a 14-MeV neutron source for materials testing was a critical element of the program and that the need would shift from desirable to essential in about the same time frame as ITER construction. The 14-MeV neutron source was considered necessary for three reasons: 1) to confirm predictions of materials performance obtained from fission-reactor irradiations; 2) to complete development of advanced materials; and 3) to extend the engineering design databases on materials performance to the goals for fusion damage levels. These conclusions were based on the previous fusion program strategy for development of a fusion DEMO by about 2025. Implementation of this strategy included initiation of a conceptual design

study (CDA) of a 14-MeV neutron source to be carried out by international collaboration under the auspices of the International Energy Agency. It was agreed by the international community that an accelerator-based D-Li source was the most appropriate concept for this purpose. An interim conceptual design of this neutron source has just been completed as a joint effort of the European Community, Japan and the United States. A cost estimate of this device is currently being developed as part of the CDA and should be available in a few months. Since preliminary information suggests that the cost will be in the range of $0.5-1 B, it is difficult to see how the United States could contribute a significant share of the construction cost under the present budget. It is recommended that the U.S. materials program address this problem over the next year. This should be done with involvement of the international community. Possible resolutions include downsizing of the facility, participation as a limited financial partner, or seeking funding outside of the fusion program.

Conclusion: *A 14-MeV neutron source for materials testing, developed as part of the international fusion program, has been a major element of the U.S. program. Under the revised strategy with a constrained budget, it is recommended that the United States, jointly with its international partners, re-evaluate the priorities of this facility.*

H.2 Fusion Technologies

The safety, environmental attractiveness, and economic competitiveness of fusion energy will depend to a large extent on the blanket system since this is the largest component exposed to the high neutron fluence. The overall goal of the fusion technologies area is to develop a blanket system that meets the performance requirements of tritium self-sufficiency and efficient energy recovery, and at the same time achieves the desired level of safety and environmental attractiveness. Meeting this goal involves several possible design concepts, a number of candidate materials, and a variety of scientific disciplines. The development of blankets is at a relatively early stage so there is a considerable amount of research still required before key issues can be resolved and a suitable blanket system can be defined. The design concepts can be conveniently divided into blanket systems that use liquid metals as breeding materials and/or coolants and blanket systems that use lithium-bearing ceramics (solid breeders) as tritium breeding materials. The issues and required research for each of these categories, including tritium systems and safety-related research, are briefly described below.

Liquid Metal Blankets

There are two liquid metals, lithium and a PbLi alloy, that have been identified as having the greatest long term potential. Both liquid metals provide good tritium breeding capability, and both can also be used as coolants in self-cooled concepts. It is also possible to use a separate coolant, such as helium or water (with PbLi only), and the liquid metals then only need to be circulated at a low rate sufficient to remove the tritium bred during operation. The leading candidate designs for

pure Li as the breeder/coolant use a vanadium alloy (V-4Cr-4Ti) as the structural material. Designs for PbLi have used ferritic steel as the structural material. Within the United States, research on liquid metal blanket concepts has focused on the self-cooled concept with pure Li as the coolant/breeder and V-4Cr-4Ti as the structure. This system combines the features of a low activation system with high temperature operation (650-700 C) which translates into an efficient energy recovery system. The self-cooled concept operates at low coolant pressure (~1 Mpa) and has a relatively simple design layout that offers the potential for high reliability and long component lifetime.

There are several issues that need to be resolved in order to demonstrate the promise of the Li/V option:

Materials -- Self-cooled blankets require the use of an electrically insulating coating material on the inside of cooling channels to reduce the MHD pressure drop of flowing liquid metals in the high magnetic fields characteristic of fusion power systems. The coatings can be quite thin (1-10 mm) but must be have sufficient electrical resistivity to effectively limit the flow of currents in the structure. They must be thermodynamically stable in the liquid metal environment and should be self-repairing in case cracks or defects are formed during operation. The research items that are to be addressed are determination of the conditions and kinetics of the formation of the candidate coatings, chemical compatibility and corrosion in liquid lithium, and irradiation effects on the electrical resistivity and microstructure.

Magnetohydrodynamics (MHD) -- The flow behavior of liquid metals in high magnetic fields is dominated by MHD forces for which there is limited understanding for systems with insulated walls in fusion-relevant magnetic fields. Research needs include demonstration of the effectiveness of insulator coatings and fundamental studies of the flow patterns that develop in coolant channel geometries typical of the fusion blanket. The flow pattern studies are important to the understanding of heat transfer behavior in self-cooled blankets.

Safety -- Liquid Li is chemically reactive with air and water, and therefore special safety considerations are necessary to insure safe operation. Research is needed to determine the behavior of liquid Li during possible accidents. Information to be obtained includes chemical reaction rates in different possible environments.

Tritium -- Tritium is highly soluble in liquid Li, and the most challenging aspect of the tritium system is identifying a suitable means for separating the tritium from the Li. For PbLi, the primary challenge is to insure tritium containment. Development of a tritium barrier is a key issue for PbLi blankets with water as a coolant. Research is needed to determine the absolute tritium solubility as a function of temperature and to identify approaches that can be used to effectively remove the tritium so the inventory in the system can be maintained at acceptable levels.

The work that has been performed in the United States is complementary to liquid metal work in other countries. The European Community has emphasized the development of the PbLi option, both self-cooled and separately cooled. The United States has focused on the self-cooled Li concept and U.S./E.C. collaborative programs have been underway since 1987 in the areas of MHD research and insulator coating development. The Russian Federation has been investigating both the Li and PbLi options. U.S./R.F. collaborative programs on blanket technology have been underway since 1989.

Solid Breeder Blankets

There are several solid breeder materials that have been considered, including Li_2O, $LiAlO_2$, Li_4SiO_4, Li_2ZrO_3, and Li_2TiO_3. The desirable features are good thermal conductivity and neutron energy multiplication for efficient energy recovery, a net tritium breeding ratio greater than one, and rapid release of tritium at typical blanket operating temperatures. There are a number of chemical properties that affect tritium release including tritium bulk diffusivity, surface recombination and release, and chemical interaction with the purge gas. Design studies have indicated that an effective neutron multiplier is required to provide tritium self-sufficiency in the solid breeder blanket concepts. Beryllium is the primary candidate as a neutron multiplier to enhance the tritium breeding capability. All these materials should remain stable over an extended time in the fusion environment. The coolants being considered in solid breeder designs are helium and water, with helium being preferred for long-term applications. The structural material most often used in solid breeder designs is ferritic stainless steel; however, SiC/SiC composites provide an attractive structure option for He-cooled solid breeders concepts if development proves successful. In the United States, the leading solid breeder concept uses Li_2TiO_3 (similar to Li_2ZrO_3) as the breeding material, beryllium as the neutron multiplier, reduced activation ferritic stainless steel as the structure, and helium as the coolant. Li_2TiO_3 is believed to have excellent tritium release characteristics, appears to be quite stable during neutron irradiation, and it is a low-activation material; however, the data base is very limited. This blanket concept is similar to others being considered in the European Community and Japan. The ceramic breeder development program has involved extensive international collaboration, particularly with the European Community and Japan.

There are several issues that need to be resolved in order to demonstrate the promise of the solid breeder options:

Materials -- The behavior of the physical and mechanical properties of candidate solid breeders and beryllium, both before and after irradiation, needs to be better understood. In addition, research is needed on the chemical compatibility between the blanket materials and the structure at fusion relevant conditions.

Heat Transfer Characteristics -- Heat transfer across interfaces between solid materials is crucial to developing a blanket that provides efficient energy recovery and that maintains the material temperatures in the optimum operating range. The unique materials and complex geometries of the fusion blanket result in a limited understanding of interfacial heat transfer. Research is needed to determine heat transfer across interfaces as a function of material morphology, surface characteristics, and interfacial stress. The effects of irradiation on these properties must also be determined.

Safety -- Beryllium at elevated temperatures is chemically reactive with air and water, and therefore special safety considerations are necessary to insure safe operation. Research is needed to determine the behavior of beryllium during possible accidents. Information to be obtained includes chemical reaction rates in different possible environments.

Tritium -- In contrast to liquid metals, with solid breeders no coolant is circulated during operation so the tritium bred during operation must be released in the blanket itself. Beryllium will also produce tritium albeit at much lower rates than in the solid breeders. Tritium release in beryllium is an important issue; over a period of time this tritium can add substantially to the blanket tritium inventory. Over the years, there have been several reactor experiments that have explored tritium release in solid breeder materials at normal operating temperatures. Additional research is needed to explore the tritium release characteristics at the limits of temperature operation to establish the operating windows for the leading candidates after high fluence irradiations. Tritium release characteristics of beryllium are less well understood than in solid breeders, and thus research on beryllium is also required.

The work that has been performed in the United States is complementary to solid breeder blanket work in other countries. The European Community has emphasized the development of the $LiAlO_2$ and Li_4SiO_4 options, but most recently has been investigating Li_2ZrO_3. U.S./E.C. collaborative programs have been underway since 1986 in the areas of solid breeder characterization and irradiation response. Japan has been investigating the Li_2O option, but recently has begun to investigate the ternary ceramics. U.S./Japan collaborative programs have been underway since 1986.

Conclusion: *The safety and environmental attractiveness of fusion will depend to a large extent on the blanket system since this is the largest component exposed to a high neutron fluence. Development of a blanket system that meets performance requirements of tritium self-sufficiency and efficient energy recovery, while meeting the safety and environmental goals remains a critical issue in the development of fusion energy.*

Tritium Systems

Tritium systems development is critical to the development of fusion. Tritium systems must be developed for processing tritium in the fuel cycle as well as for processing tritium from the blanket system. The tritium systems for the fueling system dominate the tritium systems. The United States has provided a lead role for several years in the development of tritium processing systems. The Tritium Systems Test Assembly (TSTA) in the United States is a world class facility for tritium processing. This technology has been transferred to the TFTR facility for the current DT test program. Extensive international collaborations with the Japanese have been conducted with the TSTA. Development of tritium systems for fusion applications should be continued. Future work should include further development of the fundamental aspects of blanket processing systems since this area has received only limited attention.

Safety-Related Research

Safety-related research is also an important part of the fusion technology program. Since a key aspect of the development of low-activation materials and technology for the U.S. fusion program relates to the safety goals of fusion energy, it is important to maintain this element of the program to guide and evaluate the safety benefits. The United States has provided leadership in the international community in this area, including contributions to the ITER project. It is important that the safety programs be closely coordinated with the materials and technology programs to provide maximum benefit. Continued involvement with the international safety effort is important to development of guidelines and rules for application to fusion systems.

H.3 Plasma Technologies

Plasma technologies are defined as those enabling technologies that have impact on plasma containment (magnetics), plasma control (heating and fueling), and plasma output (power and particle handling). The application of new plasma technologies has been essential to the advancements in fusion. In the 1960s hot plasmas were of short duration with many impurities, and the state of the vacuum vessel walls was not characterized. During the 1970s auxiliary heating systems provided controlled multi-kilovolt plasmas. More recently, high field copper and superconducting magnets, improvements to fueling, proper choice of plasma facing materials, and wall conditioning techniques have led to significantly higher temperatures and longer pulse lengths in tokamaks and other configurations.

ITER is presently the primary vehicle for plasma technology development in the U.S. program. The United States has the lead in two of the seven major ITER deliverables: the central solenoid model coil and the divertor cassette prototype. It also plays an important role in other ITER plasma technologies. However, any current or next generation device benefits from the U.S. plasma technology R&D. The basic U.S. concept of the "cabled" niobium-tin superconductor is embodied

in the design of superconducting fusion devices. Heating and fueling systems are required for any magnetic confinement concept. In Electron Cyclotron Heating (ECH), the United States holds the world record for pulsed gyrotron energy output and CW operation. Our Ion Cyclotron Heating (ICH) antennas are found on numerous fusion devices around the world. The United States is also preeminent in pellet fueling technology. Finally, plasma-material interactions are intrinsic to all magnetic fusion concepts. The United States is a leader in actively cooled plasma facing component development.

Superconducting Magnet Technology

The primary goal of the magnet program is the development of niobium-tin superconductors and radiation-resistant insulation systems for reliable operation in magnetic confinement device environments. The purpose of the program is to provide one of the enabling technologies for future devices. Superconducting magnets are a requirement for long pulse and steady state future devices.

The major development issues have been to establish high confidence in the long term reliability of components, and to assure stable conductor operation under pulsed field and disruption conditions. The United States has a special long-term expertise to contribute in these areas. For example, the conductor and fabrication concepts adopted by ITER were originated under pre-ITER U.S. base program activities.

The current application of the U.S. generated concepts is far broader than ITER. For example, the basic concept of “cabled” niobium-tin conductors is embodied in the design of all current superconducting tokamaks -- TPX, JT-60 Super-Upgrade, and the Korean STARX. With retention of a strong magnet technology infrastructure, and good-faith participation in the EDA, the United States would be welcome in any such future international device.

Maintaining a base program is required for long term continuity. Projects generally contribute to maturity of a technology, but projects are rarely organized to incubate or sustain development. Developments generally arise and are optimized in “base programs” -- projects then carry only the last stage development and/or scale-up, and drop out as soon as they have what they need.

The laboratories and universities provide continuity of knowledge, innovation, and characterization of superconducting strand, structural materials, helium technology, and conductor behavior. Many of the more than 100 university graduates trained over the years in these areas supported by the OFE magnet programs have found jobs in the industrial superconducting infrastructure.

Superconducting strand fabrication requires a continuity of industrial production. The Japanese have been very successful in their industrial production because they maintained a continuity through their base program. For the United States to remain competitive, we believe we must retain

a continuity of industrial wire fabrication, and we must understand and optimize that production by basic work in the university/laboratory infrastructure.

Plasma Facing Component Technology

Plasma facing component technology is a fundamental enabling element of any fusion science program. Plasma materials interaction issues are present in all magnetic fusion devices. The interaction between the plasma and the material which faces it is the single most significant interaction that limits both the advancement of fusion science and the development of the fusion energy option. Any probable improvements in fusion concepts result in smaller, steady state devices which will stress plasma facing materials technology beyond its present capacity to withstand heat loads on the order of 5 MW/m^2. Utilization of any future improvements in plasma performance is also inextricably linked to improvements in plasma facing materials.

The goal of plasma facing component (PFC) technology is the development of reliable, steady-state components that can handle the heat and particle emission from the plasma without contaminating the plasma. The technology issues include: enhanced heat removal technology; development of plasma facing materials (PFMs) and joining technologies for attaching PFMs to heat sink materials; assessment of thermal and mechanical fatigue; development of in-situ repair techniques; development of non-destructive examination techniques for assuring reliability of components; establishment of a database; and models for plasma-material interactions.

Plasma facing component development requires the use of specialized facilities capable of addressing the issues. The minimum, most cost-effective set of test facilities required for development of reliable, long-lived PFCs consists of high power density tokamaks, small linear plasma simulation devices, ion/electron beam high heat flux test stands, pulsed, high power density plasma guns for simulation of disruption thermal loads, and fission reactor for neutron irradiation damage studies. The United States has world-class facilities in each of these areas: DIII-D (DiMES), PISCES and TPE, EB-1200, PLADIS, and HFIR. All of the U.S. tokamaks and future device designs (including the TPX design) and several international machines have used these facilities to develop their plasma facing components. Plasma facing component R&D has led to improvements in low Z materials, understanding of erosion, wall conditioning techniques, hydrogen effects in materials, and enhanced heat removal (all crucial to plasma performance).

The United States is the world leader in plasma facing component development. It is leading the international effort to develop ITER divertor components. The U.S. plasma facing component technology program has collaborated on the design and fabrication of components for TFTR [Carbon-Fiber-Composite (CFC) materials], DIII-D (CFC materials), Tore Supra (steady-state heat-removal components), TEXTOR (pumped limiters), JET (beryllium plasma facing material),

JT-60U (CFC materials), and LHD (stellarator PFCs). The components (inner-wall limiter, pump limiter, and ergodic divertor dump plates) on Tore Supra are the only steady-state components in any operating tokamak. They were designed for 2 MW of heat removal and a peak heat flux of 15 MW/m^2 based on experience gained from short pulse devices. The U.S. PFC community is involved in tokamak edge plasma diagnostics, analysis of samples from tokamaks for surface contamination, tritium/deuterium retention, failure modes studies, and erosion measurements.

Possibilities for continuing the technology development beyond the work in progress include development of reduced activation materials and novel concepts for heat removal. Concept improvement and advanced concepts will likely lead to higher heat fluxes that are more challenging technically. This implies a need for advanced plasma facing materials (liquid surfaces, infiltrated surfaces) to improve component lifetime. Safety concerns about water cooling imply the need for developing alternate coolants (liquid metals, helium gas). Erosion and tritium studies are needed for advanced materials to address safety issues. Methods for rapid in-situ repair will increase the useful lifetime of plasma facing components.

Ion Cyclotron Heating (ICH) Technology

The goal of the ICH technology program is the development of reliable, high-power ($\geq$ 50 MW), steady-state plasma heating and current drive systems. These systems can be used for:

- efficient and cost-effective core heating of plasma ions (or electrons);
- current drive to enable long-pulse or steady-state tokamak operation;
- current profile control to access and sustain advanced operating modes (e.g., reversed-shear).

They are currently being used for these applications on a number of U.S. and international machines.

ICH is a significant part of the R&D programs on major confinement experiments both here (DIII-D, TFTR, and C-MOD) and abroad. Support from the ICH base technology program enables U. S. RF scientists and engineers to have a unique role in the world program, in which knowledge gained from one experiment can easily be applied to another. Examples of such synergism are:

- Fast tuning and matching R&D -- we are collaborating on the design, analysis, and testing of the JET and DIII-D RF control systems, as well as working on the design of the ITER tuning and matching system.

- Long-pulse antennas –- we have furnished a new water-cooled long-pulse RF antenna (which has demonstrated excellent characteristics in initial testing there) to Tore Supra, and are collaborating with the Tore Supra staff on RF experiments. Experience with such antennas influences the design and fabrication of antennas for other machines (e.g., DIII-D), and is also being used in our design studies for the ITER antenna.

- Advanced technology –- we are supplying advanced components to international experiments such as ASDEX-U, CHS, W7AS, and TEXTOR, and collaborating with their researchers to evaluate the components' performance.

Knowledge gained from fusion ICH R&D is also being applied to non-fusion applications of RF-generated or heated plasmas, such as plasma processing of semiconductors and other materials

There are several key development issues for ICH. Fast tuning and matching of multiple-strap, high-power antennas in the presence of changes in the plasma properties (e.g., from ELMs or plasma motion) is a high priority for both present-day and future applications, especially ITER. R&D in this area is well underway on DIII-D, JET, and other machines. The development and testing of long-pulse or steady-state advanced antennas (e.g., the folded waveguide) with much higher RF power capabilities (>20 MW/m^2; reliable operation at 10 MW/m^2) will make ICH much more attractive due to the reduced number of ports required to deliver a given amount of power.

The United States has particular expertise in antenna design, and in calculation of the interaction of the large RF fields from the antenna with the core and edge plasma regions. In addition, we are leaders in the technology of antenna and RF component fabrication, and have developed advanced methods of antenna construction. The United States can take a major role in the design and construction of the ITER ICH system. Expertise exists in laboratories and industry to supply a cost-effective, reliable system to meet ITER requirements. ICH is likely to be a necessary component to advanced-mode plasma operation. Continuation of existing programs will keep the United States in the forefront of both the ICH and advanced-physics plasma operation modes. Future international collaborative programs with LHD, W7X, and STARX are also possibilities. Based on past experience, our collaboration with these programs would be welcomed and would allow cost-effective testing of U. S.-developed RF expertise on major fusion devices.

Electron Cyclotron Heating (ECH) Technology

ECH has many advantages for plasma heating and current drive applications. For heating, advantages of ECH include the well-known physics of the coupling of the wave to the plasma and the known location of the power deposition within the plasma. ECH can also be used for current

drive and is especially useful for off-axis current drive. Such off-axis current drive is required in advanced tokamak scenarios. This would be an important application in the U.S. domestic fusion program in the future. ECH can be used for plasma stabilization and profile control, negative shear, etc. It is often used for heating stellarators, a major alternate concept. ECH has been selected as one of the main auxiliary heating methods for ITER. It will also be used for start-up and current drive.

ECH is in use at the DIII-D Tokamak at GA, T10 and T15 in Russia, Tore Supra in France, TCV in Switzerland, ASDEX-U in Germany, and JFT2M in Japan. It is in use on all major stellarators and helical devices such as W7AS in Germany, and LHD, CHS and Heliotron E in Japan. ECH is under development for ITER. The United States exports gyrotrons and transmission line components to Europe and Japan for use in these experiments.

The goal of the ECH technology development program is the development of 1 MW output power, continuous wave (CW) gyrotrons and the related systems components such as transmission lines and windows. Gyrotrons are needed at frequencies of 110 GHz for heating present day plasma devices, particularly the DIII-D tokamak at General Atomics, and at 170 GHz for ITER. ECH is a critical enabling technology for ITER, advanced tokamaks and alternate concepts such as stellarators. Maintaining a US capability, including industrial capability, in this enabling technology is extremely important and valuable.

The main development issue is demonstrating true CW operation at the 1 Megawatt power level, along with a transmission line and tokamak window. A second major issue is continuity of funding of industry. The US has a world renowned consortium of industry (CPI, formerly Varian), national labs, universities and small businesses in the ECH technology program. CPI has the only experience in the world with high average power / CW operation of a gyrotron. A CW power level of 300 kW has been achieved at 28 GHz and 200 kW at 140 GHz. Prototype development of a 110 GHz gyrotron has achieved over 100 kW of true CW operation and pulsed operation at 0.35 MW for 10 s, a record for gyrotron pulsed output energy. Extension of these results to the 1 MW, CW power level is very promising. The ITER prototype gyrotron under development at MIT has recently achieved 1 MW operation at 170 GHz in short pulse operation. This work validates the design and allows industrial development to go forward when funded.

ECH systems will be needed for ITER, DIII-D, Alcator C-Mod and other fusion devices throughout the world. Industrial participation is necessary for technology development and pays off for the United States in the long run since it allows sales abroad. Gyrotrons and transmission lines are possible components that the United States could contribute as a junior partner in ITER. This is especially true if we have the best product, which is the case at the present time. Without industrial participation, the United States could still contribute in the area of basic research. This is

very unattractive but it would allow us to maintain our expertise in this important technological area and develop applications in the future.

Plasma Fueling Technology

Plasma fueling based on pellet injection is being pursued for a number of purposes:

- efficient fueling -- minimize tritium inventory and plasma exhaust throughput;
- plasma density profile control for improved reactivity and advanced confinement regimes (PEP-mode, shear reversal, etc.);
- rapid plasma termination for disruption mitigation - "killer pellets."

In the United States, pellet fueling R&D programs exist on DIII-D, TFTR, and C-MOD. In addition, the U.S. pellet injector development program plays an integrating role in the United States and world fusion programs by:

- collaborating on advanced pellet injector development with European partners;
- furnishing an improved centrifuge type fueling system for Tore Supra, and collaborating with the CEA on long pulse pellet fueling experiments;
- being the lead contributor to the design of the ITER fueling system.

The technologies developed in the U. S. fusion program have also been applied in areas outside of fusion research. These include the development of cryogenic pellet-based surface cleaning techniques and high Z pellet sources for advanced x-ray lithography systems. A viable base development program is essential for developing future spin-offs and to maintain our leadership role in fusion fueling research.

The long range objective is to develop reliable, steady state, tritium compatible plasma fueling systems to refuel fusion plasmas and control the fusion burn. An intermediate objective is to develop and supply pellet injection systems for use on magnetic confinement devices in the United States and abroad.

Issues which are currently being addressed for both long term and near term objectives are:

- the development of long pulse or steady state cryogenic pellet feed systems;
- tritium pellet fabrication and acceleration;
- reliable centrifuge and pneumatic drivers;
- higher pellet speeds (up to 4-5 km/s) for more demanding long term applications; and
- development of high Z pellet operating scenarios for disruption control studies.

The United States has the preeminent position in this field and is responsible for all of the R&D needs associated with ITER. Specific development expertise includes centrifuge and conventional

pneumatic injectors, two-stage light gas guns and MHD railguns for higher performance, and tritium applications (unique). In addition, the U.S. program has a long history of applying fueling systems and conducting research on plasma confinement devices in the United States and abroad (enabling technologies).

Because of its unique position in this field, the United States can take the lead role in the design and construction of the ITER pellet fueling system. Expertise exists at U.S. laboratories and in industry to supply a cost-effective, reliable system to meet projected ITER needs. Potential future applications for this technology also include LHD (in Japan), and TEXTOR (in Germany). U.S. collaboration on these programs is welcomed and would allow access to unique fusion facilities not planned in the domestic program .

Conclusion: *Plasma technology development is crucial to insure optimal advancement of plasma science and to establish fusion's feasibility as a viable energy source. The United States has developed unique expertise in these enabling technologies which can contribute to both domestic and international programs. Industry must continue to play a valuable role in plasma technologies.*